IPCC Fourth Assessment Report

Expert Review of the First-Order Draft

Chapter 3

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-1	A	0	0			I have four general observations. 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)	Accept, will broaden scenarios range, use less defensive language and include IMCP, look into including MARKAL scenarios; email colleagues re additional scenarios to include in database- put out notice on NIES website. Need to mine database for important massages for use in ch3 rather than just statistical use. Specific response requires specific comment. Will review database in more detail and will deal with specific comments.
3-2	A	0	0			In sections related to CCS, references to the SRCCS are missing, extensive material related to scenarios included in the SRCCS are neglected and focus lies instead on a narrow band of publications (often forthcoming) material. The ideas concerning negative emissions are vaguely expressed. Literature is quite limited (mostly forthcoming publications) in this area. If we could really attain net-negative emissions globally in order to attain very low stabilisation targets, it would be a great step forward, so great, in fact, that we need to exercise the greatest care to speak very precisely. Eg. when and where could it happen? With regard to terminology there are inconsistencies within the chapter concerning the use of the terms 'storage' and 'sequestration' related to CCS. Note that the SRCCS uses 'storage' in the context of CCS (rather than sequestration). (Kenneth Möllersten, Swedish Energy Agency)	Missing and SRCCS will be referenced (Riahi). Will talk about CCS role in mitigation context more adequately in later sections.
3-3	A	0	0			Chapter 3 appears to have taken the approach of reviewing changes to the literature since the publication of SRES. This is a good approach. However, it does not adequately reflect recent developments in the economics of long term scenarios. In particular, the treatment of technical change is poor and does not take into account	Morita looks at post SRES tech analysis across models, including CCS, to bring emissions down. Publication has been accepted and will consider material and incl

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						the useful material and recent literature that has been presented in chapters 2 and 11 of the WGIII first order draft, on technology and technical change. Also, there is no discussion of mitigation actions and policies discussed in chapter 13 of WGIII first order draft and their implications for long term scenarios. (Jonathan Köhler, Tyndall Centre, University of Cambridge)	material from ch11. Last part of comment belongs in ch 11 and 13.
3-4	A	0	0	0	0	According to the World Energy Outlook (IEA 2005), more efficient vehicles would cost all consumers about 1100 billion dollars over the next 30 years, but would bring double benefit, in reducing oil consumption but also in reducing oil prices. The gross savings is the difference between the cost of 935 billion barrels at 39$ each in the reference scenario and the cost of 883 billion barrels at 33$ each in the alternative policy scenario, ie $7326 billion. Taking into account 1100 billion investment on the demand side, there remains a net benefit of more than 6 trillion dollars. This information should probably find its way in this chapter, although it may also be relevant for others such as chapters 5, 11 (in particular 11.7.5) and 12. (Cédric Philibert, International Energy Agency)	Accepted, 2005 WEO will be reflected in the chapter. 2006 WEO will not be available for review in time.(?) The specifics of this comment are relevant to the transport chapter.
3-5	A	0	0	0	0	You refer to the IMCP but do not include the reference to the synthesis paper by Edenhofer et al (2006). I think this paper can add something to the section on technological development in relation to scenarios (Monique Hoogwijk, Ecofys)	Accept
3-6	A	0	0	0	0	To make the handshake between chapter 3 and chapters 4 - 10 I think chapter 3 should focus more on the results regarding the different types of mitigation options, what energy mix is used. I am aware that this differs largely among scenarios and models, but some highlights and robuust conclusions can be drawn. I am thinking of biomass energy (mostly high share, e.g. work from MESSAGE, IMCP, IMAGE, Azar), which often has a large share, CCS which is of high importance for low-level stabilisation scenarios, the impact of BECS, the mitigation options in different sectors. These type of conclusions could be related to what is conclused in Ch 4 - 10. For bioenergy, Chapter 4 does some statements (Ch 4 p 43 line 23) (Monique Hoogwijk, Ecofys)	Consider adding new material, will consider structure of energy system on mitigation. Refer to comments for section 3.4. Will look at new model runs in greater detail and highlight some as per marker scenarios. Text on analysis of baseline effect on mitigation cost will be considered for addition. (IIASA, MIT and RIVM analyses). (Van Vuuren, Riahi, Warren)
3-7	A	0	0	0	0	Regarding biomass energy, I think a reference to the submitted paper from Smith, sims, Schlamadinger, Carl might be useful. (Monique Hoogwijk, Ecofys)	Agree (Rose)
3-8	A	0	0			Overall, the chapter is substantially improved from the ZOD (Tom Kram, MNP)	agree
3-9	A	0	0			In the overall discussion on emission scenarios it might be appropriate to include three sections dealing with (1) importance of updating scenarios especially in the	Add text to 3.6 in discussion on SR/LR. Request Fatih Birol provide text as CA on

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						context of the current energy markets, (2) importance of linking shoter term scenario (2030) to longer term one (2100): Shorter term scenario such as IEA's provide a very detailed insights on policy and technology available in the energy sector and likely evolution. The richness of policy and technology characterisation enables to give policy makers and other stakeholders precise estimate of impacts and policy measures, and (3) importance of the energy sector, which is reflected in other chapters but not adequately reflected in these overview. (Fatih Birol, International Energy Agency)	ch3.
3-10	A	0	0	0	0	I'm confused. The title of this chapter is Isues related to long-term mitigation yet mitigation is hardly mentioned in the executive summary. The major first part of the chapter deals with an update on SRES scenarios. As the authors point out, there has been a special report on this subject so unless this is a chapter specifically on scenarios then it would seem that scenarios are perhaps being given too much weight in this chapter. Certainly any discussion of scenarios should center on 1) where the new findings agree with the TAR and special report and what is new. Still, given the title of the chapter and the broad range of expertise of authors in the chapter I would hope for a more balanced coverage of mitigation. (Jeff Price, California State University, Chico)	Will summarise 3.1 and 3.2 and change balance of 3.1, 3.2 and 3.3. Bring mitigation issues closer to front and cut down material on baselines. (Nakicenovic, Delachesnaye and Van Vuuren –swap section leads for editing)
3-11	A	0	0	0	0	I think I have missed the publication of Schaeffer et al, on the albedo with regard to sinks and energy crops. I think that the issues regarding albedo should be in Chapter 3, I made the same remark to Chapter 9, maybe more there, but some consistency (Monique Hoogwijk, Ecofys)	Accepted – should be discussed in both ch3 and ch9 (maybe ch8) with more detailed discussion appropriate for ch9. Will coordinate this with other chapters. (Rose)
3-12	A	0	0	0	0	I really like the summarising conclusion at the end of sections, but I do think the executive summary can benefit more from these concluding sections (Monique Hoogwijk, Ecofys)	agree
3-13	A	0	0			Discussion on why it is important to prepare a short term scenario for a long term scenario is missing. (Fatih Birol, International Energy Agency)	agree
3-14	A	0	0	0	0	no nuclear energy pathway. Nowadays it seems to be all biomass and co2 sequestration in order to reach stringent mitigation targets. (Peter Kolp, IIASA)	Technology details not incorporated in ch3 but will examine scenario database to draw out information on nuclear potential.
3-15	A	0	0			In TAR annual-mean radiative forcing is estimated to be -0.4 Wm2 for anthropogenic sulphate, almost the same order of magnitude than anthropogenic methane mean radiative forcing. As they say, uncertainties remain relatively large and these arise from difficulties in determining the concentration and radiative	Point of relevance true. Ancilliary cost of SO2 reduction – mention briefly in ch3 and cross refer comment to WG1. (Van Vuuren to footnote)

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						characteristics of atmospheric aerosols and the fraction of the aerosols that are of anthropogenic origin. It may be convenient discuss the implications for mitigation of this uncertainty. (Jorge Gasca, Mexican Petroleum Institute)	
3-16	A	0	0	0	0	no nuclear energy pathway. Nowadays it seems to be all biomass and co2 sequestration in order to reach stringent mitigation targets. (Peter Kolp, IIASA)	See previous response
3-17	A	0	0			Transitions: A low –carbon energy system can be achieved through carbon sequestering and a shift to low-carbon fuels: nuclear and renewables. In economic models the changing costs and benefits of various options are estimated, the result of which may be used for calculating optimal paths. Accepting uncertainties, long-term goals may be formulated on the basis but this is not how it works in politics where goals get formulated on the basis of the state of development of solutions (acceptable costs for certain gains). Through its innovation policy governments support the development of new technologies and systems low in GHG emissions. This means that governments are open to radical innovation and even system innovations (transitions). Such policies are partly undertaken to create new business or, in the case of energy, to become more self-sufficient. In this regard, governments are even involved in a strategic game with the US championing fuel cells and hydrogen and Japan and Germany championing PV and wind power. Through their innovation policies and environmental policies government give simultaneous support to options of system improvement and system innovation. Support for system innovation could be undertaken in a more systematic, comprehensive manner, using visions and programmes for system innovation. This is what the Dutch government is doing through transition management at the level of five different ministries (Rotmans et al,. 2001; Kemp and Loorbach, 2004; Rotmans, 2003; Rotmans, 2005). Transition management consists of a deliberate attempt to work towards a transition offering sustainability benefits, not just environmental benefits but also economic and social benefits. The basic steering philosophy is that of modulation, not dictatorship or planning-and-control. Transition management joins in with ongoing dynamics and builds on bottom-up initiatives. Ongoing developments are exploited strategically. Transition management for sustainability tries to orient societal dynamics to participatory defined sustainability goals for functional systems (energy, transport, agriculture). The goals and policies to further the goals are not set into stone but constantly assessed and periodically adjusted in development rounds. Existing and possible	Transition strategies text to be improved in 3.6. (Hourcade +Nakicenovic) To examine literature and incorporate any relevant ideas as per comments.

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						policy actions are evaluated against two criteria: first, the immediate contribution to official policy goals (for example in terms of kilotons of CO2 reduction and reduced vulnerability through climate change adaptation measures), and second, the contribution of the policies to the overall transition process. Learning, maintaining variety and institutional change are important policy aims and policy goals are used as means (Kemp and Rotmans, 2004). (Rene and Jan Kemp and Rothman , Erasmus University)	
3-18	A	0	0			Transitions The notion and concept of transitions is not used at all in the text, it is only mentioned a few times but in a rather loose manner. This is a missed chance, because the concept of transition or system innovation (a radical, but incremental, long-term shift from one dynamic equilibrium to another one) can be applied to explore the shift from a fossil fuel based energy system to a low-carbon energy system in a more systematic manner. Further, the concept of transition management can be used to investigate the relations between mitigation and adaptation strategy, i.e. to demonstrate that mitigation and adaptation are highly interlinked and that they can be considered as two parallel but connected tracks. (Rene and Jan Kemp and Rothman , Erasmus University)	To examine literature and incorporate any relevant ideas as per comments.
3-19	A	0	0			Transition Management References Kemp, René, and Jan Rotmans (2004) Managing the Transition to Sustainable Mobility, in Boelie Elzen, Frank Geels and Ken Green (eds.) System Innovation and the Transition to Sustainability: Theory, Evidence and Policy, Edgar Elgar, Cheltenham, 137-167. Kemp, René, Saeed Parto and Robert B. Gibson (2004), Governance for Sustainable Development: Moving from theory to practice, International Journal of Sustainable Development, Vol 8 (Nos 1/2): 13-30. Kemp, R., and J. Rotmans (2005) 'The management of the co-evolution of technical, environmental and social systems', M. Weber and J. Hemmelskamp (eds.) Towards Environmental Innovation Systems, Springer Verlag, Heidelberg/New York, 33-55. References: Kemp, R., and D. Loorbach (2005) ' Dutch Policies to Manage the transition to Sustainable Energy', in Jahrbuch Ökologische Ökonomik 4 Innovationen und Nachhaltigkeit, MetropolisVerlag, Marburg, 123-150. Kemp, R., D. Loorbach and J. Rotmans (2005), Transition management as a model for managing processes of co-evolution, paper for special issue on (co)-	To examine literature and incorporate any relevant ideas as per comments.

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						evolutionary approach to sustainable development of The International Journal of Sustainable Development and World Ecology. Kemp, R., Loorbach, D., Rotmans, J. (2005). 'Transition management as a model for managing processes of co-evolution towards sustainable development.' The International Journal of Sustainable Development and World Ecology (Special Issue on Co-evolution), in press. References: Loorbach, D. and J. Rotmans (2006). Managing transitions for sustainable development. In Olsthoorn, X. and Wieczorek, A.J. (eds.), Understanding Industrial Transformation. Views from different disciplines., Dordrecht, Springer. Martens, W.J.M. and Rotmans, J. (2005), 'Transitions in a Globalising World', Futures 37, 1133-1144. Robalino, David A., and Robert J. Lempert (2000), 'Carrots and sticks for new technology: Abating greenhouse gas emissions in a heterogeneous and uncertain world', Integrated Assessment 11-19. Rotmans, Jan, René Kemp, and Marjolein van Asselt (2001) 'More Evolution than Revolution. Transition Management in Public Policy', Foresight 3(1): 15-31. Rotmans, Jan (2005) 'Societal Innovation: between dream and reality lies complexity', Shortened inaugural speech, Rotterdam School of Management, ERIM, Erasmus University Rotterdam. Rotmans, J. (2003), 'Transitiemanagement: sleutel naar een duurzame samenleving', van Gorcum Uitgeverij, Assen. Rotmans, J., D. Loorbach and R. van der Brugge (2005). "Transitiemanagement en duurzame ontwikkeling: Co-evolutionaire sturing in het licht van complexiteit." Beleidswetenschap 2, vol. 19, 3-23. Van Asselt, M.B.A., Rotmans, J. and Rothman, D.S. (2005), 'Scenario Innovation: Experiences from a European Experimental Garden', Taylor & Francis, U.K. Van der Brugge, R., J. Rotmans and D. Loorbach (2005). "The transition in Dutch water management." Regional Environmental Change Volume 5 (1). Van Notten, P. (2005), 'Writing on the wall: scenario development in times of discontinuity', PhD-dissertation, Thela Thesis, Amsterdam, The Netherlands. (Rene and Jan Kemp and Rothman , Erasmus University)	
3-20	A	0	0			Transition management aims for generating "momentum" for sustainability transitions. Not all companies will of course contribute to a transition, but once a new development takes shape, more and more companies will follow suit, including companies invested in the old system. This is already happening in the area of	To examine literature and incorporate any relevant ideas as per comments.

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						energy where oil companies are moving into the business of renewables. When this happens the transition process becomes a force of its own. This is a critical phase in a transition in which also unwanted path dependencies occur. Society has to develop antennas (via 'assessment tools') for systemic effects. Transition management requires continuous anticipation and adaptation. An important task for research is to analyse the co-evolution of policy and technical change in GHG mitigation as part of wider transition processes. This can be done through agent-based models in which actor behaviour is specified ex ante (as in Robalino and Lempert, 2000) or though narrative studies. By making more realistic assumptions about policy such studies may become an input to government policy, leading governments to pursue transition policies for functional systems and systems of governance in a more systematic manner. Such analyses would draw on political economy and political science and complex adaptive systems theory. Conclusively, transition management seems to be a promising approach that could be used within the context of the IPCC. It enables the coupling of 'soft', qualitative long-term goals with 'hard', quantitative objectives. In the Netherlands it has been used quite successfully, in particular with regard to the Dutch energy transition. Practically, this means that a short-term energy policy and a long-term energy policy are combined. The short-term energy policy is rather pragmatic and operational, whereas the long-term energy policy is strategic and aimed at achieving a sustainable energy system. Sustainable then involves three dimensions: economic (efficient and competitive), ecological (50% less CO2-emissions by 2050 and social (dependable, reliable and guaranteed supplies). This long-term sustainability policy is also denoted as a 'shadow-line' that takes place largely outside the realm of the political arena, but still is legitimized and authorized by the political arena. Within the IPCC this distinction could also be used: a short-term, pragmatic policy aimed at achieving Kyoto-goals while focusing on (regional and local) adaptation, and a long-term, strategic policy aimed at substantial mitigation (in orders of magnitude 50-80% reduction of CO2-emissions), which we call a transition policy. (Rene and Jan Kemp and Rothman , Erasmus University)	
3-21	A	0	0			The scenarios used are limited to the energyside. WGII will also be including scenarios for impacts. Specifically, there will be the inclusion of non-linear scenarios for impacts (see Burkett et al. 2005). Virginia R. Burkett, Douglas A. Wilcox, Robert Stottlemyer, Wylie Barrowa, Dan Fagre, Jill Baron, Jeff Price, Jennifer L. Nielsen, Craig D. Allen, David L.	Summarise WG2 ch19information and make link in 3.5. (update table 3.9). (Warren)

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						Peterson, Greg Ruggerone, Thomas Doyle. Nonlinear dynamics in ecosystem response to climatic change: Case studies and policy implications. Ecological Complexity (in press). Note, this is distinct from non-linear scenarios in the climate system (eg, shutdown of THC, massive methane release). Examples are 1. collapse of coral reefs, 2. sudden, significant SLR and loss of coastal wetlands. Such scenarios need to be discussed in WGII for continuity in the IPCC report and for inclusion of ever more probable futures. (Paul Epstein, Harvard Medical School)	
3-22	A	0	0			The field of scenario development is broad and diverse. What strikes us is that only a small part of this broadness and diversity is covered by this IPCC-chapter. This is not only disappointing but also self-referential. So in our view both the scenario context and scope within the IPCC needs to be broadened. It should go much beyond sketching the dichotomy between narrative story lines and quantitative model analyses, which is outmoded. Van Notten (2005) for instance, has developed a typology of scenarios, based on three dimensions: the goal, the process design and the scenario content. He analysed 30 international scenario studies (including the IPCC-SRES scenarios) and categorized them according to this scenario typology, and visualized them by using a 'scenario cartwheel'. This is very useful material for the IPCC-Assessment, and exemplary for quite a few useful scenario references that are missed, of which you will find many in the book of van Notten (2005). That also holds for the recently published book on scenario innovation by van Asselt, Rotmans and Rothman on new scenario methods and new types of scenarios for a sustainable Europe, which is not even mentioned in the highly incomplete list of scenario literature, while this represents quite innovative scenario material, both in methodological and content terms. This incompleteness also holds for the historical retrospective of scenarios that is one-sided and gives a misleading picture of the past and current scenario field. The whole field of sustainability assessment and sustainability science is basically ignored, illustrated by missing examples such as the global sustainability assessment by Rotmans and de Vries (1997) and the European sustainability assessment by Kasemir et al. (2003). Quite a few other peer-reviewed and representative resources can be mentioned that are basically ignored in the current IPCC-assessment. (Rene and Jan Kemp and Rothman , Erasmus University)	See previous comments 3-16 to 3-20
3-23	A	0	0			Key elements of transition management are: o Long-term thinking (at least 25 years) as a framework for short-term policy.	See previous comments 3-16 to 3-20

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						o Backcasting: the setting of short-term and longer-term goals based on long-term sustainability visions and short-term possibilities. o Thinking in terms of more than one domain (multi-domain) and different scale levels (multi-level); how developments in one domain (level) gel with developments in other domains (levels); trying to change the strategic orientation of regime actors. o The use of societal experiments with transition options o An orientation towards system innovation. o Learning about a variety of options (which requires a wide playing field). Through processes of co-production and co-ordination, a transition to alternative systems may emerge without a collective plan or blueprint. Such an adaptive multi-level approach, whereby uncertainties and risk are acknowledged and dealt with, appears especially suitable for dealing with complex and structural societal problems (Kemp and Loorbach, 2004). The model that is currently used in the Netherlands to implement transition management consists of 4 basic activity-clusters (Loorbach, 2002, Loorbach and Rotmans, 2004). Collective choices are made "along the way" informed by learning experiences at different levels. Different trajectories are explored and flexibility is maintained, fitting with national and regional considerations. Transition management is a new steering concept that relies on 'darwinististic' processes of variation and selection. It makes use of "bottom-up" developments and long-term goals both at the national and local level. Learning and institutional change are key elements which means that transition management not so much concerned with specific outcomes but rather with mechanisms for change. The basic philosophy is that or goal-oriented modulation: the utilisation of ongoing developments for societal goals. (Rene and Jan Kemp and Rothman , Erasmus University)	
3-24	A	0	0			John Drexhage asked about the role of emissions trading. Brian Fisher replied that the literature in respect to emissions trading and taxes are if they are implementable and used they do substantially reduced the costs of meeting a target. He did say more detailed information on the transaction costs and the effectiveness of those regimes was needed qualifying it with that it is a work in progress. (Capetown Industry Expert Meeting, Industry)	agree
3-25	A	0	0			In trying to capture the scale of the challenge of stabilisation, one possible graphic is that of "population vs per-capita emissions" in different regions, because it encapsulates several dimensions of the challenge including current inequalities, potential for future growth, relative scales of industrialised and developing country	Review editor suggested refer comment to ch 1 or 2

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						contributions, and divergence within each group. The most recent version of the graphic is published in M.Grubb, "Kyoto and the Future of International Climate Change Responses: From Here to Where?", International Review for Environmental Strategies, Vol. 5, No. 1. But if the authors are interested I could supply the data and package for generating the graphic, with or without attribution. (Michael Grubb, (a) Carbon Trust(b) Cambridge University(c) Imperial College London)	
3-26	A	0	0			The chapter is very transparent about the methods and assumptions used. But this transparency reveals the significant flaws in using published emissions scenarios only - these are not representative in the areas of which I have some knowledge ie economic and population growth. The impact on estimates of emissions (and subsequently climate change) are uncertain but you would think there would be some upward bias. Unless this problem is addressed in an acceptable way, the validity of IPCC work will remain under question. This would be unfortunate as climate change is one of the most important issues facing the world, if not the most important. The credibility of the report should be such that the debate can focus on appropriate policy interventions not the validity of the underlying numbers. Furthermore, the IPCC report is a wonderful example of international collaboration and clearly uses a lot of good science. It would be a pity if this good work was undermined by question marks about the validity of one aspect of the report. The ways to resolve this need further discussion. Possibilities include: Removing or modifying those scenarios that are clearly inappropriate (eg because of economic or population growth rate assumptions that no longer appear realistic). Removing or modifying those scenarios which predict GHG emissions that depart significantly from more recent estimates of actual GHG measures. Ensure the range of scenarios used are representative of the range of economic and population growth assumptions from authoritative bodies such as the United Nations and the World Bank (and other key variables in the emission scenarios). Use Purchasing Power Parities (PPPs) to adjust national economies at the 1990 base. If assumptions are made about growth rates rather than levels for subsequent years, this, I believe, will address the main residual flaw from the non-use of PPPs. But Nordhaus' suggestion of using PPPs at the regional level is worth examining. In summary, I think more work needs to be done to ensure the scenarios are consistent with authoritative assumptions about population and economic growth. (Dennis Trewin, 0)	Agree, will quote new literature. (Nakicenovic)
3-27	A	0	0			Chapter 3 appears to be well-done, given that it had to work with the SRES	We are not generating new scenarios but

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						scenarios. My second "General Comment" above indicates my concern about the use of the SRES scenarios in stabilization analysis. I have other misgivings about the SRES scenarios as well, and think that AR4 needed to develop some new ones (see next paragraph). However, I have been given to understand that an IPCC decision required that the SRES scenarios be retained for AR4. This has meant that the analysis of stabilization in Chapters 3 and 11 may be substantially biased to the extent that many of the reference scenarios have already built in very rapid rates of decline in energy intensity and large amounts of carbon-free energy, all policy-free. I would add a further point. The combination of GDP growth rates and population growth rates in scenario families A1 and B1 result in implausibly high 110 year average annual rates of growth of GDP per capita. I question whether such century-long growth rates can be justified either empirically or in terms of economic growth theory-whether or not "endogenous". My impression is that the implied rates are globally unsustainable for a century-long period. However that may be, I am even more disturbed by the failure to include a scenario with both relatively low GDP growth rates and population growth rates. I think, for example, that a scenario with a 2.0% global rate of GDP growth (1990-2100) and a population of 7- 8 billion in 2100 (as in A1 and B1) is more plausible than scenarios A1, B1 (GDP growth rates of 2.5-3.0%). It is also more plausible than A2, which has a population reaching an unsustainable and implausible 15 billion in 2100. A reference scenario such as that which I have described would also yield a better perspective on the stabilization effort required, assuming its (policy-free) rates of energy intensity decline and carbon-free energy were modest. (Christopher Green, McGill University)	assessing existing literature. Therefore comment disregarded.
3-28	A	0	0			There is much new literature about regional abatement costs of allocation schemes, which are not described in this Chapter. 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	Comments to be refered to ch13

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						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 Analysis, The Hague. (Michel den Elzen, The Netherlands Environmental Agency)	
3-29	A	0	0			Sometimes rather long, informative texts, which can be reduced. Reader is most interested in what's new compared to the TAR. (Michel den Elzen, The Netherlands Environmental Agency)	agree
3-30	A	0	0			Issue of peaking, overshooting and stabilising concentration scenarios is missing here. In particular peaking instead of stabilizing is a way to reduce the climate risks. See Meinshausen (2006) - Exeter avoiding dangerous climate change; den Elzen and Meinshausen (2005) - MNP report (www.mnp.nl\en); O'Neill -PNAS; Wigley- OECD paper. Letter I make some suggestion how to include this in section 3.3.1. (Michel den Elzen, The Netherlands Environmental Agency)	Noted- pathways discussion to be elaborated using new literature. (Van Vuuren + Warren)
3-31	A	0	0			General comment: Chapter 3 is very good, thorough and multi-faceted summary of mitigation issues. (Sanna Syri, VTT)	noted
3-32	A	0	0			Chapter 3 describes the regional costs of 4 IPCC SRES regions (based on EMF study). The regional costs are highly depending on the assumed regime for future commitments. This is not described here. Here, the outcomes of one regime based on full IET and equal marginal costs across the regions is presented. This seems rather ad-hoc choice, as there are many allocation schemes 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.). These regimes	Consider here if not undertaken in ch13. (Riahi)

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						are explained in more detail in Chapter 13, and therefore it might be better to discuss the regional costs in more detail in Chapter 13, and not here. See also detailed comments on specific paragraph on page 37. You can contact me for more next about this (michel.den.elzen@mnp.nl) (Michel den Elzen, The Netherlands Environmental Agency)	
3-33	A	0	0			Overall, this is a very clear and good chapter, which was a pleasure to read (Sjak Smulders, Tilburg University)	noted
3-34	A	0	0			This chapter is generally well organised and in fairly good shape. (Pat Finnegan, Grian)	noted
3-35	A	0	0			It is noted that all emission scenarios assume a significant increase of GDP over the 21 century. However, this seems not to take into account the possible loss of GDP induced by damage linked to the impacts of climate change. It is proposed to include a remark that the emission scenarios published until now in the literature do not consider any losses related to the impacts of climate change. As those impacts might be considerable in monteary terms (a figure included in the AR4 in chapter 2 suggests that the costs will be in the range of trillion $ per decade in the near future and the projected increase in temperature suggests that those costs will grow in the next decades with a tendency to double every 10 years) this might significant reduce the growth in GDP or even result in a reduction of the GDP). (Radunsky Klaus, Umweltbundesamt)	Linking to impacts via 3.5 (Corfee-Morlot)
3-36	A	0	0			I am very concerned that the focus of Chapter 3, is only on the next 100 years. The reality illustrated by the analysis of Wigley, Richels and Edmonds, and similar analyses provided for example on pages 223-224 of the TAR Climate Change 2001, The Scientific Basis, BUT IGNORED HERE, is that the problem is much longer term than this. Furthermore, the problem is 10x larger in the long term (~50,000 EJ / 50 years) than in the short term (~5000 EJ / 50 years). As part of the resolution of this problem, we need to introduce technologies in the present century that can almost fully replace carbon-emitting technologies in the next century. Thus we need to be advancing new energy technologies with very high total potential, and we have to be moving to energy uses that are consistent with very low CO2 emission. While it is important to pay attention to the near term, this report must absolutely also keep the much larger long term challenge in focus. It is critical that analyses looking to 2200 be included in the mix of the discussion in this chapter. See the attached analysis of future non-carbon energy needs, labeled "WRE Analysis.pdf". (Robert Goldston, Princeton Plasma Physics Laboratory)	Scoping issue to be dealt with in ch2. In addition will add enhanced definition of stabilisation on p30.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-37	A	0	0			Some abbreviations come without full explanation, e.g. GHG, NCP in page 8 line 35, and in page 50: EST in line 19 and EIT in line 21 and so on. Maybe it is better to give a full explanation when an abbreviation appears at the first time in the chapter. (Tieju Ma, International Institute for Applied Systems Analysis)	Accept - editorial
3-38	A	0	0			The chapter has too many references at the bibliograhy and in general is too detailed. The conclusion of 3.2.3 are clear and give also an overview of the problem. The figures are too many and not easy to read. (Marco Mazzotti, Institute of Process Engineering)	Accept – editorial. Reject comment on references; will modify text as necessary to reflect literature.
3-39	A	0	0			I do not offer detailed comments, but a broad observation. Much of this chapter has the flavour of being quite defensive, including defensive of the SRES scenarios against it critics. The defense is good, but is that really the purpose of this chapter? In addition, the chapter gives the impression, more than many, of being a collation of different contributions rather than an integrated assessment of literature pertaining to long term mitigation and stabilisation. For the issues of long term stabilisation, I really wonder how useful detailed discussion of baselines and amounts of carbon that need to be cut is - the question is whether, how, and under what kind of investment and cost profiles the system can evolve in different and lower emitting directions, and also what kind of trends and policies would foreclose options or make stabilisation more difficult and costly. (Michael Grubb, Cambridge University)	Note comment above – rebalancing of text in 3.1 – 3.3 as per response to 3-10.
3-40	A	0	0			The chapter could be better focused. Many references are missing from the reference list, hampering the review process. Some of the listed references are not peer-reviewed, however, and I suspect this is true for a higher share of the non-listed references. (Richard Tol, Hamburg University)	See response 3-39
3-2	B	0	0	0	0	In the issues related to mitigation in the long term context there is a leitmotive on renewable development. Also it is good to develop renewable energies we have to build a bridge between the current fossil dominated energy mix and the future one. Besides the potential of renewable may be limited by high cost and necessary time for technology deployment. It is worth to cite in a much broader manner the development of low carbon technologies including nuclear and clean fossil fuel. Cite B. Magné and M. Moreaux (LEERNA, IDEI), "Long Run Energy Trajectories: Assessing the Nuclear Option in Response to Global Warming" World Congress of environmental and resources economists, Monterey, California, June 24-27, 2002. (Nicole DELLERO, AREVA- Erratum)	Accepted, will review cited literature.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-45	A	1	0			Table 3.2 makes no mention of the "Cement" industry. This is an oversight. The cement industry holds good potential for CO2 capture. (Richard Doctor, Argonne National Laboratory)	
3-41	A	1	1	5		This chapter starts with a fascinating overview of scenario development but it is too long and too detailed for the amount of space alloted the chapter. Given that much of the detail is available in the special report then it is not necessary to repeat it here. (Jeff Price, California State University, Chico)	See response 3-39
3-42	A	1	1	30	1	I have to wonder if it is really necessary to spend 30 pages reviewing, often at a textbook level, the ins and outs of the SRES scenarios and their history. The level of detail is fascinating but most of it (on the order of 75% could likely be cut with little impact on the crux of this chapter. The authors could then include the material as supplementary information on the IPCC website if theu truly felt it was necessary. Inclusion of this material should not come at the expense of information on mitigation or damage avoidance, for example. (Jeff Price, California State University, Chico)	Accepted: we will bring some of the materials on new stabilization scenarios upfront and reduce the pages allocated to description of SRES and TAR scenarios.
3-43	A	1	27	1	27	3.4.1 is missing. (Leo Schrattenholzer, IIASA)	Accepted: Section numbers will be corrected.
3-44	A	1	28			It seems subsection 3.4.1 is missing. It is also missing inside the chapter. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted: Section numbers will be corrected.
3-46	A	2	5	2	26	The Executive Summary (page 2) is very incomplete. One find also partial conclusions (paragraph 3.2.3, page 23) which take again some ideas of the executive summary and add new findings. However, there is no conclusion or summary on pages 30-64 whereas the text is often rich and dense. Probably the chapter could be improved by giving a complete and compact executive summary taking again the main findings of the whole chapter. Perhaps partial conclusions (as in 3.2.3, page 29) could be added at the end of each section (3.1, 3.2,…). This seems to me very important because some readers, in particular decision-policy-makers, have generally no time to enter the detail of the text. (Norbert LADOUX, University of Toulouse and IDEI)	Accepted: Executive Summary will be revised to better reflect changes in the chapter due to comments and important messages. It is not clear right now whether it would be appropriate to end each section with a conclusion.
3-47	A	2	5			How about adding the list of the six different integrated assessment models, their names and references. (Tieju Ma, International Institute for Applied Systems Analysis)	Rejected: It is not clear to which passage in the chapter this refers to. Presumably to the 6 SRES models. These are adequately described in the SRES and the related references. No need to repeat in Chapter 3.
3-48	A	2	12			Executive Summary: The ES seems rather weak as it reports only one new finding	Accepted: Executive Summary will be revised

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						compared to the TAR: lower population projections. This is little news from a chapter draft that is more than 100 pages (including figures and tables). (Hans-Martin Fuessel, Stanford University)	to better reflect changes in the chapter due to comments and important messages. It is not clear right now whether it would be appropriate to end each section with a conclusion. Nonetheless, it is true that the new scenarios are not fundamentally different from the previous literature. Lower populations, new approaches to deal with economic growth and multi-gas stabilization scenarios are some of the changes.
3-49	A	2	12			Summary: In section 1.7.8, difference between short-tem and long-term is mentioned. As for the scenario, the similar things about purposes, resolutions and so on can be mentioned? (Toshihiko Masui, National Institute for Environmental Studies)	Noted: We will consider an explanation of the purposes and resolution of shorter-term scenarios although the heterogeneity is very large.
3-50	A	2	16	2	16	The way the reference is made is inconsistent with chapter 1. In chapter 1, the same publication is referred to as IPCC, 2001, instead of Moria et al., 2001. This applies to all the other IPCC references and they should be consistent. (Asami Miketa, International Atomic Energy Agency)	Noted: This is an issue of the overall reference style, especially references to IPCC reports and their chapters.
3-51	A	2	18		19	What should be relevant is how representative the scenarios are of authoritative estimates of the driving flaws. The reliance on scenarios in the literature only would be a major flaw of the report. (Dennis Trewin, 0)	Rejected: Development of scenario driving forces is also a part of scenario description. Their development is not independent of other scenario assumptions. The reliance on scenarios in the literature is part of our terms.
3-52	A	2	19	2	19	It is proposed to substitute "of" by "compared to". (Radunsky Klaus, Umweltbundesamt)	Accepted
3-53	A	2	21	2	23	It is said that “the focus of the chapter is on scenarios that stabilize atmospheric concentrations of GHG and other relevant anthropogenic substances that are radiatively active in the atmosphere such as sulfur aerosols”. However in 3.2.2.4.1 Sulfur Dioxide Emissions Scenarios the only reference to sulfur aerosols is in the first two lines. May be it is better to say that “the focus of the chapter is on scenarios that stabilize atmospheric concentrations of GHG and other relevant anthropogenic substances that are radiatively active in the atmosphere or that contribute to the formation of substances that are radiativly active such as sulfur dioxide” (Jorge Gasca, Mexican Petroleum Institute)	Noted: This needs to be harmonized with text in 3.2.2.4.1 and will be considered in revising Executive Summary
3-54	A	2	24	2	24	It is proposed to substitute "change" by "progress".	Accepted.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Radunsky Klaus, Umweltbundesamt)	
3-55	A	2	26	2	28	Wording "have not changed very much" seems very loose. (Nick Campbell (Batch 2), ARKEMA SA)	Accepted. We will improve on the language and give examples what has changed.
3-56	A	2	33			This doesn't mean it is right. The vast majority of economists would say PPPs should be used. Given that, in theory, they should be used, the issue is how should they be used in practice. (Dennis Trewin, 0)	Noted: The jury is still out what would be the best way to proceed in the context of stabilization scenarios. However, it is a factual statement to observe that there are still very few PPP long-term scenarios. It is not possible to assess the literature that does not exist. However, we consider bringing to the Executive Summary some more PPP discussion.
3-57	A	2	35	2	36	Instead of using the term all gases it would be better to mention the gases or use a narrower term like GHGs. The term “all gases” is too generic. (Junichi Fujino, National Institute for Environmental Studies)	Noted: We will reword as suggested.
3-58	A	2	35	2	35	Should read "emission ranges" not changes (H-Holger Rogner, IAEA)	Accepted.
3-59	A	2	35	2	36	.. that include all gases … is this all kyoto GHG gases ? (Peter Kolp, IIASA)	Noted: We will reword as suggested in 2-57
3-60	A	2	35	2	36	.. that include all gases … is this all kyoto GHG gases ? (Peter Kolp, IIASA)	The same as above: 3-59.
3-61	A	2	40	2	49	Future greenhouse gas emissions and the evolution of their underlying driving forces are highly uncertain, so research priority should be given to exploring hypothesized interactions and linkages between key variables by using scenarios analysis, and how these might be affected by policy interventions. (James Bero, BASF Corporation)	Noted: We will consider the new wording.
3-62	A	2	51			"still" span most…??? (Peter Bosch, IPCC TSU WGIII)	Rejected: It is not clear how
3-63	A	2	53	2	53	This should be www.cger.nies.go.jp (Asami Miketa, International Atomic Energy Agency)	Rejected: Actually there is a “-“ in the address pathname. The path name will be updated anyway: http://www-cger-nies.go.jp
3-64	A	3	15	3	49	The first full paragraph and the third on this page directly repeat the first two paragraphs on page 2, and should be deleted. (Michael Jefferson, World Renewable Energy Network/Congresses)	Noted: we will make sure to avoid such repetitions in the revised Executive Summary.
3-65	A	3	22	3	22	I suppose that the sentence does not intend to suggest that the focus of the chapter is on scenarios that stabilize sulfur aerosols. Written as it is, this is being suggested.	Accepted: This is indeed not the focus of the chapter and we will reword. Nevertheless, it

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						The sentence should be rephrased as what is relevant for GHG is not for aerosol emissions. (Philippe Tulkens, TERI School of Advanced Studies)	should be noted that sulphur emissions are indeed lower in the new scenarios.
3-66	A	3	27	3	27	(EMF-21) scenarios. Isn't it EMF-19 and EMF-21 scenarios (Peter Kolp, IIASA)	Accepted; We will consider in revising Executive Summary – emf-19 was on technology and emf-21 on multi-gas.
3-67	A	3	27	3	27	(EMF-21) scenarios. Isn't it EMF-19 and EMF-21 scenarios (Peter Kolp, IIASA)	Same as 3-66.
3-68	A	3	36	3	37	The meaning of this sentence is not clear. Does the sentence imply that the reference scenarios recently also include climate counter measures? In that case the term “such policies” may be reworded to convey the meaning. (Junichi Fujino, National Institute for Environmental Studies)	Noted: The meaning of the sentence is that new scenarios include climate policies so that the traditional way of looking at baselines that do not have any climate policies is not longer possible for all scenarios.
3-69	A	3	43		44	I am not clear how lower population projections will be used in the new emissions scenarios exercise. My impression that they will not be used except to analyse the representative of existing scenarios. (Dennis Trewin, 0)	Noted: This is an open issue, but there are new scenarios with lower population projections. We will have to see how that effects the emissions because of the possible compensating mechanisms.
3-70	A	3	48	3	48	Should read "emission ranges" not changes (H-Holger Rogner, IAEA)	Accepted.
3-71	A	4	20			I have not found the Davis 2002 reference.[Same comment at page 5, line 45]. On page 6, line 37, Raskin 2005? (Michael Jefferson, World Renewable Energy Network/Congresses)	Accepted: Reference is indeed missing.
3-72	A	4	47	23		(and no more are planned – IT IS NOT THE STATE OF ART). New concept such are Tidal Lagoon (to avoid environmental impact of tidal dams) are studied in UK (project in Swansea Bay). A 300 MW chinese plant (offshore tidal lagoon) in the waters near the mouth of the Yalu River has been annonced in 2004. An other project is studied for South Korea. (MICHEL PAILLARD, IFREMER)	This appears to be a misplaced comment. It probably refers to another chapter.
3-83	A	5	0	5		Table 3.8: Finland: national scenarios have been made by VTT. One customer has been Tekes. References are Savolainen et al. 2003, and Lehtilä et al. 2005 (main Chapter). Scenarios were made for -10%, -20 % and -30% reduction from 1990 by 2030. All six gases of Kyoto protocol were considered in calculations. (Sanna Syri, VTT)	Noted: we will consider including these references.
3-73	A	5	11	5	13	More precision is needed in referring to definitions of scenarios. I would suggest inserting the word "reference" before the word scenario in line 11; inserting the	Noted: A good suggestion for improving the language that we will consider in the next

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						word "quantitative" before the word "projections" and inserting the words "fully fledged" before "alternative futures" in line 12; and adding the following additional sentence after the sentence ending in "outcomes": "The 'reference scenario' may be accompanied by alternative scenarios in which one or more exogeneous variables or parameters have been changed to reflect a major source of uncertainly." (Kenneth Ruffing, N/A)	rewrite.
3-74	A	5	22	5	34	Current text says nothing on time-horizons (typically very long for climate chnage scenarios, which is different from many other scenario exercises. Also dgree of spatial resolution (regional differentiation) is worth mentioning; might best fit in section 3.1.1.1 (Bert Metz, IPCC)	Noted: We will expand the text on other approaches. However, the stabilization scenarios are mostly century-scale and global in nature.
3-75	A	5	22	7	21	3.1.1.1 to 3.1.1.4 give a historic descriptive line, without conclusions. Can this decription not be summarised in a table of concepts, providing some examples with each concept. (Peter Bosch, IPCC TSU WGIII)	Noted: We will consider such a table and ways of shortening these sections.
3-76	A	5	26			Ruskin et al is missing from the reference list (Danny Harvey, University of Toronto)	Accepted: The reference needs to be added and spelled correctly (Raskin).
3-77	A	5	26	5	26	Is figure 3.1 really necessary. It is so simple that is would appear that it could be described in less space than it takes to reproduce the figure, especially since it comes from the special report. (Jeff Price, California State University, Chico)	Noted: We will consider deleting and simply providing a reference.
3-78	A	5	30			Should be: (Raskin et al., 2005) Not "Ruskin et al. Also, add citation to references (Paul Raskin, Tellus Institute)	Accepted: The reference needs to be added and spelled correctly (Raskin).
3-79	A	5	36			Fig 3.1 does not clarify much in the dichotomy as described in the lines 24-34, because the word scenarios is in the middle which is used in line 24 in another sense. Revise sentence in line 25. (Peter Bosch, IPCC TSU WGIII)	Noted: We will consider deleting and simply providing a reference.
3-80	A	5	38	6	37	The distinction between section 3.1.1.2 and 3.1.1.3 is not clear; both seem to deal with storylines for global scenarios (not modelling as the title of 3.1.1.2 suggests); seems logical to merge these sections. Mentioning of EMF and IMCP in line 33 on page 6 does not make sense in this context; would fit into 3.1.1.4 (Bert Metz, IPCC)	Accepted: imcp and emf can be deleted from page 6. We will consider shortening the text and perhaps merging the sections.
3-81	A	5	38	6	14	3.1.1.2 is general descriptive. Can be shortened. (Peter Bosch, IPCC TSU WGIII)	Accepted: may be merged with the following section.
3-82	A	5	40	6	15	Again, this section reads like a textbook and, while interesting, it is too detailed and	Accepted: see response to 3-80 and 3-81.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						much of it is not relevant for the amount of space it takes from other material in the chapter. (Jeff Price, California State University, Chico)	
3-84	A	6	10			Footnote 2 This is like a dinner without the first course! How about: 'The Rapids' (Wack et al, May, 1973); 'The World of Internal Contradictions' (Wack et al., 1974 - not to be confused with the scenario of the same name formulated by Kahn et al./Hudson Institute which did not consider turbulance in oil markets and consequential disruptions); 'Producer Miscalculation' (Jefferson et al., October, 1976). There are a number of other examples which could be cited between 1977 and 1990 when the Footnote's listing begins - among them 'Oil Price Collapse', (Jillings et al., 1985) which had its origins in an October, 1979 scenario ('European Relapse'). (Michael Jefferson, World Renewable Energy Network/Congresses)	Noted: However, it is not clear at this stage whether we will expand the foodnote or delete.
3-85	A	6	16	6	16	Is there are a possibility of having a clearer name/term. Can we say "Future scenarios for the globe" "Future scenarios at the global level."? (Junichi Fujino, National Institute for Environmental Studies)	Rejected: This is an established concept used in TAR WGIII, ch 2. However, we will consider shortening this discussion.
3-86	A	6	18	6	37	Too much history that is available elsewhere, this section could be significantly shortened. (Jeff Price, California State University, Chico)	Noted: We will consider shortening this discussion.
3-87	A	6	19			Should be: (Raskin et al., 2005) Not "Ruskin et al. Also, add citation to references (Paul Raskin, Tellus Institute)	Accepted: Reference will be added and corrected..
3-88	A	6	19	6	19	(Ruskin et al. 2005): there's no entry for this reference in the References (Peter Kolp, IIASA)	Accepted: Reference will be added and corrected.
3-89	A	6	19	6	19	p. 6 L.19. The reference to Ruskin et al. (2005) is missing in the reference list. It should be added. (Philippe Tulkens, TERI School of Advanced Studies)	Accepted: Reference will be added and corrected.
3-90	A	6	19	6	19	(Ruskin et al. 2005): there's no entry for this reference in the References (Peter Kolp, IIASA)	Accepted: Referee will be added and corrected.
3-91	A	6	37			Should be: (Raskin et al., 2005) Not "Ruskin et al. Also, add citation to references (Paul Raskin, Tellus Institute)	Accepted: Reference will be added and corrected.
3-92	A	6	41			On page 5 (line 44) it is stated the Shell scenarios were "principally based on" narrative stories with illustrative quantifications … Here on page 6 the statement that they were primarily qualitative and narartive-based fails to communicate the point that the quantifications made many of the key points really telling. Thus the	Noted: We will consider to shorten the text and make if more clear.

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						impact of higher oil prices/supply disruptions were calculated by the scenario team to have an impact of several percentage points on annual GDP change for specified European countries in the mid-1970s and again following the 1979 oil 'crisis' (up to 6 percentage points lower than some of the same countries operating companies projected). Inflationary impacts, oil price impacts, and - after some delay - oil demand impact quantification proved very powerful - and 'prophetic'. Neither Pierre Wack nor (coming several years after this process had begun) Peter Schwartz were involved with this quantification, their roles being very important (in Wack's case, of critical importance) in the initial creation of narrative storylines. It is also not the case that (as implied here) that global modelling work was not undertaken. As early as 1974 a 3,400 equation linear programming model, which was considered too much of a 'black box' and too unwieldy (its initial run took 19 hours 54 minutes central processing unit time) for acceptable use, and this was finally handed over to the Workshop on Alternative Strategies under Carroll Wilson at Harvard. A global 'mini-model' was, however, found useful. Thus the FOD text seems to downplay the extent and importance of quantification, perhaps because those who have mainly written about this scenario work have slightly overstated the roles of the main authors and overlooked the roles of others. (Michael Jefferson, World Renewable Energy Network/Congresses)	
3-93	A	6	41		44	you are saying twice the same in this sentence (Peter Bosch, IPCC TSU WGIII)	Noted: We will avoid duplicaton in the revision of the text.
3-94	A	6	46	6	47	It seems a “)” is missing. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted.
3-95	A	6	48			Should be Swart, Raskin, and Robinson not Swart and Ruskin (Paul Raskin, Tellus Institute)	Accepted: Reference will be corrected.
3-96	A	7	23	8	44	There is some overlap in this section: lines 5-13 on page 8 cover the same issue as lines 32-44 on page 8. What I miss in this section is literature that compares different "future worlds" (all with some degree of intervention) and compare those in terms of sustainability (including the prevailing climate, incomes, equity, etc). This would be a nice way to circumvent this intervention vs non-intervention problem (Bert Metz, IPCC)	Noted: However, this would be difficlult to do as these futures scenarios do not focus on climate issues. Equity implications are probably more easy to deal with.
3-97	A	7	30	7	31	Important to note that CO2-equivalent concentration is directly proportional to radiative forcing and should not be confused with equivalent emissions (using GWPs) (Tom Kram, MNP)	Accepted.

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3-98	A	7	42	7	42	It took almost 8 pages to get to the point where the authors say the mitigation scenarios are the focus of the chapter. It would seem that the text to this point could be shortened by at least half if not by close to 75%. (Jeff Price, California State University, Chico)	Noted; We will consider shortening this introductory text.
3-99	A	8	6	8	9	As early as 1976, drawing (inter alia) on work done by Willis Harman and Arnold Mitchell at Stanford Research Institute (the VALS program), Shell developed scenarios in which lifestyle and values changes were required and assumed to achieve a lower emphasis on material well-being in order to reach a more sustainable path of development. This work took up a considerable amount of effort between late 1976 and early 1979, as was perhaps a diversion from what was unfolding on the international oil scene.The reference to Lazarus (1993) gives a misleading impression. (Michael Jefferson, World Renewable Energy Network/Congresses)	Noted.
3-100	A	8	6	8	6	"... the admissible temperature increase (1 C)": Most tolerable windows approach/safe-landing analyses have concentrated on a ceiling of 2 C. (Thomas Bruckner, Technical University of Berlin)	This does not appear to refer to the page and line as given.
3-101	A	8	21		23	Repeats what is said on page 3, line 31. (Peter Bosch, IPCC TSU WGIII)	Accepted.
3-102	A	8	22	8	23	Sentence incomplete (H-Holger Rogner, IAEA)	Rejected: It is indeed a sentence.
3-103	A	8	40			There is only Morita and Robinson in the ref list (Danny Harvey, University of Toronto)	Accepted: The correct reference is to the IPCC TAR WGIII Ch 2 by Morita, Robinson et al.
3-104	A	8	46	8	46	Suggest deleting "and the lock-in effects of infrastructure choices" from the title of section 3.1.3. I found no mention of lock-in in 3.1.3. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Noted: Either lock-in discussion will be included in the section or we will consider changing the title.
3-105	A	8	46			Section 3.1.3: confusing that title mentions only infrastrucutre lock-in, while text also refers to technology lock-in. Electricity grid can support fossil and non-fossil electricity based systems, but technology/supply chains are currently locked in by fossil. similar for institutional lock-in, now not addressed until section 3.1.6 (Tom Kram, MNP)	Noted: Either lock-in discussion will be included in the section or we will consider changing the title
3-106	A	8	46	13	27	Sections 3.1.2 to 3.1.6 cover issues related to scenario building for development. These sections are related (3.1.3, 3.1.5 and 3.1.6 for instance cover issues that are very close; 3.1.5 is not only on mitigation!) Section 3.1.4 is specifically addressing growth rate assumptions, which seems to belong more to the discussion in 3.2.1.2 about baseline assumptions for economic growth. They all in fact further elaborate the issue that is raised in section 3.1.2, namely the question of sustainable	Accepted: Some of the literature from ch 12 would indeed be useful here. Also, we will consider revising the sections.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						development pathways as non-intervention scenarios and how realistic these scenarios are. Because of this the sections do not fit very well into an introductory section 3.1. It might be better to group them under a separate 3.2 (Scenarios for development or something like that) and to integrate the material in the various sections better (with some issues such as growth rate assumptions in 3.2 then referring back to this new section). More non-climate literature may have to be looked at. Ch 12 has alot of relevant literature on this topic. Use it also in ch 3 (Bert Metz, IPCC)	
3-107	A	8	48			Section 3.1.3. Nevertheless, this section---due to the extreme criticality of the subject for a way out of the problem---needs to be much more developed, in my view. It is neither sufficiently precise enough on the existing damage done by technological lock-in in the North, nor sufficiently comprehensive enough on the options available to avoid this situation while still simultaneously finessing many development problems in the South---e.g. (particularly) concentration on the development of distributed renewable energy systems. (Pat Finnegan, Grian)	Noted: This will considered in rewriting the secton.
3-108	A	9	15	9	15	You can refer to our most recent work, World Energy Outlook 2005, instead of WEO2004. (Fatih Birol, International Energy Agency)	Noted. Fatih Birol is now CA and should be asked to contribute the appropriate text.
3-109	A	9	29	9	34	This sentence lacks clarity. Though the message is conveyed that developing countries need to consider leap frogging opportunities, the sentences above are not continuous and are confusing. (Junichi Fujino, National Institute for Environmental Studies)	Accepted: We will rewrite.
3-110	A	9	29	9	34	Something missing linking the two sentences (H-Holger Rogner, IAEA)	Accepted: We will rewrite.
3-111	A	9	32	9	32	You can refer to our most recent work, World Energy Outlook 2005, instead of WEO2004. (Fatih Birol, International Energy Agency)	Noted. Fatih Birol is now CA and should be asked to contribute the appropriate text.
3-112	A	9	34	10	46	The text deals with the issue of convergence mainly from a standard neoclassical standpoint. An extensive analysis of the problem is also performed by economists such as Nelson and Fagerberg, which argue within an evolutionary paradigm. Their theoretical and empirical work should also be included. The following literature can serve as a starting point: Fagerberg, J. (1995): User-producer interaction, learning, and competitive advantage, in: Cambridge Journal of Economics, Vol. 19, S. 243-256. Fagerberg, J.; Godinho, M. (2005): Innovation and Catching-Up, in: Fagerberg, J. et al. (eds.): The Oxford Handbook of Innovation, Oxford University	Noted: The evolutionary perspective will be considered as well.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						Press, Oxford, pp. 514-542. UNIDO (2005): Capability building for catching-up, UNIDO Industrial Development Report 2005, Vienna, ISBN: 92-1-106431-7. (Rainer Walz, Fraunhofer Institute Systems and Innovation Research)	
3-113	A	9	36	10	48	Again, this material reads as if it is coming from a textbook. Unless this chapter is new to IPCC as a whole then the inclusion of references going back to the 1980s (or most anything pre-2000) does not meet the need of conveying information on mitigation in a small amount of space. This section, like much of the other material before it, could easily be drastically cut. (Jeff Price, California State University, Chico)	Noted: The passages would need to be shortened.
3-114	A	9	36			3.1.4 New Economic Growth theories. The section refers only to standard or neoclassical theories that assume that the economy is a closed system. Within the 70 and 90 there was an interesting discussion on Limits to Growth and Steady States Economics that still continues. For instance, non- growth economics have been an issue for economists since D. Ricardo and Stuart Mill. I suggest that an Assessment Report should report this literature and comments briefly the arguments of these authors. See specially "Economic growth theory and the Georgescu-Roegen paradigm" in Bioeconomics and Sustainability, Essays, Kozo Mayumi and John M. Gowdy eds. 1999 Edward Elgar. (Juan Llanes, Havana University)	Noted: The literature will be considered.
3-115	A	9	36			Development paradigms is defined in Ch. 2: section 2.2.5 (framing, ca.1.5 page). Ch3: does not mention the word development paradigm, although in the explanation of scenario storylines (3.1.1.2 and more specifically 3.1.4 to 3.1.6) the concept is used. Ch 12, p18,line 33 refers to chapter 3 for development paradigms. Hence the following proposal: In Ch2 clarify section 2.2.5 to make it fit with the use of the concepts in Chapter 3. In chapter 3 refer back to chapter 2 and use explicitly the notion of development paradigms. (Peter Bosch, IPCC TSU WGIII)	Noted: A reasonable proposal. We will refer to ch 2.
3-116	A	9	38			You omit capital deepening, and perhaps but too much emphasis on demography. (Richard Tol, Hamburg University)	Noted: Capital deepening issue will be considered for the next revision.
3-117	A	9	44			"newly discovered" what is this supposed to mean? Marshall, Clark and Arrow may have speculated about increasing returns to scale, but it was Romer who solved the model (Richard Tol, Hamburg University)	Accepted: "Newly discovered" needs to be deleted.
3-118	A	10	5	10	48	A series of recent assessments demonstrates divergence, not convergence, as well as providing explanatory models (World Bank 2002, Income Poverty - Trends in	Noted: Divergence needs to be mentioned.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						inequality, http://www.worldbank.org/poverty/data/trends/inequal.htm. Halloy, S.R.P. and Lockwood, J.A., 2005. Ethical implications of the laws of pattern abundance distribution. E:CO (Emergence: Complexity and Organization), 7, 41-53. UN-SD, 2005. Progress towards the Millennium Development Goals, 1990-2005. United Nations Statistics Division.http://unstats.un.org/unsd/mi/mi_coverfinal.htm) (Stephan Halloy, Universidad Mayor de San Andrés)	
3-119	A	10	15		24	"Catch-up…the expected findings in the literature" This is a vague and misleading statement. In contrast, I think the current agreement in the literature is that relative income differences that we see nowadays are highly persistent and not very likely to vanish away (this is consistent with what you write in lines 11-12, and 21-14). Convergence is slow and only arises as a process after some major disturbances (the central example of course is World War II in Europe). Even over a time span of centuries, income differences are persistent and largely depend on persistent differences in institutions (see the very influencial studies by Acemoglu, Johnson and Robinson, and also Easterly and Levine) (Sjak Smulders, Tilburg University)	Noted: Catch-up is about growth rates and does not imply that relative income differences will disappear. We will consider repharsing.
3-120	A	10	20	10	24	Last ten years in economic literature there was a lot of discussion on "club convergence" (See for example Sala-i-Martin, 1996. The Economic Journal N 437). This phenomenon is important for emission scenarios. It was not taken into account in a special IPCC report. (Alexander Golub, Environmental Defense)	Noted.
3-121	A	10	25		33	You omit institutions. (Richard Tol, Hamburg University)	Noted: Given the space limitations, the mention cannot be too long. This should be treated in Ch 2, see comment 3-130.
3-122	A	10	30	10	34	I do not see why this is making a difference. It might be better to frame the debate in terms of mutual vulnerability or even in terms of enlightened self interest of the industrialized countries (Gert de Gans, Kerkinactie)	Rejected: This does make a difference some degree of affluence is required for diffusion of less carbon-intensive technologies. We will however consider adding the “enlightened self interest”.
3-123	A	10	34		36	This is disingenious. Yes, SRES assumed global convergence in line with regional convergence, particularly European convergence. The sentence is formulated as a justification of this assumption, whereas in fact it is bloody stupid to confuse continents and globes. Europe's recent history is NOT representive for the history of the world, and unlikely to be representative for its future. Besides, this is based on material that was not peer-reviewed; in fact, Keywan got severely criticised at	Noted: We will consider this criticism in the next rewrite, but it is perfectly permissible to compare conditional convergence among large world regions (eg. China and India compared to Europe and North America) with recent historical experience.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						this meeting for sprouting such nonsense. (Richard Tol, Hamburg University)	
3-124	A	10	34		40	I am not sure how the annual rate of income convergence is defined. It seems the high annual rate of 2% among 90 regions of Europe is used to justify the higher rate of convergence (among the 11 regions) in the A1 set of scenarios. But the averaging effect would mean that you would expect to have less dispersion among 11 entities than 90 entities. So the convergence assumption may be a factor leading to higher economic growth rate assumptions than the World Bank, etc. (Dennis Trewin, 0)	Noted: The reference is to conditional convergence.
3-125	A	10	34	10	36	In the sentence, there seems to be a contradiction as the evidence of OECD regions cannot be used to check the consistency of world convergence, only of developed countries convergence. (Juan Carlos Ciscar, IPTS, European Commission)	Noted: We will consider this criticism in the next rewrite, but it is perfectly permissible to compare conditional convergence among large world regions (eg. China and India compared to Europe and North America) with recent historical experience.
3-126	A	10	36	10	36	"Rate of convergence" could be defined in various different ways. It should be clear how it is calculated, otherwise no one can check if the numbers are correct. (Asami Miketa, International Atomic Energy Agency)	Noted: this refers to conditional convergence of growth rates in the sense that lower-income regions have higher growth rates than the high income ones have.
3-127	A	10	36	10	38	SRES had only 4 regions - aggregated from a larger but different spatial resolution of the underlying models. 11 regiosn refers to IIASA and MESSAGE/Scenario generator?? (H-Holger Rogner, IAEA)	Noted: It also refers to other SRES models as each had a higher regional resolution than 4 Macro regions.
3-128	A	10	40			The author mentioned that “less convergence generally yields higher emissions”. How about giving some explanation about why or what main reasons are for this result? (Tieju Ma, International Institute for Applied Systems Analysis)	Noted: Lower incomes means that carbon-saving technologies are less affordable.
3-129	A	11	12	11	15	What is meant by 'the energy content' of industries? The energy content of a products would be the energy physically contained within a product (for example the heating value of paper or wooden furniture). The 'energy conctent of an industry' is much more difficult to grasp. (Kenneth Möllersten, Swedish Energy Agency)	Noted: The language needs to be more precise, the text refers to carbon intensiveness and should refer to emissions.
3-130	A	11	16	11	19	the history of the recent developments in the economics modelling community are described in Köhler, Grubb, Popp and Edenhofer 2006 the transition to endogenous technical change in climate economy models, Energy Journal Special Issue on the IMCP, forthcoming	Accepted: Reference will be added here as well.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Jonathan Köhler, Tyndall Centre, University of Cambridge)	
3-131	A	11	20	13		Several chapters mention institutional issues, sometimes in the context of mitigative or adaptive capacity. The most logical place to deal with the topic upfront is in Ch.2. The main question here is the role and importance of institutions for mitigation. Hence the following proposal: Ch 3, p11-13 (3.1.6 Institutional frameworks): Keep here page 12 up to line 19. Integrate p12, line 20-55 into chapter 2, p51. Keep page 13 line 5-27. (Peter Bosch, IPCC TSU WGIII)	Noted: Suggestion will be considered.
3-132	A	11	50			The section on institutions ignores the body of economic research from the WorldBank, and all academic research that uses the WorldBank's institutions data. (Richard Tol, Hamburg University)	Noted: World Bank will be quoted.
3-133	A	12	16	12	16	NAS forthcoming reference not found in references (Peter Kolp, IIASA)	Accepted: Reference will be included.
3-134	A	12	16	12	16	NAS forthcoming reference not found in references (Peter Kolp, IIASA)	Accepted. Reference will be included.
3-135	A	12	31	12	35	Duplication, identical text in Chapter 2, page 11 (H-Holger Rogner, IAEA)	Noted.
3-136	A	12	31	12	32	The policy implication is i.e. formulated by… should this be … implications is e.g. formulated ? (Peter Kolp, IIASA)	Noted.
3-137	A	12	31	12	32	The policy implication is i.e. formulated by… should this be … implications is e.g. formulated ? (Peter Kolp, IIASA)	Duplicaton.
3-138	A	12	42			"mitigate to climate change", maybe the "to" should be deleted. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted.
3-139	A	13	29			Section 3.2. jumps into population projections, but a brief introduction on how emissions result from pop, income, energy-intensity and supply mix would be helpful background. (Tom Kram, MNP)	Accepted, will add discussions on Kaya-Identity
3-140	A	13	35	13	35	This sub section there is only a passing reference to migration. It would be beneficial to discuss the impact of migration on population numbers in greater detail. An important reference that is omitted is the UN World Economic and Social Survey 2004, which discusses international migration in detail. (Junichi Fujino, National Institute for Environmental Studies)	Noted, will add reference, but a comprehensive discussion is not possible given the space limitation and lack of literature
3-141	A	14	0			At several occasions, acronyms such as "ALM" and "REF" need to be explained at their first occurrence.	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Hans-Martin Fuessel, Stanford University)	
3-142	A	14	43			Not true. Fisher et al. are about to publish a paper with higher population projections, based on a new model; note that this also affects statements in the executive summary and in the introduction. (Richard Tol, Hamburg University)	Noted, reference will be added (page 25, line 38), text will be moidified deleting the word "only".
3-143	A	15	9	15	38	After the previous section clearly indicated that more recent population projections are significantly lower that used in SRES, it is useless to show a comparison (text + fig 3.3) in which the impact of that is masked by the fact that many post-TAR scenarios still use the SRES population assumptions. Replace this with a real comparison between scenarios with old and with new population projections. (Bert Metz, IPCC)	Accepted, some discussion on the implications of updated populations scenarios will be added
3-144	A	15	11			Section 3.2.1.1.3 would be helpful to note by how much the range in emsiins would change from lower POP, keeping everything else the same (plus argument why the latter is not very plausible) (Tom Kram, MNP)	See 3-144
3-145	A	15	36		38	The alternative to developing a new A2 scenario is to eliminate it or modify it because it is outside the range of authoritative estimates. Another alternative is to develop a new set scenarios based around lower level population growth assumptions. Having projections at both the upper and lower end of authoritative population estimates provides some sensitivity of the climate change models to different assumptions on population estimates. (Dennis Trewin, 0)	Rejected, going beyond the scope of the section
3-146	A	15	38	15	38	, and a set of …. 2004). Note, the study of Hilderink has never been used for the population projections of the IMAGE IPCC SRES scenarios, other IMAGE/integrated assessment scenario, therefore maybe less relevant to mention here. (Michel den Elzen, The Netherlands Environmental Agency)	Rejected, we think reference is relevant
3-147	A	16	0	19	0	I endorse the view that choice of PPP or MER as a basis for conversion, with no change to assumptions about income convergence should be neutral in a properly specified model. I propose to write a submission to this effect for the inquiry currently being undertaken by Lord Stern in the UK. (John Quiggin, University of Queensland)	Noted
3-156	A	16	0	17		There is way too much information that is only of academic interest or more suitable for a textbook on the subject than is needed in a chapter that is supposed to be a review of the state of the literature since 2000. A mention of the debate over PPP and MER MIGHT be necessary but it does not call for the amount of text	Accepted, text will be shortened

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						given to it. (Jeff Price, California State University, Chico)	
3-148	A	16	4			should read "data ARE" (Danny Harvey, University of Toronto)	Accepted
3-149	A	16	10	16	11	What is "purchasing power parity index"? If this is different from purchasing power parity itself, a definition should be given. Using "the observed market exchange rate in a fixed year" is an option to convert from one monetary unit to another when it is in constant price (if it is in current price, it does not have to use a fixed year exchange rate). Somewhere in the paragraph "constant prices" has to be mentioned. Also, it seems that the current standard practice of converting GDP in constant prices from national currency to international dollar appears to be using PPP in a fixed year (for example, IEA's statistics, Maddison statistics etc), given that there is no statistics except PWT which publishes PPP based GDP in constant prices. (Asami Miketa, International Atomic Energy Agency)	Accepted
3-150	A	16	11			The Box makes reference to different set of PPP rates among the institutions. The differences are not great. All four could be regarded as authoritative. It would be difficult to be criticised if the World Bank rates were used. (Dennis Trewin, 0)	Noted, will review literature on ppp differences and change text accordiongly
3-151	A	16	15		18	Please provide references. Please link this discussion to the one on convergence. (Richard Tol, Hamburg University)	Accepted Reference to the paper including the statistical analysis to be added (Nakicenovic et al., 2006). Rejected, the number of PPP scenarios is not relevant in the context of convergence
3-152	A	16	19	17	28	This section underplays the difference between SRES assumptions for economic growth rates and the realised and recently projected numbers. First of all, the section does not make clear what the difference is with the economic growth realised (the period 1990- 2005). This would show, I think that particularly for Africa (do not hide this fact by only referring to the ALM region) SRES assumptions were much higher than reality (of course this does not have much impact on global GHG emissions). Second, when comparing SRES assumptions with more recent scenario study assumptions these newer ones tend to be lower, and particularly for Asia and Africa (I presume). This can also be seen from figure 3.5 (add figures with growth rate numbers, because much of the text is expressed in those terms). The remark that DoE high 2003 was the same as SRES high is strange, since DoE 2004 is lower; drop that remark. The section does not analyse	Rejected, doing historical analysis goes beyond the scope of the section, Noted, problems of the ALM region are already discussed (going more spatially explicit levels not possible since not included in SRES) Accepted, DOE 2003 will be removed Accepted, will add a discussion on potential implications for emissions

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						what the impact of these lower growth assumtions could be (as for section on population: show the difference for recent (lower) groth rates with SRES). (Bert Metz, IPCC)	
3-153	A	16	22			should read "data SUGGEST" (Danny Harvey, University of Toronto)	Accepted
3-154	A	16	30			Figure 3.4: It is confusing that the figure shows abolute values and the discussion is in terms of growth rates. Also, the units on the vertical axis suggest that the graphs show GDP per capita rather than total GDP. (Leo Schrattenholzer, IIASA)	Accepted, Y-axis will be changed – will add growth rates to left-hand bars
3-155	A	16	30	0	0	The units in Figure 3.4 seem odd (thousands US$(1990)). What are the lines showing? GDP/capita? (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	See 3-154
3-157	A	17	7	17	7	Qualify 'global economic growth rates': Average over which time period? (Leo Schrattenholzer, IIASA)	Noted, will add timeframe
3-158	A	17	7	17	7	Add 'in press' after 'O'Neill'. (Leo Schrattenholzer, IIASA)	Accepted
3-159	A	17	13		14	My interpretation of the graph is that scenario A1 is well outside the US DOE high growth projection. The cumulative effect is apparent from Figure 3.5 which only goes to 2030. The economic growth assumptions in A1 appear unrealistic. This may be due to the economic convergence assumption and/or the use MERs rather than PPPs for the 1990 based period. (Dennis Trewin, 0)	Noted, will modify text accordingly
3-160	A	17	16		26	The phenomena described in this paragraph are due to the (inappropriate) use of MERs rather than PPPs in the 1990 base for economic projections. The use of MERs will, in a relative sense, increase the size of the more industrialised economies (eg OECD) and decrease the size of the less developed economies (eg ASIA, ALM). The higher starting point for the OECD economies, for example, will lead to lower growth rates. The converse effect will happen for those economies with lower starting points. This problem could be largely solved by using PPPs to adjust the 1990 base and using realistic economic growth assumptions for the various regions. (Dennis Trewin, 0)	Noted, will discuss proposals in the literature of handling PPP.
3-161	A	17	16	17	26	Some more background on differences, where do they come from? What subregions differ the most? (Tom Kram, MNP)	Noted, sentence will be deleted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-162	A	17	28			Figure 3.5: Again confusing for the same reasons. (Leo Schrattenholzer, IIASA)	Accepted, will change caption accordingly
3-163	A	17	28	18	45	section 3.2.1.2.2 Critique of the use of MER in the SRES. There a lot of text about why models should use PPP and very little why to use MER. (Peter Kolp, IIASA)	Noted, will improve ballance
3-164	A	17	28	18	45	section 3.2.1.2.2 Critique of the use of MER in the SRES. There a lot of text about why models should use PPP and very little why to use MER. (Peter Kolp, IIASA)	See previous comment
3-165	A	17	28	0	0	Are the data in constant prices. e.g. $(1990)US? (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Accepted, will improve caption
3-166	A	17	30	18	45	Thissection (3.2.1.2.2) is couched as confined to the MER/PPP controversy, especially pitting Professor David Henderson against the MER-based work in SRES. However, Henderson in his contribution to the Quarterly Newsletter of The Royal Economic Society, and elsewhere, has criticised the treatment of economic issues by the IPCC on a broader front and these criticisms should be addressed on that broader front. Richard Tol, Chris Judge, and Defra - together with Nebojsa Nakicenovic - have countered Henderson's criticisms on the MER/PPP front and Terry Barker on some others.(See UK House of Lords Select Committee on Economic Affairs: "The Economics of Climate Change", July, 2005, especially Vol.II). But somewhere the more all-embracing criticisms could be addressed. This source could usefully be cited to demonstrate that IPCC authors AND reviewers take such comments seriously! (Michael Jefferson, World Renewable Energy Network/Congresses)	Decided to keep material in Ch3
3-167	A	17	30			Section 3.2.1.2.2 does not say much about the important issue, raised in p.17/l.53, of whether using MER based scenarios are reasonable or believable. The next session reports on different views and lack of consensus, probably not adequate to answer concerns raised. (Tom Kram, MNP)	Rejected, the literature does not give a coherent answer to whether to use MER or PPP.
3-168	A	17	30	18	45	Discussion of publications that show different results for emissions depending on use of PPP or MER metrics (in particular McKibbin, but maybe others) is not correct. The suggestion that McKibbin has not calibrated his relationships correctly is wrong. He has used a very different method for calculating long-term growth. His point is that the growth and convergence calculations used by the SRES models is the wrong approach (in which you don't find problems with PPP vs MER). This needs to be analysed properly. Also other publications on the PPP-MER issue	Noted, the text will be revised, and references will be added

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						warrant better analysis. There are also publications on this issue that have not been mentioned. (Bert Metz, IPCC)	
3-169	A	17	31	17	31	Spell oiut MER the first time it appears. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted
3-170	A	17	33	17	35	The European Commissions "World Energy, Technology and Climate Policy Outlook 2030" using the POLES model is based on PPP as well see http://europa.eu.int/comm/research/energy/pdf/weto_final_report.pdf (Peter Russ, IPTS, Joint Research Centre, European Commission)	Accepted, will add reference
3-171	A	17	35	17	35	You can refer to our most recent work, World Energy Outlook 2005, instead of WEO2004. (Fatih Birol, International Energy Agency)	Accepted, will add reference
3-172	A	17	40	17	40	per capita incomeinstead of per capital (H-Holger Rogner, IAEA)	Accepted
3-173	A	17	45	17	45	The use of PPP data would not be impossible if PPP data is available, which is the case. It does not make sense that the lack of projections prevents use of the PPP in the models. (Asami Miketa, International Atomic Energy Agency)	Noted, will rephrase text
3-174	A	17	45		46	It is not impossible to use PPP data in the way I have described above. I am not familiar with how the economic models work but application along the lines suggested by Nordhaus (2005) should go a long way towards addressing criticisms of the non-use of PPPs. (Dennis Trewin, 0)	Noted, will rephrase text
3-175	A	17	46		48	This is unbalanced. You say that Castles and Henderson criticized, SRES people dismissed that criticism (true so far) and then you continue by saying that yet other people looked at this as well, but you do not tell the reader what they conclude. Besides, you miss the works of Dixon/Rimmer (forthcoming Energy & Environment), Nordhaus (cond. accepted, Energy Economics), and Tol (forthcoming, Climatic Change), who, like McKibbin, lean against SRES. Smith et al. (2005, Climatic Change) show that PPP or MER has an effect on sulfur emissions. (Richard Tol, Hamburg University)	Noted, will add references
3-187	A	18	0			Your discussion of MER versus PPP reaches the right and very clear and insightful conclusion in lines 28-30. The whole discussion started by Castles and Henderson is overdone! (Sjak Smulders, Tilburg University)	Noted

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3-176	A	18	11			"data ARE" (Danny Harvey, University of Toronto)	Accepted
3-177	A	18	13	18	13	According to my experience, most people understand the difference between PPP and market exchange rates for a given year. The situation is different when talking about the difference between the two in the course of time. In particular, it appears that "constant PPP" has not been defined satisfactorily. I don't think that the AR4 is the right place for doing this, but I propose to at least briefly summarize the definition of the Törnqvist index here. (Leo Schrattenholzer, IIASA)	Rejected, beyond scope
3-178	A	18	14			Timmer (2005) is not peer-reviewed. If IPCC rules do allow you to quote Timmer and Riahi, then what happened to Tol's presentation at the same conference? If you can cite presentations, then why not cite Dale Simbeck's 4 fingers description of SRES? (Richard Tol, Hamburg University)	Accepted
3-179	A	18	14	18	14	No reference to Timmer 2005. (Asami Miketa, International Atomic Energy Agency)	Accepted
3-180	A	18	14			I have not heard of Timmer so cannot comment on his competence. But Nordhaus is an internationally recognised economist. Timmer's work is not included among the references so I cannot assess whether it has been peer reviewed by economists. (Dennis Trewin, 0)	Noted, reference will be removed
3-181	A	18	29	18	30	Nevertheless, there are may be some discrepancies between scenarios built on market exchange rate and scenarios built on PPP. These discrepancies may be explained by the persistent differences between long run MER and PPP due to different share of non tradable sectors in different economies. (Alexander Golub, Environmental Defense)	Noted
3-182	A	18	32		34	The report must address the criticism of the non-use of PPPs or its validity will continue to be criticised. I have suggested a practical way of doing this in my comments. An alternative is to recognise the problem but use the work of Manne and Richels and McKibben et al to make an empirical assessment that, because of counteracting influences, the effects may not be great. I have also seen a Norwegian study making similar conclusions. But it suggests a small upward bias in emissions. To improve the credibility of the report, it would be best to recognise this and quantify it even if it is small. (Dennis Trewin, 0)	Rejected, text also reflects the points made by the comment
3-183	A	18	44			"at most only mildly" this is quite exagerated; the modelling teams that did go through the bother of recalibrating their model to PPP did find changes in	Accepted, will change wording ;Second comment is wrong: Tol does not reject the “no

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						emissions, sometimes large, sometime small; other modelling teams (IMAGE, IIASA) did not recalibrate but argue on first principles, or as Tol (forthcoming, Climatic Change) argues, on a misinterpretation of first principles (Richard Tol, Hamburg University)	change" vision in his paper.
3-184	A	18	47	19	17	When we consider energy use as a driver, along with mentioning the emerging energy systems we also need to mention energy supply endowments and likely future costs of fossil fuels vis a vis cleaner fuels. (Junichi Fujino, National Institute for Environmental Studies)	Accepted, will change text accordingly -
3-185	A	18	47	19	18	In this section on energy use projections the confusion created in the sections on population and growth rates (not clearly shown what the difference is between the newest assumprtions and SRES) accumulates. There is no clear distinction between post-TAR scenarios using lower population and lower growth rates with the SRES results. Fig 3.6 only shows the difference between SRES and post-SRES (in which many scenarios still used the SRES assumptions). (Bert Metz, IPCC)	Accepted, sentence will be added whether new scenarios include lower population and economic projections
3-186	A	18	53	18	53	Say whether 'highest' is meant absolute or relative (to GDP or energy use) (Leo Schrattenholzer, IIASA)	Rejected, text is clear
3-188	A	19	14			The author mentioned "the median is now somewhat lower". What's the main reason for this? Is it mainly because projections on population are now lower. (Tieju Ma, International Institute for Applied Systems Analysis)	See 3-185
3-189	A	19	20	21	10	This section on land-use describes many new studies on projected land-use, suggests that these are quite different from SRES, but lacks a discussion on the significance of these newer assumptions on the emissions projections (for instance by looking at the land-use contributions in SRES and estimating the possible impact of these different land-use projections, taking into account the lower population projections and lower economic growth projections). Table 3.1 is not very useful; it raises more questions than it answers. table can best be deleted and major factors discussed in the text. (Bert Metz, IPCC)	Accepted – more discussion summarizing changes in driver trends/expectations and modeling is appropriate for since-SRES modeling. Emissions implications can be addressed in Sec. 3.2.2.2. Table 3.1 will be reconsidered for modification or removal.
3-190	A	19	22	19	25	Land use is driven not only by market forces but also by non-market forces for preservation and conservation. The establishment of a new park or reserve can not be considered to have been driven by market forces unless it were part of a CDM development. (Jeff Price, California State University, Chico)	Accepted – non-market values should be noted
3-191	A	20	7	20	12	It may not be categorically correct to assume only growing consumtion of livestock. It would be useful to mention the other view point of the growing trend	Noted – assume commenter meant "vegetarianism." Will discuss incorporation

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						of egetarianism as societies become more well off. Under sustainable scenarios (B2) such a trend may be more visible. (Junichi Fujino, National Institute for Environmental Studies)	with other authors.
3-192	A	20	12	20	12	Typo error. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted
3-193	A	20	25	20	29	The authors should verify that there statements are in line with other writing teams both in WG3 (Ch 9) and in WG2 (Jeff Price, California State University, Chico)	Noted – agree needs to be done with WG2 and WG3 Ch8 and Ch9
3-194	A	20	39	20	40	The increase in agricultural acreage will also be for biofuels and that the statements mesh with those made in other chapters in WG 2 and WG 3 (some of which say things different than stated here).. (Jeff Price, California State University, Chico)	Will be taken into account with previous comment (3-193)
3-195	A	20	49	21	10	Consider also Moreira, 2005 Global biomass energy potential. Mitigation and Adaptation Strategies for Global Change(Special Issue, forthcoming).. for this discussion. In particular, consider the coupling of biomass-based energy and carbon capture and storage as a negative source of CO2 emission. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Noted – will consider Moreira (2005) – thank you. Negative emissions biomass/CCS being considered currently very briefly in Sec. 3.3.3.4. We plan to revisit following review of Moreira and possibly other literature.
3-196	A	21	21	22	38	this section (in lines 23-34 on page 21) compares TAR+pre-TAR with post TAR non-intervention scenarios (shown in fig 3.8). That is the wrong comparison. I would expect a comparison between SRES and post-SRES (using the most recent population and most recent growth rate assumptions; at least not using the SRES assumptions for these drivers). That would give an impression about the most recent emissions range compared to SRES (and that is what this assessment is about). The analysis presented in lines 36-43 on page 21 could then become more than speculation. I realise this would require an in-depth assessment of individual scenario studies, but that may be the only way to get real insights. (Bert Metz, IPCC)	Accepted, text will be revised – there are unfortunately too few scenarios that use new revised population assumptions thus a statistical analysis of post-TAR scenarios is not possible
3-197	A	21	27		28	Are there grounds for eliminating those scenarios? (Dennis Trewin, 0)	No.
3-198	A	21	37		38	This opens the work up to criticism. Shouldn't there be some adjustment for the lower demographic projections that authoritative bodies have now published? (Dennis Trewin, 0)	Noted, text will indicate that most of the recent emissions scenarios did not incorporate the new popuation projections yet.
3-199	A	22	10	22	10	In figure 3.9, it would be interesting to highlight why such important differences are observed in 1990 since these differences in 1990 explain most of the differences along the whole trajectories. (Peter Wittoeck, Belgian Federal Administration)	Accepted, text will be added

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3-200	A	22	12	22	36	These paragraphs are difficult to understand. What are "Three baseline emissions projections"(line 13)? Is fig 3.10 left panel showing the range of different model baselines using a best guess baseline? Then say so more clearly. SRES is a multiple baseline scenario set. Can't that be said simpler? Is fig 3.10 right panel supposed to show the ranges based on probabilistic studies? If so, then show these ranges more clearly (hard to see now). On this latter point: why don't we see a discussion on the high end of the range of the probabilistic scenarios (that goes much beyond the A2 SRES line). Why is this section not connected to the discussion on page 21 lines 23-34? (Bert Metz, IPCC)	Noted, text will edited for clarity and streamlined
3-201	A	22	14	22	15	Consistent with the comment above, I would suggest inserting the words "or 'reference scenarios' "after the word "baseline", and the words "and behaviour" after the word "trends" at the end of line 14; insert the words "sometimes with alternatives based on changing a limited number of exogenous variable or parameters," after the word "continue", insert the word "comprehensively" before the word "different" all in line 15. (Kenneth Ruffing, N/A)	See 3-200
3-202	A	22	32	22	32	I think the characterisation in this paragraph would be more accurate if the following sentence were added: "However, by most measures the range of the newer scenarios is somewhat narrower; many are characterised by lower population projections than in A1 or A2, lower developing country growth rates than in B1, and lower carbon and/or energy intensities. (Kenneth Ruffing, N/A)	See 3-200
3-203	A	22	40	23	42	The remark "" Although the SRES effort was ……, the treatment of land-use was poor in terms of the modelling of land-use drivers …." should have been made already in section 3.2.1.4. Aren't there any other baseline scenario studies, even not for the MA? The recent IMAGE results discussed in lines 51 on page 22 to line 10 on page 23, are not compared with SRES results, meaning no assessment is made of the question how good the SRES scenarios are in terms of land-use contributions. The paragraphs in lines 13-42 on page 23 don not provide a clear picture on the recent projections of land-use emissions, and are not linked to the overall land-use emissions quoted in the first part of the section. This leaves the reader with a lot of question marks. (Bert Metz, IPCC)	Accepted – all excellent points. Better discussion of newer projections is forthcoming, at which point section consistency can be addressed.
3-204	A	23	13	23	26	The discussion on the possible, yet uncertain and contested, future changes in terrestrial carbon stocks as a reult od climate change (e.g. leading to massive forest	Accepted – will be incorporated

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						die-back and associated carbon releases) warrants at least a few words here (Tom Kram, MNP)	
3-205	A	23	44	25	22	It would be useful to mention the major issues in estimating Non CO2 gases especially in developing countries. Also some studies show mitigation opportunities for Non CO2 gases. Some are in traditional forms (especially in developing countries for Methane) while in developed countries many technologies are being applied in large scale projects. (Junichi Fujino, National Institute for Environmental Studies)	Accepted – will include short description on major issues in estimating Non CO2 gases
3-206	A	23	44			Section 3.2.2.3 for the first time introduces the non-CO2 GHGs. Yet this is arguably amongst the most interesting and eagerly awaited innovations in the literature. It is recommended to introduce this more prominently and earlier in the chapter. (Tom Kram, MNP)	Accepted – will add short introduction in Sec 3.1.2
3-207	A	23	44	25	22	A problem with the methane section is that a comparison is presented of emission scenarios from EMF21 and SRES, but at the same time it is said that these results are incomparable (EMF uses single baseline, SRES multiple baseline approach). So, how should one interpret this. What is the conclusion w.r.t validity of SRES results for future studies?. Same problem exists for N2O. For both the question is if the newer insights in population growth and land-use change have an effect on emissions projections?(not discussed) (Bert Metz, IPCC)	Noted – could provide additional description on differrces in baselines, both EMF21 and SRES but this will subject to space constraints.
3-208	A	24	6	24	8	non-CO2 and CO2 emissions cannot be made comparable with GWP, because the GWP is a simple, one-dimensional index that cannot capture the time-evolving differences between the two. I would reword as: "In order to aggregate CO2 and non-CO2 emissions, the GWP is commonly used in spite of the fact that no single index can properly combine the effects of non-CO2 and CO2 emissions" You can cite a couple of papers by Smith and Tom Wigley in Climatic Change around 2000 or 2001 in this. (Danny Harvey, University of Toronto)	Rejected / Noted – practice is to use SAR GWPs for reporting in CO2 eq. Could report data in gas specific units to avoid GWPs.
3-209	A	24	9	24	10	The information on non-CO2 emissions said to be in Table 3.2 is not in that table nor any of the other tables attached to this draft. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Accepted – will include proper table.
3-210	A	24	9	24	15	Is this table really necessary? All I see is a list of sectors with no emission estimates at all. (Jeff Price, California State University, Chico)	Accepted – will include proper table.
3-211	A	24	11			I'm not sure that GWP are "common practice" as I have not counted; nor have you,	Rejected – the common practice for reporting

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						it seems. Anyway, whether common practice or not, GWPs are wrong. (Richard Tol, Hamburg University)	equivilant CO2 emissions is from UNFCCC and Kyoto Protocol.
3-212	A	24	29	24	29	"are do" change to "are due' (H-Holger Rogner, IAEA)	Accepted.
3-213	A	24	40	25	22	Is this materrial better found in WGI or is it really the job of this chapter to discuss it? (Jeff Price, California State University, Chico)	Rejected – emissions projections are the job of Chap 3.
3-214	A	25	5	25	15	Please, make reference to the recent IPCC Special Report on Fluorinated Gases. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted.
3-215	A	25	26	27	18	The discussion of SO2 and NOx projections lead to conclusion"s that do not seem to match the figures: :For SO2 it is "peak lower and earlier", while it also seems that the long-term decline of SRES is not supported by newer literature. for NOx: "shifted downward, particularly in short term", but the figure suggest lower emissions in the long-term, not the short (Bert Metz, IPCC)	Accepted, Figures will be modeified to give the individual scenarios of Smith et al.. In addition text will be modified accordingly.
3-216	A	25	29	25	29	p. 25. L. 29. Aerosols not only change radiative forcing but also impact cloud and precipitation patterns. This should be reminded here and a reference to the relevant sections in WG I and WG II reports might also be helpful. (Philippe Tulkens, TERI School of Advanced Studies)	Accepted, text will be added
3-217	A	25	37			Given China alone has accounted for about 80% of the World's increase in coal use since 2000, on the back of revived economic growth rates, the reference to growth rates having declined considerably recently appears to have little real meaning. (Michael Jefferson, World Renewable Energy Network/Congresses)	Rejected, the growth rates refer to sulphur emissions, which have declined considerably.
3-218	A	25	50	25	50	Re: footnote 5 -- " ….emissions from these sources have been added from xx to the original Cofala et al. values." What is xx? (Lourdes Maurice, US Government)	Accepted, number will be added
3-219	A	26	40	26	41	It is mentioned - as I read it - that NOx emissions from transport will still increase. However, the projections of the WBCSD Sustainable Mobility Project show an ongoing decrease in OECD regions (please see page 38 of the Mobility 2030 report) and a decrease in Non-OECD countries by 2015 (please see page 40 of this report). This is similar for CO, VOC and PM-10 emissions. - literature: WBCSD 2004: Mobility 2030 - meeting the challenges to sustainability. (Stephan Herbst, Toyota Motor Europe)	Noted, reference will be reviewed
3-220	A	27	6			this is a reference to mitaigation cases; doesn't bellong here (Bert Metz, IPCC)	Rejected, The reference is on Nox mitigation, which is also part of baselines
3-221	A	27	20	29	9	what is missing in this section to draw conclusions on the most reasonable baseline	Accepted, conclusions on the relevance of the

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						scenarios for OC/BC, while the analysis seems to indicate that the Rao and Streets projections are more relevant than the Liousse ones. This is what the reader expects from an assessment (Bert Metz, IPCC)	alternative studies will be added
3-222	A	27	52			Table 3.3 lacks unit, What is global here? (Peter Bosch, IPCC TSU WGIII)	Accepted, text will be added
3-223	A	28	54	28	55	It is not likely that the climatic effects of non-absorbing aerosols (such as carbonaceous aerosols) are closely related to the forcing pattern. Rather, the spatial pattern of impacts from such aerosols is almost identical to that associated with CO2 increases (although opposite in sign). This is because the spatial patter depends on regional climate feedback process that are triggered by overall climatic change, not by the regional forcing. I showed this in a comrpehensive examination of the patterns of temperature change associated with CO2 and aerosol only in 8 different coupled atm-ocean GCMs. Reference: Harvey, L.D.D. 2004. Characterizing the annual-mean climatic effect of anthropogenic CO2 and aerosol emissions in eight coupled atmosphere-ocean GCMs. Climate Dynamics 23:569-599. (Danny Harvey, University of Toronto)	Noted, will forward this info to WG1 as it focuses on climate implications that are beyond the scope of this chapter
3-224	A	29	3	29	5	This sentence does not make sense to me. I think that the issue should be the efficacy of emission reductions (not emission projections) in reducing climatic change (rather than in formulating reliable policy recommendations). (Danny Harvey, University of Toronto)	Accepted, text will be edited
3-225	A	29	11	29	52	In this concluding section the messages need to be changed in conformity with a different type of analysis as suggested in the comments on the section on baselines:1) emissions of new scenarios with lower population and lower economocic growth need to be compared with SRES; 2) conclusions about the areas where SRES growth assumptions were too high; 3) conclusions about the new insights in land-use baseline scenarios compared to SRES; 4) conclusions about NOx, SO2 and OC/BC baseline scenarios in line with text (and NOT saying new parameterisations/ models for NOx are needed because that is not in the respective section); 5) (and probably the most important conclusion that is now missing) what is the conclusion with respect to using SRES baseline scnarios in policy analysis or other uses; it seems the literature indicates that the storylines of SRES can be used and the modelling does not have to change, but for quite a few parameters better values need to be used (in other words: using the original SRES quantifications is not longer recommended) (at least that sounds more logical based on the material	On 1) see 3-196 On 2) Accepted On 3) Conclusions on land-use will be added On 4) Accepted On 5) Accepted, will add some discussion of the validity of SRES.

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						presented). (Bert Metz, IPCC)	
3-226	A	29	15	29	16	I think the characterisation in this bullet would be more accurate if the following changes were made: insert the words "the majority of" before the word "scenarios" in line 15; replace the words "not to have changed" with the words "to have narrowed somewhat." (Kenneth Ruffing, N/A)	Accepted
3-227	A	29	27		30	I think this understates the difference in economic growth rates. The problem with A1 scenarios which are well outside the range provided authoritative commentators. (Dennis Trewin, 0)	Accepted, will add additional discussion
3-228	A	29	34			It is not an emerging debate in the economic literature! (Dennis Trewin, 0)	Rejected, the use of ppp in economic modeling is subject to debate
3-229	A	29	35			"A number of studies ... sust small" This is misleading. (Richard Tol, Hamburg University)	Accepted, will rephrase
3-252	A	30	0			Figure 3.15: The caption does not explain what the two grey rectangles on the bottom stand for. (Hans-Martin Fuessel, Stanford University)	Accepted, will add explanation
3-253	A	30	0			first two paragraphs: I don't see how one can argue that there is a choice what what stabilization target to use in the cause effect chain. The UNFCCC clearly states that its goal is to stabilize GHG CONCENTRATIONS at a safe level. To determine a safe concentration, one does not need to know what the correct climate sensitivity is; rather, one needs to a know a plausible upper bound to climate sensitivity (how plausible depends on the risks one is willing to impost on future generations). Thus, if 2 C is adopted as the maximum possible safe temperature change (most estimates fall between 1 C and 2 C), and if 4 C is accepted as a climate sensitivity with a 10% chance of being equalled or exceeded (some estimates indicate a much greater probability), and if one is prepared to run a 10% risk of major harm to future generation, then it immediately follows that GHG concentrations corresponding to half that of a CO2 doubling represent the allowable threshold. Since we have already exceeded this forcing, and since a tighter climate damage threshold or a lower risk tolerance will lower further, where is the uncertainty on what we should do if we are to comply with the UNFCCC? (Danny Harvey, University of Toronto)	Rejected, alternative interpretation of climate risks is possible
3-230	A	30	1	47	19	You might want to include the box on IEA's World Alternative Policy Scenario. The World Alternative Policy Scenario is an internally consistent scenario, deriving	Accepted, will be added to 3.6

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						conclusions from stated assumptions, within a rigorous modelling framework.The World Alternative Policy Scenario analyses the impact of a range of potential government policies and measures which are already being contemplated, but are not yet adopted. They aim to address energy-security and environmental concerns in all regions. Global primary energy demand in 2030 reaches 14.7 billion toe – 1.6 billion toe, or about 10%, less than in the Reference Scenario. At 1.2% per year, the average annual rate of demand growth is 0.4 percentage points less than in the Reference Scenario. The effect of energy-saving and fuel diversification policies on energy demand grows throughout the projection period, as the stock of energy capital is gradually replaced and new measures are introduced.Oil and gas demand in the Alternative Policy Scenario are both about 10% lower in 2030 than in the Reference Scenario. Coal use falls much more, by 23%, due to lower demand in power generation: the use of more efficient technology reduces the demand for electricity, and generators choose to use more carbon-free fuels. On the other hand, the use of non-hydro renewables, excluding biomass, is 27% higher in 2030 than in the Reference Scenario. Biomass and nuclear energy also grow. Most of the net increase in renewable use results from OECD government policies aimed at promoting their use in the power sector and in transport. Lower overall energy consumption and a larger share of carbon-free fuels in the primary energy mix yield a 16%, or 5.8 gigatonnes, reduction in global carbondioxide emissions compared with the Reference Scenario. (Fatih Birol, International Energy Agency)	
3-231	A	30	1	30	40	Duplication: Dealt with in Chapter 1 (H-Holger Rogner, IAEA)	Noted, discuss overlap with chapter 1
3-232	A	30	1	45	45	Section 3.3. The results of the Innovation Modeling Comparison Project are now available for consideration by the authors in this chapter and of greatest relevance to this section. The main results are covered in Edenhofer, O., et al. (2006). "Induced Technological Change: Exploring its Implications for the Economics of Atmospheric Stabilization." Energy Journal (Special Issue: Endogenous Technological Change and the Economics of Atmospheric Stabilization), but several of the individual papers in this Special Issue also carry additional points that may of interest. (Michael Grubb, Cambridge University)	Accepted
3-233	A	30	7			Section 3.3.1. I miss quite the studies of multi-gas and CO2 only emission pathways leading to stabilization of GHGs concentrations or climate targets. Below I give some suggestions how to include these, but in general I would expect in this	Noted, will check references for inclusion into the chapter

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						section a paragraph briefly describing the main findings of these studies (some even include costs estimates). For example (CO2-only emission pathways): O'Neill, B. C. and Oppenheimer, M., 2004. Climate change impacts are sensitive to the concentration stabilization path. PNAS, 101(47): 16411-16416; Wigley, T.M.L., Richels, R. and Edmonds, J.A., 1996. Economic and environmental choices in the stabilization of CO2 concentrations: choosing the "right" emissions pathway. Nature, 379: 240-243; Wigley, T. M. L., 2003. Modeling climate change under no-policy and policy emissions pathways, Benefits of climate policy: improving information for policy makers. Organization for Economic Co-operation and Development (OECD), Paris, France; And studies of multi-gas emission pathways based on MACs: den Elzen, M.G.J and Meinshausen, M., 2005. Meeting the EU 2 C climate target: global and regional emission implications. MNP-report 728001031 (www.mnp.nl/en), Netherlands Environmental Assessment Agency (MNP), Bilthoven, the Netherlands. Den Elzen, M.G.J and Meinshausen, M., 2006. Multi-gas emission pathways for meeting the EU 2 C climate target. In: H.J. Schellnhuber, W. Cramer, N. Nakicenovic, T. Wigley and G. Yohe (Editors), Avoiding Dangerous Climate Change. Cambridge University Press, Cambridge, UK. Other multi-gas studies: Meinshausen, M., 2006. What Does a 2 C Target Mean for Greenhouse Gas Concentrations? A Brief Analysis Based on Multi-Gas Emission Pathways and Several Climate Sensitivity Uncertainty Estimates. In: H.J. Schellnhuber, W. Cramer, N. Nakicenovic, T. Wigley and G. Yohe (Editors), Avoiding Dangerous Climate Change, Cambridge, UK. Meinshausen, M., Hare, W.L., Wigley, T.M.L., van Vuuren, D.P., den Elzen, M.G.J and Swart, R., 2005. Multi-gas emission pathways to meet climate targets. Climatic change, in press. There are even more studies about emission pathways. (Michel den Elzen, The Netherlands Environmental Agency)	
3-234	A	30	7	31	13	This is a very useful discussion of targets as used in mitigation scenarios. There is ambiguity in this section, however, as to what is the use of the "targets" being considered. Is the target the objective of a "policy architecture" or is it the objective of a mitigation scenario. I suggest that its use is here is the latter, and that this be made clear in this section. Use of policy targets and their corresponding policy architecture raises additional considerations that are not sufficiently discussed in this section to support the former. This is alluded to in lines 46 to 50, however, the generalization proposed in this section appears much more idealized than the actual policymaking process.	Accepted, will add a discussion with respect to long-term targets vis-a-vis short-term targets

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						(Haroon Kheshgi, ExoonMobil Research and Engineering Company)	
3-235	A	30	7	31	13	Suggest that this section consider a broader set of objectives than only stabilization targets, consistent with Art 2 of the UNFCCC and considerations of the impacts of and adaptation to climate change. The pace of climate change is an important factor that should be considered in studying objectives. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted, add brief text, but overall focus will remain stabilization
3-236	A	30	7			Section 3.3.1. (notably ll.30-40) mix the issue of stabilization metrics and associated uncertainties with the recent attempts at introducing risk-based target seting. As these are largely unconnected, it is suggested to remove the risk paragraph here and add it to discussion of targets setting as a policy process. (Tom Kram, MNP)	Accepted, paragraph on uncertainty will be moved to 3.6 and cross-referenced
3-237	A	30	9			"UNFCCC call", maybe it should be "UNFCCC's call". (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted
3-238	A	30	16	30	16	"Emission stabilization". Is this a misprint for concentrations stabilization? Cf. line 12 above. Due to the long lifetime of many climate gases this is of course very different. (Terje Berntsen, CICERO)	Accepted
3-239	A	30	20	30	20	Is the UNFCCC, 2002 really the first study mentioning this? It would be better to mention a study that tries to quantify these uncertain and impacts effects along the cause-effect chain. Maybe this has already be done in the earlier IPCC scientific assessments (Michel den Elzen, The Netherlands Environmental Agency)	Noted, will ask reviewer for references
3-240	A	30	20	30	20	Is Matthews and van Ypersele (difficult to find) really the first study mentioning this. Here, again mention also the earlier IPCC scientific assessments, or other studies analyzing the impacts of the uncertainties along the cause-effect chain. I know some studies in the early 1990s that conclude the same. (Michel den Elzen, The Netherlands Environmental Agency)	Noted, will ask reviewer for references
3-241	A	30	22	30	22	p. 30. L22. The reference given for Matthews and van Ypersele (2003) seems incomplete and does not indicate whether this is peer-reviewed literature. The reference should be completed. (Philippe Tulkens, TERI School of Advanced Studies)	Noted, will ask reviewer for references
3-242	A	30	28			missing reference (Danny Harvey, University of Toronto)	Accepted, refrence to TAR will be added
3-243	A	30	30	30	40	Possible inconsistency with respect to PDFs. Here, the PDFs are not explained in detail, whereas on page 56 line 39-52, the PDFs are discussed in much more detail. (Michel den Elzen, The Netherlands Environmental Agency)	Accepted, will be solved in 3.6

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3-244	A	30	32	30	36	Reformulate (as this is not sited correctly). For example, den Elzen and Meinshausen (2005) used the probability density function of climate sensitivity by Wigley and Raper (2001), which is built to match the IPCC-TAR 1.5 to 4.5oC uncertainty range, in the MAGICC climate model (Wigley and Raper, 2001) to estimate that an emission pathway leading to a 550 ppm CO2 equivalent stabilization level has a risk of 75per cent of overshooting a limit of 2oC ΔT, a 33per cent risk of overshooting 3oC ΔT and 10per cent of overshooting 4oC ΔT. Using a second probability density function derived by Murphy et al. (2004) would imply a 100% risk of overshooting 2 oC and beyond. (Michel den Elzen, The Netherlands Environmental Agency)	Noted, details will be provided in 3.5/3.6
3-245	A	30	32			year is 2004 in reference list (Danny Harvey, University of Toronto)	Noted, reference will be checked
3-246	A	30	33	30	33	Hare and Meinshausen (2004), Reference: Hare, W.L. and Meinshausen, M., 2004. How much warming are we committed to and how much can be avoided? Potsdam Institute for Climate Impact Research (PIK), Potsdam, Germany. (Michel den Elzen, The Netherlands Environmental Agency)	Accepted, text will be added
3-247	A	30	46	30	46	It is not a model comparison, but a comparison of the different targets (Michel den Elzen, The Netherlands Environmental Agency)	Accepted, text will be re-written
3-248	A	30	47	30	47	I am not sure whether in policy making a set of targets is chosen, I think you miss some of the discussions of choosing long-term targets as being described in Chapter 13 (see also Pershing and Tudela, 2003, Corfee-Morlot and Höhne, 2003, and others). The same holds for the rest of this paragraph. (Michel den Elzen, The Netherlands Environmental Agency)	Accepted, text will be re-written
3-249	A	30	48	30	49	Unless it by far dominates global GHG emissions, how can a country set at temperature target other than for the negotiation of GHG reduction targets? Rephrase (H-Holger Rogner, IAEA)	Rejected, countries do use temperature targets
3-250	A	30	49	30	50	Suggest reformulation to specify it in more detail. In order to meet the target with at least a 50% certainty, the target is likely to be translated into maximum emission levels around 2015-2020 in order to avoid global reduction rates exceeding more than 2.5%/year, followed by substantial overall reductions by as much as 30 to 60% in 2050 compared to 1990 levels (den Elzen and Meinshausen, 2005; Meinshausen et al., 2005). Here, a 'peaking strategy' is followed, allowing concentrations to peak then decrease before stabilizing, i.e. going up to 480-500 ppm CO2-equivalent before going down to levels such as 400 or 450 ppm equivalent later on. This overshooting is partially reasoned by the already substantial present concentration	Noted, section will be re-written, and also moved to 3.5

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						levels and the attempt to avoid drastic sudden reductions in the presented emission pathways. With such a peaking strategy it is actually possible to prevent some of the temperature increase that would still occur after this peak ('global warming in the pipeline'). In this way, it is possible, to increase the likelihood of meeting the long-term temperature target, or vice versa, decrease the probability of exceeding the target. REFERENCES: den Elzen, M.G.J and Meinshausen, M., 2005. Meeting the EU 2 C climate target: global and regional emission implications. MNP-report 728001031 (www.mnp.nl/en), Netherlands Environmental Assessment Agency (MNP), Bilthoven, the Netherlands.; Meinshausen, M., Hare, W.L., Wigley, T.M.L., van Vuuren, D.P., den Elzen, M.G.J and Swart, R., 2005. Multi-gas emission pathways to meet climate targets. Climatic change, in press. (Michel den Elzen, The Netherlands Environmental Agency)	
3-251	A	30	52	30	53	In the different model-studies on stabilization targets the choice of different targets … outcomes. [To distinguish the policy-process and model-analyses] (Michel den Elzen, The Netherlands Environmental Agency)	Noted, section will be re-written, and also moved to 3.5
3-254	A	31	0	31		This could be added to the discussion of disadvantages and advantages: Aaheim et al found that the cost of using the GWPs compared optimal weights depends on the ambition of climate policies. Reference: Aaheim, H. Asbjørn, Jan S. Fuglestvedt and Odd Godal, 2005. Costs Savings of a Flexible Multi-Gas Climate Policy. Energy Journal, (In press). (Jan Fuglestvedt, CICERO)	Accepted
3-255	A	31	0			The main conclusion of section 3.2.2. could also be given briefly in the Executive Summary. (Jan Fuglestvedt, CICERO)	Accepted
3-256	A	31	0			In the discussion of GWPs one could add the following argument: As long as the target of climate policies is not defined (neither in terms of which aspects of climate change that is important (indicator), or the level or timing) then it is difficult to evaluate GWP as an emission metric and not very meaningful to say that one metric is better than the other. (Jan Fuglestvedt, CICERO)	Accepted
3-257	A	31	0			I think it would be very useful if the authors of section 3.3.2 have contact and discussions with the authors of the GWP section in the WGI report. (Jan Fuglestvedt, CICERO)	Accepted
3-258	A	31	0	31		This could be added to the discussion of disadvantages and advantages: Aaheim et al found that the cost of using the GWPs compared optimal weights depends on the ambition of climate policies. Reference: Aaheim, H. Asbjørn, Jan S. Fuglestvedt	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						and Odd Godal, 2005. Costs Savings of a Flexible Multi-Gas Climate Policy. Energy Journal, (In press). (Jan Fuglestvedt, CICERO)	
3-259	A	31	0			The main conclusion of section 3.2.2. could also be given briefly in the Executive Summary. (Jan Fuglestvedt, CICERO)	Accepted
3-260	A	31	0			In the discussion of GWPs one could add the following argument: As long as the target of climate policies is not defined (neither in terms of which aspects of climate change that is important (indicator), or the level or timing) then it is difficult to evaluate GWP as an emission metric and not very meaningful to say that one metric is better than the other. (Jan Fuglestvedt, CICERO)	Accepted
3-261	A	31	0			I think it would be very useful if the authors of section 3.3.2 have contact and discussions with the authors of the GWP section in the WGI report. (Jan Fuglestvedt, CICERO)	Accepted
3-301	A	31	0			References to EMF-21 and impacts of multigas and substitution metrics seem to be scattered through several subsections - perhaps could be consolidated more. For example, include section 3.3.2 in section 3.3.3.2, as the latter currently contains some arguments expressed in the former. (Geoffrey Blanford, Stanford University)	Rejected, inconsistent with current structure of the chapter
3-262	A	31	5			The Schaeffer result strikes me as trivial. The cost-effective way to stabilize temperature is to stabilize temperature. Any other stabilization target (e.g., radiative forcing) is necessarily second-best. (Richard Tol, Hamburg University)	Rejected, will add different reference
3-263	A	31	5	31	8	Wigley (2003) analyzed overshoot pathways that overshoot levels with 50 ppm CO2-only (for all GHGs, even more) for the 550 ppm CO2-only (about 625-650 CO2-eq. ppm) pathway, i.e. peak at 600 ppm CO2-only. These pathways lead to reduced mitigation costs, while the associated increased warming lead to a reduction in the benefits of averted climate change. Here the overshooting is reasoned to make the trade-offs between the climate risks and abatement costs. Reference: Wigley, T. M. L., 2003. Modeling climate change under no-policy and policy emissions pathways, Benefits of climate policy: improving information for policy makers. Organization for Economic Co-operation and Development (OECD), Paris, France (Michel den Elzen, The Netherlands Environmental Agency)	Noted, we will use these suggestion to re-write the text
3-264	A	31	5	31	8	The study of Schaeffer shows that such a peaking strategy also leads to costs	

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						benefits. This is the new element of this study, and is now also explored by other coming studies. Please reformulate the sentences here (Michel den Elzen, The Netherlands Environmental Agency)	
3-265	A	31	5	31	8	Schaeffer et al. (2005) is an interesting study, which unfortunately has not been published, but was not the first study about peaking and overshoot pathways as an effective way to reach concentration stabilization targets. It is better to refer here to: den Elzen, M.G.J and Meinshausen, M., 2005. Meeting the EU 2 C climate target: global and regional emission implications. MNP-report 728001031 (www.mnp.nl/en), Netherlands Environmental Assessment Agency (MNP), Bilthoven, the Netherlands. Meinshausen, M., 2006. What Does a 2 C Target Mean for Greenhouse Gas Concentrations? A Brief Analysis Based on Multi-Gas Emission Pathways and Several Climate Sensitivity Uncertainty Estimates. In: H.J. Schellnhuber, W. Cramer, N. Nakicenovic, T. Wigley and G. Yohe (Editors), Avoiding Dangerous Climate Change, Cambridge, UK. (Michel den Elzen, The Netherlands Environmental Agency)	Noted, text will be improved using suggested text.
3-266	A	31	5	31	8	O'Neill and Oppenheimer (2004) analyses overshoot pathways that even exceed 100 ppm for the ultimate concentration stabilization levels 500, 600 and 700 ppm CO2-eq., and showed that the associated incremental warming may significantly increase the risks of exceeding critical climate thresholds to which ecosystems are known to be able to adapt. Reference: O'Neill, B. C. and Oppenheimer, M., 2004. Climate change impacts are sensitive to the concentration stabilization path. PNAS, 101(47): 16411-16416; (Michel den Elzen, The Netherlands Environmental Agency)	Noted, we will use these suggestion to re-write the text
3-267	A	31	5			Schaeffer et al, 2005 does not exist (Bert Metz, IPCC)	Accepted. Reference will be replaced.
3-268	A	31	10			Section 3.3.2 is based on the premise that we have the luxury of trading off smaller emission reductions in one gas against larger emission reductions in another. Since, as argued above, we are already in violation of the UNFCCC, we do not have this luxury. This alternative viewpoint, which follows from the changes that I recommend to Section 3.3.1, should be acknowledged. If (as I believe) we are already in violation, then it is particularly important to focus now on reductions in emissions of methane BECAUSE it is short-lived - so that we can get closer to compliance. This conclusion is contrary to the suggestion made in this section that early reductions in methane have no climatic benefits which, as explained above, is not correct from a scientific point of view. (Danny Harvey, University of Toronto)	Rejected. Started of from very specific interpretation of UNFCCC.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-269	A	31	13			Section 3.3.2. should be expanded to underline the essential difference between equivalent concentration targets (one-on-one translation of radiaitive forcing) and equivalent emissions through GWPs. Here it seems to be tied exclusively to the issue of optimality, but this is an unsufficient to incomplete treatment of the topic, that is infamous for raising confusion and misunderstanding among researchers and policy analysts. (Tom Kram, MNP)	Accepted. Add footnote for more explanation.
3-270	A	31	18	31	18	Fuglesvedt is not in the references list (Michel den Elzen, The Netherlands Environmental Agency)	Accepted. Will add reference
3-271	A	31	20			"GWp", the "p" is subscribed. Is it the author's purpose or a mistyping? (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. Will correct typo.
3-272	A	31	21	31	24	There is a large body of literature discussing the GWPs and altenatives from an economic point of view. There are two short commentaries that summarizes much of this discussion in an excellent way. These could be referred to here: Godal, O.: 2003, 'The IPCC's assessment of multidisciplinary issues: The case of greenhouse gas indices', Clim. Change 58, 243–249. Bradford, D.F.: Time, money and tradeoffs. Nature, 2001, 410, 649-650. (Jan Fuglestvedt, CICERO)	Accepted. Will use references.
3-273	A	31	21	31	24	There is a large body of literature discussing the GWPs and altenatives from an economic point of view. There are two short commentaries that summarizes much of this discussion in an excellent way. These could be referred to here: Godal, O.: 2003, 'The IPCC's assessment of multidisciplinary issues: The case of greenhouse gas indices', Clim. Change 58, 243–249. Bradford, D.F.: Time, money and tradeoffs. Nature, 2001, 410, 649-650. (Jan Fuglestvedt, CICERO)	Accepted. Will use references.
3-274	A	31	24	31	25	Why would methane emission reductions have no climatic benefit just because methane has a short lifetime? Quite the opposite is the case - emission reductions would have a quick (and sustained) benefit because of its short lifetime. The magnitude of the benefit depends on the reduction in radiative forcing, which is not insignificant. Either there is an error in the cited paper, or the paper has been mis-interpreted here. (Danny Harvey, University of Toronto)	Noted. Will clarify language (take out 'appear')
3-275	A	31	26		55	The discussion misses that alternatives to GWPs are older than GWPs themselves (Eckaus, Schmalensee). It also ignores the cost-benefit approach to GWPs (Reilly and Richards, Kandlikar, Fankhauser, Hope, Tol). (Richard Tol, Hamburg University)	Noted. Add relevant references.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-276	A	31	26			There is a fundamental difference between GWPs and the "other methods" discussed here. The "other methods" requires that a long-term stabilization target is set, while GWPs do not require that (cf. GWPs in the Kyoto Protocol). This should be mentioned in the text. It is not entirely clear if the whole of section 3.3 is about stabilization scenarios, or if it is about mitigation scenarios in general. This distinction is made on page 32, lines 41-45, but should also be reflected in section 3.3.2 (Terje Berntsen, CICERO)	Accepted, will add some text
3-277	A	31	26	31	38	This section should acknowledge that the studies referred to a only looking at minimization of cost under the sole objective of stabilization. If other objectives are introduced (e.g. rate of change), then different results for, e.g., methane arise. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted, will include other reference
3-278	A	31	29	31	29	p. 31. L. 29 A proper reference should be given for EMF-21, I could not find it in the reference list. The reference is given in the text on p. 33 and 35. The text should specify that EMF-21 means Weyant and de la Chesnaye (2005). (Philippe Tulkens, TERI School of Advanced Studies)	Accepted
3-279	A	31	32	31	32	It would be useful to very briefly mention what these optimizations take into account (mitigation costs, lifetime, forcing strenght, and an exogenously given forcing ceiling). (Jan Fuglestvedt, CICERO)	Accepted
3-280	A	31	32	31	32	It would be useful to very briefly mention what these optimizations take into account (mitigation costs, lifetime, forcing strenght, and an exogenously given forcing ceiling). (Jan Fuglestvedt, CICERO)	Accepted
3-281	A	31	34			I suggest adding "and F-gases" after "for methane", since there are significant deviations also here. (Jan Fuglestvedt, CICERO)	Accepted
3-282	A	31	34			I suggest adding "and F-gases" after "for methane", since there are significant deviations also here. (Jan Fuglestvedt, CICERO)	Accepted
3-283	A	31	35			I suggest mentioning the reason for the deviation; the differences in lifetime compared to CO2. (Jan Fuglestvedt, CICERO)	Accepted
3-284	A	31	35			I suggest mentioning the reason for the deviation; the differences in lifetime compared to CO2.	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Jan Fuglestvedt, CICERO)	
3-285	A	31	38			I suggest mentioning that the shortlived have low weights in the beginning of the period that increases towards the end, and that the opposite is the case for the gases with longer lifetime than CO2. (Jan Fuglestvedt, CICERO)	Accepted
3-286	A	31	38			I suggest mentioning that the shortlived have low weights in the beginning of the period that increases towards the end, and that the opposite is the case for the gases with longer lifetime than CO2. (Jan Fuglestvedt, CICERO)	Accepted
3-287	A	31	40	31	40	Figure 3.16 difficult to read. This holds in general for some of the spaghetti-figures. (Michel den Elzen, The Netherlands Environmental Agency)	Rejected, colors help to identify major trends
3-288	A	31	42	31	42	Cost effectiveness conclusions in this section are often contingent on the idealized objectives of model studies, and may not be robust. Suggest deleting the first sentence. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Noted, will add …"necessarily" lead to….
3-289	A	31	42	31	43	neither cited paper is in the reference list (Danny Harvey, University of Toronto)	Rejected, no references in the text
3-290	A	31	44			The sentence with "what flexibility" is unclear and should be re-written. (Jan Fuglestvedt, CICERO)	Accepted, will either define the term "waht-flexibity" or delete
3-291	A	31	44			The sentence with "what flexibility" is unclear and should be re-written. (Jan Fuglestvedt, CICERO)	Accepted, will either define the term "waht-flexibity" or delete
3-292	A	31	47	31	47	Person et al. is missing in reference list. (Kenneth Möllersten, Swedish Energy Agency)	Accepted, will add reference
3-293	A	31	47			Regarding "the disadvantages of GWPs are likely to be outweighed by the advantages": In this weighing the political and practical aspects are compared to the scientific/economic aspects. Thus, it would be useful if these considerations are discussed somewhat more explicitely. (The following sentence "this can be done by..." is OK, but some more discussion would be useful.) (Jan Fuglestvedt, CICERO)	Accepted
3-294	A	31	47			Person et al 2004 can not be found on the list of references. (Jan Fuglestvedt, CICERO)	Accepted
3-295	A	31	47			Regarding "the disadvantages of GWPs are likely to be outweighed by the advantages": In this weighing the political and practical aspects are compared to the scientific/economic aspects. Thus, it would be useful if these considerations are discussed somewhat more explicitely. (The following sentence "this can be done by..." is OK, but some more discussion would be useful.)	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Jan Fuglestvedt, CICERO)	
3-296	A	31	47			Person et al 2004 can not be found on the list of references. (Jan Fuglestvedt, CICERO)	Accepted
3-297	A	31	49	31	54	GWP instead of GW subscript p (H-Holger Rogner, IAEA)	Accepted
3-298	A	31	51			Regarding "not focusing on": We think it would be better to change this to "not needing". (Jan Fuglestvedt, CICERO)	Accepted
3-299	A	31	51			Regarding "not focusing on": We think it would be better to change this to "not needing". (Jan Fuglestvedt, CICERO)	Accepted
3-300	A	31	52			The word “that” refers to “one particular target”, while he following statement “in this case leads to considerable reductions of CH4 early in the scenario period” refers to the GWP based strategy. Thus it is contradictory to the following sentence. (Terje Berntsen, CICERO)	Accepted, will re-write
3-302	A	32	4	37	55	This section is largely occuppied with scenarios that are in blatant violation of the UNFCCC, which, as everyone here knows, requires stabilization of GHG concentrations at levels that prevent dangerous interference in the cliamte system. Since no credible argument has been presented anywhere that I am aware of that 4 C warming is not a dangerous warming, and since all the published attempts to probabilistically estimate climate sensitivity come up with at least a 10% risk of 4 C warming for a CO2 doubling, it follows that a CO2 doubling (3.7 W/m2 forcing) is dangerous interference. Why then proceed with such an extended discussion of scenarios the allow even greater forcing? Granted, this is what much of the literature does - but you can say that briefly and move on by tying the discussion back to the UNFCCC and the likelihood of present non-compliance. The real issue that needs to be addressed with regard to non-CO2 vs CO2 emisisons is, How much can we reduce their emissions and total forcing in the immediate future while working to bring down CO2 emissions, whose benefits show up later? (Danny Harvey, University of Toronto)	Reject, starts off a particular interpretation of the UNFCCC
3-303	A	32	5			What is the assumption and the reasoning towards the reduction of sulphur cooling? Clarify. (Peter Bosch, IPCC TSU WGIII)	Accepted, will re-write and clarify
3-304	A	32	7			Section 3.3.3. should be expanded to cover more of the recent new literature on low stabilization scenarios (like Azar, van Vuuren), another strand of new analysis relevant for the policy debate on climate targets. Overall, the single stabilization	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						target adopted as central case by EMF-21 attracts most of the discussion, which may give the impression that this may be preferred or most likely level (which in fact it may become by givimg it the central role). (Tom Kram, MNP)	
3-305	A	32	8			p. 32. Section 3.3.3. The progress made since the TAR on the cost evaluation of the implementation of the cost of the Kyoto Protocol presented in Chapter 11 should be referred to in this section. It should be stated explicitely that the IPCC assessment did revisit previous cost estimates for the Kyoto period and included recent estimates for the Kyoto period. The conclusion of this process is that recent estimates project a cost that is even lower than previously projected. Section 3.3.3.3 concentrates on mid to long-term periods, the short term should also be referred to. (Philippe Tulkens, TERI School of Advanced Studies)	Rejected, this chapter focuses on long-term issues of mitigation
3-306	A	32	8	43	47	The structure of section 3.3.3 is confusing. It deals with individual building blocks of mitigation, e.g. the different gases and sectors, as well as multigas stabilisation where all these gases/ sectors are integrated. These things are not clearly separated, which leads to a non-coherent picture. The other main problem is that the material is not clearly organised around stabilisation levels, which is probably the most policy relevant way. Restructuring of the section is needed.Discussion about "building blocks" is useful, but should stay clear of multigas results. It might be best to fist present the multigas stabilisation scenarios from integrated models and after that zoom in on the various sectors/ gases, allowing to commnet on the role of that gas/sector over time and the comparison with more bottom-ip/ detailed mitigation potential information (see for instance the land-use section). Results of stabilisation scenarios (multigas and CO2 only) should be organised by stabilisation level and baseline (provide a conversion table for ppmv CO2 eq to W.m2) (Bert Metz, IPCC)	Noted, we will restructure the building blocks – will explore the relationship between CO2 conc. And CO2 eq. Conc. for alternative targets
3-307	A	32	10	32	10	While it is not clear if this level of heading will be retained (it is not in the table of contents), the heading of "Energy and Industry CO2" does not seem to apply to this section. Non-CO2 gases, and emissions from sectors outside of energy and industry (as used in chapters 4 and 7) are considered in this section. Suggest that the heading be renamed to represent the topics covered. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Noted, we will re-write the heading to be consistent with section
3-308	A	32	12	32	20	This material is duplicated elsewhere in the chapter. (Jeff Price, California State University, Chico)	Accepted
3-309	A	32	12		20	Consider moving this paragraph more to the front of the chapter, instead of placing it under industry.	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Peter Bosch, IPCC TSU WGIII)	
3-310	A	32	15			Please add a few sentences about quality control for the database (I have very little faith in these data) and the procedure of data collection (below standard as far as I know). (Richard Tol, Hamburg University)	Accepted, will add text concerning validation process with respect of individual sources of data.
3-311	A	32	15	32	15	(Morita & Lee 1998a,b) references missing in the references section (Peter Kolp, IIASA)	Accepted
3-312	A	32	15	32	15	(Morita & Lee 1998a,b) references missing in the references section (Peter Kolp, IIASA)	Accepted
3-313	A	32	21	33	34	I find the mix of all intervention scenarios in a single picture not very interesting because such a mix does not allow one to disentangle differences due to model uncertainty from differences due to the stenght of the emission reduction. I recommend to display a figure for each group of intervention scenario and therefore to change figures 3.17 and 3.18 accordingly. (Peter Wittoeck, Belgian Federal Administration)	Accepted
3-314	A	32	44	32	54	The assessment of the study of Riley might be misleading in so far, as it is well known that the present configuration of the Kyoto Protocol is not effective in limiting temperature change at all and that its impact in its present configuration is actually neglegible. The added value of the Kyoto Protocol is that it is a very significant first step. Next steps are already enshrined in its current structure and this has also been reflected in the decisions of COP/MOP 1 in December 2005. (Radunsky Klaus, Umweltbundesamt)	Rejected, text will be removed
3-315	A	32	49	32	49	The reference Riley et al. (2005) is not quoted in the reference list. Should it be corrected by Reilly et al. ? (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	Accepted
3-316	A	32	49	32	54	I would recommend to substantiate this statement or to make it more precise (in terms of overal emission reductions, participating countries, timing of reductions, …). Otherwise the reader is not able to understand on which elements the statement is based. The word 'effective' should also be explained (does it mean 'cost-effective', in the sense that it entails lower world abatement costs ?) (Peter Wittoeck, Belgian Federal Administration)	Rejected, text will be removed
3-317	A	32	51	32	54	This material belongs in ch 13 (what are effective regimes?) (Bert Metz, IPCC)	Accepted, text will be removed
3-318	A	32	54	33	8	This material is about the role of different mitigation options. This belongs to a discussion on what options are important for what stabilisation levels (which is also addressed on page 33, lines 20-33 and needs to be organised better)	Accepted, text will be removed

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Bert Metz, IPCC)	
3-319	A	32	54			standard term is "CO2 capture and storage (CCS)" (as in IPCC Special Report) (Bert Metz, IPCC)	Accepted
3-320	A	33	3	33	4	"Akimoto, K., T. Tomoda, Y. Fujii and K. Yamaji, Assessment of Global Warming Mitigation Options with Integrated Assessment Model DNE21, Energy Economics, 26(4), pp. 635-653, 2004" also indicates the description of "As suggested by others, a portfolio of emissions reduction options is needed." I recommend referring the literature. (Keigo Akimoto, Resaerch Institute of Innovative Technology for the Earth (RITE))	Accepted
3-321	A	33	8			The sentence "The median of the stabilization scenarios … mitigation scenarios" is useless. (Bert Metz, IPCC)	Reject, will re-write to make meaning clearer
3-322	A	33	18	33	18	In figure 3.18, some intervention scenarios seem to lead to negative CO2 emissions. How is this possible ? Is this entirely due to CCS ? We also recommend to highlight in the figure the horizontal axis at the zero coordinate since it is quite unusual to encounter negative values in such a figure. The same remark applies to some other figures. (Peter Wittoeck, Belgian Federal Administration)	Accepted, will add explanation of biomass+CCS and sink enhancement
3-323	A	33	18	33	18	In figure 3.18, as well as in some other figures, the blue enveloppe (TAR, intervention range) is truncated between 2060 and 2080. The reader may think that this is due to a particular shape of the extreme upper scenario. A closer look at other figures reveals that such a truncation is due to time horizon of some scenarios (models). A remedy to that problem needs to be found (by extrapolating the extreme scenarios ?) (Peter Wittoeck, Belgian Federal Administration)	Rejected, all scenarios illustrated in the figure have a time-horizon up to 2100
3-324	A	33	18			Fig 3.18 is prety useless in its current form. It would be much more informative to show the mitigation emission trajectories per stabilisation level and compare that with the figures from TAR (do we see different trajectories in the newer literature?) (Bert Metz, IPCC)	Accepted, will calrify the figure and add additional analysis and illustrations for alternative stabilization targets
3-325	A	33	20			This is misleading. Many a model reports that 350 ppm, even 450 ppm is infeasible. (Richard Tol, Hamburg University)	Rejected, earlier studies have not taken into account BECS, and hence see 350/450 as infeasible
3-326	A	33	20	33	25	See also Moreira, 2005, Global biomass energy potential. Mitigation and Adaptation Strategies for Global Change(Special Issue, forthcoming). for this discussion.	Accepted, will review reference if it is made accessible

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	
3-327	A	33	20	33	33	One might question if it is appropriate to refer to 3-5 papers from 3 research groups as an 'increasing body of literature'. It is a bold statement that the few publications referred to in this para have 'changed perceptions' especially as most of them are still forthcoming. (Kenneth Möllersten, Swedish Energy Agency)	Accepted, will edit text
3-328	A	33	20	33	34	what about using nuclear energy to achive stringent targets ? (Peter Kolp, IIASA)	Rejected, nuclear energy is part of the portfolio of these and higher stabilization scecnarios
3-329	A	33	20	33	34	what about using nuclear energy to achive stringent targets ? (Peter Kolp, IIASA)	Rejected, nuclear energy is part of the portfolio of these and higher stabilization scecnarios
3-330	A	33	25	33	26	The phrase "If biomass is grown sustainably" should be removed. It unnecessarily introduces a potentially confusing definitionial and accounting question. CCS of biomass-derived CO2 will result in negative emissions. The question of whether forests will regrow to sequester additional carbon is an important one, but it is not dependent on CCS. (Reid Miner, NCASI)	Accepted, will edit text for clarity
3-331	A	33	27	33	27	Obersteiner et al. 2002 is missing in reference list. (Kenneth Möllersten, Swedish Energy Agency)	Accepted
3-332	A	33	27			why say "might"? Isn't it obvious that, if we want to stabilize at 350 ppmv and we are already above it, we need negative emissions (unless we want to wait 1000 years)? (Danny Harvey, University of Toronto)	Rejected, value judgement
3-333	A	33	28	33	28	What is the GET model (Peter Kolp, IIASA)	Accepted, will duplicate reference to clarify the source of model
3-334	A	33	28	33	28	What is the GET model (Peter Kolp, IIASA)	Accepted, will duplicate reference to clarify the source of model
3-335	A	33	28			Are these models really integrated assessment models?? Timer forms part of the IMAGE integrated assessment modelling system but is not a IAM,….MESSAGE?? (Peter Russ, IPTS, Joint Research Centre, European Commission)	Accepted, will change into "modelling systems"
3-336	A	33	35	33	42	The above description of the alternative stabilitation targets is of particular interest. Therefore, in the comparison exercise, it would be extremely useful to have a paragraph (or more) on the possible link between 4.5 W/m2 radiative forcing stabilization and a concentration target for CO2 (or GHG), i.e., a range of concentration levels associated with 4.5 W/m2.	Accepted, will explore the relation between CO2 conc. And Co2-eq. concentration

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Peter Wittoeck, Belgian Federal Administration)	
3-337	A	34	9	34	20	Suggest that the range of cumulative CO2 emissions be compared to the range for a given CO2 concentration pathway (as for example given this the TAR, WG1 Ch3) to compare this range of models with that of carbon cycle models (without differences in CO2 concentration pathway). It is not clear from this assessment if the range of models considered spans the range of carbon cycle models (the scenario models may be parameterized to some central estimate from the carbon cycle models). The range given by in the TAR WG1 (see Kheshgi, H. S., and Jain, A. K.: 2003, 'Projecting future climate change: implications of carbon cycle model intercomparisons', Global Biogeochemical Cycles 17, 1047, doi:10.1029/2001GB001842) is roughly the same width, and this would be add to pathway uncertainty. I suspect that the range of models considered in Figure 3.19 does not span the range of carbon cycle science models. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted, will add to the discussion pointed out in comment 3-336
3-338	A	34	15			Reverse order of stabilisation levels (in light of beginning of sentence) (Bert Metz, IPCC)	Accepted
3-339	A	34	17	34	20	This point is not illustrated by fig 3.20 (in lower half of figure the 15-85 percentile ranges are not different for 450, 550, 650 ppm), nor by fig 3.19 (the relative uncertainty in maximum allowable cumulative emissions for 450 ppm is not different from that for 550 ppm or 650 ppm) (Bert Metz, IPCC)	Accepted, Figure 19. illustrates reduced flexibility, will revise text to reflect that Fig. 3.20 does not indicate the point.
3-340	A	34	24			It is misleading to call the results for low level stabilisation scenarios "outliers" (Bert Metz, IPCC)	Accepted, will change text to small number of scenarios
3-341	A	34	38	34	51	This discussion and figure 3.21 are not providing clear insights. What policy makers want to know is what the implications are in terms of emissions reduction (as % of baseline) for the various stabilisation levels and various baseline choices (because the latter is a very important factor). presenting ranges of 15-50 GTC (line 39) or "a few GTC to 40 GTC (line 46) is too general. Try to develop text/figures that convey the key points better. (Bert Metz, IPCC)	Accepted, will add discussions and illustrations showing the implication of alternative stbilization levels and choice of baselines
3-342	A	34	53	36	35	This section should also include a paragraph on the available low cost opportunities in developing countries to mitigate non CO2 gases especially Methane. (Junichi Fujino, National Institute for Environmental Studies)	Accepted, will add the point if literature supports the point
3-343	A	35	18	35	18	Typo error. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted, will change “gazes” into “gases”
3-344	A	35	20			The exact same text is used here and in Section 3.6.1.6.2, the latter section could	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						refer to this section (Terje Berntsen, CICERO)	
3-345	A	35	21			Maybe it should be “gases”. (Tieju Ma, International Institute for Applied Systems Analysis)	Yes, accepted
3-346	A	35	23	35	41	This paragraph duplicates with section 3.3.2 (Bert Metz, IPCC)	Accepted, will remove text
3-347	A	35	30			Case (2) does not seem to be justified. Although near-term warming may be LESS critical than long term warming, we are already seeing evidence of adverse ecological impacts and there are published studies (presumably reviewed by WG II) of worse impacts to come within the next few decades. Thus, reductions of short-lived gases in order to get quick benefits are still justified. It does not logically follow that they are not justified just because near term impacts of warming are less than long term impacts. Secondly, what evidence is there to suggest that tirggering reductions in short lived gases would not be "economically efficient"? I wouldn't be suprised if the supposed evidence is next to zero or as dependent on the modeller's assumptions as on any real data. (Danny Harvey, University of Toronto)	See 3-346
3-348	A	35	34	35	38	The condition explained as part 2 of this if statement is very unclear. How far into the future is “further decades” and what is meant by “given, currently unknown, concentration thresholds”. Since the same text is used in Section 3.6.1.6.2 the comment applies also there (Terje Berntsen, CICERO)	See 3-346
3-349	A	35	50			Did all models use cost minimisation, as suggested by the text (by the way better to say "least-cost approaches" than "cost- effective approaches") (Bert Metz, IPCC)	Accept, will be re-written
3-350	A	35	50			using full " what " flexibility. Jargon, please explain. (Peter Bosch, IPCC TSU WGIII)	Accept, will be re-written
3-351	A	36	24			The literature (e.g. Manne and Richels, 2001) indeed shows that with intertemporal optimization the value of reducing CH4 towards the end of the period. However, the “end of the period” is often just when the concentrations (or temperature change) should be stabilized. Mitigating short-lived gases then (and thus neglecting mitigation of the long-lived gases) will probably make it very costly to keep the stable conditions after that as there will still be significant emissions of long lived gases (eg. CO2, N2O) that will continue to accumulate in the atmosphere. This short-coming of the economical analysis should be mentioned in the comparison between trade-offs between gases using the standard GWP and the intertemporal	Accepted, text will explain the shortcoming of using GWPs versus physical relationships between gases

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						optimization approach. This comment also applies to page 31 line 36. (Terje Berntsen, CICERO)	
3-352	A	36	24	36	35	It is confusing to present the results for the GWP based scenarioss together with those for the cost-minimization scenarios. Both are relevant, but please separate. (Bert Metz, IPCC)	Accepted
3-353	A	36	37			section 3.3.3.3 Note that, in addition to the EMF-21 study the results from the international Model comparison project are now available, reported in Köhler et al and Edenhofer et al. reference details above (Jonathan Köhler, Tyndall Centre, University of Cambridge)	Accepted
3-354	A	36	37			Section 3.3.3.3. Some explanations should be given on the notion of cost expressed in terms of GDP loss relative to a reference scenario. Section 2.5.2 in Chapter 2 identifies different types of costs. It should be specified in section 3.3.3.3 what type of cost is being considered. Moreover, the meaning of for instance 7% of GDP for 2050 should be explained. Are such estimates given in US$ of 2050 using a GDP deflator? By how much is the real GDP projected to increase over the same period? This is very important to put the cost estimate into context. Past cost assessement exercices on the Kyoto Protocol implementation in the USA led to great confusion on what exactly is being projected by the model. In the special issue of Enegy Journal (1999) on this topic, costs are expressed in % of GDP in 2010. What did it mean exactly? Was it assumend that there was no cost before 2008 and then suddenly the total cost is being felt over the 5 year commitment period? 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 interpreted. (Philippe Tulkens, TERI School of Advanced Studies)	Accepted, further explanation will be added
3-355	A	36	41			The results of co-benefit studies should be presented somewhere! (unless there is absolutely no literature) (Bert Metz, IPCC)	Noted, see table 3.7
3-356	A	36	43	36	53	Not very helpful text, as the conclusion in ll. 43-44 that costs may or may not be related to GDP growth, is not followed by a somewhat structured discussion what may lie behind this somewhat surprising observation. (Tom Kram, MNP)	Accepted, implications of baseline for costs will be analyzed
3-357	A	36	43	37	26	This section on costs of mitigation/ stabilization is very confusing. It throws all scenario results together, stabilisation, non-stabilization, different baselines. It would be important to disaggregate the results according to stabilisation level and also accordsing to baseline assumption, so they can be compared to the respective	Accepted, 1) see 3-356, 2) discussion on the implication of stabilization level will be added

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						TAR information (CH8 and SyR). The paragraph from line 12-15 on page 37 is puzzling. How can you conclude the TAR results are robust without disaggregating the studies? And, as stated in lines 50-51 0n page 37, costs for multigas stabilisation are lower than for CO2 only, so that should show in the cost data. (Bert Metz, IPCC)	
3-358	A	36	44		49	I do not agree with this argument. Whether climate change policy is more or less costly when growth is higher does not depend on justy the rate of economic growth, but on the marginal productivity of investment. Suppose growth is higher because a new technology has allowed the economy to grow faster. At the same time this technology allows for complementary investment in capital and R&D such that the marginal returns to investment are high. Then any investment that is diverted away from investment in this new technology is more costly. Hence, if resources are spend on climate change this might crowd out investment in the new technology and this is more costly than in a situation with low growth and low marginal productivity of investment. (Sjak Smulders, Tilburg University)	Rejected, not supported by literature
3-359	A	37	12	37	12	Compared to the pre-TAR, i.e., pre-SRES, SRES and post SRES … I'm having difficulties to decode this pre/post SRES/TAR sequence (Peter Kolp, IIASA)	Accepted
3-360	A	37	12	37	12	Compared to the pre-TAR, i.e., pre-SRES, SRES and post SRES … I'm having difficulties to decode this pre/post SRES/TAR sequence (Peter Kolp, IIASA)	Accepted
3-361	A	37	16		24	Usually the argument is the other way around: poor countries adopt technologies already available in the rich countries at low cost or can even leapfrog technologies, so that marginal adjustment costs are lower. (Sjak Smulders, Tilburg University)	Rejected, figure illustrates results from the literature. Note though that we will modify the section to illustrate the implications of baselines and alternative stabilization levels for costs.
3-362	A	37	17	37	24	The regional costs are highly depending on the assumed regime. There are many types of regimes, and this in fact is a complete new subject. Here, the outcomes of one regime based on full IET and equal marginal costs across the regions. This seems rather ad-hoc choice, as there are many allocation schemes based on various equity principles and allocation schemes (i.e. Multi-Stage, Triptych, Contraction & Convergence, costs-allocation etc) These regimes are explained in more detail in Chapter 13, and therefore it might be better to discuss the regional costs in more detail in Chapter 13. If you want to discuss regional costs, then also some of the new work on regional mitigation costs needs to be mentioned. See next comment.	Noted, discussion on costs will mainly implications of baselines and alternative stabilization. Regional discussion will be revied accordingly.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Michel den Elzen, The Netherlands Environmental Agency)	
3-363	A	37	17	37	24	Suggested text: The number of studies analyzing the regional abatement costs for various allocation designs for concentration stabilization at 450 ppm CO2 or 550 ppm CO2 –equivalent or below is limited. Of the studies cited above, Nakicenovic et al. (2003) presents regional costs for 400 and 450 ppm CO2 only, and Persson et al. (2006) for 450 ppm CO2 only. Criqui et al. (2003) and den Elzen et al. (2005b) are the only studies that take all GHGs into account, presenting regional costs for different allocation schemes for 550 and 650 ppm CO2-equivalent. Besides these studies, there are also studies with macro-economic models, that focus primarily on the Contraction & Convergence regime for higher global CO2 only emissions targets, as has been done by: Böhringer and Welsch, 1999; Böhringer and Löschel, 2003), or for a 450 ppm CO2 only profile and converging per capita emissions by 2024 (see also sensitivity analysis, this study), as in Bollen et al. (2004). Next there are macro-economic studies that focus on different emission scenarios for the US, Annex I (minus US) and the developing countries, as for example has been done by Buchner and Carraro (2004). (Michel den Elzen, The Netherlands Environmental Agency)	Accepted, will review references and include if appropriate
3-364	A	37	17	37	24	REFERENCES: Regional-costs 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	Accepted, will review references and include if appropriate

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						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 Analysis, The Hague. (Michel den Elzen, The Netherlands Environmental Agency)	
3-365	A	37	17	37	17	It is proposed to use the following wording: Since several scenarios include in the database information on regions, … (Radunsky Klaus, Umweltbundesamt)	Accepted
3-366	A	37	20	37	24	The explanation for the narrower range of GDP loss in OECD countries is not very convincing… Isn't it rather because assumptions on GDP, energy needs, population, etc. are more difficult to establish for developing countries than for OECD ones, as suggested in section 3.2.1 ? (Peter Wittoeck, Belgian Federal Administration)	Noted, discussion on costs will mainly implications of baselines and alternative stabilization. Regional discussion will be revied accordingly.
3-367	A	37	20	37	24	The explanation for a wider spread in cost for developing countries than for OECD is not convincing. (Bert Metz, IPCC)	Noted, discussion on costs will mainly implications of baselines and alternative stabilization. Regional discussion will be revied accordingly.
3-368	A	37	26			Figure 3.24: The caption does not explain what kind of mitigation scenarios are depicted here. Do they have the same mitigation target or a different one? In the latter case, Figure 3.24 would compare apples with pears, and I would propose to delete the figure. (Hans-Martin Fuessel, Stanford University)	Accepted, discussion on costs will mainly implications of baselines and alternative stabilization. Regional discussion will be revied accordingly.
3-369	A	37	26	37	26	Figures 3.23, 3.24, 3.25 and 3.26 seem to treat the different models differently I.e. different colours for Fund in 3.23 and 3.24. Can they be made consistent? In addition the outcomes in 3.24 do not seem consistent with those in 3.25: surely GDP costs should be higher in 3.25 (CO2 only) compared with 3.24 (multigas mitigation) or is 3.24 CO2 only? If so why the difference with 3.25? (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Accepted, we will make colours of 3.25 and 3.26 consistent. Values are correct in both figures.
3-370	A	37	28	37	35	Paragraph fails to note that in EMF-21 very different baselines are used ('modelrs choice') which limits the value of absolute reduction cost estimates. Core question was to explore if and by how much abatement costs may be reduced by going from CO2-only to multi-gas strategies. More emphasis on the latter aspects is thus justified.	Accepted

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						(Tom Kram, MNP)	
3-371	A	37	28	37	45	These results need to be disaggregated according to baseline (which TAR showed has a big influence) and need discussion. Why is it that there are such big differences? Is that because of the mitigation options that models have built in? Or is it due to model representation or assumptions? (Bert Metz, IPCC)	Accepted, will illustrate the implications of baselines for costs
3-372	A	37	31	37	31	The text would be clearer if the term "marginal cost" was replaced by carbon tax, if that is what is meant. They are the same only under certain stringent theoretical assumptions. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Accepted, will add explanation
3-373	A	37	37	37	37	From figure 3.25 (and 3.26), it appears that the GTEM model is very extreme. This is not a problem per se. However, the fact that this model provides significant (relative) costs for the year 2000 is an issue, because this artificially drives down the overal enveloppe. (Peter Wittoeck, Belgian Federal Administration)	Rejected, we reflect the EMF 21 literature here, which includes the GTEM – will add some explanation for the specific behaviour
3-374	A	37	49	37	55	The explanation for the differences is much too brief and of no help to the policy maker or to the understanding of these extraordinary results. Figure 3.25 has one model (GTEM?) with -5% of GDP in 2000, rising to -15% in 2020; this is simply odd and discredits the IPCC if it is to be included without explanation and dismissal. Figure 3.24 has 2 models with GDP losses over 25% by 2100; these are much greater than any losses reported in the TAR. Why no explanation? What are the carbon tax rates for these losses? What happens to the carbon tax revenues? The models are probably extremely stylised with no proper treatment of public sector finances. The job of the IPCC is to assess such literature not just report it. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Accepted, will add some explanation for the specific behaviour
3-375	A	38	2	41	46	This section lacks any discussion of the fact that the net effect of trying to reduce climate change through reforestation is less than the effect expected based on the reduction in atmospheric CO2 concentration, due to the fact that the surface albedo is usually reduced, increasing the absorption of solar radiation. A reference on this point is ..... FIND REF (Danny Harvey, University of Toronto)	Accepted, reference will be added
3-376	A	38	6	41	50	This sectionis interesting because it discusses the insights in the role of land-use mitigation over time and in various regions. It also summarises the "bottom-up information" on mitigation potential in agriulture and forestry (some of this might	Noted – the text order needs to be discussed with co-authors, as does the role of IA and non-IA modeling frameworks in this section.

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						better be left to Ch 8 and 9) and does discuss biofuels. It would help if first the results fro integrated models are discussed, referrring back to their overall results and then confront that with the more detailed information. More emphasis could be given to comparing/ explaining the differences between integrated models in terms of how much land-use mitigation they show (e.g expressed in the % land-use mitigation) for particular stabilisation levels in the light of detailed knowledge on costs (because the integrated models use cost curves). (Bert Metz, IPCC)	More detailed results being sought from the authors of the cited literature to facilitate, among other things, the kind of comparisons suggested by the commenter.
3-377	A	38	37			insert "so-called" before "optimal". There is really no such thing as an optimal emission pathway (optimal for whom? Future generations, faced with a 10 m sea level rise, would surely not regard as optimal the pathway that the current generation regards as "optimal", especially when distant impacts are largely discounted to zero). Also, you are implying that stabilization pathway are not "optimal" by saying "Stabilization and optimal ... policies". I strongly object to this impllication. (Danny Harvey, University of Toronto)	Accepted – this is a matter of semantics, where the economic terminology is confusing and misleading the reader. Clarification is required and using different language is appropriate.
3-378	A	39	12	39	12	Figure 3.3-14 is this figure 3.28 ? (Peter Kolp, IIASA)	Accepted – figure mislabelled in text
3-379	A	39	12	39	12	Figure 3.3-14 is this figure 3.28 ? (Peter Kolp, IIASA)	Duplicate comment – same as previous
3-380	A	41	16	41	29	very difficult to understand (Bert Metz, IPCC)	Accepted – agree that the text is pretty dense as is. Will revisit.
3-381	A	41	25	41	25	what are "forward thinking models" ? (Peter Kolp, IIASA)	Accepted – definitions required
3-382	A	41	25	41	25	what are "forward thinking models" ? (Peter Kolp, IIASA)	Duplicate comment – same as previous
3-383	A	41	38	41	40	The authors should explain what exactly is 'particularly enticing' about the 'negative emissions strategy' and furthermore what exacly lies behind the term 'strategy'. 'Strategy' implies some kind of plan or tactic. Both these issues need to be addressed in the light of the chapter's theme 'long-term context'. The idea of negative emissions may certainly sound enticing. However, one must be careful not to overlook or subconsciously suppress salient points The in this area is very limited. At the heart of the process is Biomass Energy with CO2 Capture and Storage. If we examine this in some fundamental way we can start with a tree having appropriate nutrients, water and solar energy available in an atmosphere containing CO2. Under such conditions, photosynthesis occurs, taking CO2 from	Accepted – some additional discussion merited. However, detail as described by the commenter is probably more appropriate for Ch.8. Therefore, coordination on this point is required.

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						the atmosphere and converting it into organic chemicals that are sequestered in the tree. Thus, the tree, over its several years of life, may be thought of as permanently sequestering CO2 as long as the tree is alive. This is certainly a carbon dioxide-negative process. The tree is harvested and its most valuable storehouse of organic chemicals is transported (no costs are mentioned in the chapter, references?) to a power plant or other conversion facility (biomass energy is typically small-scale and this severely restricts opportunities for CCS, no mention in the chapter where it may be feasible that this might occur, today or in the future. No mention of literature sources addressing these issues). Upon burning the biomass most of its carbon is transformed into CO2. Then, according to the negative emission 'strategy' (?), the CO2 is extracted from the exhaust of the CHP plant and permanently stored. Alternatively, the extraction occurs integrated with pre-processing of the fuel whereupon the fuel combustion can take place GHG emissions-free. Certainly, the entire process from the birth of the tree to the permanent storage of the CO2 extracted from would be a carbon dioxide negative process. However, 'negative' cannot simply be taken for granted. If the biomass extraction leads to deforestation, then indeed the process can only be carbon-neutral. Provided that another tree is born to replace 'our' one the process becomes carbon-negative. But what of the carbon-balance of the entire process? The second thing that comes to mind is the question “What is so new and enticing about carbon dioxide negative processes?” when we have had them since the very beginning of life on Earth. Nevertheless, if we could really attain net-negative emissions globally in order to attain very low stabilisation targets, it would be a great step forward, so great, in fact, that we need to exercise the greatest care to speak very precisely. Mother Nature has already shown us the way, but we must recognize the constraints of scientific and technological feasibility, economic feasibility, early opportunities for implementation, environmental feasibility, social feasibility, political feasibility and any other feasibilities (references?). In the context of this chapter, an adequate coverage of this 'enticing strategy' must consider the entire process from the birth of the tree to the final storage of the CO2, an elaborate life cycle approach paying full attention to all of the feasibility constraints. (Kenneth Möllersten, Swedish Energy Agency)	
3-384	A	41	39	41	39	Modify the sentence by: "…that combines biomass energy and CO2 geological storage" (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	Taken into account with next comment
3-385	A	41	39			standard term is "CO2 capture and storage (CCS)" (as in IPCC Special Report)	Accepted

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						(Bert Metz, IPCC)	
3-386	A	41	42	41	50	good to mention this, but it is more important to give an idea how these factors can affect (quantitatively) the estimated contribution of land-use mitigation in the longer term. One additional factor also needs to be taken into account: albedo changes (see Schaeffer, Ph D Thesis Utrecht University, 2005). (Bert Metz, IPCC)	Noted – further consideration appropriate. This text includes very broad topic areas and space in the chapter is constrained. Also, quantitivative effects are uncertain in some cases. A condensed more substantive presentation than the current text will be considered, as well as required consistency with other WGs and chapters.
3-387	A	41	52	43	47	This section discusses the combined effects of mitigation strategies on climate and air quality, also in monetary terms. That is good and should probably be reflected better in the title of the section. Be careful to focus on the long-term results and leave the short term material (e.g lines 49- 54 on page 42) to Ch 11.As far as the terminology is concerned (ancillary benefits vs co-benefits), the resolution in TAR (use ancillary, because most of the studies have only looked at climate mitigation with air quality benefots as a spin off) does not longer apply I think. The current literature should have many more studies where both issues are considered together (is that true?) and therefore the preferred term for AR4 could then be "co-benefits". Is there no literature on other co-benefits of long-term mitigation/ stabilisation, such as energy security? if so, that would be important to cover, also because it provides a link to the isues of development paths and development obejectives that are supposed to be covered in the chapter. (Bert Metz, IPCC)	Noted, title of sub-section will be revised. Benefits/co-benefits of mitigation to be dealt with in new section 3.5
3-388	A	42	22	42	25	is this true for biomass burning ? (Peter Kolp, IIASA)	Noted
3-389	A	42	22	42	25	is this true for biomass burning ? (Peter Kolp, IIASA)	Noted
3-390	A	42	50	43	30	The following national-scale studies on Finland could be added: 1. S.Syri et al. Atmospheric Environment 36 (2002) 3059-3069: Implementation of the Kyoto protocol would reduce acidification of ecosystems by 6-11% and harmful ozone levels by 3% from current reduction policies. 2. A. Lehtilä et al. The role of technology in greenhouse gas emissions reduction: the case of Finland, Energy 30 (2005):2738-2758: a -20% GHG emission target by 2030 would reduce sulfur emissions by 11-16% from "Kyoto forever" scenario and NOx emissions by 8-9% in Finland. Particulate emissions would not be affected notably.	Accepted, will review reference and include if appropriate

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Sanna Syri, VTT)	
3-391	A	43	6	43	24	This is important material and should be highlighted in the executive summary (Danny Harvey, University of Toronto)	Accepted
3-392	A	43	27	43	28	Siri -> Syri (Sanna Syri, VTT)	Accepted
3-393	A	43	30			Table 3.7: This table seems to be incomplete, in particular the column on "Ancillary Benefits". (Hans-Martin Fuessel, Stanford University)	Accepted, table will be updated
3-394	A	43	30	43	30	Citations in the table that actually do not have anciallry benefit numbers should be deleted. (Jeff Price, California State University, Chico)	Noted, will fill table wherever possible, and delete those, which don't have anc. Benefits
3-395	A	43	49			The analysis of the factor of CO2 emission changes in scenarios is necessary not only at a global level but also at a national level. "Which factor has a key role for reducing CO2 emission drastically", "plausible or feasible change rate of the factor" and "introduction rate of low carbon energy" are significant information for the researchers who develop the long-term scenarios. (Reina Kawase, Kyoto University)	Factors that contribute to CO2 emissions will be considered.
3-396	A	43	49			How much it costs to reduce CO2 emission is of concern for policy makers. So the social cost in climate stabilization scenarios at a national level need to be reviewed. (Reina Kawase, Kyoto University)	Considered. Several lieteratures were reviewed on the costs of CO2 reduction, but few leteratures state about social costs explicitly. Assessment on CO2 reduction costs on technology will be considered. Assessment on socila costs will be added if available.
3-397	A	43	49	45	45	A lot of material covered in this section is about short/ medium term mitigation scenarios (till 2030 or so). That belongs to ch 11. The explicit statement on page 32, line 24 needs to be corrected as well.The remaining material (I noticed a lot aof material was dropped compared to the interim version I saw and some of that material may be very useful) on long-term studies (2050 and beyond) needs to be analysed in the context of the earler findings on long-term mitiagtion/ stabilisation: are these national findings inline with the general conclusions or are there differences? What do these national studies say about more mitigation options for certain groups of countries? Are there cost estimates that compare well with the general findings or do we see large differences between countries? etc. Only then this section would have added value (Bert Metz, IPCC)	Basically regional/country scenarios with 2050 and beyond are reviewed in Chapter 3. Some scenarios till 2030 are included if it is difficult to find country scenarios beyond 2050 and necessary to include from the long-term context. The statement on page32, line 24 is corrected accordingly. Sort-term -> Medium/long term. Dropped materials that were in the interim version will be reconsidered. Different mitigation options in different country scenarios exist, e.g. difference in

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
							share of nuclear, CCS, hydrogen, biomass etc. Also social system differs. Although available cost data are limited, these cost data will be included.
3-398	A	43	51	45	45	What are the main conclusions regarding national and regional scenarios. This section does not give results where do these scenarios meet, or where do they NOT meet. This is interesting information for linkages Ch 4 - 10 and Ch 3. In addition, renewable energy scenarios or work in teh EU like the FORRES project might be interesting to add (EC DG TREN, 2005), work from Fraunhofer, EEG (Vienna University) and Ecofys. (Monique Hoogwijk, Ecofys)	Several scenarios have desirable taregets. They include future technologies under development. These scenarios are included if their assumptions are clear and they have specific stragetegies to achieve taregtes. More relevant works will be considered.
3-407	A	44	0			Table 3.8, Row_18 As scenarios in Japan, medium-term scenarios are reviewed. Recently, some research groups in Japan released long-term climate stabilization scenarios towards 2050 or 2100. The following scenarios should be listed in Table 3.8. Ministry of the Environment started “Japan low carbon society scenarios toward 2050” project, and Ministry of the Economy and Trade and Industry issued “Strategic Technology Roadmap -Energy Technology Vision 2100”. These two projects formulates long-term climate stabilization scenario in Japan. Japan Atomic Industrial Forum (JAIF) analyzed energy supply and demand 2050 in Japan by using two scenario exercises. You can get the related information and material from here. http://www.iae.or.jp/2100.html, http://2050.nies.go.jp/index_e.html, http://www.jaif.or.jp/english/news/2005/0317vision.html (Reina Kawase, Kyoto University)	Noted. The papers will be considered.
3-399	A	44	18	44	18	Plenty of other national and regional scenario studies have been performed. Table 3.8 does certainly not give an overview of all of them. Unfortunately, to our knowledge, no such recent overview exists in the literature. (Peter Wittoeck, Belgian Federal Administration)	Table 3.8 will be checked and modified including addition of new scenarios.
3-400	A	44	21			Table 3.8. List of national scenario: In this table, the "Model" which is adopted in "Advisory Committee for Natural Resources and Energy:ACNRE" is "ECONOMATE" and another bottom-up type submodule. The core of the model is "ECONOMATE".(Source: Ministry of Economy, Trade and Industry(METI),March 2005,Japan's Energy Outlook for 2030 (Japanese)) (Please see attached file "Japan's Energy Outlook 2030.pdf")	Table 3.8 will be checked and modified including addition of new scenarios.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Ryoichi Komiyama, The Institute of Energy Economics, Japan (IEEJ))	
3-401	A	44	21	44	21	After studying this table I am not sure it really yields much information useful to readers fo this chapter. Perhaps the authors could consider deleting it or substantially modifying it. (Jeff Price, California State University, Chico)	Table 3.8 will be checked and modified including addition of new scenarios. The table will be reformed to give information of differences between country/regional scenarios.
3-402	A	44	21			Table 3.8 It should be added to indicate the source of Citezens' Open Mode Projects for Alternative and Sustainable Scenarios. Citizens' Open Model Projects for Alternative and Sustainable Scenarios, 2004, COMPASS. http://www.isep.or.jp/shimin-enecho/presen_pdf/COMPASS_finalreport_en050428.pdf (Masatake Uezono, Citizens' Alliance for saving the Atmosphere and the Earth)	Table 3.8 will be checked and modified including addition of new scenarios.
3-403	A	44	35	44	38	Lehtilä et al. 2005. The study assessed the cost-optimal ways of reducing Finnish GHG emissions to -10%, -20% and -30% from the Kyoto target by 2030. (Sanna Syri, VTT)	New paper is reviewed and data will be assessed with other scenarios.
3-404	A	44	40	44	55	This text clearly belongs in chapter 11. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	**Accepted, text revised**
3-405	A	44	42			one major analysis of emission reductions in the EU and world-wide that does not look on the transport sector but the whole energy system is the study by Criqui et. al. Three models IMAGE, GEM_E3 and POLES were used to analyse "Greenhouse Gas Reduction Pathways in the UNFCCC process upto 2025". (http://europa.eu.int/comm/environment/climat/pdf/pm_techreport2025.pdf). Even if you might not want to discuss results it deserves being mentioned. (Peter Russ, IPTS, Joint Research Centre, European Commission)	Noted. The report will be considered.
3-406	A	44	50	44	51	I could not understand this (FÉLIX HERNÁNDEZ, IEG-CSIC)	The sentence will be modified.
3-424	A	45	0			The headline of subsection 3.4.1 is missing. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. To include heading "3.4.1. Carbon free energy and decarbonization"
3-408	A	45	6	45	11	The authors are commended for their careful reading of the literature on the work by Hanson et al. In the bibliography, "Argonne ILL" is used. The state name of "Illinois" or the two letter zip-code abbreviation of "IL" should be employed. (Richard Doctor, Argonne National Laboratory)	Noted.
3-409	A	45	7	45	7	It is proposed to check the reference to "Mintzer, et al (2004)" because the literature	Noted.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						does not include such reference but a reference to the year 2003. (Radunsky Klaus, Umweltbundesamt)	
3-410	A	45	20	45	30	Continued from point 1. : Literature "The costs of mitigating carbon emissions in China: findings from China MARKAL-MACRO modeling (CHEN Wenying, Energy Policy 33 (2005) 885–896)" discussed the integrated role of structure adjustment, technology efficiency, and new and renewable energy to energy intensity improvement, and concluded that the annual decrease rate of carbon intensity per GDP in China during 1980 to 2000 was around 5%, and it is expected to be 3% in the next 50 years (2000-2050). (Wenying Chen, Energy, Environment, and Economics Research Institue, Tsinghua Univerisity)	Accepted, the papers and discussions will be considered.
3-411	A	45	20		30	Again, the OECD study includes macro-scenarios with and without climate commitment (Haakon Vennemo, ECON)	Noted.
3-412	A	45	20	45	30	2. This paragraph indicates that most scenarios in development countries don't specify carbon emission limits. However, in order to study the mitigation cost, mitigation scenarios should be defined, both in OECD and developing countries. Literature "The costs of mitigating carbon emissions in China: findings from China MARKAL-MACRO modeling (CHEN Wenying, Energy Policy 33 (2005) 885–896) " defined several abatment scenarios for China to assess the marginal abatement cost curves as well as GDP losses. (Wenying Chen, Energy, Environment, and Economics Research Institue, Tsinghua Univerisity)	Accepted, the papers and discussions will be considered.
3-413	A	45	20	45	30	1. Future carbon emission reduciton in China not only rely on energy technology efficiency improvement, but also highly depend on future economic structure adjustment (decreasing share of industry while increasing share of service sector), industry sector's structure adjustment (increasing share of light industy, and increasing share of high-value added products), and development of new and renewable energy. Past development experience in China showed that technology efficiency improvement contributed to around 1/3 while structure changes contributed 2/3 to the overall energy efficiency(energy intensity) improvement. Apart from disccusion the role of technology efficiency improvement, this chpater had better add some words on other important factors,especially structure changes (this is one of the major differences between developing and developed countries). (Wenying Chen, Energy, Environment, and Economics Research Institue, Tsinghua Univerisity)	Accepted, the papers and discussions will be considered.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-414	A	45	30	45	30	Incomplete sentence (H-Holger Rogner, IAEA)	Deleted.
3-415	A	45	30	45	30	Either delete "Some" or add the missing part of the sentence. (Radunsky Klaus, Umweltbundesamt)	Deleted.
3-416	A	45	33	45	33	p. 45. L. 33. The reference for Shukla seems to be a presentation given in an IPCC expert workshop. Is such literature that is accessible to the reader acceptable for AR4 ? A paper was reference would be better. (Philippe Tulkens, TERI School of Advanced Studies)	Noted
3-417	A	45	38	64	19	I agree with the text when it concludes for example that "reduction of green house gas emissions is highly dependant upon both technological innovation and practice" (see page 49). However I think that the text neglects too much the dynamic general equilibrium models, in particular the last generation of them which explicitly introduce endogenous growth and endogenous technical change. There is no doubt that the technological choices made by firms and the efforts put in Research and Development depend basically of the incentives given by the public authorities. But conversely these public incentives and the resulting technological choices have complex dynamic effects on all variables in the economic: growth, extraction path of natural resources, emissions, … The best framework used in economy to capture these inter-relations is probably given by the dynamic general equilibrium models. Since the seminal model of Nordhaus-Boyer, several new models have been recently developed. For instance, in a recent paper, Reyer Gerlah and Wietze Lise ("Induced Technological Change Under Carbon Taxes", Ecological Economics, 2005) show that "without induced technological change, carbon taxes have a modest effect on emissions, while with induced technological change, they accelerate the substitution of carbon-free energy for fossil fuels substantially". Similarly, in "Entice-BR: The Effects of Backstop Technology R&D on Climate Policy Models" (forthcoming in Energy Economics), David Popp includes policy-induced energy R&D in a model with a backstop energy technology. He shows that, "while induced technological change is important, larger welfare gains come from simply adding an alternative technology to the model". A reasonable climate policy would stimulate technological change to an extent that would make these policies inexpensive. An other example is the paper of Ottmar Edenhofer, Nico Bauer, Elmar Kriegher, "The impact of technological change on climate protection and welfare: Insights from the model MIND", Ecological Economics 54 (2005). The authors show that	Accepted. Additional results by the dynamic general equilibrium models and additional papers on technological change will be considered.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						"Achieving the ambitious policy goals necessary to avoid dangerous climate change becomes feasible without significant welfare losses", if one incorporates technological change in a portofolio of mitigation options. They claim that "a better understanding of technological change should be a priority on the research agenda". My opinion is that these types of models are not sufficiently analysed in this chapter. (Norbert LADOUX, University of Toulouse and IDEI)	
3-418	A	45	45	45	45	Section 3.3 contains a large amount of information and implies a number of policy-relevant conclusions, e.g. that inclusion of multi-gas and land-use options lowers the projected cost of stabilization. These conclusions currently are not highlighted either at the end of the section or in the Executive Summary. It would be valuable to add a list of conclusions, as was done at the end of Section 3.2, and then highlight the most important of them in the Executive Summary. While reference scenarios are interesting, information about the potential costs of mitigation and the factors that affect those costs is far more important. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Good suggestion. Conclusions will be considered.
3-419	A	45	46			I am missing a conclusion section like 3.2.3 here. Idem at the very end of the chapter after 3.6. (Peter Bosch, IPCC TSU WGIII)	Conclusions including highlighting potential options to mitigate GHG will be considered.
3-420	A	45	47	51	50	The hierarchy of these two sections (3.4.2 and 3.4.3) is not quite clear. More importantly, this section should elaborate on the importance of having both "push" and "pull" policies, ie support to R&D as well as carbon costs, and/or other more technology-specific instruments (ie renewable energy porfolios, tradable certificates or feed-in tariffs) to pull new climate-friendly technology into the marketplace. "There is no guarantee either that strategies focussing on research and development (including dissemination efforts) of carbon-free technologies will ever be successful." (Philibert, Cédric, 2005, Energy demand, energy technologies and climate stabilisation, Proceedings of the IPCC Expert Meeting on Industrial Technology Development, Transfer and Diffusion, September 21-23, 2004, Tokyo) (Cédric Philibert, International Energy Agency)	Accepted. Will be considered in revised text.
3-421	A	45	47			General comments about Section 3.4: Overall, it seems less thorough/coherent than other sections of Chapter 3 - probably reflects comparative lack of research on technology modeling issues, but this need should be prominently emphasized. Specifically, I would like to see the argument developed that because ETC is defined by the response of private actors to contemporaneous price signals, long-	Discussed with Ch2 and Ch11 to remove the overlaps.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						term basic R&D is unlikely to arise through such channels. That is, an emissions penalty applied today cannot induce investment in tomorrow's solutions, implying a need for a more direct technology strategy. Also interesting would be a discussion about the impact of firms' decision-making with regard to capital investments on the rate of technology diffusion, and how this is represented in the various modeling scenarios. Finally, there are many crossover opportunities between this section and section 2.9 in the framing chapter - should be more clear what this section is trying to cover that 2.9 does not. Similarly section 11.3 has many of the same ideas. (Geoffrey Blanford, Stanford University)	
3-422	A	45	47	51	50	The section on the role of technologies contains a lot of generic material on how technology changes that belongs in chapter 2 (and in chapter 2- and ch 11- there is material on results of scenario studies that belongs in chapter 3). So there needs to be a consolidtion of material. This chapter should concentrate on how technology is being treated in integrated models that are used for long-term mitigation analaysis. The point made in lines 22 on page 46 till line 12 on page 47 is important, but can be made in a shorter text, particularly becaue there is a good figure (and round off the numbers please). The section on decarbonisation trends (3.4.1.1) could be much shorter (less than 3 figures too) and would fit much better in section 3.3.3 to zoom in on the decarbonisation of the energy sector as an important component of mitigation/ stabilisation strategies, unless it is only used as an example to illustrate the way technological change is handled in long-term mitigation scenarios (then some of it could be retained here). The paragraphs on page 51, lines 5-32 belong in section 3.3.3.3 on costs, not here. Investment patterns (ie the differences of energy system investment in stabilisation vs basline scenarios) should be covered. (Bert Metz, IPCC)	Accepted. Changes will be incorporated.
3-423	A	45	50	51	50	section 3.4 may find it interesting to consider the extensive review of the mainstream economic literature on endogenous technological change and its empirical basis by Kohler et al (2006) (Michael Grubb, Cambridge University)	Accepted. Will ask reviewer for references and incorporate relevant material.
3-425	A	46	1			"four" should read "five". (Sjak Smulders, Tilburg University)	Accepted. Will be corrected.
3-426	A	46	5	46	20	mitigation in transportation technologies should also be discussed here (Jonathan Köhler, Tyndall Centre, University of Cambridge)	Accepted. Will be included.
3-427	A	46	5	46	5	Change "In principle there are four different ways technology can help reduce future GHG emissions:' to "The ways in which technology can help reduce future	Accepted. Will be included.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						GHG emissions include:" The list that follow has five items, and is not comprehensive. It considers only energy related CO2 emissions options. The previous section highlighted the importance of non-CO2 gases and land-use options, both of which are also affected by technology. A bullet point should be added to the list to include these options. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	
3-428	A	46	5			Text says four ways, but there are five bullet points. Moreover, this classification is good, but perhaps excludes a sixth category like "new configurations", e.g. hydrogen as a carrier, fuel cells, storage, long-distance electric transmission, etc. These kinds of developments can act as complements to the listed technology categories, and in particular give them leverage on the transportation sector. (Geoffrey Blanford, Stanford University)	Accepted. Will be included.
3-429	A	46	5	46	5	Modify the sentence by: "In principle, there are five…" (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	Accepted.
3-430	A	46	5			"there are four different". Maybe it should be "five" since five items are listed following this line. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted.
3-431	A	46	8	46	10	what are "fossil intensive" sources ? Maybe it's carbon/emission intensive … (Peter Kolp, IIASA)	Accepted. Will be corrected to "carbon intensive…"
3-432	A	46	8	46	10	what are "fossil intensive" sources ? Maybe it's carbon/emission intensive … (Peter Kolp, IIASA)	Repeat of above.
3-433	A	46	12	46	14	From a life-cycle perspective if biomass harvested is not replaced, the result would not be net removal. (Kenneth Möllersten, Swedish Energy Agency)	Accepted. Will clarify and improve language.
3-434	A	46	13		25	This is not very precise and not very scientific. It reads as a policy report, just saying "R&D is important, we need more" rather than explaining why how and where it si important. Please cite evidence for the link between marginal increases in R&D and energy intensity reductions, and other climate-related things. (Sjak Smulders, Tilburg University)	
3-435	A	46	18	46	20	Also less publically accepted nuclear energy can help reduce future GHG emissions. Don't mix issues in one sentence. (Leo Schrattenholzer, IIASA)	Accepted. Will be split in two sentences.
3-436	A	46	18	46	20	increasing the nuclear energy share will help to reduce GHG emissions. That's not depending on the introduction of inherently save reactors, fuel cycles, etc. (Peter Kolp, IIASA)	Same as above.
3-437	A	46	18	46	20	increasing the nuclear energy share will help to reduce GHG emissions. That's not	Same as above.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						depending on the introduction of inherently save reactors, fuel cycles, etc. (Peter Kolp, IIASA)	
3-438	A	46	22	47	18	This discussion of projection with frozen technologies covers the same ground, but with different references to that in chapter 2 section 2.9.1.1 (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Accepted. Will refer to chapter 2 text.
3-439	A	46	23	46	24	Emission intensities of what? (Leo Schrattenholzer, IIASA)	Accepted. Will check and change.
3-440	A	46	24	46	24	For the benefit of readers who are not insiders, it should be explained why the case is unrealistic. Also, it should be said that global values are meant. Finally, I propose to eliminate the expression 'utterly' as non-scientific. (Leo Schrattenholzer, IIASA)	Accepted.
3-441	A	46	24			"Here...". Please explain what you are going to do first before writing down your assumptions. The following text can be made more logical (e.g. line 50-53 higher up) and can be shortened. (Peter Bosch, IPCC TSU WGIII)	Accepted. Will revise text.
3-442	A	46	26			Figure 3.4-1 should read figure 3.30 (Sjak Smulders, Tilburg University)	Accepted. Will be changed.
3-443	A	46	26			I could not find the “Figure 3.4-1” mentioned here. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. Will be changed.
3-444	A	46	27	46	27	… in the database. Which database ? (Peter Kolp, IIASA)	Accepted. Will be changed to … “scenario database.”
3-445	A	46	27	46	27	… in the database. Which database ? (Peter Kolp, IIASA)	Same as above.
3-446	A	46	31	47	12	This part is not very clear. It is a decomposition analysis, but the explanation in the text needs improvement. E.g. sentence in line 34-35 is incomple. The "steps" are not clearly described. What is held constant, how is structural change in line 36-37 defined??? Nevertheless, the material is very interesting. (Sjak Smulders, Tilburg University)	Accepted. Will revise text.
3-447	A	47	15	47	20	This comment is very cursory and does not reflect the current literature. there are detailed discussions of this issue in Köhler et al and Edenhofer et al. reference details above (Jonathan Köhler, Tyndall Centre, University of Cambridge)	Accepted. Will ask reviewer for references and incorporate relevant material.
3-448	A	47	21	48	17	The estimates of decarbonization rate in this section do not take into account the IEA's World Energy Outlook 2004, which shows no decarbonization globally between 2002 and 2030. In the IEA's reference case, natural gas' share of primary	Accepted. Will mention about IEA’s outlook results.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						energy grows from 21 to 25%, but half of that growth is replacement of non-carbon energy, whose share of primary energy declines from 21 to 19%. (IEA, WEO 2004, Pg. 430). A crude calculation assigning ratios of 5:4:3:0 to coal, oil natural gas, and renewables shows a slight increase in carbon intensity between 2002 and 2030. If one further considers that a growing portion of oil's share of primary energy will come from heavy crude and tar sands, higher carbon content sources, it is hard to justify the steady reduction in carbon intensity projected in this section. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	
3-449	A	47	21	49	9	The discussion of "decarbonization" should also assess energy outlooks and trends that show increasing carbon per unit primary energy from the recent past through 2030 (e.g. "recarbonization" in the IEA energy outlook). Major factors affecting this are the limited growth rate of nuclear, and bioenergy in outlooks (relative to fossil fuels). (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted. Will mention about IEA's outlook results.
3-450	A	47	30	47	34	It seems like over the last 20 years decarbonization rate increased. (Alexander Golub, Environmental Defense)	
3-451	A	47	31	47	32	Like in the previous two sections … not clear to me what sections you're refering to (Peter Kolp, IIASA)	Accepted. Will be changed to "Like in sections 3.2 and 3.3..."
3-452	A	47	31	47	32	Like in the previous two sections … not clear to me what sections you're refering to (Peter Kolp, IIASA)	Same as above.
3-453	A	47	43	47	44	… presented in the previous two sections) … not clear to me what sections you're refering to (Peter Kolp, IIASA)	Accepted. Will be changed to "Like in sections 3.2 and 3.3..."
3-454	A	47	43	47	44	… presented in the previous two sections) … not clear to me what sections you're refering to (Peter Kolp, IIASA)	Same as above.
3-455	A	48	25	52	20	P48 and further partly duplicate with ch2. General sections, that is with no specific reference to the long-term can be taken out. (Peter Bosch, IPCC TSU WGIII)	Discussed with ch2 to remove overlaps.
3-456	A	48	37	48	37	It is proposed to insert "is" after "GHG intensities". (Radunsky Klaus, Umweltbundesamt)	Accepted. Will be inserted.
3-457	A	48	37			I think an "is" should be added before "one of the major …". (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. Will be inserted.
3-458	A	48	39			Section 3.4.2 This section is very cursory and does not reflect the current literature. (Jonathan Köhler, Tyndall Centre, University of Cambridge)	Accepted. Will ask reviewer for references and incorporate relevant material.
3-459	A	48	39			Section 3.4.2 comment:I propose to refer to Chapter 11, Section 11.3.	Accepted. Will refer to ch 11.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Leo Schrattenholzer, IIASA)	
3-460	A	48	41			Is there any data support for the "woefully inadequate" statement? Numbers on energy- or carbon-related research in relation to total R&D investment would seem to be most appropriate. Not sure of the relevance of the quoted statistics in section 3.4.2.1. In general, section 3.4.2 seems weak on its own - either bulk it up or combine with another section. Perhaps mention the Hoffert papers, new since TAR. (Geoffrey Blanford, Stanford University)	Accepted, text to be revised
3-461	A	48	44	48	44	The first '10' appears wrong. (Leo Schrattenholzer, IIASA)	Accepted. Will be deleted.
3-462	A	48	44			"world' 30 poorest countries", I think it should be "world's 30 …" ; and "10 Over past 10 years", it seems the first "10" should be deleted. (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. Will be corrected.
3-463	A	48	45			The section on R&D statistics should have some reference. Suggest OECD: 2002, OECD Science, Technology and Industry Outlook, OECD Publication Service, Paris, 325 pp. It might be noted that essentially all R+D growth is in the private sector and that this is dominated by the health and high-tech areas, not energy. Some statistics on energy R&D should also be considered (e.g. from J Dooley at PNNL). The US and Japan dominate spending on energy R+D. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted. Will be incorporated.
3-464	A	48	46	48	46	I propose to eliminate the expression 'woefully' as non-scientific (Leo Schrattenholzer, IIASA)	Accepted. Will be deleted.
3-465	A	48	48	49	9	Are those data published somewhere? If they are, what are the references? (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. Will include reference.
3-466	A	48	48	49	9	This does not not contribute to a clear point particlarly the final phrase of line 54, the paragraph at the top of page 49 does not appear to be relevant. (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	Accepted. Text will be revised.
3-467	A	49	5	49	9	Delete this paragraph. The facts about pharmaceutical industry R&D spending are no doubt correct, but they have no relevance to GHG mitigation. (Lenny Bernstein, L. S. Bernstein & Associates, L.L.C.)	Accepted. Text will be revised.
3-468	A	49	5	49	9	It is proposed to delete the information on money spent in the pharmaceutical and biotechnology industries because there is no rational just to pick one branch and the issue of opportunity costs is much broader and beyond the scope of the AR4. (Radunsky Klaus, Umweltbundesamt)	Accepted. Text will be revised.
3-469	A	49	5		10	Are all these figures relevant here?	Accepted. Text will be revised.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Peter Bosch, IPCC TSU WGIII)	
3-470	A	49	11	49	25	This section should not only discuss the development of new exotic technologies and the investments and efforts needed to achieve emission reductions through these technologies but also should discuss available traditional knowledge and low cost technologies (especially in developing countries) that are currently being used. These technologies through investments in them can be developed as viable mitigation options. (Junichi Fujino, National Institute for Environmental Studies)	This aspect is covered in section 3.4.3.3.
3-471	A	49	15	49	15	This section (without references) advocates "support" for market introduction of technology broadly. Perhaps more important is that the private sector capture rewards for technologies that are successful (and suffer the consequences for those that are not successful) in the marketplace (see Flannery, B. P., and Kheshgi, H. S. (2004). An industry perspective on successful development and global commercialization of innovative technologies for GHG mitigation. Intergovernmental Panel on Climate Change Workshop on Industry Technology Development, Transfer and Diffusion, Tokyo, IPCC.) There are different roles for public and private investment in R+D. Suggest changing "Support" to "Investment". (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Will check references. “Support” includes investments and also need for investments highlighted in the paragraph already.
3-472	A	49	21	49	25	This paragraph, without reference, states a 50-year timeframe over which known technologies can make deep cuts. The timescales for some technologies is shorter than 50 years. Some technologies under research will be needed within 50 years simply to provide for energy needs. Of course plasma physics and nanotechnology (eg catalysis) have been contributing for decades. Suggest that this paragraph be reconsidered and reflect the ways in which R+D can contribute rather than the implicit assumption that R+D will not contribute over the next 50 years. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted. Will revise the text. Will check references.
3-473	A	49	27			Section 3.4.3 comment:I propose to include the following reference: International Energy Agency, 2003: Creating Markets for Energy Technologies. IEA / OECD, Paris. (Leo Schrattenholzer, IIASA)	Accepted. Will refer to the suugested material.
3-474	A	49	37	49	38	delete "make" (H-Holger Rogner, IAEA)	Accepted. ‘make’ will be changed to ‘making’
3-475	A	49	47	50	15	Learning by doing is also covered, more extensively , in chapter 2, section 2.9.2.1. Cross reference. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University	Accepted. Will add reference.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						of Cambridge)	
3-476	A	50	5	50	7	Add to first sentence: ...developments and is also supported by the systems of innovation approach. (Rainer Walz, Fraunhofer Institute Systems and Innovation Research)	Will ask reviewer to clarify the meaning of 'systems of innovation approach' and ask for reference to quote.
3-477	A	50	12			"learning by doing is a strong argument for early action". This is not true, unless you get the dynamics wrong. Please read Goulder and Mathai. (Richard Tol, Hamburg University)	Will ask reviewer for exact paper to refer to and incorporate accordingly.
3-478	A	50	17	0	0	Similarly the same ground is covered in 2.9.2.3 (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Accepted. Will refer to ch2.
3-479	A	50	19	50	19	Spell out 'EST'. (Leo Schrattenholzer, IIASA)	Accepted. Will expand to "Environmentally Sound technologies"
3-480	A	50	19	50	21	Acronyms EST and EIT not defined. (Geoffrey Blanford, Stanford University)	Accepted. Will expand to "Environmentally Sound technologies" and "Economies in transition"
3-481	A	50	21	50	25	Please, add technology lock-in to the list of barriers. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted. Will be added.
3-482	A	50	21	50	21	Spell out 'EIT'. (Leo Schrattenholzer, IIASA)	Accepted. Will expand to "Economies in transition"
3-483	A	50	35	50	41	Energy security' matters, as discussed above, may also be worth mentioning specifically, in terms of a national policy driver that can have strong alignment with climate goals, even if climate mitigation is not the primary rationale for policy development (unless this is already understood to be part of development - some other chapters mention energy security directly). (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	Accepted. Will be included.
3-484	A	50	53	50	53	Typo error (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Accepted. Will change to "Dynamics of technology…"
3-485	A	50	53			Section 3.4.3.4. is not covering the available literature very well, e.g. several EU projects explored different aspects of technolgy dynamics, including mixed market-push / technology pull analyses and uncertainty aspects of RD&D stimulation programs. These discussions are very pertinent to the current discussions on the role for technology strategies in climate policy making. (Tom Kram, MNP)	Accepted. Will ask reviewer for references and included accordingly.
3-486	A	50	53			"Dynamics technology …" maybe it should be "Dynamics of technology …". (Tieju Ma, International Institute for Applied Systems Analysis)	Accepted. Will change to "Dynamics of technology…"

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3-487	A	50	53	0	0	This section covers the same ground as 11.3.4 and 11.5. There are important common themes in the coverage in chapters 2, 3 and 11 that require coordination and agreement as to which literature and results appear where. The problem is partly that the induced technological change (ITC) literature is developing very rapidly, with many papers under review and in press. Furthermore it is clear that different assumptions about the treatment of technological change yield very different estimates as to its effect, for reasons that need to be explained. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Discussed with ch2 and ch11 to remove any overlaps. Will include ITC in the discussion.
3-488	A	51	5	51	50	The European Commissions"World Energy, Technology and Climate Policy Outlook 2030" http://europa.eu.int/comm/research/energy/pdf/weto_final_report.pdf is containing a section where the impact of exogenous assumptions on the global emissions is shown.Also here the conclusion is that advanced technology can lower significantly the cost of emission reduction. However, it is also shown that technology allown will not lead to emission reduction (leakage effects, etc.) (Peter Russ, IPTS, Joint Research Centre, European Commission)	Accepted. Will refer to the suggested material and include accordingly.
3-489	A	51	13			"reduce costs substantially" This is not true. It is a partial equilibrium result, that falls apart in general equilibrium. Please read Bovenberg and Goulder; Smulders. Note that page 59 does refer to Smulders and does get the sign right. (Richard Tol, Hamburg University)	Accepted. Will refer to the suggested material and include accordingly.
3-490	A	51	21		32	The studies cited all point in the same direction. However, these studies are biased in their set-up. They only allow for induced technological change that saves on emissions. If one would make growth in the economy depend on investments in new technologies, then climate change policy crowds out "regular" R&D and climate change policy is more costly with endogenous technological change than without. This mechanism has not been studied in calibrated climate change models, but the mechanism has been demonstrated in general models (see Smulders 1998 and Smulders and De Nooij 2003). (Sjak Smulders, Tilburg University)	Accepted. Will refer to the suggested material and include accordingly.
3-491	A	51	21	51	50	Clarke and Weyant (2002), p. 332) make the important point that in the presence of market failure to innovate sufficently (because private firms cannot capture all the benefits of their R&D and innovation) means that solutions of optimising models with ITC are no longer optimal. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Will check with reviewer for clarification.

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3-492	A	51	21	51	50	A table of ITC effects on GDP or welfare costs and on permit prices or carbon tax rates would be helpful. There is new literature to be included here, see (Edenhofer, 2006) (Energy Journal IMCP special issue). (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	Will check with reviewer for clarification.
3-493	A	51	35	51	50	In recent economic literature increasing return to scale has been broadly discussed in the context of cost of emission reduction. It was noticed that increasing return may significantly reduce the cost of climate mitigation. There are several factors that lead to increasing return, some of them associated to new technologies. Some references to Grubler and Gritsevsky, 2002 in Grubler et al. technological change and environment; Buonanno et al 2003. Resource and Energy Economics, N 25; Golub 2005, World Resource Review. N 1. (Alexander Golub, Environmental Defense)	Accepted. Will refer to the suggested material and include accordingly.
3-494	A	51	43	51	44	However, technology learning is a cost burden to current and near-term generations while the learning benefits are reaped by future generations. So there is a real cost increse in the sort run which later is balanced by the gains. (H-Holger Rogner, IAEA)	Yes, in short run investment would be required for RD&D. This point is covered in section 3.4.2.
3-495	A	51	47	51	50	The author mentioned that current technologies can reduce up to 5GtC emission a year. Is this because the efficiency of current technologies is improved? (Tieju Ma, International Institute for Applied Systems Analysis)	The reduction using current technology includes utilizing fuller potential of existing resources and efficiency improvement.
3-496	A	51	52	54	22	These issues referred in the above comment are hardly addressed, although the preliminary early October version of the chapter did contain relevant material. In addition, section 3.5.2 is currently very incomplete, with an unavailable table. You may have considered that some of this material (the impacts and valuation stuff in particular) does not belong to WG III. Of course that is true, WG II is the place where these things are discussed primarily. However, summarising that information (and making sure it is in full conformity with the WG II report as it evolves) would serve the purpose of bringing the elements of decision making together and serve the user community on a very complex and important aspect of climate policy making. You may also face limited availability of literature on these issues. Of course that is well understood, you can not go beyond what has been published. However, the policy makers decided at IPCC 22 in the outline of ch 3 to include "interaction of mitigation and adaptation,...not only present costs of mitigation but also avoided climate change damage and costs of adaptation should be included "; so their expectations on these issues are appearently high. (Bert Metz, IPCC)	agree- material to be added to section 3.5.2 & to include a summary of key points from relevant part of WGII

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3-497	A	51	52	54	24	It is hard to know how to comment on section 3.5 without knowing the proposed relationship of this work to the background in Chapter 2 and, more specifically, the work in WGII, most obviously Chapter 18 but also 19 and 20. (Michael Grubb, Cambridge University)	agree
3-498	A	51	52	54	22	This section was designed to bring together the pieces that decision makers need to have when deciding what response policy to aim for, i.e. it was supposed to bring together the information about mitigation,stabilisation, adaptation and avoided (and remaining) impacts. This would help decisison makers to take decisions on stabilisation levels to aim for and the mix of mitigation and adaptation responses.This would mean discussion of: methods to decide on long-term stabilization targest, using information on mitigation and adaptation costs and benefits of avoided climate change damages (both CBA as cost-effectiveness approaches; including summaries of quantitative findings from WG II on damages and adaptation) and a good discussion on trade-offs and synergies between adaptation and mitigation. (Bert Metz, IPCC)	agree but lead on decision frameworks to be in WGII/Ch2 (also Ch. 18/WGII) and cross-referenced here.
3-499	A	51	54			Here it is acknowledged that there is no such thing as an "optimal" path (and you even put the term in quotes!). This seems to vindicate my earlier points. (Danny Harvey, University of Toronto)	Noted
3-500	A	52	22	53	51	This discussion is repeated in many Chapters (2, 11, etc). It is better to put all the material in one chapter and increase cross-referencing. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Yes - but the issues are in the agreed outline from the plenary and require some development in this chapter for completeness
3-501	A	52	22			p. 52. Section 3.5.1. Some reference should be given to all sections dealing with the same question in other chapters and in the report from WG II. This is essential to ensure coherence of the messages. (Philippe Tulkens, TERI School of Advanced Studies)	Ok - agree. We will adapt the text; also to cross-reference WGII unavoidable impacts at different GMT change and connect to mitigation scenario discussion.
3-502	A	52	23			Section 3.5.1. fails to mention the all-important temporal issue in the mitigation/adaptation discussion, an important limiting factor to any atempt at balanced overall strategies. Concepts like committed warming and unavoidable climate impacts deserve more attention here, anticipating further timing discussions in 3.6.1.. (Tom Kram, MNP)	Noted – will cross reference to chapter 18 WGII and WGI
3-503	A	52	23	53	15	Given the importance of this section to the non-market sector and ecosystem	Agree - more space will be devoted to these

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						services, and in turn to article 2 on avoiding dangerous anthropogenic interference on ecosystems, then the entire section needs to be alloted more space and table 3.9 expanded. It ranks as equally important to material that received far more text pages. As those pages with basic information are reduced they could then be alloted to this section. (Jeff Price, California State University, Chico)	issues in the next version of the report
3-504	A	52	24	53	51	This section refers a lot to Tol, but it omits five papers in which he shows how emission reduction would increase impacts (Tol and Dowlatabadi, 2001, Integrated Assessment; Tol and Yohe, Exeter book; Tol, 2005, Environment and Development Economics; Tol, 2005, Environmental Science and Policy; Tol, forthcoming, Mitigation and Adaptation Strategies). (Richard Tol, Hamburg University)	We agree to look at these references and to the extent they are relevant to the issues of the section, to take them into account in the SOD
3-505	A	52	49	52	41	It should also be noted that many analysts and activists have not wanted to focus on adaptation because it would seem to be "giving up" on mitigation. (I'm not sure that there is any citation to this effect, but it's common knowledge in the climate policy community). (Paul Baer, Stanford University)	Ok - to add a sentence to acknowledge this view in politics of climate change.
3-506	A	52	53			This implies that there is such a thing as an "optimal" trade-off between mitigation and adaptation. Given that mitigation costs are borne primarily by different persons and groups than those who will be forced to adapt, and that adaptation will not prevent many persons, communities and ecosystems from being harmed, there is unlikely to be the kind of consensus implied by analytic "optimality." (Paul Baer, Stanford University)	Noted
3-507	A	53	15			Table 3.9: Earlier references for "Shift in area extent by type of ecosystem" include: • H.-M. Füssel, J. G. van Minnen: Climate impact response functions for terrestrial ecosystems. Integrated Assessment 2(4):183-197, 2001 • F.L. Toth, T. Bruckner, H.-M. Füssel, M. Leimbach, G. Petschel-Held, H.-J. Schellnhuber: Exploring Options for Global Climate Policy: A New Analytical Framework. Environment 44(5):22-34, 2002 (Hans-Martin Fuessel, Stanford University)	Ok - references to be added to the table.
3-508	A	53	38	53	40	Tol 2005 (Energy Policy) gives higher estimates of damage than in Tol 2000. (Alexander Golub, Environmental Defense)	ok to check this reference and include it.
3-509	A	53	44			Ideas for harmonizing adaptation and mitigation deserve special attention. Specifically, this includes alternative energy sources for distributed generation. Such measures can increase adaptive ability, energy security, carry benefits for health and development, and help create markets for clean energy technologies thus	Ok agree with this point but the lead development of these issues is likely to be in another chapter (eg ch.2) and we will just follow this lead, summarising some key points

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						forward mitigation. www.climatechangefutures.org, pp103-104. Epstein, PR, Mills, E. 2005. Climate Change Futures: Health , Ecological and Economic Dimensions, Center for Health and the Global Environment, Harvard Medical School, Boston, MA. (Paul Epstein, Harvard Medical School)	here.
3-510	A	53	45			insert "that" after "shows" (Danny Harvey, University of Toronto)	done
3-511	A	53	52	54	22	Please refer to Chapter 11, discussion on air pollution and climate change from Markus Amann. Improve consistency (Monique Hoogwijk, Ecofys)	this comment is not relevant to this section - it pertains to ancillary benefits which is covered in another table and section (see section starting on p. 41, section 3335, and Table 3.7.
3-512	A	54	5	54	22	I look forward to seeing this table as it will likely be of major importance and significance to this chapter. Again more space needs to be allocated to this topic. If space is of concern (it always is) then other portions should be shortened so more information can go in this section. (Jeff Price, California State University, Chico)	agree - table will be modified to a more practical format which corresponds directly to work of WGII.
3-513	A	54	5	54	22	How in the world is this going to be done? Particularly with regard to avoided damages, the possible impacts of both a non-stabilization scenario like A1F1 and a stabilization scenario (whether at 550 or any other level) vary (first) with the uncertainty in climate sensitivity, and (second) with unknown probabilities for melting ice caps, etc. (Paul Baer, Stanford University)	see comment above; we are modifying the table and will use Meinhausen's work on risk of overshooting temperature for different ppm stabilisation levels as a bridge to connect impact and key vulnerability assessment (WGII) with mitigation scenarios in this chapter.
3-514	A	54	24	64	25	section 3.6 appears to be very mixed without a coherent thread, and it also has major duplication in some areas with Chapter 2 (in particular - eg. on discounting) and also areas of WGII (Chapters 2, 18, 19 in particular). Eg. I am not sure we need another rendition of basic principles of decision-making. Surely the framing should belong elsewhere, and this section shoudl concentrate on insights emerging when the fact-base is put together? The linkage with chapter 11 remains to be developed. (Michael Grubb, Cambridge University)	The articulation between the subsections will be improved ; about the discount rate, what will conserved in this chapter is the discussion of its practical influence of empirical models weighting policy costs and benefits
3-515	A	54	31	54	34	Noting here, in addition, that conclusions are already being drawn eg the Tyndall briefing (Kohler et al, 2005; referenced above ch2, l. 19) on the importance of near term investment in technological change in the next 5 years, in addition to the theoretical approaches to this type of decision. I note that this issue is also picked	The importance of investment decisions and of government policies will be quoted as part as the key drivers to be incorporated in the analysis

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						up within 3.6.1.5. (This latter reflects greater linkage with real-world issues, noting IEA (2003) World Energy Investment Outlook's reinforces the importance of government stimulating investment conditions (through policy and incentives) for renewable energy, for example, 'if renewables are to play their expected role in climate change abatement and energy security' (p 92). (Kirsty Hamilton, retainer to UK Business Council for Sustainable Energy; Associate Fellow, Chatham House.)	
3-516	A	54	46			Section 3.6.1. Maybe here the literature about overshoot and peaking scenarios can be described here in more detail. See comments above. (Michel den Elzen, The Netherlands Environmental Agency)	This will be done in function of the new material
3-517	A	54	46	64	25	The title of this section was modified compared to the approved outline. That is not helpful, because it suggests a shift in emphasis (which may not be intended). The section is meant to discuss the links between short-term and long-term action (i.e. what is needed in the shorter term to get to long-term stabilisation). The section was also specifically meant to provide a "bridge" between ch 3 and the short/ medium chapters 4-11. Many ofthe issue discussed in this section however seem to address other issues. The (useful) section on "timing" (3.6.1) is dealing 1) with decision making on long-term targets and affiliated emissions trajectories (an issue that was designed to be in section 3.5); 2) with technological change (ATC, ITC) which belongs in sevction 3.4 (and is actually also discussed there); and, 3) with timing of specific mitigation options such as CO2 capture and storage (NOT geological sequestration, please) and non-CO2 gases that belongs in 3.3.3 (where it also is addressed already) The hedging part (3.6.2) is ok for this section. But what is missing is the link between the sectoral chapters and the long-term stabilisation scenarios. I strongly suggest to try and use the Pacala/Socolow "wedges" approach (see Science 13 August 2004:Vol. 305. no. 5686, pp. 968 - 972) . By extracting 2050 numbers for the various mitigation options fromthe stabilisation scenario studies and "translating" them in specific actions between now and 2050, based on the chapter 4-10 material an impression can be given about the magnitude of the challenge and a check can be made if , from bottom-up studies, the potential for such actions is there. In TAR this aspect has received almost no attention, while it is critical for policy. (Bert Metz, IPCC)	This remark will be incorporated in the new structure of this section
3-518	A	54	53	54	54	Modify the sentence by: "…and the influence of CO2 capture and storage, biological carbon sequestration and non-CO2 gases options on the time profile of	OK: thanks for the suggestion

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						decarbonisation efforts." (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	
3-519	A	55	4			insert "so-called" before "optimal" (Danny Harvey, University of Toronto)	OK, this will be done
3-520	A	55	19	55	24	Discount rates are not "the main" difference between cost-benefit analysis and threshold-based (cost-effectiveness) approaches. There are several other crucial differences: (1) cost-benefit analysis requires valuation of damages including loss of life, loss of biodiversity, etc; (2) cost-benefit analysis requires a unique probability distribution for impacts; and (3) cost-benefit analysis relies on hypothetical compensation for its moral justification. (Paul Baer, Stanford University)	The abruptness of the sentence about the fact that the "main" difference between cost-benefit and cost-effectiveness analysis will be corrected and other factors will be mentioned. However, it should be clear a) that cost-benefit analysis can, in theory at least, incorporate thresholds (this is a matter of primary information) and account for situations in which, for whatever reason, compensations cannot operate b) in a stochastic framework, cost-effectiveness analysis also uses a unique probability distribution; but this distribution is applied to targets or dangerous thresholds, not to impacts. This is why the difference between both approaches, in theory, should not be as drastic as it seems, even though this difference is very significant in many of the existing cost-benefit analysis.
3-521	A	55	25	56	11	This discussion is repeated in many Chapters (2, 11, etc). It is better to put all the material in one chapter and increase cross-referencing. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Yes, we will conserve here only what is useful to understand the findings of empirical models
3-522	A	55	25			Section 3.6.1.2: A cross-reference should be added to Section 2.5.4.1 for a more extensive discussion of discounting in the climate change context. (Hans-Martin Fuessel, Stanford University)	OK, this will be done
3-523	A	55	25			Subchapter 3.6.1.2 is well written and should not change significantly when aligning corrresponding subchapters in chapter 2.5. (Radunsky Klaus, Umweltbundesamt)	Many thanks; we think indeed that the alignment with 2.5 should not alter the content of this section
3-524	A	55	32			Tol (2004) may not be the best source. Hasselmann, Hope, and Yang have argued this more forcefully. In fact, in a commentary to Yang, Tol has argued against	Thanks for the reference, we will check that. We will probably introduce a remark about the

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						double-discounting. (Richard Tol, Hamburg University)	risks of double-discounting unless the core of this discussion is ultimately introduced in chapter 2
3-525	A	55	32	55	38	I would suggest to refer to the discussion in chapter 2, instead of adding new references here (but they could be added in chapter 2). There is a strong case for a decreasing discount rate due to uncertainties on future growth rates, and for increasing valuation of environmental assets that are non reproducible nor substitutable - but not for a zero discount rate. (Cédric Philibert, International Energy Agency)	Yes, we will conserve here only what is useful to understand the findings of empirical models
3-526	A	55	32	55	33	I don't think this is true. I think most concerns about non-zero discount rates arise because of moral concerns about itreating harms to persons in the future as of less moral weight than harms to present persons. Furthermore, the examples later in this paragraph do not address uncertainty about the future either. (Paul Baer, Stanford University)	well taken: a copy paste mistake probably explain why the “moral concerns’ issues disappeared from the paragraph which comes back to that in an indirect form quoting Guesnerie and intergenerational equity
3-527	A	55	45	55	47	The distribution of wealth and income should be added to this list. (Paul Baer, Stanford University)	This will be done
3-528	A	56	15			"cost-effectiveness" is the proper English word; "cost-efficiency" is French English (Richard Tol, Hamburg University)	Thanks; not that the proper French word for French English is Franglais!
3-529	A	56	15	56	18	If one means by consensus "unanimity" this is certainly true. But if one means "enough agreement to come to a policy decision", there is no special reason to think that such agreement can't be reached. If this is intended as a contrast to cost-benefit analysis, there would certainly be no consensus on a discount rate or any number of other important parameters either. (Paul Baer, Stanford University)	Thanks for pointing out possible misinterpretation. The idea was to say that, since there is no automatic agreement about what is tolerable, some form of ‘negotiation’ or trade-off remains necessary.
3-530	A	56	29	56	29	References to the tolerable windows approach should also include two more recent publications: (a) F.L. Toth (guest editor), 2003: Integrated Assessment of Climate Protection Strategies (ICLIPS). Climatic Change 56(1-2), special issue -- and (b) F.L. Toth, T. Bruckner, H.-M. Füssel, M. Leimbach, G. Petschel-Held, H.-J. Schellnhuber, 2002: "Exploring Options for Global Climate Policy: A New Analytical Framework". Environment 44(5):22-34 (Hans-Martin Fuessel, Stanford University)	Thanks for signaling these publications
3-531	A	56	29			I agree that concentration ceilings are a poor surrogate for dangerous climatic change, but they are not a poor surrogate for dangerous anthropogenic interference in the climate system. However, the goal of the UNFCCC is to stabilize concentrations at levels that prevent dangerous interference so, if we are going to	We do not see a big problem here. This sentence simply means that passing from concentration ceiling to temperature allows for making explicit some key parameters of the

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						be relevant to the UNFCCC, we have to deal with concentrations. To determine which concentrations are dangerous, we need to decide what amount of climatic change is dangerous, then adopt a plausible upper limit to climate sensitivity, and use the two of them to decide on concentration limits which, adopting the upper limit to climate sensitivity, still avoids unacceptable harm. The discussion here seems to have forgotten the actual wording of the UNFCCC. (Danny Harvey, University of Toronto)	analyis. May be we will have to change the expression 'poor surrogate' which apparently can be mistinterpreted
3-532	A	56	35	56	36	Moreover … takes into account the rate of climate change … does it ? - the global mean temperature ? (Peter Kolp, IIASA)	OK we meant that it can be used to account for ….
3-533	A	56	35	56	36	Moreover … takes into account the rate of climate change … does it ? - the global mean temperature ? (Peter Kolp, IIASA)	cf 532
3-534	A	56	38	56	53	Maybe mention here the study of Meinshausen and Hare about the linkage between risks to overshoot temperature targets and concentration stabilization levels? (Michel den Elzen, The Netherlands Environmental Agency)	We will certainly do so
3-535	A	56	39	56	52	Suggest that this section refer to AR4 WG1 section on climate sensitivity. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	This will be done
3-536	A	56	45	56	45	Erratum:note in uncertainty (FÉLIX HERNÁNDEZ, IEG-CSIC)	Thanks
3-537	A	57	14			Ambrosi did not estimate the willingness to pay; they just assumed some number. (Richard Tol, Hamburg University)	no, they did not assume 'some' numbers: they first calculate the implicit willingness to pay consistent with the adoption of three given concentration ceiling (corresponding to three "ex-ante' views of the problem) and then conducted a stochastic analysis with arrival of new information about risks. This modeling technique interpret in monetary terms the claim for certain targets and allows for overshoots.
3-538	A	57	17			missing reference (Danny Harvey, University of Toronto)	OK
3-539	A	57	19	57	23	Schneider came to similar conclusion applying probabalistic approach. (Alexander Golub, Environmental Defense)	We will look at the reference
3-540	A	57	40	57	41	What are "power functions with integer exponents or polynomial ones"? Power	you are right; however people generally make

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						functions with integer exponents are a special case of polynomial functions, hence it is sufficient to refer to the latter. (Hans-Martin Fuessel, Stanford University)	the difference to qualify the specifications
3-541	A	58	10			All these studies speculate on impacts. In contrast, Link and Tol (2004, Portuguese Economic Journal) use an impacts model, and reach the opposite conclusion. (Richard Tol, Hamburg University)	Some of the works quoted here are theoretical in nature. Others do use impact analysis even though they do not conduct it in the same way as Link and Tol. We will reinforce here the qualitative nature of the insights given that, concerning agriculture for example, the assessment of damages and benefits is very conditional upon a large set of assumptions about expectations, the adaptative capacities under uncertainty, the functioning of the markets and the formal or informal insurance frameworks.
3-542	A	58	19	58	21	It should be incorporated here the following discussion. Due to the possibility of a climate catastrophe in the short time it may be worthwhile to invest in technology development that can yield negative CO2 emission. At least large scale demonstration of these technologies should be performed in a way that if large scale use become necessary enough know-how is available. (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	thanks, we will see where and how to better insert this remarks
3-543	A	58	40	0	0	The point about innovation market failure applied here too. (Terry Barker, 4CMR Centre for Climate Change Mitigation Research, University of Cambridge)	this section will be substantially changes anyway
3-544	A	60	14	60	15	Modify the title by: "Timing of action on non-CO2 gases, CO2 capture and storage, biological carbon sequestration, and their implications for de-carbonisation pathways" (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	Thanks for the suggestion, we will follow it
3-545	A	60	15	60	38	Aaheim, H.A., K.A. Brekke, T. Lystad and A. Torvanger (2001), The trade-off between short- and long-lived greenhouse gases under uncertainty and learning, Working Paper No. 10, CICERO, Oslo, Norway could be referred to. (Asbjørn Torvanger, CICERO)	Ok this reference will be added
3-546	A	60	18	60	18	Suppress the word 'decarbonisation" as CO2 capture and storage (CCS) is a way to de-carbonise the energy system. Use instead: "… of using options other than abandoning fossil fuel energy for achieving climate objectives".	we will duly consider this suggestion

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						(CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	
3-547	A	60	19	60	20	Modify the sentence by: "…; and biological carbon sequestration through…". (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	we will duly consider this suggestion
3-548	A	60	35	60	38	It is proposed to provide additional information on the impact of pathways to stabilisation and temperature in 2100 and rate of temperature change as such information is not inlcuded in chapter 3 but it might be quite relevant. (Radunsky Klaus, Umweltbundesamt)	We will see whether, given space constraints, it is possible to provide more substantive information here. This would be useful indeed.
3-549	A	60	40	60	49	How much can the overall cost be reduced? What is the technical potential? What is the techo-economic potential? (Kenneth Möllersten, Swedish Energy Agency)	Given the overall structure of the report, this is not the right place to discuss this.
3-550	A	60	40	61	19	Here there is no space to discuss coupling of biological and geological carbon sequestration (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)	An extensive discussion of this point should be conducted elsewhere in the report
3-551	A	60	40	61	20	Split this chapter into two chapters: 3.6.1.6.1 Role of biological carbon sequestration, 3.6.1.6.2 Role of CO2 geological storage. By the way, do you want to restrict to geological storage or rather say "Role of CO2 capture and storage (CCS)"?. (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	we will split the section only if there is material enough to do so. We will obviously extend the analysis to capture and storage
3-552	A	60	42	60	49	This paragraph is focused on geological storage only, but may require to address all the storage options. However make clear that ocean storage and its ecological impact are still in the research phase (see IPCC SRCCS 2005 - summary for policymakers p.6 point 8) and that this option has much more uncertainties that the geological storage option. The third storage option, mineral carbonation, is also in the research phase, but certain applications in using waste streams are in the demonstration phase (p6 point 9). Geological storage is the only option that has been proven to be technically and economically feasible under specific conditions (p5 point 7). (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	This section only presents the results of models which incorporate capture, storage and sequestration in their analysis of the optimal hedging strategy. These works make a lot of assumptions about the costs and potentials of these options but do not discuss their scientific basis.
3-553	A	60	42	60	42	Modify the sentence by: "…on the potential for carbon capture and (geologic?) storage, revealing that CCS has the potential to reduce overall mitigation costs and increase flexibility in achieving greenhouse gas emission reductions (see IPCC SRCCS 2005 - summary for policymakers p.2 point 1), but that there is still significant uncertainty about technical options, costs, regulatory aspects, environmental issues and social acceptance". (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	OK, we will duly consider this suggestion

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-554	A	60	45			At the end of the paragraph, add: "Harvey (2003, 2004) has shown that sole reliance on deep ocean carbon sequestration to reduce emissions to the atmosphere will eventually cause atmospheric CO2 concentrations that are likely to violate the UNFCCC, as well as causing significant changes in ocean chemistry that are likely to have adverse effects". References: Harvey, L.D.D, 2004. Declining temporal effectiveness of carbon sequestration: Implications for compliance with the United Nations Framework Convention on Climate Change, Climatic Change 63: 259-290. Harvey, L.D.D., 2003. Impact of deep-ocean carbon sequestration on atmospheric CO2 and on surface-water chemistry, Geophysical Research Letters 30(5), doi:10.1029/2002GLO16224. (Danny Harvey, University of Toronto)	although this is not the place to discuss basic science, we will introduce this caveat
3-555	A	60	47	60	49	No reference is given for Keller. Suggest that this section refer to the SRCCS rather than assumptions about leakage and highly idealized models. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	we will correct that
3-556	A	60	47	60	49	To facilitate understanding the following wording is proposed: Keller cautions that under the assumption of leakage from geological sinks, net damages over long time horizons are dependent on assumptions with regard to the level of cabon stored. (Radunsky Klaus, Umweltbundesamt)	OK, we will duly consider this suggestion
3-557	A	61	21	61	21	Change the number of the paragraph from 3.6.1.6.2 to 3.6.1.6.3 (see above comments) (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	this will depend upon our ultimate solution to your previous remark
3-558	A	61	29	61	44	It is noted that almost the same text has also been included in chapter 3.3.3.2 on page 35, lines 18 to 37. It is proposed to consolidate both texts and to include in one chapter only reference to the other part, thus shortening the whole report. (Radunsky Klaus, Umweltbundesamt)	Thanks for the remark and you precise reading
3-559	A	61	46			Section: 3.6.1.2: I miss a reference to Yohe et al. (Science) (Michel den Elzen, The Netherlands Environmental Agency)	Accepted, Yohe should also have been quoted in other sections
3-560	A	62	4	62	8	This section seems to idealize how a policymaker may view hedging. Emission trajectories are only one of many inputs to a decision making process. Suggest that this section consider portfolio strategies for hedging and technology initiatives (introduced in Chapter 2). (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	
3-561	A	62	16	62	23	I miss quite the studies of multi-gas and CO2 only emission pathways leading to stabilization of GHGs concentrations or climate targets. In general I would expect	Accepted

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						in this section a paragraph briefly describing the main findings of these studies (some even include costs estimates). See also my comments above about peaking and stabilization. For example (CO2-only emission pathways): O'Neill, B. C. and Oppenheimer, M., 2004. Climate change impacts are sensitive to the concentration stabilization path. PNAS, 101(47): 16411-16416; Wigley, T.M.L., Richels, R. and Edmonds, J.A., 1996. Economic and environmental choices in the stabilization of CO2 concentrations: choosing the "right" emissions pathway. Nature, 379: 240-243; Wigley, T. M. L., 2003. Modeling climate change under no-policy and policy emissions pathways, Benefits of climate policy: improving information for policy makers. Organization for Economic Co-operation and Development (OECD), Paris, France; And studies of multi-gas emission pathways based on MACs: den Elzen, M.G.J and Meinshausen, M., 2005. Meeting the EU 2 C climate target: global and regional emission implications. MNP-report 728001031 (www.mnp.nl/en), Netherlands Environmental Assessment Agency (MNP), Bilthoven, the Netherlands. Den Elzen, M.G.J and Meinshausen, M., 2006. Multi-gas emission pathways for meeting the EU 2 C climate target. In: H.J. Schellnhuber, W. Cramer, N. Nakicenovic, T. Wigley and G. Yohe (Editors), Avoiding Dangerous Climate Change. Cambridge University Press, Cambridge, UK. Other multi-gas studies: Meinshausen, M., 2006. What Does a 2 C Target Mean for Greenhouse Gas Concentrations? A Brief Analysis Based on Multi-Gas Emission Pathways and Several Climate Sensitivity Uncertainty Estimates. In: H.J. Schellnhuber, W. Cramer, N. Nakicenovic, T. Wigley and G. Yohe (Editors), Avoiding Dangerous Climate Change, Cambridge, UK. Meinshausen, M., Hare, W.L., Wigley, T.M.L., van Vuuren, D.P., den Elzen, M.G.J and Swart, R., 2005. Multi-gas emission pathways to meet climate targets. Climatic change, in press. There are even more studies about emission pathways. (Michel den Elzen, The Netherlands Environmental Agency)	
3-562	A	62	46	62	46	Is "Figure 3.6-1" the same as Figure 3.34? (Hans-Martin Fuessel, Stanford University)	Yes, noted
3-563	A	62	46	63	9	It is noted that figure 3.34 is practically identical with figure 2.3.1. It is proposed to use only one figure and to include reference. Furthermore the term "caricature" should be substituted by the term "schematic figure". (Radunsky Klaus, Umweltbundesamt)	Noted
3-564	A	63	5	63	7	This explanation of Figure 3.34 is important and should be included in the Figure caption. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Noted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
3-565	A	63	9			Figure 3.34: Essentially the same figure (though with a more sensible design in the middle section) appears in the Chapter 2 FOD (as Figure 2.3.1). Even the text in the two chapters is almost identical (including the same punctuation errors). (Hans-Martin Fuessel, Stanford University)	Noted, see above
3-566	A	63	9			Figure 3.34 is also shown in Chapter 2.(fig 2.3.1) (Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	Noted, see above
3-567	A	63	9			Figure 3.34 should include the explanation given in Ch3, p63, lines 5-7. Note that this figure is redundant with Figure 2.3.1. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Noted, see above
3-568	A	63	26			3.6.2.1 should include a broader set of literature, in particular relating to ways to make emission reductions dependent from actual costs. The section ends up with a recognition of the stock nature of the climate change issue but fails to draw relevant conclusions from this important feature. Because it's a stock pollutant any short term action has little impact per se and it is therefore difficult to justify achieving any precise level of emissions or abatement in the short run. However, abatement costs in any short period of time are likely to grow rapidly with the amount of abatement. Fixed objectives thus bear the risk of leading to either excesive or insufficient action. This risk could be reduced if the level of abatement in any period could be made dependent on abatement costs. Ambitious short term and ambitious but indicative long term objectives combined with some price capping mechanisms could be seen as a robust and long term method to adjust the level of action - although uncertainties on possible damages will remain and prevent achieving a true optimum. (Cédric Philibert, International Energy Agency)	Noted, section being rewritten
3-569	A	63	26	64	25	This section does not appear to cover the important topic of hedging strategies very well. It seems to tangentially review a small set of studies that look at game theory in negotiating international policy. Suggest deleting this section. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted, section will be rewritten
3-570	A	63	41	63	42	It might be appropriate to note that the modelling by Read and Lermitet al involved an over-simple characterization of the carbon cycle, which led to a significant overestimate of the effectiveness of bio-energy based of forest plantations alone. See the authors' own comment in the preface to a forthcoming special issue of Mitigation and adaptation strategies for global change (www.accstrategy.org). (Kenneth Möllersten, Swedish Energy Agency)	Accepted
3-571	A	63	48	64	7	Game theory is discussed in other two chapters also. It is better to bring all discussion to one site and use cross-referencing.	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Considerations by the writing team
						(Jose Moreira, Institute of Electrotechnology and Energy - University of Sao Paulo)	
3-572	A	63	48	64	15	It's not clear how these insights on coalitions from game theory relate to the topic of that sub-section - "empirical studies of hedging strategies". (Cédric Philibert, International Energy Agency)	Accepted, text will be clarified
3-573	A	63	50	64	7	The result of the article concerning the possibility, within a sequential framework, of having developing countries participation in the beyond Kyoto period could be mentioned. (Juan Carlos Ciscar, IPTS, European Commission)	Accepted
3-574	A	63	53	63	53	The term 'concluded' seems somehow strong, because the result comes from an illustration of the methodology. A suggestion would be to state 'could undertake' in line 54, instead of 'undertake'. (Juan Carlos Ciscar, IPTS, European Commission)	Accepted
3-575	A	63	54	63	54	Define Nash equilibrium, and assess/explain why this results occurs and what is meant by 5 and 10%. This should be explained and assessed fully if such results are to be reported. (Haroon Kheshgi, ExoonMobil Research and Engineering Company)	Accepted
3-576	A	63	54	63	54	As Nash equilibrium is a non-cooperation context, the text would be "...case of non-cooperation…" (Juan Carlos Ciscar, IPTS, European Commission)	Accepted
3-577	A	64	5	64	7	it is somewhat surprising to me to see "somewhat surprising" here used. To me a value statement that shouldn't be apear here (Peter Russ, IPTS, Joint Research Centre, European Commission)	Accepted
3-578	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)	Accepted
3-579	A	65	51			Check REFERENCES, some are not cited in the text. (Michel den Elzen, The Netherlands Environmental Agency)	Accepted
3-580	A	66	52	66	54	Reference should read: Criqui, P.; Russ, P., Deybe, D.:"Impacts of multi-gas strategies for greenhouse gas emission abatement: insights from a partial equilibrium model"; the title was changed from the initial proposal!, co-authors are missing! to be published (soon) in Special Issue Energy Journal on EMF 21 (Peter Russ, IPTS, Joint Research Centre, European Commission)	Accepted
3-581	A	67	41	67	41	Please spell my family name correctly. (Leo Schrattenholzer, IIASA)	Accepted
3-582	A	68	16			Replace in REFERENCES list: RIVM (National Institute of Public Health and the	Accepted

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						Environment, Netherlands) by MNP (Netherlands Environmental Assessment Agency) (Michel den Elzen, The Netherlands Environmental Agency)	
3-583	A	68	42	68	44	The viewpoint that today's technology is suffcient to effect necessary emissions reductions. This seems unlikely - even if capital was unlimited, today's technologies would not alone lead to desired emissions reduction. (Lourdes Maurice, US Government)	Accepted
3-584	A	69	18	69	18	I'm sorry for the simple comment, but the reference to Hoogwijk should be Hoogwijk, A. Faaij, B. Eickhout, B. de Vries, W. Turkenburg etc (Monique Hoogwijk, Ecofys)	Accepted
3-585	A	70	21	70	27	Distinguish between the three references to 'IEA, 2002'. (Leo Schrattenholzer, IIASA)	Accepted
3-586	A	70	21	70	22	Add the reference of IPPC Special Report on CO2 Capture and Storage, 2005. (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	Accepted
3-587	A	70	27	70	28	Please quote WEO2005 and use uniform way for the reference, i.e. International Energy Agency, 2002: World Energy Outlook 2002. IEA/OECD, Paris. (Fatih Birol, International Energy Agency)	Accepted
3-588	A	73	15	73	19	Add publication years that enable readers to identify the correct one. (In the text, 2005 is mentioned.) (Leo Schrattenholzer, IIASA)	Accepted
3-589	A	74	33	74	33	Replace (MEIS) by (MIES) (CZERNICHOWSKI-LAURIOL Isabelle, BRGM)	Accepted
3-590	A	77	29	77	29	'2000' instead of '200'. (Leo Schrattenholzer, IIASA)	Accepted