IPCC WGII
Fourth Assessment Report
Climate Change Impacts, Adaptation and Vulnerability

Government and Expert Review of Second Order Draft

Specific Comments

EXPERT REVIEW COMMENTS

Chapter 19

August 2006

Organization of the review comments file

Comments are organized as follows:

(a) First are the comments from the Co-Chairs and TSU. These:
  (i) track the development of the ZOD and FOD, and your responses to review comments on each of these drafts, and then
  (ii) present comments on the Second-Order Draft

(b) Second are the comments from the Expert Reviewers, organized in the same format as your FOD comments file.

Discussion of expert review comments and record keeping

IT IS RECOMMENDED THAT:

- AUTHORS BEGIN WORK ON THE COMMENTS IMMEDIATELY. SUBSTANTIVE COMMENTS NEED TO BE SEPARATED FROM NON-SUBSTANTIVE, AND THE TWO SHOULD BE TREATED DIFFERENTLY
- CONTACT IS MADE BETWEEN AUTHORS AND THEIR REVIEW EDITORS IN AUGUST

Substantive comments

- The chapter writing team should discuss all substantive expert review comments, by email and/or at Cape Town.
- Substantive comments require full and proper consideration. The *Principles Governing IPCC Work* state that:
  - genuine controversies should be reflected adequately in the text of the Report and
  - it is the role of the Review Editors to advise the lead authors on how to handle contentious/controversial issues
- You must record the outcome of these discussions in this document, under the column 'Notes of the Writing Team'.

Non-substantive comments

- For non-substantive comments, a very brief entry should be made in the column 'Notes of the Writing Team'. The following terms are acceptable:
  - Addressed
  - Not applicable
  - Text removed
  - A tick to denote a comment has been addressed (somewhere on the document this should be stated)

General

- The record should be kept in this document, ideally electronically.
- The document becomes part of the traceable account of the Working Group II Fourth Assessment. When completed to the satisfaction of the Review Editors, a copy should be returned to the TSU by the **8th December 2006**.

Chapter 19

Comments from the Co-Chairs/TSU are laid out as follows: first we comment on whether the SOD addresses the comments we made on the ZOD; second we comment on whether the SOD addresses the comments we made on the FOD; our concluding comments on the Second-Order Draft are at the end.

	Chapter 19 ZOD comments by CCs and TSU	Has this been addressed in the SOD?	Author responses:
19.Z1	This draft clearly represents some thought and time by the authors. The TAR ducked prioritising the vulnerabilities (e.g. indicating where these are) but the AR4 should not. To indicate where there are no apparent thresholds is valid (and useful). Similarly it is useful to indicate where the current information is not adequate to discriminate them. Please recall that this is an assessment of what we currently know or do not know regarding key vulnerabilities. It is not an exercise in determining KVs per se.	What we don't know is included in the SOD	Addressed.
19.Z2	This chapter is very dense. Please make it easier for the reader to grasp. Some real-world examples to illustrate the points the authors are making would help greatly; and avoiding fancy words that are not necessary: e.g. why not use 'measures' rather 'numeraires'; and we doubt the value or creating typologies where these confuse and over-simplify more than clarify (more on this later)	Chapter reads well until section 4 which is a bit heavy going.	Care has been taken to improve readability, particularly in section 4, though the latter section does present methods and thus is necessarily a bit heavier going.
19.Z3	Much of the strength of the chapter could lie in tables. We suggest more use of these to get some of the density out of the text	5 tables in ZOD now 3 tables in SOD but two very big summary tables.	Modification of the tables has continued in this version. Text has been edited to remove excess density.
19.Z4	The current draft is already 14 pages over the maximum length (see pages calculations in the Blue Book)	SOD draft 10 pages over target at 41 pages	Care has been taken to cut as much as possible.
19.Z5	Please be very careful to follow the plenary outline. Not doing so will only store up criticism in the future. We fought hard and long to get delegates to accept inclusion of this chapter. They talked long and hard about its content. By using the same headings as in the outline you'll avoid much incipient criticism: Of course, how much attention the literatures suggests giving each topic is a matter for you, the assessors.	On the whole the issues highlighted in the PAO are included in the chapter but the headings are not stated as in the PAO with the exception of section 4: assessment of response strategies to avoid key vulnerabilities. Article 2 and reasons for concern are present as are risks for regions and sectors, and priorities for research	We have chosen—as we were told that the content, not the headings, is what really mattered--not to use the same headings as the PAO, but to present the requested information in an order we believe is logical. As the comment suggests, we have focused on including the desired information.
19.Z6	Too much space is spent on discussion of methodology. We suggest section 2 be reduced to about half its current length, to allow more emphasis on conclusions rather than methods	ZOD = 15 pages SOD = 3	Addressed.
19.Z7	Section 19.3 starts to get to grips with the whole matter of key vulnerabilities, although it is too long. The authors need to keep their eyes on what is 'key' and what is not. Some of the impacts described and elaborated on are not clearly key. For example, concerning 'Thermal Properties of Lakes', authors need to explain why what they are describing is key. There need to be tables to summarize Section 19.3, to reduce its length and to make it less of	Thermal properties of lakes removed. SOD is better at explaining why these particular impacts are key. Summary tables 19.1 and 19.2 are in section 3.	We now present our application of our criteria for key in the summary table.

	a shopping list, so that the authors can concentrate on drawing out the really key vulnerabilities		
19.Z8	Much of the richness from the Buenos Aires meeting is not yet captured by the chapter (see the set of ppt from the meeting) e.g. the three approaches suggested by T.R.Carter regarding identification of thresholds at regional levels; and e.g. Corfee - Morlot's criticisms of various of these; and finally, Bill Hare's regional and sectoral burning embers (also repeated at the Exeter meeting in February)	In section 3 much specific information has been moved to tables 19.1 and 19.2. The remainder of the section is divided into subsections which cover the categories of key vulnerabilities covered at the BA meeting – market systems (focussing mostly on agriculture); societal systems; geophysical systems.	Addressed.
19.Z9	Thresholds: a)The Types 1 and 2 distinction is not made clear, and needs more explanation. The distinction is based on the type of climate change (shift in mean climate vs non-linear events). It is quite a simple distinction (and we believe as we describe later that it misses the opportunity of more valuable distinctions/descriptions of thresholds) b)The 2-type climate typology fails to make clear the following: i) that there are frequently non-linear climate and weather aspects (e.g. of variabilities) in Type 1; ii) that there are frequently non-linear effects in Type 1 (e.g. many thresholds may be due to multiple stresses reaching exceedance levels, where climate change is only one variable, but triggers non-linear effects). c) By typing the thresholds according to the type of climate change (type of cause), the assessment is missing a whole level of complexity that stems from thresholds being embedded in the exposure unit (type of effect). We would like to discuss this with you. d) There is a useful discussion of thresholds and the previous literature on this (which you miss) in Chapter 2, and we recommend you study this, which considers non-climate thresholds of the kind in ii) above. You should recall the presentations at Buenos Aires and Exeter, in order to distinguish between thresholds that are: i) irreversible; ii) exceed adaptive capacity; iii) multiply vertically through systems (e.g. yield, to production to food supply, to food security); or iv) horizontally through systems: e.g. water to food to health	Type 1 and 2 terminology is not used in the SOD	Addressed.
19.Z10	The concentration on physical system thresholds is again shown in the selection of global thresholds. Are there not economic ones: e.g. global food supply; global water; global political security (note that even if the conclusion is that these global systems are not	Economic systems included in SOD	Addressed.

	threatened by climate change, this is still a valid part of the assessment, namely where there are systems that are apparently resilient).		
19.Z11	Section 19.3.2 regional is missing: presumably much information can now be obtained from regional chapter ZOD drafts	Now in SOD although section 19.3 has changed a lot regional vulnerabilities doesn't have it's own subsection (apart from under 19.3.3) however, different regions are discussed within 19.3	Regional vulnerabilities are now treated separately.
19.Z12	S19.3.3. sectoral thresholds: subsection treatment is very uneven here and there is not a clear conclusion about where the key vulnerabilities are. The literature is very much more extensive than has been cited. Excess attention is given to lakes and wetlands.	Structure of 19.3 has changed a lot in the SOD – better balance within sections	Addressed.
19.Z13	S 19.3.4 the authors should use diagrams and table to illustrate conclusions and reduce text length	19.3.4 is much shorter in the SOD (now 19.3.2.3) and is included in T19.1	Addressed.
19.Z14	S 19.4 adaptation: no obvious conclusions are drawn here yet. What is adaptation doing, or what can it do regarding key vulnerabilities/thresholds: a) to avoid their exceedance; b) to delay their exceedance. We suggest you use diagrammatic examples of this	No diagrams included. Discussion of adaptation generally focuses on agriculture and concludes that the more managed a system is the greater adaptive capacity in most cases.	Areas with promise for high adaptive capacity identified, those with lower potential also identified and relationship between magnitude and rates of climate change and adaptive potential mentioned in many places as well.
19.Z15	Section 19.5: the risk of triggering key vulnerabilities (proposed in the chapter outline) is missing completely (dropped?)	No section 19.5 in SOD – trigger risks mentioned briefly in SOD	This is addressed in section 19.4.2.
19.Z16	There are no clear conclusions reached about where/what/when are the key vulnerabilities, what regions, what systems, at what levels of forcing, at what points in time, at what rates, etc. There is too much space spent on methods	Tables 19.1 and 19.2 identify the key vulnerabilities, thresholds, rates etc.	Addressed in the new amalgamated Table 19.1 and accompanying text..
19.Z17	We suggest that the next step is to quarry the draft regional and sectoral ZoD drafts for substance on system and regional key vulnerabilities.		Done.
19.Z18	Much of the supporting detail that is needed in the chapter can presumably be provided by Contributing Authors. These names and their slots need to be identified now.	CAs identified	Addressed.
19.Z19	There is relatively little consideration of rates of change (rather than levels) as thresholds.	Still small relative to thresholds but a greater attempt has been made to include rates of change in the chapter (e.g., T19.2)	Included where possible—this reflects a sampling of what is available in the literature.
19.Z20	Some material seems less than essential to the chapter e.g. much of Section 19.2.1.3 (Expert frameworks) and the section on Perceptions of Risk	ZOD section 19.2.1.3 and Perceptions of risk are no longer in the SOD	Addressed.
19.Z21	There are many pre-TAR references in this chapter. For example, page 9, lines 16-41, there are references going back to 1991	Still many pre-TAR references, going back to 1980. Other references are in the list but not cited in the text	References have been cleaned up, focus is and continues to be on post-TAR references, but important pre-TAR references are included when needed for clarity or context.
19.Z22	We suggest you discuss with the Ch 20 writing team the exposure		We define our focus in this chapter as

	unit you adopt to measure 'dangerous to 'what''. For example, what is dangerous could be defined in policy terms now adopted by the international community as (e.g.) threats to achieving the Millennium Development Goals or threats to current levels of sustainability (e.g. as measured by the new Global Sustainability Index)		assessing key vulnerabilities, subject to the criteria laid out in section 19.2. Section 19.1 also discusses the scope of the chapter and the relevance to discussion of "danger" with respect to Article 2, UNFCCC.
	Chapter 19 FOD comments by CCs and TSU	**Has this been addressed in the SOD?**	**Author responses:**
19.F1	Very substantial condensing is needed, which will help to focus on the essentials. Text is currently 65 pp of Word (41 pp printed) needs reducing to 20 pp printed	Still over length by 10 pages	Text has been cut as much as possible given reviewer demands for explanations and materials.
19.F2	Suggest you start with sections 19.1.3 and 4 and 5; then follow with 19.1.2 then 1. NB this is not a chapter about Art 2, but about much wider sets of important impacts; and Art 2 is one example of a target. Starting with current 19.1.1 may well provoke negative misunderstanding by some readers; and NB in 3, even 1 years time, Art 2 may have been replaced by another policy targe	Suggestion incorporated	Addressed.
19.F3	Global vulnerabilites should start with global aggregate assessments of 'IPCC' scenarios of projected T and P change (not the extreme low-prob scenarios). The former are the basis of the rest of the report and should be so also in this chapter. The global KV part of this chapter should assess KVs (for example) in the context of i) aggregate global costs (eg Nordhaus, etc) asking for example where is the point of inflexion between global net gain and global net cost, is this significant or not,), ii) aggregate global econ-soc but non monetary estimates (eg the millions at risk analysis; and migration analyses), and iii) aggregate non econ estimates (see Millenium Asst)	No longer a global KV section Aggregate global cost related to changes in GMT are discussed. Aggregate impacts briefly discussed in SOD – 19.1.2.4, 19.3.7, 19.3.2.3, and more detailed examples in T19.1 Millions at risk are included in the SOD	Addressed.
19.F4	Then, continue (in much condensed revision) with discontinuity scenarios. Anyway actually have almost no modelled impact assessments for these	Suggestion incorporated into SOD	Addressed.
19.F5	The assessment of exceedance avoided by a) mitigation and b) adaptation is very weak. There should be much more on this and the previous sections should be condensed substantially. A) Mitigation: There should be analysis of impacts for different ppm concn pathways (see for example, the global millions at risk for Hadley 550 and 750 (Arnell et al 2001); and see same for the SRES scenarios (Parry et al 2004), from which ppm equivalents	This is still the case in the SOD. Mitigation: coral bleaching (not avoided), MOC collapse, WAIS disintegration (avoided under 450 ppm scenario only) briefly discussed in 19.4.2.3	Section 19.4 discusses adaptation and mitigation, presents figures showing estimates of thresholds that might be crossed for various temperatures and for a few scenarios of emissions—including overshoot scenarios.

	can be inferred with A1B=750; B2=650 and B1=550 (Swart, Mitchell, Morita and Raper GEC, 2002). There have now been several impact assessments for various mitigation/stabilisation pathways. And B) Adaptation: there should be assessment of how much adaptation can avoid KVs (by either avoiding exceedance [raising tolerable ceiling] or delaying exceedance)	Probability of exceeding 2degC warming under various stabilization discussed Discussion on exceedance of thresholds not resultant impacts. Too much is spent on methods. Section 19.4 much less enjoyable to read than previous sections Adaptation: dominated by examples from agriculture; states that developed countries have greater adaptive capacity than developing. More detail and specific examples should be included where possible	
19.F6	The question this chapter must address is: how sensitive are KVs `(their avoidance or their delay of occurrence) to elasticities in mitigation and/or adaptation	Discussion of exceedance of 2degC increase included in 19.4.2.3 p36 THC shutdown in 19.4.2.4 Above 450 ppm = increase probability of large scale singularity	We address this as much as is possible based on the literature, but are very wary of the lack of credibility associated with over precision of thresholds or emissions levels.
19.F7	You do not analyse the impact literatue to see whether there is any clustering of effects at certain times as forcing increases. Surely this is a key aspect of KVs. An example I worked on is given in a report on UK agric I sent you a while ago [Parry, et al., (2002) Investigation of thresholds of impact of climate change on agriculture in England and Wales. Jackson Envt Inst, Research Report 4, Univ of East Anglia, U.K.]. The impact literature will tell you (or you need to ask other authors) about whether there is any 'lumping together' of important effects with certain amounts of C change (but less so between these amounts), i.e. where effects are triggered non-linearly or step-wise.	Clustering effects not covered but in some way T19.2 provides indications of impacts at various T change thresholds	Table 19.1 provides this sort of information.
19.F8	Why not use sectoral or regional burning embers diagrams, such as in Ch4 (ecos) or ch 11 (Australia)	Not done	Now incorporated into Table 19.1
19.F9	There needs to be a summary table of the effects expected under different amounts of ppm The literature on this is now extensive. Much of it is summarised in the Rachel Warren/Hare papers from Exeter and in the OECD Benefits project (eg Hitz and Smith, et al)	Not done	This comment ignores the uncertain relationship between ppm and temperature/impacts. Nevertheless, we now cautiousely report ranges of temperature change from WGI and the integrated assessment literature associated with different emissions scenarios in 19.4.3.
19.F10	Key next step is to draw conclusions: You urgently need to go through all core chapters to pull out KVs for each sector and region		See Table 19.1

	(and assess whether there is any lumping of these which might lead to macro-region KVs or multi--sector KVs or global KVs))		
19.F11	Below is copy of comment by M. Parry on ZERO-ORDER DRAFT in Jan 2005 [with note on whether comments have been addressefd in the FOD]: General comments: 1) suggest section 2 be reduced to about half its currenbt length, to allow more emphasis on conclusions rather than methods.[FOD is even longer] 2) Much of the richness form the Buenos Aires meeting is not captured in the paper (see the set of ppt from the meeting) eg the 3 approaches suggested by T.R.Carter regarding identification of thresholds at regional levels; and eg Cofee -Morlot's criticisms of various of these. [FOD is better but still misses much; FOD remains too obsessed with a) Art2; b) global extreme scenarios c) probability of the event rather than of the impact flowing from it] 3) S 19.3: Why only physical system thresholds at global level? Why not economic: eg global food supply; global water; global political security (NB even if the conclusion is that these global systems are NOT threatened by climate change, this is still a valid part of the assessment, viz where there are systems that are apparently resilient. [this criticism not anwered at all in FOD] 4) S 19.3.2 regional is missing: presumably much info can now be obtained from regional chapter ZoD drafts [FOD now better on this] 5) S 19.3.3. sectoral thresholds: subsection treatment is very uneven here and there is not a clear conclusion about where the key vulnerabilities are. The literature is very much more extensive than has been cited. Excess attention to lakes and wetlands. [FOD is better on this but fails to address clustering of impacts] 6) S 19.3.4 use diagrams and table to illustrate conclusions and reduce text length 7) S 19.4 adaptation: no obvious conclusions drawn here, yet. What is/can adaptation do regarding key vulnerabilities/thresholds: a) avoid their exceedance; b) delay their exceedance. Use diagrammatic examples of this. [FOD still does not address this] 8) Section 19.5 risk of triggering key vulnerabilities, proposed in LA outline is missing completely (dropped?) 9) No clear conclusions are reached about where/what/when are the the key vulnerabilities; too much space is spent on method and insufficient on current knowledge sabout how this can inform policy. 10) Suggest next stape is to quarry the draft regional and sectoral ZoD drafts for substance on system and regional key	See ZOD comments a) SOD is less obsessed with Article 2 but it still features b) Extreme/abrupt events still dominate c) still the case	These comments repeat those above.

	vulnerabilities. 11) At the end of the chapter the reader needs some balanced evaluation of where current knowledge indicates the main key vulnerabilities to be: what regions, what systems, at what levels of forcing, at what points in time, at what rates (NB there is relatively little consideration of rates of change (rather than levels) as thresholds) . The TAR ducked prioritising the vulnerabilities (eg indicating where these are) but the AR4 should not. And, of course, to indicate where there are apparently NOT thresholds is just as valid (and useful) a set of conclusions; or where the current information is not adequate to discriminate them. [this FOD chapter yet has to develop its conclusions]	KVs clearly identified in Tables 19.1 and 19.2	
	Chapter 19 SOD comments by CCs and TSU		**Author responses:**
19.S1	**LENGTH:**	41 pages (10 pages over length (target 31))	Authors have focused on shortening chapter.
19.S2	**ARE PAO HEADINGS PRESENT?**	Some PAO headings are included but others aren't. The topics outline are covered but not all the headings are included in the chapter	See 19.Z5
19.S3	**HAVE MOST GENERAL COMMENTS OF ERs FROM ZOD AND FOD BEEN COVERED?**	yes	Thanks
19.S4	**ARE REFERENCES BROADLY COMPLETE?**	Several incorrectly referenced, missing years/full citation. Others in the reference list which aren't cited in the text.	References have been edited.
19.S5	**IS THERE LINE-OF-SIGHT TEXT ➔ ES AND TEXT+ES ➔ TS+SPM?**	Some statements can only be found in the ES P16 of SPM ln39-40. In Ch 19 this statement related to mitigation and there's no mention of delay = greater costs P7 of SPM ln15-16: 1-2 deg inc = reduced agri production at lower latitudes not stated in Ch 19. Up to 1degC inc should ref T19.2. P49 of TS bullet one = 19.3.3,19.3.7 and ES Bullet 3 = 19.3.7 bullet 4 = ES bullet 5 = ES and 19.3.3.1 P50 1st para – 19.2, 19.3.3, 19.4	These text pieces have all changed and great effort was added to dite line of sight cross-references for the latest available drafts we have been able to obtain..
19.S6	Generally well written chapter, easy to read – section 19.4 needs some checking for sentence structure. This section is also heavy-going – too much emphasis on methods and too much jargon.		Authors have focused on improving readability, particularly in 19.4.
19.S7	Confusion over what is a KV. The ES implies it's an impact, the introduction talks about vulnerable people and		Clarity and consistency of discussion of

	systems; and section 19.1.2.1 ploughs through 2 sensible TAR definitions of KVs and then dismisses them to define KVs as severe impacts. Please make it clear which definition is being used throughout the chapter.	KV has been improved and mentioned ins several key places.
19.S8	References are not cited consistently in the chapter	Improved
19.S9	Many references to WG1 and WG2 chapters are incomplete	Improved
19.S10	Some sections with very few or no references e.g., p27 ln26-46	Improved
19.S11	Pretty much word-for-word repetition of section 19.3.7 in the ES. Please reword the ES.	Rewritten.
19.S12	**TO DO:** • Revise section 19.4 to improve the readability if the section is to match the rest of the chapter. This is too technical and spends too much time concentrating on methods and not enough on examples. • Clarify the definition of KVs at the beginning of the chapter and be consistent in that definition in the ES and Introduction. You need to screw these definitions down very tightly; then use the definitions as a filter to draw out KVs from the systems and sector chapters (NB the US comment seems pertinent here: the tables would be much stronger if based on the literature assessed in other chapters); • Reverting to the TAR areas of concern later in the chapter is confusing. You logically start with them; then you develop more precise definitions of KV; then you give examples of these and sort them by increments of T change. Seems more logical to go straight toward examining elasticity of KVs to adaptation and mitigation (and skip a repeat of areas of concern…especially given the need to cut length) • This examining of elasticity (mentioned in point above) is poorly done by comparison with the preceding section: you should examine current published knowledge of: a) how far adaptation can raise KV exceedance levels by increasing resilience, or b) how mitigation can delay exceedance; and how raising and delaying are potentially complementary (or otherwise) strategies, by drawing on Ch18 conclusions. There is good recent literature on all this. • Avoid 'operationalising' or re-defining Art 2 stuff: it will only lead to endless objections from some governments that it is not in IPCC's remit to do this. • Many references are incomplete in the reference list and many references to other chapters are incomplete in the text. Where AR4 chapters are referenced please provide a section number also where possible. • Reword ES so it's not repeating section 19.3.7 word-for-word. • Shorten by 10 pages	Comments repeated from above.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-1	A	0	0	0	0	The chapter now reads excellently well. 1990 and current temperature baselines are both used in the chapter - although these are only 0.1C apart it may be tidier to only use 1990 (for example). (Rachel Warren, School of Environmental Sciences)	Temperatures are now related to a 1990-2000 baseline, as discussed in Box 19.1.
E-19-2	A	0	0			The chapter represents and advance over the FOD, but it makes excessive reliance upon reference to sources that either do not constitute the best peer-reviewed science in academic journals or have other problems which seriously limit the current review process. The latter category includes reference to numerous papers which are either in press or (in some cases) still under review at journals such as Climatic Change. As such they are not in the public domain, and cannot be checked; neither can the interpretation placed on these sources by the authors of this chapter be subected to critical review, yet this is probably the last opportunity for review independent of the IPCC community. On the first point, there are far too many sources underpinning the chapter that are authored by the autors of this chapter or WG Coordinating Lead Authors. This places the authors in an unacceptable conflict of interest, and seriously weakens the extent to which this can be accepeted as an independent, disinterested review of the best available literature. There are also too many sources that are 'working papers' or similar outputs which are neither journal articles nor boooks published by university presses or commercial, scholarly presses. In one case (Pittock, 2005) we have a source authored by a chapter author and published by the research institute from which he has retired and with which he is still associated. (A specific point is made with respect to this below). IPCC Reports should be based upon th best available literature; this chapter falls short in that regard, including, for example, such low quality publications as a Discussion Paper published by a highly partisan Australian think tank that is a member of the Climate Action Network (Hamilton et al, 2001). Unfortunatley, it is probablly too late in the process to rewrite the chapter to eliminatetotally reliance upon such sources, but best efforts should be made in the interets of the credibility of the chapter. (Aynsley Kellow, University of Tasmania)	Copies of gray literature are maintained by IPCC and are available. Authors have focused on expanding the breadth of literature cited, and this includes new literature that is in the process of publication, most of which are not by the LAs. However, many LAs and CLAs were selected by the TSU precisely because they had the knowledge of the subjects evidenced by peer reviewed publications. To eliminate that would be to eliminate a substantial fraction of the quality of the assessment.
E-19-3	A	0				This is the most important chapter in the WGII report, because it relates directly to Article 2 of the UNFCCC and because it summarizes the most important impacts of climatic change. It is therefore absolutely crucial that the risks and impacts are not downplayed. Unfortunately, many of the risks are downplayed here and in Chapters 4, 5, and 6, with which this chapter obviously has to be consistent. I have submitted specific comments pertaining to these chapters, and hopefully they will be addressed in the final draft. I have not taken the time to review the regional	Individual points will be addressed in responses below.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						chapters, but presumably if some of the risks are downplayed in the sectoral chapters, they may also be downplayed in the regional chapters. My main concerns involve sea level rise, impacts on coral reefs, and threats to tropical rainforests. I think that these risks are not adequately addressed in this chapter too. (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-4	A	0				This draft is so different from the first draft that it is hard to judge the response to earlier comments. This lack of continuity could be seen as a vulnerability in the process. Nonetheless the chapter does read more easily than before. (Michael Manton, Monash University)	No change suggested.
E-19-5	A	0				This chapter provides an analysis of risks associated with climate change that is a perspective not often considered by physical scientists. As one of the latter, I look forward to studying the final version carefully and I think it will be valuable educational material for me and many of my colleagues. My criticisms of the chapter focus on the areas of the chapter I am most familiar with, and from my reading of these areas I do think the draft would benefit from general attention to two areas: (i) precision of langauge, especially when using 'confidence' statements that are actually defined in terms of probability of being correct, (ii) where discussing events in the physical climate system, it is very important to be consistent with the assessments of WGI. This is not always the case at present. (Richard Wood, Hadley Centre)	We agree with these suggestions. Considerable effort has been devoted to improve precision of language and consistency with WGI.
E-19-6	A	0				This chapter employs an inconsistent definition (or rather lack of a definition) of "key vulnerabilities". According to WG2 definitions, "vulnerability" means "susceptibility to harm". Therefore, it is not the actual harm (ie impact) but the susceptibility to sustain harm that should be described as a key vulnerabiloty. However Section E and chapter 19 use "key vulnerability" interchangeably with vulnerability, impact, coping limits, or adaptive capacity. This makes this section unnecessarily weak. It mixes an assessment of "susceptibility" (ie something worth watching out for because of the potential damage it could do) with projections of actual impacts. It would be much more robust to clearly separate "suceptibility to significant harm" (=key vulnerability), and the likelihood that the change that would cause such harm would actually occur (which would have to, and could, rely much more explicitly on robust and extensive supporting information and assessments by WG1; and the key vulnerability would to a first degree be independent of changes in knowledge about the actual likelihood of events - within reason). Please consider this issue carefully, I really think the current approach does an injustice to the very important concepts that this section deals with. Consistent with this general issue, I also suggest that the authors reconsider phrases used in the	The definition of “key vulnerabilities” has been clarified and made consistent through the chapter. It is also explicitly linked to the WGII glossary definition of “vulnerability”—and key impacts and risks are explicitly separated and defined.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						ES and underlyinig chapter that go against a clear definition. Examples include phrases such as "impacts that can be considered key vulnerabilities", "probabilities associated with key vulnerabilities". (Andy Reisinger, IPCC SYR TSU)	
E-19-7	A	0				There are two sections that discuss changes in the Atlantic thermohaline circulation/MOC: one in Ch 12 and one in Ch 19. I don't think they are fully consistent with each other, and neither is fully consistent with what WGI says on the topic. Given that the impacts of MOC change extend well beyond Europe (e.g. big impact on tropical precip) I would suggest the material is consolidated in Ch 19 - with possible improvement in usability and overall saving of space. I have made specific comments on the sections in both chapters, which I hope will be helpful wherever the material ends up. (Richard Wood, Hadley Centre)	We have made our discussion of MOC consistent with the literature assessed by WGI, which we expect Ch. 12 has done as well.
E-19-8	A	0				It does not help the chapter, the WG report, or the whole AR4 to use such a broad (ill-defined) interpretation of 'vulnerability'. Yes, the literature uses the term to mean a system, an impact, or a physical process, but why does this justify the IPCC being so woolly. A counter example is the definition of climate change, where the IPCC has selected a definition that nicely separates detection from attribution. The lack of precision in Ch 19 blurs the interpretation of an important concept, and leads to items (physical mechansims) being listed in WG2 that should be in WG1. Under the ch 19 definition, the first key vulnerability should be rising global temperature. (Michael Manton, Monash University)	The definition of “key vulnerabilities” has been clarified and made consistent through the chapter. It is also explicitly linked to the WGII glossary definition of “vulnerability”.
E-19-9	A	0				I strongly recommend that all of the thresholds for dangerous or unacceptable global mean temperature change, which are currently given with respect to temperatures of the 1990s, be given with respect to preindustrial temperatures instead. There are several reasons for doing this. First, and prehaps most importantly, temperature thresholds in Chapter 4 (Table 4.2) are given with respect to pre-industrial temperature, so consistency requires the same in Chapter 19 (on the other hand, Chapter 5 does not specify the reference time period for their temperature changes, I point that I have raised in my comments on that chapter). Second, radiative forcings are given in WG1 (Chapter 2) with respect to pre-industrial (1750) conditions (Table 2.1). Third, the concept of climate sensitivity applies to the same reference period as used for radiative forcing, which is 1750 in WG1 Chapter 2. Fourth, transient climate model simulations generally begin with pre-industrial conditions. In short, there is greater consistency and transferability between climate model simulations, climate sensitivity, radiative forcing, and	Unfortunately, we cannot please everyone with respect to this choice (see comment E-19-91, e.g.). As explained in the text, we make this choice to reflect the most common metric found in the literature, and are careful that no misunderstanding will occur of what is the baseline period for temperature increases..

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						temperature change if everything is given with reference to pre-industrial conditions. (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-10	A	0				Having read three drafts of this chapter, I still think that it has no place whatsoever in a scientific report. The chapter is decidedly biased, and the only improvement over the previous draft is that it now admits so much. I have argued this before, and I expect you to just continue to ignore these concerns. (Richard Tol, Hamburg University)	The IPCC Plenary-approved outline gives the title for this chapter as "Assessing Key Vulnerabilities and the Risk from Climate Change". As we now clearly state in the text, Vulnerability to climate change is the degree to which systems are susceptible to, and unable to cope with, adverse impacts from climate change. As we also now clearly state, given this focus on vulnerability, the analytic emphasis of this chapter is on people and systems that may be *adversely* affected by climate change, particularly where impacts could have serious and/or irreversible consequences. Positive impacts are addressed where relevant to this assessment of key vulnerabilities. A detailed description of positive and negative climate impacts in all sectors and regions is beyond the scope of this chapter, and readers are encouraged to turn to the executive summaries of the sectoral and regional chapters of this Report for this information. We give cross references to such places.
E-19-11	A	0				GENERAL COMMENT: I had very much trouble in getting the point and logic of Chapter 19. The problems can be condensed in four entities. 1. The style is heavy and illogical. Paragraphs are extensively long and at many places very unclear. One example is the second para of the introduction which is a real mess. It remains unclear water the authors aim to achieve. 2. Sections are disconnected. The chapter starts with some almost random methodological opinions which are not at all coordinated or in line with the very good chapter 2 of the assessment report draft, neither make use of other relevant chapters such as 17, 18 and 20. Then a section with two lists of key vulnerabilities is given, which are nor was comprehensive, nor connected by any means to what stated before or after. Totally disconnected! The same style continues. A very	We have focused on improving clarity and brevity of text. An effort has been made to improve the logical flow of the chapter.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						strange decision of the authors has been to write such a chapter as response strategies, which pretends to be methodological but is as mentioned fully disconnected from e.g. chapter 2 which in turn is analytical and systematic and nice to read. As a methodological summary, the response strategies section is extremely weak and does not even address the scope of the chapter. There are many serious key vulnerabilities on the globe with respect to climate change but the chapter fails in bringing them to the readers. 3. Response strategies come "out-of-the-blue". 4. The chapter has a serious geographic and topical bias. One example: the text mentions the words "ice" and "glacier" the total of 75 times, but does not mention words such as "desert", "dryland", "steppe", "slums" not a single time. Hence the chapter gives an impression that the world's rapidly growing urban slums with over one billion people are not vulnerable to climate change. It also gives a view that world's drylands, deserts and steppes are neither vulnerable although millions of people have moved away of the Sahelian zone in Africa, although India and China are investing very heavily in reducing climatic sensitivity of their large arid areas by massive infrastructure projects with extreme social and economic cost, although the whole Middle East and North Africa, Australia, Southern Africa and many other regions of the world are gravely concerned for ardification and prolonged droughts. I strongly disagree with the view that the chapter now gives. The authors should study carefully the regional chapters which are generally of good quality and try to provide a more balanced view of the global key vulnerabilities. As a continuation, let me mention one more example. The chapter mentions that "Flooding: large river flooding in Northern North America and Eurasia becomes frequent, especially in winter". This might be true, but this should be put in some perspective. The Red Cross & Red Crescent has estimated in the World Disaster Report of 2002 that whereas 700 million people were exposed to disastrous floods annually in the 1970s, the figure has tripled by the 1990s. We are talking about a number that exceeds 2 billions today. The arctic problems are serious of course but the total population of the arctic zone is around 4 million and the social & economic & political vulnerability of these societies is minimal in comparison to most of the world. Please note that these are only a very few examples of an array that could be taken. As a conclusion, chapter 19 needs a major rewriting and complete reconsideration of scope and content in order to justify its place in the IPCC 4th Assessment Report. (Olli Varis, Helsinki University of Technology)	This is part of our approved outline, and now is more clearly linked to avoidance of KVs. We agree that it is important to address regional key vulnerabilities, and have added examples of these to section 19.3.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-12	A	0				General comment. Definitional qualifications early in 2the chapter work well and the text is now much improved.In the same way, Tables 19.1 and 19.2 represent a significant improvement to the first-order draft. General comment. As with soil carbon, the recent results of FACE studies on carbon dioxide fertilisation should be discussed at greater length. Their implications for impacts, adaptation and earlier assessments thereof should also be judged. General comment. This second-order draft still gives too little attention to the recent results on carbon balance in soils. It would be important to discuss these results more extensively than has been done now in the second order draft, and to judge their potential implications for impacts and adaptation, as well as earlier predictions thereof. For example, Technical Summary gives them relatively greater role than chapter 19. (Jouni Paavola, University of Leeds)	We believe this is largely a WGI issue, and carbon cycle issues are included here, but cannot be extensively treated.
E-19-13	A	0				Comparing with approved outline and this draft of Chapter 19, I find that there are some different contents, like key impacts were not mentioned in the draft, for this reason, the definition of key vulnerability might change, because of lack of key impacts. I hope lead authors can give a compellent explanation (Lin Erda, Chinese Academy of Agricultural Science)	We agree, and now explicitly discuss in several places the linkage between impacts, vulnerability, and risk, and the criteria that can make them "key."
E-19-14	A	0				COMMENT: Would suggest scaling temperature to preindustrial rather than 1990. This has been done in Chapter and other chapters (William Hare, Potsdam Institute for Climate Impact Research (PIK))	See E-19-9
E-19-15	A	0				Chapter 19 is well-written and comprehensive. In general, it could have been made clear that climate change impacts are brought about in concert with the other two major components of Global Change, viz. Land Use Change and Environmental Pollution, including feedbacks among the three. This complication appears here and there in text, but could be more explicitly presented. (Ulf Molau, Göteborg University)	Synergisms and multiple stressors, now explicitly treated in the chapter.
E-19-16	A	0				Although there is a nice box (19.2) explaining the different baselines for temperature change, it would seem sensible for the whole chapter (indeed the whole assessment) to use a common baseline period. It seems that the text of ch 19 uses current levels while the tables use 1990. (Michael Manton, Monash University)	See E-19-9. We now clarify in this box that we use 1990-2000 levels.
E-19-17	A	0				A general weakness in AR4 WGII is the usage of the term "biodiversity", but this is less problamitic here than in other chapters of WGII. In most cases, species richness is (alpha diversity) is addressed. Following the current definitions of biodiversity (e.g. UNEP), the concept includes an array of scales and hierarchy, e.g., genetic diversity, ecosystem diversity, temporal diversity, functional diversity,	This appears to be more of a comment for the WGII Report as a whole. In general, we have focused revisions on improving precision of language.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						taxonomic diversity etc. The components dealt with in Ch. 9 are species richness and ecosystem diversity; the usage is in most cases relatively clear to the specialists, but perhaps less so for most users. Genetic diversity is a "dark horse" that does not surface anywhere in AR4. (Ulf Molau, Göteborg University)	
E-19-18	A	1		40		The following references were alluded to in the comments, but may not have been provided within comments; nor do they seem to be in the report: Goklany, IM. 1995. Strategies to Enhance Adaptability: Technological Change, Economic Growth and Free Trade. Climatic Change 30: 427-449. Goklany, IM. 2000. Potential Consequences of Increasing Atmospheric CO2 Concentration Compared to Other Environmental Problems. Technology 7S: 189-213. Goklany, IM. 2003. Relative Contributions of Global Warming to Various Climate Sensitive Risks, and Their Implications for Adaptation and Mitigation. Energy & Environment 14: 797-822. Goklany, IM. 2005a. A Climate Policy for the Short and Medium Term: Stabilization or Adaptation? Energy & Environment 16: 667-680. Goklany, IM. 2005b. Is Climate Change the 21st Century's Most Urgent Environmental Problem? Lindenwood Economic Policy Lecture, Series 7, Lindenwood University, St. Charles, MO, also forthcoming in Society (Transaction Publications)] Goklany, IM. 2005c. Is a Richer-but-warmer World Better than Poorer-but-cooler Worlds? 25th Annual North American Conference of the US Association for Energy Economics/International Association of Energy Economics, September 21-23, 2005. Goklany, IM. 2006a. Integrated Strategies to Reduce Vulnerability and Advance Adaptation, Mitigation, and Sustainable Development. Mitigation and Adaptation Response Strategies for Global Change, forthcoming. Goklany, IM. 2006b. Death and Death Rates Due to Extreme Weather Events:	No suggested change, and ther are citations in the report from this reviewer..

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						Global and U.S. Trends, 1900-2004, Climate Change and Disaster Losses Workshop, 25-26 May 2006, Hohenkammer, Germany. Levy, P.E., et al. 2004. Modelling the impact of future changes in climate, CO2 concentration and land use on natural ecosystems and the terrestrial carbon sink. Global Environmental Change 14 (1): 21-30. (Indur Goklany, US Department of the Interior)	
E-19-19	A	2	6	2	7	"definition of DAI connot be based on scientific arguments along, but must incorporate value judgement…", I think this conclusion give a correct position of IPCC, Yes IPCC will not decide what is DAI, It should be decide by UNFCCC, IPCC can provide evidences, so I think this conclusion is very important for this chapter. (Lin Erda, Chinese Academy of Agricultural Science)	We take care to carefully draw distinctions between scientific and normative judgments in this chapter, particularly related to Article 2.
E-19-20	A	2	12	2	17	please add a sentence to say this chapter need to assess key impacts, and their risk of occurrence, approaches to determining and so on. (Lin Erda, Chinese Academy of Agricultural Science)	Relevant text removed, but this information is incorporated in the text.
E-19-21	A	2	19	2	26	Considering that this chapter is (per the first sentence in this chapter) directed toward a discussion of key vulnerabilities to climate change that merit particular attention by policy makers, a critical factor has been omitted from the criteria listed here, namely, the importance of climate change in determining the magnitude and timing of the vulnerability in question (among other things). The reason why this is important is that policy makers have to be able to understand, among other things, the opportunity costs of response measures. Consider, for instance, global hunger – certainly a key vulnerability. It is projected to be a problem in the future and climate change is projected to add to this problem (e.g., Parry et al. 1999, 2004). Policy makers are owed not only estimates of the future population at risk (PAR) of hunger due to CC, but also the PAR in its absence (Goklany 2000, 2003, 2005a). Without such context, it would be impossible for policy makers to make a reasoned judgment – we are positing that we, indeed, want reasoned judgments from policy makers – on how much resources to spare for different response strategies that would directly or indirectly address the key vulnerability. It is not inconceivable that in some cases despite the fact that CC may contribute to the PAR for hunger, other factors may contribute larger amounts to that PAR More importantly, the non-CC related contributions to the PAR may be reduced more effectively and/or efficiently than might reductions in CC (Goklany 2006a). Accordingly, we recommend adding the following new criterion: "importance of CC in the context of other factors affecting the key vulnerability."	This context is discussed in section 19.1.1.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Indur Goklany, US Department of the Interior)	
E-19-22	A	2	28	2	47	The separation of systematic and normative thresholds brings the same problems in climate change as it does in air quality assessment. Very few impacts are really systematic. Indeed the difference between a jump and a smooth change is usually a matter of how one draws the time or scale axis. The initial discussion then tends to suggest that the temperature levels in the dot-points are systematic thresholds, without much scientific justification. (Michael Manton, Monash University)	This discussion has been removed.
E-19-23	A	2	33			Append a new sentence at the end of this para which would read as follows: "However, it should be noted that a key geophysical vulnerability may not translate into a key socio-economic vulnerability of equal significance." As an example consider the melting of various ice sheets that, if it occurs, will take centuries to millennia (p. 19 of this chapter). The socioeconomic impacts of such melting can be substantially mitigated due to the long time frames involved. (Indur Goklany, US Department of the Interior)	Identical to G-19-35. This is not appropriate here, and the example given is speculation. This is addressed in the chapter, and confidences are given to focus on uncertainties..
E-19-24	A	2	36	2	38	This sentence makes some statements that don't seem fully justified. Given that the chapter is largely focused on future climate change caused by GHG emissions, one can assume that the authors use "climate change" in this sentence in an anthropogenic sense. If yes, then this would be inconsistent with the IPCC definition. If no, then the statement would appear to have no real role in this chapter, or it would need to be clearly stated that these are just examples of what climate change (in IPCC sense) can do but no implication is made that these are necessarily anthropogenic changes. Furthermore, neither chapter 1 nor chapter 8 actually provide a clear statement that climate change to 2006 has led to attributable increases in human mortality (with the exception of the 2003 heat wave), so there is some inconsistency that needs to be corrected (let alone the question of attributing any increase in mortality to anthropogenic climate forcing). Finally, it appears somewhat strange to me to mix loss of human lives with loss of glaciers; they appear to be on a different scale of significance. It's may not be intuitively clear to readers why loss of glaciers in itself would constitute a key vulnerability without any further explanation or comment. (Andy Reisinger, IPCC SYR TSU)	This language has been refined and clarified. Statements are now consistent with other chapters.
E-19-25	A	2	36	2	37	The unfortunate definition (or lack of definition) of vulnerability leads to a silly list of inconsistent items: we have a human impact, a hydrological impact, and some phyical events. The thrust and importance of impacts is lost by mixing these items. (Hydrology being an impact is curious, but defensible. TCs are not.) (Michael Manton, Monash University)	This now refers to key impacts. The definition of "key vulnerabilities" has been clarified and made consistent through the chapter. It is also explicitly linked to the WGII glossary definition of "vulnerability".

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-26	A	2	37			To the best of our knowledge, there has been no global increase in "observed" deaths due to climate change. Yes, there are estimates – based on simplistic models – that CC contributes to human mortality (McMichael et al.2004), but these are not observed increases. They are based on modeling. In fact, despite the recent spate of deadly extreme weather events such as the 2003 European heat wave and the hurricanes of 2004 and 2005, data from EM-DAT, the International Disaster Database maintained by the Office of Foreign Disaster Aid and Center for Research on the Epidemiology of Disasters at the Université Catholique de Louvain, Brussels, Belgium, indicates that aggregate mortality and mortality rates due to extreme weather events are generally lower today than they used to be. Globally, mortality and mortality rates have declined by 95 percent or more since the 1920s. The largest improvements came from declines in mortality due to droughts and floods, which apparently were responsible for 95 percent of all deaths caused by extreme events during the 20th century. For windstorms, which contributed most of the remaining 5 percent of fatalities, mortality rates are also lower today but there are no clear trends for mortality. Cumulatively, the declines more than compensated for increases due to the 2003 heat wave. With regard to the U.S., current mortality and mortality rates due to extreme temperatures, tornados, lightning, floods and hurricanes are also below their peak levels of a few decades ago. The declines for the last four categories range from 55 to 95 percent. [Goklany 2006a, 2005a.] Similar information on this had been conveyed on the "first order draft." (Indur Goklany, US Department of the Interior)	Chapter text has been revised for consistency with Chapter 8. The purpose of this chapter is not to compare the effects of climate change with the effects of socio-economic development, but to assess the additional effects of climate change, where overall socio-economic development is considered in the baseline. Whether a climate change impact would be either greater or smaller than welfare gains or losses associated with particular development scenarios is beyond the scope of this chapter--and is dealt with in Chapter 20 and in Working Group III. In this case, overall mortality rates are irrelevant to the discussion of the marginal implications of climate change. Likewise, comparison of the costs of mitigation or adaptation relative to the projected income from the growth rates in the GDP are also beyond the scope of this chapter.
E-19-27	A	2	37			Insert "many" prior to "glaciers". (Indur Goklany, US Department of the Interior)	We disagree.
E-19-28	A	2	38			We are unaware of empirical data with sufficiently long coverage that indicates any global increase in intense tropical cyclones. By long term, we mean data that covers more than 3 to 4 decades. Any thing less than that is indistinguishable from short term fluctuations not necessarily connected to climate change (anthropogenic or otherwise). (Indur Goklany, US Department of the Interior)	Reference to cyclones removed here.
E-19-29	A	2	39	2	46	The unfortunate approach to representing uncertainties in the AR4 means that statements are made categorically and then have a tag added: this will happen (with low confidence). This is a misuse of the English language and is likely (with some confidence) to cause confusion. (Michael Manton, Monash University)	This is a comment for WGII generally, and we have been very attentive to making our confidence statemts consistent with the phrasing of conclusions..
E-19-	A	2	39	2	41	Key vulnerabilities in this temperature range include major loss of coral reefs and	These topics are covered in 19.3.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
30						substantial biodiversity loss (Hare, W. L. (2006). "Relationship between increases in global mean temperature and impacts on ecosystems, food production, water and socio-economic systems" in Schellnhuber, J., W. Cramer, N. Nakicenovic, G. Yohe, and T. B. Wigley, eds. Avoiding Dangerous Climate Change. Cambridge:) and a significant risk of rapid loss of ice from the Greenland Ice Sheet. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	
E-19-31	A	2	39	2	40	I strongly doubt this conclusion has a high confidence, because I can not find evidences from the draft and tables, meanwhile we don't know if this conclusion including adaptation or not, say exceeding the adaptive capacity need more carefulness. (Lin Erda, Chinese Academy of Agricultural Science)	Please refer to Section 19.3 and Table 19.1 where such issues are addressed.
E-19-32	A	2	39	2	41	Change "will exacerbate current key vulnerabilities" to "**may** exacerbate **some** key vulnerabilities **while relieving others, at least temporarily**". Consider, for example, for temperature increases less than 2o C, global carbon sink capacity (and habitat lost to cropland) may be augmented (e.g., Levy et al 2004). [REF: Levy, P.E., et al. (2004). "Modelling the impact of future changes in climate, CO2 concentration and land use on natural ecosystems and the terrestrial carbon sink," Global Environmental Change 14 (1): 21-30.] Similarly, it's not clear that moderate temperature changes may not reduce net global hunger [Parry et al.. 2004, Fig.14 Panel A]. . (Indur Goklany, US Department of the Interior)	This is changed to key impacts and linked directly to the first bullet. Confidence statements reflect level of uncertainty.
E-19-33	A	2	39		44	"will exacerbate current key vulnerabilities" it is rather easy to have "high confidence" in a vague statement, as "I am rather certain that something will happen" -- please limit your confidence statements to conclusions that have information -- interestingly, you do not attach any confidence level to deglaciation (Richard Tol, Hamburg University)	Text revised for clarity. Confidence statements added and carefully chosen to phrasing of conclusions.
E-19-34	A	2	40	2	41	Unless the above change is made, the confidence levels on these lines are overstated. (Indur Goklany, US Department of the Interior)	Text revised. Confidence statements reflect level of uncertainty.
E-19-35	A	2	44	2	44	The phrase "triggering" partial deglaciation is misleading because it implies a trigger, ie a non-linear threshold. I'm not aware of such a threshold and have not found reference to a threshold in the underlying chapter, or WG1, or the literature. Perhaps it might be better to say "initiate" or simply "lead to". "Trigger" sounds very dramatic and has an air of irreversibility (which we don't know - we know that complete deglaciation could well be irreversible, but we do not (yet) know whether reaching 3 deg C for a century and then coming down again to 2 deg C would lead to irreversible melting. It might, but it's not borne out by the current literature.)	Text revised, and made consistent with WGI.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Andy Reisinger, IPCC SYR TSU)	
E-19-36	A	2	45	2	46	For greater accuracy, rewrite this bullet to read as follows: "The higher the global mean temperature (GMT) increase over 4o C above ~1990 levels, the greater the likelihood of exceeding the current adaptive capacity of many systems". We note that if higher GMTs are associated with higher levels of economic growth and technological prowess, adaptive capacity might keep pace with climate change impacts, at least for some systems (Goklany 2006a, 2005c). . (Indur Goklany, US Department of the Interior)	We address the effects of baseline scenarios n adaptive capacity—see E 19-26 response.
E-19-37	A	2	51			Why is this restricted to "planned adaptation"? (Richard Tol, Hamburg University)	Planned removed.
E-19-38	A	2	51			Eliminate "planned". There is no reason why adaptations have to be "planned". They could be spontaneous. (Indur Goklany, US Department of the Interior)	Planned removed.
E-19-39	A	2		4		ES: please add sourcing for statements (Clair Hanson, IPCC TSU)	Done.
E-19-40	A	2		6		Please be consistent with definition of KVs. On page 2 it gives the impression they are just impacts, then on page 5 there is another definition which relates to systems and populations, this is followed on page 6 by two further definitions and a third which encompasses systems, regions, impacts etc (Clair Hanson, IPCC TSU)	The definition of "key vulnerabilities" has been clarified and made consistent through the chapter. It is also explicitly linked to the WGII glossary definition of "vulnerability".
E-19-41	A	3	1	3	3	Change "However" to "Although", and append at the end of this sentence the following: "such capacity should be enhanced with the passage of time under any of the SRES scenarios because of advances in economic and technological development, among other things (Goklany 2005b, 2006)." We believe this is a critical point, because there should be greater focus on the future – since that's when the impacts of CC will become greater. (Indur Goklany, US Department of the Interior)	Text has been revised to include some of this information.
E-19-42	A	3	20	3	21	To the list of uncertainty factors, add the following two: "adaptive capacity in light of continued economic and technological development assumed in the SRES scenarios, and the likelihood of bringing such capacity to bear ." (Indur Goklany, US Department of the Interior)	Text revised to include adaptive capacity and likelihood of bringing it to bear and relation to baseline scenarios.
E-19-43	A	3	21	3	21	A "risk management framework" is promoted without definition or any further discussion in the remainder of the chapter. (Jim Hall, University of Newcastle upon Tyne)	Linkage to Chapter 2 is now given in 19.1.1.
E-19-44	A	3	24	3	28	Suggest that this point be recast as "actions to mitigate climate change" as opposed to "reductions in greenhouse gas emissions". Actions includes a broader, and more inclusive, range of activities including for example technology R&D and	Suggested words added.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						investment in deployment of superior technologies both of which could improve our ability to manage the risk of climate change (and reduce cost) but would not show up in today's emissions. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	
E-19-45	A	3	26			insert "otherwise" after "that" [this insertion makes particular sense in light of the insertion given for line 27] (Danny Harvey, Dept of Geography, University of Toronto)	Relevant text deleted.
E-19-46	A	3	27			insert "and the loss of windows of opportunity to reduce emissions" after "technologies," (Danny Harvey, Dept of Geography, University of Toronto)	Relevant text deleted.
E-19-47	A	3	29	3	33	The phrase "triggering" partial deglaciation is misleading because it implies a trigger, ie a non-linear threshold. I'm not aware of such a threshold and have not found reference to a threshold in the underlying chapter, or WG1, or the literature. Perhaps it might be better to say "initiate" or simply "lead to". "Trigger" sounds very dramatic and has an air of irreversibility (which we don't know - we know that complete deglaciation could well be irreversible, but we do not (yet) know whether reaching 3 deg C for a century and then coming down again to 2 deg C would lead to irreversible melting. It might, but it's not borne out by the current literature.) For the same reason, I don't think it's a good idea to call deglaciation of Greenland an "event" because this "event", even in the worst case scenarios, is likely to be a rather drawn out affair lasting more than 1000 years. It leaves the statement open to the acccusation of being sensationalist - which it can easily avoid without loosing its quite important message by rewording. (Andy Reisinger, IPCC SYR TSU)	Partial deglaciation not mentioned here. Use of event removed. Entire discussion is designed to be consistent with literature assessed by WG 1.
E-19-48	A	3	29			This sentence says that deglaciation cannot be avoided. I don't think that's right. If it was, why bother with emission reduction? (Richard Tol, Hamburg University)	Wording revised for clarity.
E-19-49	A	3	29			Insert a new bullet to read as follows: "Increases in adaptive capacity would also reduce the socioeconomic risks from key vulnerabilities and DAI." (Indur Goklany, US Department of the Interior)	Not appropriate here. This is addressed elsewhere in the ES.
E-19-50	A	3	31	3	31	Suggest adding "estimated" after "currently". (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Relevant text removed.
E-19-51	A	3	32	3	33	Estimates of the probability depend not only on GHG concentrations, but also on our incomplete understanding of the relationship between GHGs and the potential events. The statement as is, is a conditional statement that assumes that understanding will not change. The estimated probability may either increase or decrease with time with the changing assessment of risk. Suggest that the	Reference to temperature added.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						statement be corrected to reflect this conditionallity. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	
E-19-52	A	3	33			However, the socioeconomic impacts of many such events (e.g., melting of ice sheets) can be substantially mitigated if these events occur over long time frames. (Indur Goklany, US Department of the Interior)	Not appropriate here. This is addressed elsewhere in the ES.
E-19-53	A	3	34	3	37	Please be clear whether you refer to CO2 only or CO2-equivalent. Also the uncertainty language is rather vague: "high confidence" that something "could" happen is ill-defined, please try to be more specific, perhaps by explicitly referring to the likely range for climate sensitivity as assessed by WG1. (Andy Reisinger, IPCC SYR TSU)	Relevant text removed.
E-19-54	A	3	34	3	37	due to same reasons, I'd doubt this conclusion "stabilization above 450ppm could cause 2 C warming", we can not find high confidence evidence from the draft, I hope to delete it (Lin Erda, Chinese Academy of Agricultural Science)	Relevant text removed.
E-19-55	A	3	34	3	37	Does this conclusion essentially state that there is "high confidence" that there is a greater than a 0% chance that 2C will be exceeded at 450ppm? Given limited knowledge it is hard to claim that anything is impossible. Suggest removing the "high confidence" and retaining the latter explanation that the probability depends on climate sensitivity. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Relevant text removed.
E-19-56	A	3	34			Why do you talk about 2C? Is this an IPCC study or an EU one? (Richard Tol, Hamburg University)	EU target is 2C above preindustrial, not 2C above 1990-2000 levels. In any case, relevant text removed.
E-19-57	A	3	34			change "could" to "will" [you are fully justified in making this change because of the way the sentence begins] (Danny Harvey, Dept of Geography, University of Toronto)	Relevant text removed.
E-19-58	A	3	39	3	40	A sixth "reason for concern" was identified be Leemans and Eickhout 2004 'Risk to regional and global ecosystems'. I think the literature justifies this as a separate reason for concern. In the TAR the literature, or at least WGII Chapter 5 was very weak in relation to the projected effects of climate change on ecosystems and species. This has changed as can be seen from Chapter 4 and from my own work. It is also justified from the point of view of article 2 as ecosystems are one of the three metrics mentioned and Article 1.1 of the UNFCCC is clearly relevant in relation to adverse effects. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Relevant text removed.
E-19-59	A	3	44			Eliminate "adversely". Alternatively, add a sentence acknowledging that many changes that have been observed to date are not necessarily adverse (as yet). For	Relevant text removed.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						instance, the northern latitudes apparently have turned greener. (Indur Goklany, US Department of the Interior)	
E-19-60	A	3	45	3	45	"mountain-top" is an akward wording, and not used later in the chapter. I suggest "alpine" as this best reflects the environment at stake. (Ulf Molau, Göteborg University)	Alpine does not capture all relevant areas. Mountain-top changed to high-mountain, consistent with Table 19.1.
E-19-61	A	3	46			Change “pose significant risks” to “increase the likelihood of significant changes”. Some of the changes might include additional biomass production (e.g., Levy et al. 2004) – which may actually increase energy supplies for species. (Indur Goklany, US Department of the Interior)	Relevant text removed.
E-19-62	A	3	47	3	47	"biodiversity hotspots" is a commonly used phrase today among biologists, but need to be precisely defined (including a note (including a note that this refers to species richness and not to other components of biodiversity, such as genetic or ecosystem diversity) (Ulf Molau, Göteborg University)	Relevant text removed.
E-19-63	A	3	49	3	51	Replace this sentence with two sentences addressing mortality and property losses separately. The first sentence should read as follows: “Despite the recent spate of deadly extreme weather events such as the 2003 European heat wave and the hurricanes of 2004 and 2005, aggregate mortality and mortality rates due to extreme weather events are generally lower today than they used to be. Globally, mortality and mortality rates have declined by 95 percent or more since the 1920s. Cumulatively, the declines from events such as droughts, windstorms and floods more than compensated for increases due to the 2003 heat wave (Goklany 2006b, 2005b) The second sentence, addressing property losses should read as follows: “In terms of real dollars, property losses from extreme weather events have increased, however, this upward trend vanishes if property losses are normalized in terms of the property at risk (see, eg., Pielke, Jr., R.A., and Landsea, C.W (1998), Normalized Hurricane Damages in the United States: 1925–95. Weather and Forecasting, 13, 621–631); Goklany (2000); Mary W. Downton, J. Zoe Barnard Miller, and Roger A. Pielke Jr. (2005), Reanalysis of U.S. National Weather Service Flood Loss Database. Natural Hazards Review. February 2005: 13-22.) (Indur Goklany, US Department of the Interior)	Relevant text removed, and this is again the baseline issue—see E-19-26 .
E-19-64	A	3	49	4	3	One form of Extreme Events that is not mentioned is the increasing frequency of extensive periods of winter rain in arctic and alpine areas (appears to be lacking also in Ch. 12 and 15. In alpine Fennoscandia, mild spells with heavy rainfall in winter have repeatedly created thick ice crusts in the snow pack that has had severe impacts on the mountain fauna, primarily by suppressing lemming peaks (lemming	Cannot include all impacts.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						population cyclicity has almost ceased since 1980, with many predators affected, e.g., snowy owl, skuas, rough-legged buzzard, arctic fox) (Ulf Molau, Göteborg University)	
E-19-65	A	3	49	4	3	It seems odd to have this update refer only to observed extremes, and not to use the new information on projected changes in future extremes, which is provided in WG1 table SPM-1. (Andy Reisinger, IPCC SYR TSU)	Most ecent available WG 1 chapters have been used for cross-referencing.
E-19-66	A	3		4		The reasons for concern have just been copied and pasted from section 19.3.7. This should be made less obvious (Clair Hanson, IPCC TSU)	Revised.
E-19-67	A	4	1			This is one-sided attribution. Weather definitely played a role, and climate change may have. But bad planning was just at least as much to blame in the heat waves, hurricanes, and river floods. (Richard Tol, Hamburg University)	Relevant text removed.
E-19-68	A	4	2	4	3	Replace these lines with the following: "socio-economic factors may be more important in terms of determining the human impact of extreme events than climate (or climate change) (Goklany 2000)." (Indur Goklany, US Department of the Interior)	Relevant text removed, this is once again a baseline issue—see E-19-26.
E-19-69	A	4	5	4	7	The point that climate impacts will be greatest in low-latitudeless developed areas that have contributed little to climate change is of political interest because it raises questions of distributive justice, but it is irrelevant to the sttement of the distribution of impact - and might be very misleading in the case of Singapore (still classed as a DC) for example. (Aynsley Kellow, University of Tasmania)	Relevant text removed.
E-19-70	A	4	6	4	7	I'm not sure that reference to historical responsibility for GHG emissions is necessary or helpful here. (Andy Reisinger, IPCC SYR TSU)	Relevant text removed.
E-19-71	A	4	6	4	7	Eliminate "that have historically contributed little to anthropogenic climate change" and add to the end of the sentence the following: "because they lack adaptive capacity (IPCC 2001)". Rationale: the issue of "historical contribution" is very complex and murky. More importantly, the real issue is what is fair. While it is possible to assign GHG emissions to nations based on where the act of burning a ton of coal, for instance, physically occurs, we should be cognizant that GHG emissions are the effluvia of a globalized economy. Economic activity in one country helps provide livelihoods and incomes for many inhabitants of other countries, and vice versa (Goklany 2006a). In fact, a substantial portion of economic growth in developing countries is	Relevant text removed.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						attributable to trade (Goklany 1995), and remittances and tourism from developed countries. Without such economic activities, U.S. emissions, for example, might be lower, but so would jobs and incomes elsewhere. Moreover, the benefits of these activities spill over into things like foreign aid; creation and support of the Internet; medicines for HIV/AIDS; development and dissemination of technologies to improve access to safe water, sanitation, and reduce tropical climate-sensitive diseases such as malaria and diarrhea; and other items now considered by some to be global public goods (ODS 2003). Therefore, in order to determine "fairness", the direct and indirect costs of activities causing climate change should be offset by the direct and indirect benefits resulting from these same activities. As an example, the costs associated with a higher risk of coastal flooding in, say, Bangladesh, should be offset by the benefits that Bangladesh obtained from technologies such as the green revolution, malaria treatment and control, etc., which probably would have been substantially delayed, if not impossible, had the developed world had not been wealthy enough, in part due to fossil fuel driven economic development, to subsidize such technological change (Goklany 2006a). Because of such complexities, we advise that any allusions to "historical responsibility" be eschewed. Alternatively, a much fuller and more mature discussion of the issue should be provided. (Indur Goklany, US Department of the Interior)	
E-19-72	A	4	23		25	This is misleading. In the TAR, there was one study (Mendelsohn) that foresaw net benefits for moderate warming. Now there are four more (Tol, Maddison, Rehdanz, Nordhaus), and in fact there is no study since the TAR that says there are net damages of moderate warming. How can this imply "greater uncertainty"? (Richard Tol, Hamburg University)	See 19.3.2.3 and 19.3.7
E-19-73	A	4	24			change "greater uncertainty" to "less confidence" (Danny Harvey, Dept of Geography, University of Toronto)	Relevant text removed.
E-19-74	A	4	27	4	33	Note that this para is inconsistent with the detailed assessment of ice sheet changes and projections in WG1. The global temperature level at which complete deglaciation of Greenland would occur has been raised from 2.7 to 3.1 deg C by WG1, based on recent literature. This does not mean that you cannot qualify such findings further, ie with reference to incomplete modelling of base lubrication. The uncertainty of the best estimate of 3.1 deg C (plus or minus 1.6 deg C) means that it is entirely correct to say that 1-2 deg C global warming above current levels "could" lead to deglaciation (but please don't use "trigger"). But the statement that thresholds may be lower than in the TAR appears inconsistent with the literature on projected ice sheet changes, and appears inconsistent with the assessment by WG1.	Relevant text removed or revised. Chapter revised for consistency with WGI.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						If the authors believe nonetheless that the statement is justified, it would need a very clear and robust reference that supports this statement. Also the rate of 1m per century is an extreme case that is not supported by palaeo data or current models; this should be stated or better qualified, right now it could be understood to be an entirely (or even most) plausible assumption. With regard to MOC shut-down, the word "trigger" is also not consistent with model projections, because models don't show an abrupt switch (as the word "trigger" would suggest). There is palaeo-evidence that it can happen, but we don't have model projections that it would happen (abruptly, that is) under anthropogenic GHG forcing either during the 21st century or beyond. (Andy Reisinger, IPCC SYR TSU)	
E-19-75	A	4	27	4	29	I'm not sure this statement is entirely consistent with the assessment of WGI (see WGI Ch 10 SOD, p4 ll 49-54, and p71 ll 1-11). I think it is important to maintain close liaison with WGI on this type of issue to prevent inconsistency. (Richard Wood, Hadley Centre)	Chapter revised for consistency with WGI
E-19-76	A	4	28			insert "such as" before "deglaciation" (Danny Harvey, Dept of Geography, University of Toronto)	Relevant text removed.
E-19-77	A	4	31	4	33	The inclusion of the net biogenic feedbacks in this sentence is misleading, it reads as if there is a threshold for this feedback, but in fact there isn't. Also, there has been made quite an improvement on the biogenic feedbacks since the TAR as there are now fully coupled carbon cycle climate simulations available. However, the conclusion is that uncertainites on climate sensitivity have been increased by including biogenic feedbacks. (Marko Scholze, University of Bristol)	Relevant text removed.
E-19-78	A	4	31	4	33	I have difficulty with the apparent claim that there is a “large-scale singularity” in net biogenic feedbacks. By singularity one would have to assume it is meant to be a critical point (as defined in non-linear systems, e.g., a turning or bifurcation point). I have yet to see any model paper that shows such a critical point for near-current conditions. This would seem to put this speculative hypothesis nowhere near the “high confidence” mentioned; I wonder if one could make even a low confidence conclusion. On MOC there continues to be an inability to simulate abrupt (high sensitivity) change in climate from change in MOC for current conditions using GCMs. Again the hypothesis that the abrupt changes experience during glacial periods may have an analog today does not appear to have been proven with any confidence. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Relevant text removed.
E-19-	A	4	31	4	33	As for WAIS, it is important to maintain consistency with WGI (see WGI SOD Ch	Relevant text removed. Chapter revised for

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
79						10 p5 ll50-54, which I think is a 'high confidence' statement, and p 69 l28 -p70 l17) (Richard Wood, Hadley Centre)	consistency with WGI.
E-19-80	A	4			9	Same issue. Indigenous people have more to fear from the reality of bad policy than from the prospect of climate change. (Richard Tol, Hamburg University)	Relevant text removed.
E-19-81	A	5	21	5	25	It is regrettable that the IPCC Plenary has restricted the scope of this chapter to 'shedding light on key vulnerabilities and climate change risks' rather than assessing the literature for positive and negative impacts — or, at least, that the IPCC remit has been interpreted in this way. This restricts the analyis in such a manner as to preclude the possibility of making it an acceptable anallysis of the risks of climate change, which it clearly purports to be. Any risk analysis must include booth posiitive and negative factors. It is true that trade-offs between positive and negative factors is approporaitely left to policy-makers, but to omit the benefit siide of the equation is to skew the analysis in a way that renders it both hopelessly biased and an inadequate basis for such rsik management decisions. To give but one example, the chapter refers to the prospect of increased mortality as the result of heatwaves, yet there is evidence (ignored in this chapter) that cold-related deaths iin Europe are two orders of magnitude greater than heat related deaths (Keatinge WR, Donaldson GC, Cordioli E, Martinelli M, Kunst AE, Mackenbach JP, et al (2000) 'Heat related mortality in warm and cold regions of Europe: observational study' BMJ 321: 670-673), raising the prospect that warming is likely to have a net beneficial effect on mortality. (It should be noted that Keatinge et al is an observational study, and thus to be preferred to model-derived results - a point which is taken up below). To omit this kind of evidence is to seriously skew the picture of 'vulnerability' and render the chapter inadequate as an exercise in risk analysis. (Aynsley Kellow, University of Tasmania)	The IPCC Plenary-approved outline gives the title for this chapter as "Assessing Key Vulnerabilities and the Risk from Climate Change". As we now clearly state in the text, Vulnerability to climate change is the degree to which systems are susceptible to, and unable to cope with, adverse impacts from climate change. As we also now clearly state, "Given this focus on vulnerability, the analytic emphasis of this chapter is on people and systems that may be *adversely* affected by climate change, particularly where impacts could have serious and/or irreversible consequences. Nevertheless, positive impacts are addressed where relevant to this assessment of key vulnerabilities. A detailed description of positive and negative climate impacts in all sectors and regions is beyond the scope of this chapter, and readers are encouraged to turn to the executive summaries of the sectoral and regional chapters of this Report for this information."
E-19-82	A	5	21		25	"Moreover, IPCC ... natural systems." This is the most important sentence of the chapter. It should be kept at all cost. Perhaps it should be clarified to "The IPCC asked us to write a biased account of climate change impacts; we complied." (Richard Tol, Hamburg University)	The sentence in question has been deleted. See the response to E-19-81 describing the purpose and scope of this chapter regarding positive impacts.
E-19-83	A	5	22	5	22	Delete 'IPCC plenary determined'. It implies reluctance and is inappropriate. (Michael Manton, Monash University)	Deleted.
E-19-84	A	5	22			insert "that" after "determined" (Danny Harvey, Dept of Geography, University of Toronto)	Sentence has been deleted.
E-19-85	A	5	29	5	29	Delete 'charge to and', as it is inappropriate (Michael Manton, Monash University)	Deleted.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-86	A	5	29			charge to' - what does this mean? 'Beyond the scope' is sufficient (Clair Hanson, IPCC TSU)	Deleted.
E-19-87	A	5	47			Article 2 of the UNFCCC does not refer to explictly to dangerous climate change (DCC). Rather, it refers to "dangerous anthropogenic interference in the climate system" (DAI). The distinction between the two is not trivial, and has important policy implications. However, almost everyone now uses the terms DAI and DCC interchangably as if they were the same thing. For this reason, I wrote a paper to clear up the confusion (Harvey, 2006a)- and to underline the significant policy implications of the difference - that has recently been accepted for publication in Climatic Change and can therefore be cited here. A copy is attached, and I suggest that the relevant distinctions be explained here. The useful excerpt from my paper is as follows: "Dangerous anthropogenic interference (DAI) in the climate system is a set of increases in GHGs concentrations that has a non-negligible possibility of provoking changes in climate that in turn have a non-negligible possibility of causing unacceptable harm to humans, human societies, or natural ecosystems. Dangerous climatic change is a change of climate that has a non-negligible possibility of causing harm to humans, human societies, or natural ecosystems. Harmful climatic change is a change in climate that does in fact cause harm to one or more of the above." In other words, a certain increase in GHGs can be considered dangerous even if it does not ultimately cause harmful or even potentially harmful climatic change (because the climate sensitvity turned out to be at the low end of the accepted likely range), but rather because it has a non-negligible possibility of provoking such change in climate. However, most people writing on the subject imply that a given GHG increase is dangerous if it actually causes harmful climatic change, or instead just directly focus on climatic change. The distinction is important, because to determine acceptable GHG concentrations to avoid DAI, one need only know a reasonable upper limit to climate senstivity, whereas to avoid DCC, one has to first know what the correct climate sensitivity is. Since most people assessing what is needed to avoid DCC adopt some middle assumption of climate sensitivity, avoiding DCC provides a less stringent constraint on allowable GHG concentrations than avoiding DAI. I am also attaching a followup paper (Harvey, 2006b, submitted to Environmental Research Letters) that highlights the importance of considering climate sensitivities somewhere near the high end of the climate sensitivitiy pdf. As well, DAI due to rising CO2 concentration occurs not only through possible changes in climate, but also through certain changes in ocean chemisty that are independent of climate sensitivity. The arguments are a bit tricky, however, because if climate sensitivity is low, the	We now refer explicitly to “dangerous anthropogenic interference.”

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						contrarians could argue that changes in ocean chemistry do not have climatic effects and therefore do not constitute interference in the climate system. However, they certainly represent interference in the broader Earth system, and it is clear from the wording of Article 2 (with its reference to protecting ecosystem) that the reason for concern with interference in the climate system is because of the impact on the broader Earth system. Thus, impacts of increasing GHG concentrations on the broader Earth system that don't require climatic change as an intermediate step are a valid consideration. Here is the except discussing this from Harvey (2006a): "increasing atmospheric GHG concentrations represent “interference” in the climate system not only through radiative forcing (leading to climatic change), but also through effects on oceanic chemistry. The absorption of anthropogenic CO2 by the oceans leads to changes in the pH of the ocean and in its saturation state with respect to calcium carbonate that in turn could eventually have profoundly negative effects on marine biological productivity. Marine biological productivity is an integral part of the climate system, through its effect on the sulfur cycle (through emissions of precursors to dimethyl sulfide), the carbon cycle (through the biological pump), other biogeochemical cycles, and on surface albedo (through enhanced absorption of solar radiation by chlorophyll). These effects are independent of any changes in climate through the radiative forcing of CO2. A disruption in the biological pump (and possibly also in the sulfur cycle) would act to warm the climate. It could be argued that if the direct climatic effects of CO2 are small, then feedback effects through changes in ocean chemistry will also be small and so do not constitute dangerous interference in the climate system. However, this is only a speculative possibility at the moment. More importantly, increasing GHG concentrations are “dangerous” due, ultimately, to their potential adverse impacts, whether or not these impacts involve the intermediate step of climatic change. Thus, it is appropriate to interpret the goal of avoiding dangerous interference in the climate system as a goal of avoiding dangerous interference in the broader Earth system, including both climate and life-support components. Thus, the afore-mentioned literature, by equating dangerous anthropogenic interference with dangerous climatic change and focusing on the latter, omits an important additional realm of dangerous anthropogenic interference in the broader Earth system arising from the increase in atmospheric CO2." (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-88	A	5	50			Add a new sentence on this line that would read as follows: “They should not be construed to be representative of views’ of reviewers, either individually or	We do not agree that this is necessary.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						collectively." (Indur Goklany, US Department of the Interior)	
E-19-89	A	6	18	6	18	There is terminology that begins in this box that should be defined and used consistently throughout. Namely, "threshold", "level", and "target". Threshold has already been used in the ES associated with a singularity, but it is apparently used here as something else (the obvious application in this context is the EU objective to control T-increase to 2C). Suggest that terminology be explicit and fully defined. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	We have made this box consistent with our use of the term "level", as this box is intended specifically to define the reference with respect to which the temperature levels in this chapter are reported, and is not intended to refer to anything else.
E-19-90	A	6	18	6	33	Suggest putting all DeltaT with respect to preindustrial as this helps to order the discussion of observed and projected changes and is consistent with the way in which Chapter 4 appears to have done its work (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Unfortunately, we cannot please everyone with respect to this choice (see comment E-19-91, e.g.). As explained in the text, we make this choice to reflect the most common metric found in the literature.
E-19-91	A	6	18	6	32	Box 19.2 is useful and should be near the front. Indeed, given the specified transforms, the whole assessment should use a common basline, unless there is a very good excuse not too. The lack of precision in impacts suggests that either current or 1990 levels should be used right throughout WG2 (Michael Manton, Monash University)	This is a comment for WGII as a whole.
E-19-92	A	6	18			Box 19.2 a new concept for current climate (1990-2000), but it looks this period is too short. Other important point for key impacts is "in most land areas, regional warming is larger than gobal warming (WGI)", need more assessment for it in this chapter (Lin Erda, Chinese Academy of Agricultural Science)	We do not attempt to define "current climate" using a ten year period (the reviewer is correct, a longer period is needed). Instead we wish to differentiate between pre-industrial temperatures and temperatures at the end of the 20th century, and have modified the text to clarify this point. Regarding the assessment of regional temperature levels, we defer to WGI, and simply report temperature levels associated with key vulnerabilities where possible.
E-19-93	A	6	24	6	28	"Pre-industrial" global temperature was not constant. Current reconstructions show that the range of temperature could have spanned about a 1C range (e.g. in NRC (2006) they compare the various reconstructions and the Esper et al. reconstruction shows a NH temperature range of about 1.0C over the preindustrial period), and current drafts of WG1 acknowledge such a variability. Clearly, the best estimates of global temperature are based on the instrumental record which dates back only to about 1850. While the 0.6C referred to here comes from the difference from a	We have modified the text to clarify the meaning of the 0.6C increase included here, and that "pre-industrial temperatures" are also defined relative to a specific reference year or period.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						specific period (late 1800s), the range could be much larger for other "pre-industrial" times. Variability in pre-1900s global temperature as well as the overlay of natural variability confounds the simplified logic surrounding the 2C control concept. Suggest that these issues not be neglected in this box. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	
E-19-94	A	6	30			insert full stop after (see WG1 chapter 11) (Clair Hanson, IPCC TSU)	There is already a period here.
E-19-95	A	6	36			SECTION 19.1.2 Good discussion! (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Thanks
E-19-96	A	6	50	7	3	The lack of precision in the definition of vulnerabilty is not helpful to anyone. Given the structure of the IPCC there is no reason for vulnerabilities to include the mechanisms covered by WG1. Poor old hydrology seems to fall in the middle. (Michael Manton, Monash University)	Definition revised in accord with AR4 Glossary. We now make clear that mechanisms covered by WGI, must be considered in Chapter 19 because they may involve key vulnerabilities in the socio-economic and biological systems.
E-19-97	A	7	3			"recent literature" please summarise this literature; the IPCC is not bound by the strictures of the European Union, is it? By mentioning only a single target, you implicitly endorse it. (Richard Tol, Hamburg University)	Phrase referred to in comment is not in text at this location.
E-19-98	A	7	5	7	8	If the reference list is exemplarily then there should be an 'e.g.' at the beginning of the references. The authors may also want to add the reference Scholze et al., 2006, in press, PNAS, in which we used a shift of the mean state by one standard deviation of the pre-industrial (1961-1990) variability as an indicator for calculating climate change risks on ecosystems. (Marko Scholze, University of Bristol)	"e.g." added to make clear list is not exhaustive. However, the proposed reference is not appropriate here. It may be appropriate in one of the regional chapters.
E-19-99	A	7	26	7	28	The connection between the first and second phrase, and the previous sentence are not clear. Suggest replacing with "Value judgements are necessarily subjective, and they may also be informed by …" (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Section rewritten; material deleted.
E-19-100	A	7	26	7	28	The connection between the first and second phrase, and the previous sentence are not clear. Suggest replacing with "Value judgements are necessarily subjective, and they may also be informed by …" (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Repeats above comment.
E-19-101	A	7	26	7	28	Shorten this sentence to read: "Value judgments are necessarily subjective". The additional phrase "they may be informed by ethical, moral, or religious arguments" is less informative than it sounds because these arguments – and more importantly -- the force that should be given to these argument is itself subjective. In fact, it is a	Section rewritten; material deleted.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						good argument for avoiding such judgeent, because otherwise one will get into discussions of theology. We urge that the IPCC stick to science instead. (Indur Goklany, US Department of the Interior)	
E-19-102	A	7	26		28	This is tautological. Value judgements are informed by value judgements. (Richard Tol, Hamburg University)	Section rewritten; material deleted.
E-19-103	A	7	27	7	28	the cited works are missing from the reference list (Danny Harvey, Dept of Geography, University of Toronto)	Citations deleted.
E-19-104	A	7	32	7	32	Flexible' is not the correct adjective. Box 19.1 shows that there are precise definitions of the things to look for (ecosystems, food, and economic development). The issue for science is to determine the relevant time scales for each process. It is not that the definition is flexible, it is scientifically difficult to estimate. (Michael Manton, Monash University)	Sentence deleted.
E-19-105	A	7	36	7	38	your statement about operationalizing Article 2 entangles two separate steps, because you refer to DAI and DCC in the same sentence without distinguishing between them. In fact, there are 3 separate steps needed in order to operationalize Article 2, only two of which have been recognized in the literature except in Harvey (2006a,b). I suggest explaining the operationalization of Article 2 somewhat along these lines: If one had complete knowledge of climate sensitivity and of impacts as a function of global mean temperature change, and if there were universal agreement as to when the impacts become unacceptable, operationalizing Article 2 would consist of two steps: (1) specifying the climate sensitivity, which links stabilized radiative forcing and hence GHG concentrations to equilibrium global mean temperature change, and (2) specifying the global mean temperature threshold for unacceptable impacts. However, both climate sensitivity and impacts as a function of global mean warming are uncertain, and furthermore, there will be disagreement as to when the impacts become unacceptable and are therefore something to be avoided. Thus, the climate sensivities and temperature thresholds need to be replaced with probability distribution functions. Having cast the climate sensitivity and temperature threshold in probabilistic terms, a third input is required in order to determine the maximum allowable GHG concentrations: the maximum acceptable probability of provoking impacts that had been previously deemed to be unacceptable. The larger the impacts deemed to just reach the threshold of unacceptability, the lower this probability should be. These three inputs are explicitly considered in Harvey (2006a,b). REFERENCES: Harvey, L.D.D. 2006a. Dangerous Anthropogenic Interference, Dangerous Climatic Change, and Harmful Climatic Change: Non-Trivial Distinctions with Significant Policy Implications. Climatic Change (accepted). Harvey, L.D.D. 2006b. Allowable CO2	The term “operationalize” has been eliminated and the discussion rewritten to emphasize some of the factors that may be considered by the Parties to the UNFCCC in interpreting Article 2, where IPCC may also be able to provide some insight.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						Concentrations Under the United Nations Framework Convention on Climate Change as a Function of the Climate Sensitivity PDF. Environmental Research Letters (submitted). (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-106	A	7	36	8	3	While this section is labeled to be about Art. 2, its content is focused on "operationalizing" Art. 2. This policy strategy appears to be the initiative of a subset of the analysis community, and if included should be considered along side of all other policy strategies in a separate section on policy strategies (this is already in the section on decision making in 19.4). Suggest that selective choice of policy strategies not be reviewed here. Also suggest that terms chosen be closer to the language of Art.2; e.g. instead of considering "operationalizing", one could consider strategies to achieving the objectives set out in Art 2 (and referring to the relevant section so 19.4 as an introductory roadmap), since by definition Art. 2 is the "objective". (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Deleted. See above.
E-19-107	A	7	36			the literature is not quite so sparse that you can cite only one reference! You can add Azar and Rodhe (1998), Hansen (1995), and Harvey (1996, 2004, 2006a,b), and the entire volume published by Cambridge, "Avoiding Dangerous Anthropogenic Climatic Change". REFERENCES: Azar, C. and Rodhe, H.: 1997, 'Targets for stabilization of atmospheric CO2', Science 276, 1818-1819. Hansen, J.: 2005, 'A slippery slope: how much global warming constitutes "dangerous anthropogenic interference"?', Clim. Change 68, 269-279. Harvey, L.D.D. 1996 "Development of a risk-hedging CO2 emission policy: Part I: Risks of unrestrained emissions", Climatic Change 34, 1-40. 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. 2006a. Dangerous Anthropogenic Interference, Dangerous Climatic Change, and Harmful Climatic Change: Non-Trivial Distinctions with Significant Policy Implications. Climatic Change (accepted). Harvey, L.D.D. 2006b. Allowable CO2 Concentrations Under the United Nations Framework Convention on Climate Change as a Function of the Climate Sensitivity PDF. Environmental Research Letters (submitted). Schellnhuber, H.J., Cramer, W., Nakicenovic, N., Wigley, T., and Yohe, G. (editors), 2006. Avoiding Dangerous Climate Change, Cambridge University Press,	Discussion revised to make clear that the cited reference (now two) is to reviews, not to the primary literature, which, while slim, is yet too long for a citation list.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						Cambridge. (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-108	A	7	43			Gardiner (2005) is missing from the reference list (Danny Harvey, Dept of Geography, University of Toronto)	Reference added
E-19-109	A	7	44	7	45	It might be worth explaining that the three subsidiary conditions referred to in Article 2 are referred to because they are three impact areas (among many) that are sensitive to rates of change as well as to absolute change. Harvey (2006a) states the following: "Article 2 of the UNFCCC, after declaring that the ultimate objective of the convention is to achieve stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system, goes on to state that, 'Such a level should be achieved within a time frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened, and to enable economic development to proceed in a sustainable manner'. By speaking of adaptation to climatic change, it is implied that the ultimate climatic change (related to the chosen GHG stabilization levels) is small enough and hence benign enough that adaptation is possible in the first place. The three subsidiary conditions (allowing ecosystems to adapt, maintaining food production, and enabling sustainable economic development) are restrictions on the rate at which non-dangerous greenhouse gas (GHG) concentrations are reached. They are related to that fact that climatic change that is not harmful (that is, sufficiently limited that adaptation is possible), were it to occur slowly, could be highly disruptive (harmful) if it were to occur too fast. These conditions thus set a constraint on rates of allowable GHG emissions, while the overall goal of capping GHG concentrations at non-dangerous levels largely represents a constraint on cumulative CO2 emissions." I think that it is quite important to make the point that, in refering to rates of change that permit adaptation, Article 2 is assuming that absolute changes are small enough that adaptation is possible. One of my consistent comments on the earlier chapters of WGII is that they begin with a list of potential impacts (some extremely serious!), then move on to a discussion of adaptation, but never with a preamble to the adaptation section stating that they are assuming that the changes are small enough that meaningful adaptation is possible, while for climate changes anticipated by the end of the century under business as usual emission scenarios, adaptation in many impact areas will be futile. This important point needs to be made in Chapter 19 somewhere, and in the context of explaining Article 2 seems like a good spot. (Danny Harvey, Dept of Geography, University of Toronto)	While the details of this comment are beyond the scope of this chapter, we have rewritten various sections to emphasize the difference between the various aspects of Article 2. Moreover, brief discussion of Article 1 contents appears now in 19.1.2.3

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-110	A	7	45	7	45	Replace 'some' by 'specific'. Article 2 gives specific guidance on what impacts to look at. (Michael Manton, Monash University)	”Some” deleted
E-19-111	A	7	45	7	45	Relevant to this specific issue is the definition of adverse effects in UNFCCC Article 1.1 1. "Adverse effects of climate change" means changes in the physical environment or biota resulting from climate change which have significant deleterious effects on the composition, resilience or productivity of natural and managed ecosystems or on the operation of socio-economic systems or on human health and welfare. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Reference to Article 1 added
E-19-112	A	7	47	7	47	Insert the phrase "by the Conference of Parties (COP) to the UNFCCC" after the word "establishment". Article 7 of the UNFCCC establishes the COP as the supreme body of the UNFCCC responsible for "the decisions necessary to promote the effective implementation of the Convention." A decision on what constitutes dangerous anthropogenic interference with the climate system certainly fall under that mandate. Inserting this phrase into the text will make it clear to readers that WG II recognizes where responsibility for defining dangerous anthropogenic interference lies. (Lenny Bernstein, L.S. Bernstein & Associate, L.L.C.)	Discussion rewritten,a s indicated above in response to comment E-19-105.
E-19-113	A	7	51	8	2	The authors may want to check the reference here: I think the 2 deg goal has first been discussed by the German Advisory Council on Global Change (WBGU). (Marko Scholze, University of Bristol)	Discussion of target deleted
E-19-114	A	8	2			Add a new sentence on this line that would read as follows: “However, the scientific basis for was weak when adopted and increasing uncertainty about climate change impacts (see lines 13-25, page 4 of this chapter) haven’t necessarily strengthened its underpinnings.” (Indur Goklany, US Department of the Interior)	Discussion deleted.
E-19-115	A	8	3			please define GMT (Clair Hanson, IPCC TSU)	Discussion deleted.
E-19-116	A	8	7	8	14	A more balanced discussion would include references to vulnerable ecosystems as well. For example, after "groups" in line 7,insert "and ecosystems". At the end of the paragraph, say something about research on particularly vulnerable ecosystems, such as coral reefs, alpine regions, and arctic ecosystems. (Danny Harvey, Dept of Geography, University of Toronto)	Particular ecosystems are discussed in 19.3.
E-19-	A	8	23	8	26	Good point!	Thank you.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
117						(Ulf Molau, Göteborg University)	
E-19-118	A	8	28	8	45	Good and useful section. (Ulf Molau, Göteborg University)	Thank you.
E-19-119	A	8	30	8	45	Since the notion of thresholds is one of the major developments compare to TAR it would be useful if authors elaborate more on this subject here. (Alexander Golub, Environmental Defense)	We regret that do not have sufficient space to provide further detail, which can be found through the citations provided..
E-19-120	A	8	50	8	50	This first sentence refers to 'impacts' as vulnerabilities; I.e. a tighter definition that in section 19.1.2.1. This is good. (Michael Manton, Monash University)	We have rewritten the definitions at the beginning of 19.1.2, and this eliminates the problem.
E-19-121	A	9	13			Co-opting Goklany is disingenious. His paper is quite at odds with what you write. Please provide an honest account of the literature. (Richard Tol, Hamburg University)	We only state that the categories or criteria are similar in the Goklany paper, not that the conclusions are the same. In any event, we have moved the reference up to avoid ambiguity on this point.
E-19-122	A	9	18			after "affected" insert ", or the significance of the ecosystem lost" (Danny Harvey, Dept of Geography, University of Toronto)	”Significance” is discussed below under the “importance” criterion.
E-19-123	A	9	26			Tol does not estimate WTP for avoiding CC impacts. Only Li et al. (2004, Ecological Economics, 48, 329-343) do. (Richard Tol, Hamburg University)	Citation switched.
E-19-124	A	10	34	10	46	I would consider poverty as confounding factor that exasserbate impacts and complicates adaptation in least developed nations. (Alexander Golub, Environmental Defense)	Sentence added.
E-19-125	A	11	8	11	16	It seems to me irrelevant. We should be concerned about vulnarability of all nations regardless thir imput into climate change so far. (Alexander Golub, Environmental Defense)	Section rewritten; comparison deleted.
E-19-126	A	11	12			Müller only re-iterates other studies. Fankhauser, Mendelsohn, and Tol have published estimates. (Richard Tol, Hamburg University)	There is no problem citing a review of other studies.
E-19-127	A	11	18	8	31	Good and important section. (Ulf Molau, Göteborg University)	Thank you.
E-19-128	A	11	18	11	31	I think, importance of the vulnerable systems should be discussed in the context of reversibility. I would suggest to merge this section with the section at page 10 , line 4-18. (Alexander Golub, Environmental Defense)	We disagree, as reveresibility is scientifically assessable criteria and “importance” a normative judgment and should be a separate criteria..
E-19-129	A	11	29	11	29	The work in Evans et al 2004a and 2004b does not disaggregate coastal effects. An account of specific coastal impacts is provided in Hall, J.W., Sayers, P.B., Walkden, M.J.A. and Panzeri, M. Impacts of climate change on coastal flood risk in	Comment out of place and object cannot be found.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						England and Wales: 2030-2100. Phil. Trans. Royal Society, A 364 (2006) 1027–1049. (Jim Hall, University of Newcastle upon Tyne)	
E-19-130	A	11	33			In consonance with the previous comment that a key criterion for selecting "key vulnerability" had been omitted, we would add the following new para: "Relative contribution of climate change to key vulnerability Considering that this chapter is (per the first sentence in this chapter) directed toward a discussion of key vulnerabilities to climate change that merit particular attention by policy makers, a critical criterion is the relative importance of climate change in determining the magnitude and timing of the vulnerability in question (among other things). The reason why this is important is that policy makers have to be able to understand, among other things, the opportunity costs of response measures. Consider, for instance, global hunger – certainly a key vulnerability. It is projected to be a problem in the future and climate change is projected to add to this problem (e.g., Parry et al.1999, 2004). Policy makers are owed not only estimates of the future population at risk (PAR) of hunger due to CC, but also the PAR in its absence (Goklany 2000, 2003, 2005). Without such context, it would be impossible for policy makers to make a reasoned judgment on the amount and timing of resources to be spared for different response strategies that would directly or indirectly address the key vulnerability. It is not inconceivable that in some cases despite the fact that CC may contribute to the PAR for hunger, other factors may contribute larger amounts to that PAR More importantly, the non-CC related contributions to the PAR may be reduced more effectively and/or efficiently than might reductions in CC (Goklany 2006a)" (Indur Goklany, US Department of the Interior)	This issue is one of the relative importance of climate impacts as opposed to other changes in the future; it is beyond the remit of this chapter to compare marginal changes of climate to baseline development activities—see E-19-26.
E-19-131	A	12	6	12	12	Explain different categorisation in Table 19.2 i.e. its heading in bold (which are different to those of Table 19.1) (Rachel Warren, School of Environmental Sciences)	Tables have been combined
E-19-132	A	12	12			Add the following sentence to the end of this para:: "However, by ignoring the time dimension, it is not possible to account for secular changes in technology, and time dependent changes in economic growth or changes in human and social capital – all determinants of adaptive capacity, a key criterion for establishing key vulnerability. Accordingly, results from most cases should be treated with caution Goklany (2006a)." (Indur Goklany, US Department of the Interior)	Covered in bit on uncertainties about socioeconomic development
E-19-	A	13	1	20	50	Somewhere could add the human amenity is impacted by lack of water/snow in	Is the former a KV?

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
133						recreational areas, losses of ecosystems and species. (Rachel Warren, School of Environmental Sciences)	
E-19-134	A	13	1	20	50	Page 18 "potential regional extinction of coral reefs" at above 4C should be moved to below 2C (Table 19.2). In Table 19.1 there is correct statement that coral reefs are threatened at 1C warming. (Rachel Warren, School of Environmental Sciences)	Have addressed
E-19-135	A	13	1	20	50	Ecosystems section of Table 19.2 needs an entry for the Arctic (I note it is currently in as a ? Regional problem). Either way an entry should reflect the endangerment of polar bear, the complete loss of summer sea ice with 1.5C warming above 1990, the transformation of tundra (77% low tundra lost at 2.8 above 1990) the damage to infrastructure, and loss of breeding habitat of large fractions of the world's shorebirds. See WG2 Ch 6 table of impacts at diff levels of T rise. (Rachel Warren, School of Environmental Sciences)	Yes, Arctic can be considered to be a KV resulting from key impacts—distinctions now made explicit.
E-19-136	A	13	1	20	20	Confusing statements about agricultural and global market impacts. In various places I read that low confidence as to where aggregate GDP increases or decreases below 2C, whilst simultaneously I read that there is potential for increased global food production (low confidence). Suggest edit entry page 16 table 19.1 to read "potential for increased or decreased global production" (if this is true ... but surely it must be since agricultural yields are the strongest driver of GDP aggregate market impacts. Currently the two table entries appear as if they may be inconsistent. (Rachel Warren, School of Environmental Sciences)	Revised
E-19-137	A	13	1			Table 19.1 Row 6 (ocean ecosystems) Column 3 (remarks…) - Again, language needs to be more specific: what does "coral reefs threatened..." actually mean? If this is meant to claim that coral reef as ecosystems and/or geomorphological structures might cease to exist, then I would suggest that there is far from consensus that this is plausible or likely at 1deg C warming. I suggest, as more accurate and less contentious term: "coral reefs degraded…." (Paul Marshall, Great Barrier Reef Marine Park Authority)	See Marine Ecosystem chapter
E-19-138	A	13	1			Table 19.1 Risks of Large Scale Discontinuities. Suggest adding "Changes in Monsoon strength and variability" Reasons were established in the TAR and science since then appears to have added to the justification for including this eg Zickfeld, K., B. Knopf, V. Petoukhov, and H. J. Schellnhuber (2005). "Is the Indian summer monsoon stable against global change?" Geophysical Research Letters 32(15): 1-5. Zhang, R., and T. L. Delworth (2005). "Simulated Tropical Response to a Substantial Weakening of the Atlantic Thermohaline Circulation." Journal of Climate 18(12): 1853-1860. Coppola, E., and F. Giorgi (2005). "Climate change in	Good point; import from WG-1

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						tropical regions from high-resolution time-slice AGCM experiments." Quarterly Journal of the Royal Meteorological Society 131: 3123-3145. May, W. (2004). Potential future changes in the Indian summer monsoon due to greenhouse warming: analysis of mechanisms in a global time-slice experiment. 389-414. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	
E-19-139	A	13	1			Table 19.1 Risks of Large Scale Discontinuities. Suggest adding "Increased Drought" as this seems to be a large scale change with high adverse consequences. The criteria for "key" would include that an increase in drought frequency, intensity and geographic synchronization would have large adverse consequence for ecosystems, food production and water resources and is likely to affect already vulnerable regions such as southern Africa, and other regions such as southern Europe and southern Australia. See eg Ciais, P., M. Reichstein, N. Viovy, A. Granier, J. Ogee, V. Allard, M. Aubinet et al. (2005). "Europe-wide reduction in primary productivity caused by the heat and drought in 2003." Nature 437(7058): 529-533. Held, I. M., T. L. Delworth, J. Lu, K. L. Findell, and T. R. Knutson (2005). " Simulation of Sahel drought in the 20th and 21st centuries." PNAS 102(50): 17891-17896. Lau, K. M., S. S. P. Shen, K. M. Kim, and H. Wang (2006). "A multimodel study of the twentieth-century simulations of Sahel drought from the 1970s to 1990s." J. Geophys. Res. 111(D7): 1-9. Lotsch, A., M. A. Friedl, B. T. Anderson, and C. J. Tucker (2005). "Response of terrestrial ecosystems to recent Northern Hemispheric drought." Geophysical Research Letters 32(6). Masson-Delmotte, V., G. Raffalli-Delerce, P. A. Danis, P. Yiou, M. Stievenard, F. Guibal, O. Mestre et al. (2005). "Changes in European precipitation seasonality and in drought frequencies revealed by a four-century-long tree-ring isotopic record from Brittany, western France." Climate Dynamics. Neelin, J. D., C. Chou, and H. Su (2004). "The "upped-ante mechanism" for tropical drought." Bulletin of the American Meteorological Society 85(5): 668-669. Neelin, J. D., M. Munnich, H. Su, J. E. Meyerson, and C. E. Holloway (2006). "Tropical drying trends in global warming models and observations." PNAS: 0601798103. Nepstad, D., P. Lefebvre, U. Lopes da Silva, J. Tomasella, P. Schlesinger, L. Solorzano, P. Moutinho et al. (2004). "Amazon drought and its implications for forest flammability and tree growth: a basin-wide analysis." Global Change Biol	Yes. Is it in AR-4. Goes in extreme events.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						10(5): 704-717. Salinger, M. (2005). "Climate variability and change: Past, present and future - An overview." Climatic Change 70(1-2): 9-29. Wang, G. (2005). "Agricultural drought in a future climate: results from 15 global climate models participating in the IPCC 4th assessment." Climate Dynamics: 1-15. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	
E-19-140	A	13	1			TABLE 19.1 Distribution of impacts. I suggest an additional row here on regional agricultural impacts. The criteria is that substantial adverse regional impacts on food production are projected for highly vulnerable regions (southern Africa, parts of South Asia, Latin America). The remarks in critical levels etc would include reference to the possibility of substantial regional food deficits that would not be readily fixed through market processes. Such impacts do not fit under the aggregated reason for concern but are highly relevant to the Article 2 issues (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Regional impacts are now covered in table
E-19-141	A	13		15		Table 19.1 should be modified in light of the above comments. To accommodate the additional criterion that we recommended for identifying and assessing "key vulnerability", we recommend adding an additional column with the following heading: "relative contribution of CC to risks". The cells under this column should be populated with assessments of the relative contribution of CC to these risks for various years in the future. For example, based on the global impacts of climate change (Arnell et al 2002, Arnell 2004, Parry et al. 2004, Levy et al. 2004) at least through the 2080s, with respect to forests, CC may lead to a global expansion of forests; with respect to hunger, the contribution of CC to the global population at risk of hunger is relatively small; with respect to public health, it could be minor; with respect to water resources, fewer people would be under water stress (see Goklany 2000, 2005a, 2005c, and references therein). (Indur Goklany, US Department of the Interior)	This is an obfuscation. Should we look at costs of migitation only relevant to total energy expenditures?
E-19-142	A	13				Table 19.1 is misleading without a context. Forest fires are also due to management; coral reefs are also threatened by mining and fishing. Without a context, you just dramatise climate change. (Richard Tol, Hamburg University)	The focus is on marginal climate change impacts, not other baseline effects unelss they are synergistic or affect adaptive capacity..
E-19-143	A	13				T19.1: remove 2nd 'at' in 3rd column under terrestrial ecosystems (Clair Hanson, IPCC TSU)	OK
E-19-144	A	13				T19.1 and throughout chapter - where other chapters are referenced please provide section numbers (Clair Hanson, IPCC TSU)	Have done

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-145	A	13				Infrastructure: Add at the end of the 3rd column: "…and/or repair during normal maintenance cycle, and/or gradual abandonment based on the rate of relative SLR and increases in risks." (Indur Goklany, US Department of the Interior)	That's a big assumption. How do you know it will happen that way. Adaptive potential already mentioned.
E-19-146	A	13				Indigenous, poor or isolated communities: The 3rd column should note that adaptive capacity should improve over time if, per the SRES scenarios, populations become wealthier and technologically more sophisticated and if there is progress toward sustainable development as exemplified, for instance, by the Millennium Development Goals (because such progress will also add to human and social capital) (Goklany 2006a, 2005a)." (Indur Goklany, US Department of the Interior)	Generally agree, but this assumes that the benefits of growth are dristributed across all groups and classes. We've seen economic growth in the US in recent years, but only among top few percent of population. Issues explicated in revised text.
E-19-147	A	13				I would agree with most of the numbers in Table 19.1 if the temperature changes were stated to be with respect to preindustrial conditions rather than with respect to 1990. The suggestion that coral reefs are not threatened until a further warming of 1 K above 1990 is completely indefensible. Observations indicate that coral reefs are seriously threatened by the amount of warming aleady reached during the 1990s. In particular, 90% of shallow-water corals in the Indian Ocean were killed during the 1998 El Nino, a year with an average temperature only 0.9 K above the early 1900s level. Overall, about 16% of coral reefs worldwide suffered near total (95-100%) mortality of coral, with only about half showing recovery since then according to "Status of Coral Reefs of the World: 2004", available online from the Australian Institute of Marine Science. Full recovery of that half requires that an El Nino like that of 1998 not re-occur for another 10 years - but even the present global mean warming may present too short a recurrence interval for El Nino's of that severity. Chapter 4 (page 37, lines 22-23) states that 30% of warm water corals have disappeared during the past 20 years and that this is due largely to warmer temperatures. Many might regard loss of 30% as a significance loss (I do!). Thus, I think that the evidence - based on what has been observed already and what is stated in Chapter 4 - compells you to change the lower limit of temperatures that threaten coral reefs to 0 C (rather than 1 C) if with respect to 1990, or 0.6 C if with respect to preindustial levels. (Danny Harvey, Dept of Geography, University of Toronto)	Findings are consistent with Ch 5.
E-19-148	A	13				Coastal Communities: In the third column replace "partially reduced" with "reduced if not eliminated". First, it leaves the original meaning intact. Second, in theory, at least, this particular vulnerability can be eliminated if people move (or are moved) inland. (Indur Goklany, US Department of the Interior)	Suggestion to pick up all the coastal ciies and move them not considered viable by authors.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-149	A	14	1			Table 19.1. The last sentence in the Remarks on critical level, timings and confidence for Health seems a bit contradictory. Would it be better to start the sentence by saying that 'Adaptive potential high, ...'? (Paul Beggs, Macquarie University)	No
E-19-150	A	14	1			Table 19.1. The Criteria for "key" for Health should include aeroallergens. Aeroallergens are discussed in the Human Health chapter (see section 8.2.7) and the chapter on Assessment of Observed Changes and Responses in Natural and Managed Systems (see section 1.3.7.5). (Paul Beggs, Macquarie University)	Not a major factor in Health ch..
E-19-151	A	14	2	14	2	2nd row, 2nd column: Some regional systems are vulnerable to hurricane (or more general severe storm events) activity, these regional systems are not necessarily coastal areas or islands. (Marko Scholze, University of Bristol)	Address in text.
E-19-152	A	14		15		Greenland and WAIS: Replace the last sentence in column 3 with the following: "Because of long time frame, adaptation potential ~~uncertain~~ **is high**, but may require massive relocation of coastal populations ~~and loss of coastal ecosystems~~ **over centuries, if not millennia. Current coastal ecosystems will likely be replaced by new coastal ecosystems at the new coastal margins**." Rationale: The substitute is more accurate. (Indur Goklany, US Department of the Interior)	Adaptive potential of coasts addresssed in draft.
E-19-153	A	14				Water supply: Replace the current entry in column 3 with the following: "Many populations under stress in the absence of climate change, are projected to see a decline in stress, particularly if adaptive measures are undertaken (Arnell 2004, Goklany 2003, 2005a, 2005c). Other populations may see an increase in stress. However, these results typically overestimate negative impacts because they do not fully account for increases in adaptive capacity due to increased economic and technological progress. This is particularly important for the poorest countries because they gain the most from a marginal increase in economic development (Goklany 2005c, 2006a)." . (Indur Goklany, US Department of the Interior)	Adaptive capacity and its relationship to baseline development mentioned often in revised text.
E-19-154	A	14				Table 19.1 MOC line. The relevant WGI chapters are 5 and 10. See WGI Box 5.1 for the assessment of whether the MOC is slowing down already (column 2 of the table), and see WGI Ch 10 ES and Box 10.1 for assessment of future MOC change. I don't think the confidence levels given in column 3 of the table are consistent with the WGI assessments. (Richard Wood, Hadley Centre)	Text revised for consistency with WGI.
E-19-	A	14				T19.1: missing reference to WG1 chapter under MOC/THC	Added in combined table.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
155						(Clair Hanson, IPCC TSU)	
E-19-156	A	14				MOC/THC: We believe the confidence levels are lower than intimated in column 3. (Indur Goklany, US Department of the Interior)	Confidence is judgment of chapter authors consistent with the literature.
E-19-157	A	14				Health: Add the following to the end of column 3: "However, these results typically overestimate negative impacts because they do not fully account for increases in adaptive capacity due to increased economic and technological progress. This is particularly important for the poorest countries because they gain the most from a marginal increase in economic development (Goklany 2005c, 2006a)." . (Indur Goklany, US Department of the Interior)	If the poor gain the most, why are there growing disparities in wealth? Regardless, relationship of baseline to adaptive capacity desecibed often.
E-19-158	A	14				Economic production/welfare: Add at the end of the 3rd column, the following sentence: "However, these results typically overestimate negative impacts because they do not fully account for increases in adaptive capacity due to increased economic and technological progress (Goklany 2005c, 2006a)." (Indur Goklany, US Department of the Interior)	Ditto above.
E-19-159	A	14				Crops and food supplies: Add the following to the end of column 3: "However, these results typically overestimate negative impacts because they do not fully account for increases in adaptive capacity due to increased economic and technological progress. This is particularly important for the poorest countries because they gain the most from a marginal increase in economic development (Goklany 2005c, 2006a)." . (Indur Goklany, US Department of the Interior)	Ditto above.
E-19-160	A	14				Cold stress kills quite a few people. (Richard Tol, Hamburg University)	Not clear if it will be reduced; also doesn't seem to kill large numbers, nor are causes—physiology versus density dependence--as clear as for cold stress as for heat stresses. We follow Chpater 8 conclusions.
E-19-161	A	14				3rd column of the row "water supply", change "marginal" to "Small", because it is not clear if you are using the term "marginal" in the sense used by economists or in the sense used by non-economists. Also, change "vital" to "disruptive" or "harmful". (Danny Harvey, Dept of Geography, University of Toronto)	No, emphasis is on "marginal.'
E-19-162	A	14				"Cessation possible next century (medium confidence)". This sentence is misleading and incorrect. I know no one who argues that a shutdown is impossible, so the correct confidence level is "near certainty". However, only a few mavericks give this a high probability, so the proper wording is "cessation next century (low confidence)" or better still "no cessation (high confidence)" (Richard Tol, Hamburg University)	Rewritten to be consistent with relevant WGI chapters.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-163	A	15				Table 19.1 Methane line. These processes are assessed in WGI Ch 7 and 8, and consistency with those chapters is important. In column 3, I think it is only part of the sea level rise that affects the hydrate stability. Specifically, I don't think the thermo- or halosteric components are important as they don't change the bottom pressure. (Richard Wood, Hadley Centre)	Rewritten to be consistent with relevant WGI chapters.
E-19-164	A	15				Methane stores destabilized: Replace the first sentence in the 3rd column with the following: "Although permafrost is already melting, methane accumulation in the atmosphere has slowed substantially in recent decades (data from Steele et al. 2006." REF: L.P. Steele, P.B. Krummel and R.L. Langenfelds, Atmospheric CH4 Concentrations from the CSIRO GASLAB Flask Sampling Network, available at <http://cdiac.ornl.gov/trends/atm_meth/csiro/csiro_gaslabch4.html>. (Indur Goklany, US Department of the Interior)	WG I
E-19-165	A	15				Biospheric positive feedbacks: This entry is misleading. It should be replaced by one titled "Biospheric feedbacks" because these feedbacks may be negative particularly if global temperature increases are low and CO2 concentrations are higher than today's. We note in passing that the biosphere seems to be absorbing more now than it did a few decades ago, particularly in the northern latitudes. The key vulnerability ought to be couched in terms of when this increase might turn negative and the degree of confidence that might be attached to that date. (Indur Goklany, US Department of the Interior)	WG I
E-19-166	A	16	0			Table 19.2. Water supply; column 2: 'Many regions… Mediterranean and semi-arid climates…' (Michael Manton, Monash University)	Ok
E-19-167	A	16	0			Table 19.2 Water Supply. Snow fed regions risk should be in <2oC range (Andes, Central Asia, South Asia, parts of Europe, New Zealand) are clearly at main risk at around 1.5-2oC above PI. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Have added row for tropical glaciers
E-19-168	A	16	1	20	40	This huge table is of critical importance to this report due to its contents inclusion in SPM-3. Given its central importance, its transparency and traceability deserve improvement. Many of the items, and how they are summarized and justified raise questions. Suggest an overall effort to improve the transparency and justification of this table. Given the lack of line numbers, I will give comments on this table by page number. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Agree, and such improvements widely attempted in FGD.
E-19-169	A	16				Water Supply: The entries should note that many populations under stress in the absence of climate change, are projected to see a decline in stress, particularly if	That's not clear

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						adaptive measures are undertaken (Arnell 2004, Goklany 2003, 2005). Other populations may see an increase in stress. However, these results typically overestimate negative impacts because they do not fully account for increases in adaptive capacity due to increased economic and technological progress. This is particularly important for the poorest countries because they gain the most from a marginal increase in economic development (Goklany 2005c, 2006a). . (Indur Goklany, US Department of the Interior)	
E-19-170	A	16				Table 19.2, 7th row ("Water Supply"), 5th column, insert "for small to modest warming" after "pricing". I am assuming that adaptation is meant to mean being able to completely compensate for the initial impact, and this is clearly not true for large enough warming. (Danny Harvey, Dept of Geography, University of Toronto)	Is about small and modest
E-19-171	A	16				Table 19.2, 3rd row ("Food Supply"), 5th column, insert "for small to modest warming" after "potential". I am assuming that adaptation is meant to mean being able to completely compensate for the initial impact, and this is clearly not true for large enough warming. (Danny Harvey, Dept of Geography, University of Toronto)	New language in new table
E-19-172	A	16				Net market impact are positive in most if not all recent studies. Nordhaus and Boyer is an exception, but they clearly state that they only make the assumptions they do because otherwise they cannot solve their model. (Richard Tol, Hamburg University)	Do not agree--we can't discount Nordhaus out of hand. This is same finding as in TAR
E-19-173	A	16				Caption to Table 19.2: the word "marginal" might be misinterpreted by some as meaning not important. The words "marginal or" can just be deleted. (Danny Harvey, Dept of Geography, University of Toronto)	Have revised
E-19-174	A	16				A. The head note for this table should note that for market and social systems the entries typically overestimate negative impacts because they do not fully account for either current adaptive capacity or, more importantly, increases in adaptive capacity due to increased economic and technological progress that would take place under any of the SRES scenarios. This is particularly important for the poorest countries because they gain the most from a marginal increase in economic development (Goklany 2005c, 2006a). B. The top row of the table should note what is assumed regarding the timing of the temperature increases. For many (if not most) systems impacts depend on both the magnitude and rate of change. This is because adaptive capacity is also a function of time (among other things), although impacts assessments often overlook this point—which is why in such assessments, it makes no difference whether the temperature is increased slowly or rapidly. Such results are suspect.	Ditto

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						C. The entries in the 2nd, 3rd and 4th columns should be based on impact estimates that fully account for any adaptations that would be taken as a mater of course in a business-as-usual world. This includes "spontaneous" or "automatic" adaptations that would likely be taken at the individual, firm, collective, governmental or intergovernmental levels. Alternatively, all the entries in these columns should be tagged as "low confidence" – and we are being charitable. (Indur Goklany, US Department of the Interior)	
E-19-175	A	17	0			Table 19.2 High Mountain communities - <2oC risks include GLOFs, avalanches and rock slides due to melting mountain permafrost (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Have added a row on tropical glaciers
E-19-176	A	17				Indigenous…communities: In the last column insert "currently" prior to "difficult", and add the following new sentence: "adaptive capacity will rise in the future if sustainable development goals are met or if economic and technological development follows paths consistent with any of the SRES scenarios (Goklany 2005c, 2006a)." (Indur Goklany, US Department of the Interior)	SRES scenarios are too aggregate to make this assertion.
E-19-177	A	17				High-Mountain Communities: At the end of the first sentence in the last column, add the following: "unless alleviated by storage projects and/or changes in current water resource management" (Indur Goklany, US Department of the Interior)	A range of adaptive potential is discussed.
E-19-178	A	17				Health: A. We have very low confidence in the entry under the 2nd column. It potentially contradicts the entry under "Food Supply" which notes that there is potential increase in world food supply despite a potential decline in low latitude food production. The world food market is increasingly getting integrated, and because of trade and increases in the wealth of developing nations, it is not evident that malnutrition will necessarily increase. Moreover, as previously noted, increases in adaptive capacity due to advances in economic and technological development are not fully accounted for in most studies. Accordingly, it's unclear that the direction of change will necessarily be negative. B. The entries should also mention cold waves. (Indur Goklany, US Department of the Interior)	Good point about malnutrition, although decrease in low-latitude production can increase risk of malnutrtion. Text revised.
E-19-179	A	18	0	20		The footnote to these tables indicating 'Some Observational Evidence' is valuable here, but iit is a flag which is frequently absent from much oof the text of the chapter. Whether we are talking about model results or observational evidence is of fundamantal importance both to the trust we might place in the 'findings' and in whether we are dealing wiith risks that are known with some degree of accuracy	Good caution. Do through sourcing and have revisited confidence levels

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						upon past observation (which in themselves are uncertain projected into the future) or the inherent uncertainty of model results, which can never be equated with observtaional evidence in terms of reliabiility. We must rely upn model results for many of the phenomena considered here, but we need to be careful about overstating our ability to know the future by failing to qualify adequately the basis for our prospectiive statements of possibilities. This is especially so with statements such as those about species extinction, which rely (in turn) upon emissions scenarios, driving climate models, driving vegetation distibution models, driving species-are models. The statement at p40, lines 7-9 deserves greater prominence (and constant reinforcement) in the text. The statement at Point 4 p41 that computer modelling indcates 'high conficence' should be treated with suspicion, on the other hand. (Aynsley Kellow, University of Tasmania)	
E-19-180	A	18	0			Table 19.2 Ocean Systems. Regional extinction of reefs appears likely for <2oC in the Indian ocean (see Hare, 2006) (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Have revised to be consistent with Ch 5
E-19-181	A	18	0			Table 19.2 Biodiversity. The reference to loss of species is not really correct: it should be to committed loss, which is usually calculated using species area relationship. The loss would most likely occur at some later time due to loss of area etc (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Text revised
E-19-182	A	18	1	18	50	Suggest explain here what is meant by bounded. Explain impacts on mountains e.g. loss cloud forest habitat. Add remark that ecosystem impacts exacerbated by other impacts e.g. drought, fire and this not included in most studies which only look at T and sometimes precip. Explain ecosyst impacts exacerbated by human adaptation to water stress, flood/sea level rise protection schemes, and reduced crop yields or need for land following climate-change-induced migration of populations of requirement for land to grow biofuels. (Rachel Warren, School of Environmental Sciences)	Text revised
E-19-183	A	18	1			Table 19.2: Biodiversity: <2°C: "Loss of up to a quarter os species" - of all species or of species in "bounded ecosystems" in the line above? Terrestrial and marine? Confusing - please rephrase! (Ulf Molau, Göteborg University)	Language is consistent with Ch 5.
E-19-184	A	18				There should be a row devoted to cropland and/or habitat conversion. This is because such conversion of habitat to cropland is currently the most important threat to terrestrial biodiversity, and it is important to look at changes wrought by CC within this context.	Baseline issue, not explicitly our remit except for synergisms.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Indur Goklany, US Department of the Interior)	
E-19-185	A	18				Table 19.2, 6th row ("Forests), 4th column, should read, "Loss of biomass in boreal, temperate, and tropical forests …". The same risks apply to column 3, so in column 3, add "Possible loss of biomass in boreal, temperatue, and tropical forests, amplifying global warming" (Cox et al. (2000) has the terrestrial biosphere becoming a net source by the time global mean warming reaches XXX above pre-industrial) (Danny Harvey, Dept of Geography, University of Toronto)	One study based on one GCM
E-19-186	A	18				Table 19.2, 6th row ("Forests"), 3rd column, insert ", including conversion of the Amazon rainforest to grassland if the mean climate becomes more El Nino-like" after "grasslands". The added statement is based on White et al. (1999) and Cox et al. (2004), which should be cited and briefly discussed in the main text. REFERENCE NOT ALREADY IN LIST: White, A., Melvin, G.R.C., and Friend, A.D. 1999. Climate change impacts on ecosystem and the terrestrial carbon sink: a new assessment. Glob. Env. Change 9, S21-S30. (Danny Harvey, Dept of Geography, University of Toronto)	Similar point; cross-reference
E-19-187	A	18				Table 19.2, 6th row ("Forests"), 3rd column, how can global forest productivity peak in associated with a warming that is causing fire, disease, and the conversion of forests to savannah and grassland? The two parts of the box seem to contradict each other. The second part should be replaced with a statement indicate the range of net source of C that might arise (I vaguely recall some work with emission showing sources due to dieback of 2-5 Gt C/yr y the end of the century). One can be reasonably confident that the terrestrial biosphere will switch from a net sink to a net source at some point (esp in light of Cox et al analysis in the "Avoiding Dangerous Climatic Change" volume) (Danny Harvey, Dept of Geography, University of Toronto)	Text revised
E-19-188	A	18				Table 19.2, 5th row ("Biodiversity"), 3rd column: Based on the analysis of Thomas et al. (2004), who indicate that 1/6 ot 1/3 of species could be committed to extinction by 2050, in association with 2.4-2.6 C globlal mean warming from preindustrial (1.8-2.0 C warming from "present"), much more than 1/3 of species could become extinct with 4 C warming. Thus, insert "or more" after "species" (Danny Harvey, Dept of Geography, University of Toronto)	Text revised
E-19-189	A	18				Table 19.2, 4th row ("Ocean Systems"). The impacts in the 3rd column should be moved to the 2nd column. We have already seen widespread intense bleaching (in 1998 overall, in 2002 in the Great Barrier Reef) and up to 30% loss of coral reefs attributed largely to the warming that has occurred so far (according to Chapter 4), so I see no justification for suggesting that the stated impacts might not occur until	Have checked Ch 5.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						4 C warming. Also, there is no justification for attributing only 75 cm SLR to 2-4 C warming. Rather, this much global mean warming may very well be sufficient to initiate the near total melting of the Greenland Ice Cap and the collapse of thr West Antarctic Ice Sheet, giving a total SLR of around 10 m!. Thus, this part of the table vastly underplays the real risks that we face. Finally, the entry in column 4 should be shifted to column 3. These changes remain valid even if the column heading is changed (as I would prefer) to indicate warmings about pre-industrial levels rather than above 1990, and it is all the more so if the warmings are meant to be above 1990. (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-190	A	18				Ocean Systems: The increase in loss of coastal wetlands due to CC-induced SLR should be compared with losses due to other causes. See, e.g., Arnell et al.(2002), Nicholls (2004), Goklany (2005a, 2005c). (Indur Goklany, US Department of the Interior)	Baseline issue—see responses to earlier.
E-19-191	A	18				Biodiversity: This is entry is based on an overly simplistic view of the threats to biodiversity. In particular, CC will not only affect species directly, it will also indirectly affect biodiversity through changes in: (a) the amount of habitat available or freed up for nature, e.g., by the reduction in cropland that might occur at lower levels of temperature change even as CO2 concentrations increase or (b) biomass production (see, e.g., Levy et al. 2004). In fact, for the next several decades, these indirect avenues may be more critical to biodiversity than the direct CC effects on species. Accordingly, we recommend a substantial rewrite that would address these countervailing factors and how they may be modulated by climate change. Accordingly, we recommend a substantial rewrite of these entries that would address these countervailing factors (see, e.g., Goklany 2005a, 2005c). For the same reasons we cannot agree with the characterization of the level of confidence (Indur Goklany, US Department of the Interior)	None of this is in the ecological literature.
E-19-192	A	19	0			Table 19.2 WAIS. <2oC risk. Am not sure that it is possible to put this so simply. Localized grounding line retreat is being observed and also in some of the EAIS catchments at present. The problem of the WAIS is the risk that this becomes unstable and hence a "local" retreat leads to a disintegration. I would say that there is a risk of this incipient at present and that this risk would grow with warming and be significant already at 1.5-2oC warming (eg substantial risk of meltwater ponding in critical ice shelves in summer and of CDW warming sufficient to melt at the basal zone of the ice shelves). (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Rewritten to be consistent with relevant WGI chapters.
E-19-	A	19	0			Table 19.2 Greenland Ice Sheet. <2oC risks include triggering of widespread or	Rewritten to be consistent with relevant WGI

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
193						complete deglaciation. It is not correct to locate this risk solely in the 2-4oC band eg of Chylek 2006 is correct and the GIS/Global temperature ratio for global warming is ca 2oC, and the local threshold for widespread or complete deglaciation is 3oC warming then this threshold would be reached for ca 1.5oC warming globally. In relation to the time scale of the deglaciation "several centuries" is not consistent with present numerical models but is consistent with the paleorecord. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	chapters.
E-19-194	A	19	1	19	40	The confidence stated for cyclone intensity is in contrast to the current WG1 report (table SPM-1) which gives this a low confidence for a less specific conclusion. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Ok. Also consider new literature.
E-19-195	A	19	1	19	30	Inconsistent entries for WAIS: text in Table 19.2 says complete deglaciation above 4-5 with several centuries for 5m slr whilst Table 19.1 reads 10m slr above 2.5-5C whilst the text on p26 line 14 suggests partial deglaciation as low as 1-2C and suggests above 4C as estimate for complete deglaciation. Suggest partial deglaciation (1-2C) needs to go into Table 19.2. Page 30 lines 27 gives partial degaciation for both ice sheets causing 4-6m slr and this could go in Table 19.2 also. (Rachel Warren, School of Environmental Sciences)	Rewritten to be consistent with relevant WGI chapters.
E-19-196	A	19	1			Table 19.2: Arctic: Replace questionmarks with condensed information from ACIA. (Ulf Molau, Göteborg University)	Ok
E-19-197	A	19	49	19	50	Remark needs rewording - do you mean that change in freq is still speculative if so do you mean whether there has been one or whether there will be one? (cf high confidence of increase in storms). Aside - are we including what is commonly known as "hurricanes" with "cyclones" ... in all oceans here? (Rachel Warren, School of Environmental Sciences)	Text revised
E-19-198	A	19		20		All entries under Extreme Events should note that impacts can be alleviated if adaptation measures are undertaken in a timely and intelligent fashion (Indur Goklany, US Department of the Interior)	Table addresses shift in risks
E-19-199	A	19				Table 19.2, under the heading "Geophysical Systems", insert a row to discuss "Shift of mean climate to a permanent El Nino-like state". This seems to be at least as likely as a complete collapse of the MOC (the probability of which is not very large according to the most recent work), and would have widespread severe impacts (drought in the Amazon and in Indonesia in particular, with implications for tropical rainforests and survival of coral reefs (which were significantly impacted by smoke from Indonesian fires in 1998)).	Does not fit in table format: no information on temperature response.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Danny Harvey, Dept of Geography, University of Toronto)	
E-19-200	A	19				Table 19.2, MOC line. Column 5. The first first sentence as it stands suggests that there is a 3 degree threshold that is consistent across the simplified models. I think that is misleading. I guess you are referring to studies like the recent ones by Schlesinger et al and Challenor et al. in the 'Avoiding Dangerous Climate Change' book. Only some of their ensemble members show a shutdown (and some members show shutdown at lower or higher thresholds). I suggest adding the word 'Some' at the start of the first sentence. I think the fact that no AOGCM shows a shutdown at any (SRES-driven) level of warming by 2100 should also be mentioned here. (Richard Wood, Hadley Centre)	Rewritten to be consistent with relevant WGI chapters.
E-19-201	A	19				Table 19.2, MOC line. Column 3 and 4. I think it is dangerous to assign confidence (which implies probability) to statements about MOC shutdown at various warming levels. E.g. from column 4 I would deduce that at a warming level 2-4 degrees above 1990, MOC shutdown would have a probability of 20% of occurring. I don't think that could be justified as a robust conclusion from the literature, and it is inconsistent with WGI Ch 10. Sadly it is simply not possible to assign robust probabilities to such an event, at the current stage of the science. I think the only solution is not to assign any confidence statements to the shutdown events. I think it is extremely important to maintain consistency across the WG's on matters like this, otherwise the integrity and reputation of the IPCC process will be compromised. (Richard Wood, Hadley Centre)	Rewritten to be consistent with relevant WGI chapters.
E-19-202	A	19				Table 19.2, MOC line. Column 2, confidence may be higher than medium - see WGI Ch 10. (Richard Wood, Hadley Centre)	Rewritten to be consistent with relevant WGI chapters.
E-19-203	A	19				Table 19.2, in the row "Tropical Cyclone Intensity", 5th row, should read: "Change in the frequency of tropical cyclones and in the location and duration of the tropical cycle season still speculative" (Danny Harvey, Dept of Geography, University of Toronto)	Text revised
E-19-204	A	19				Table 19.2, in the row "Arctic" (which is currently empty), you can put "Significant loss of summer sea ice and significant decline or extinction of polar bears and other animals dependent on sea ice" in column 2, and you can put "Complete loss of summer sea ice and extinction of polar bears and other animals dependend on sea ice" in column 3. (Danny Harvey, Dept of Geography, University of Toronto)	Move to region.
E-19-205	A	19				Table 19.2 WAIS and Greenland lines. Suggest reviewing confidence levels for consistency with WGI Ch 10. Where it is not possible to assign robust probabilities to events, I strongly suggest not assigning confidence at all.	Text revised

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Richard Wood, Hadley Centre)	
E-19-206	A	20				There should be row for “cold waves”. (Indur Goklany, US Department of the Interior)	Will address in text.
E-19-207	A	21	1	28	12	Numerous comments on Sections 19.3.2 through 19.3.6 have been provided in the foregoing in conjunction with comments on Tables 19.1 and 19.2, so we won’t repeat them. But we do expect the text to be changed to reflect our comments. (Indur Goklany, US Department of the Interior)	
E-19-208	A	21	9			"Agricultural impacts are probably the largest" I doubt it. It is definitely not so in Tol's work. (Richard Tol, Hamburg University)	Sentence deleted.
E-19-209	A	21	14	21	16	I can see that it does make sense that if there is low confidence in temperate crop yield declines below 2C then there cannot be high confidence in there being increases in yields and that therefore there is low confidence in production increasing up to 3.5C of warming. However, the two sentences are a little confusing - since one might think that low confidence in a decrease means high confidence in an increase - although I know that is not so. Suggest reword to perhaps, "agricultural production may increase up to 2-3.5C above 1990 (low confidence) but it may also decrease (low confidence). In fact yields may decrease in temperate regions below 2C. (I note that one sentence refers to production and the other to yields, which are of course quite different, but many non-agronomists (including policy makers I have talked to) think they are the same. Suggest to add footnote explaining different between yield and production. Is production likely to decrease in temperature regions below 2C or only yields? (Rachel Warren, School of Environmental Sciences)	No literature shows decrease
E-19-210	A	21	14	21	15	Eliminate “is low confidence that”; instead, add “low confidence” in parentheses at the end of that sentence. (Indur Goklany, US Department of the Interior)	Text revised
E-19-211	A	21	15			what does 'approximately above 1990' mean? (Clair Hanson, IPCC TSU)	Not exactly 1990; circa 1990
E-19-212	A	21	17	21	18	delete both occurrences of "marginal" (Danny Harvey, Dept of Geography, University of Toronto)	OK
E-19-213	A	21	22			Add the following references to the end of the line: Goklany (2005c, 2006a). (Indur Goklany, US Department of the Interior)	Not necessary
E-19-214	A	21	28	21	48	Text is too anecdotal and weak on judgements here. Provide short, 2-3 sentence discussion and judgements on all "other sectors". On the basis of Technical Summary, there is some evidence on most of them. If evidence is weak or does not exist, lack of evidence base must be brought up as the judgemen, so that research	Put in appropriate confidence levels

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						efforts can be guided to gap areas in the future. This may be the case with fisheries, for example. (Jouni Paavola, University of Leeds)	
E-19-215	A	21	37			The text should note that losses from Hurricane Katrina are probably more due to lapses in disaster management than climate change per se. In fact it should also note that over the long term, deaths due to hurricanes (and other extreme events) have declined worldwide over the past several decades (Goklany 2000, 2005b, data from EM-DAT, see Goklany 2006b). (Indur Goklany, US Department of the Interior)	Revised.
E-19-216	A	21	38			Why are reinsurers at risk? They sell more policies after each hurricane. (Richard Tol, Hamburg University)	Text revised
E-19-217	A	21	43	21	43	Maybe add "loss of ecological attractions" to causes of changes in tourism. (Janice Lough, Australian Institute of Marine Science)	Too narrow
E-19-218	A	22	3	22	11	Here the reporting of findings is too dense and non-interpretive. More words, an interpretation and a judgement is needed here. Discussion on monetary impacts should be extended. (Jouni Paavola, University of Leeds)	Have revised
E-19-219	A	22	8	22	10	I would include a new meta-analysis published by Tol in Energy Policy 2005. (Alexander Golub, Environmental Defense)	Included
E-19-220	A	22	8			See also Maddison (2003, Resource and Energy Economics, 25, 155-175), Rehdanz and Maddison (2005, Ecological Economics, 25, 111-125). (Richard Tol, Hamburg University)	Included
E-19-221	A	22	11	22	14	Two problems here. First, there is no need to aim for exclusively monetised estimate of aggregate impacts. Non-monetary (estimated loss of life, species etc.) estimates can justifiably presented in parallel with monetary estimates. The argument against monetising these non-monetary impacts is not that it would be normative: monetisation of property damages is equally normative than that of health effects. The argument for not monetising certain impacts is just that they are more transparent to decision-makers than monetised figures. (Jouni Paavola, University of Leeds)	Good point, but section is about market impacts.. Do discuss other aggregations.
E-19-222	A	22	18	22	21	Distribution of impacts discussion is inadequate / too concise. For example, Technical Summary covers them more widely in the context of extreme weather impacts and this should be done here too. Other chapters contain figures on differential impacts of extreme weather events accross countries, and there also seems to be evidence on differential impacts of extreme weather events within countries (Katrina, heatwaves). (Jouni Paavola, University of Leeds)	Disagree

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-223	A	22	37			replace 'a myriad of thresholds' --> 'myriad thresholds' (Clair Hanson, IPCC TSU)	Ok
E-19-224	A	22				19.3.2.4 - very short, can this be expanded? (Clair Hanson, IPCC TSU)	We have to cut, not add.
E-19-225	A	23	1	23	7	The treatment of malarial risks and other risks of mortality is still unsatisfactory. Reiter has consistently shown that the HAZARD posed by temperature induced changes to the distribution of insect vectors for infectious diseases fails to translate into an increased RISK in most situations, not least because numerous factors (escpecially soco-ecoonomic) intervene. (The diistinction between hazard and risk is vital here, and the term 'risk' is frequently used where 'hazard' is the appropriate word). (See Dye and Reiter (2000) in Science 289: 1697-1698, for example). Even in developing countries, the incidence in malaria seems unrelated to climate. Hay et al (2002) ('Climate change and the resurgence of malaria in the East African highlands' Nature 415: 905-909) conclude on observational eveidence that the modlelled associations of TAR are 'overly simplistic'. It would be unfortunate if AR4 were to repeat this error without so much as referring to thiis contrary evidence. (Aynsley Kellow, University of Tasmania)	Consulted with health chapter authors on revisions to health discussion..
E-19-226	A	23	2	23	5	While the two (full) sentences on these lines are accurate in a narrow kind of way, they are, in fact, misleading because they do not provide the context in which the numbers presented can be judged . As noted in (Goklany 2003, 2005a), in the absence of climate change the population at risk (PAR) of hunger is of the order of 300million (see Arnell et al. 2002, which is also based on the same sources as Parry et al (1999). And with respect to malaria, while CC may add hundreds of millions to the PAR for malaria, in the absence of CC, the PAR is in the billions (Goklany 2003, 2005). This context must be provided in the text. (Indur Goklany, US Department of the Interior)	We state that development is important.
E-19-227	A	23	3	23	3	Does this range include the Parry 2004 SRES predictions or only the Parry 1999 ones? Ref. states 1999 only but check what range would be from 2004 work on SRES implications. (Rachel Warren, School of Environmental Sciences)	Will check Parry 2004. Relying on agriculture chapter for numbers
E-19-228	A	23	3			Sen (1981) shows that Parry et al. (1999) is wrong. Hunger is because of lack of access to food, not because of lack of food. (Richard Tol, Hamburg University)	Baseline has been acknowledged.
E-19-229	A	23	4	23	5	This statement has to be supported by a reference. (Marko Scholze, University of Bristol)	Ok
E-19-	A	23	9	23	14	I would include direct impact of extreme weather events on human health, at least	Is included

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
230						heat waves. (Alexander Golub, Environmental Defense)	
E-19-231	A	23	10	23	10	level needs an s (Rachel Warren, School of Environmental Sciences)	Ok
E-19-232	A	23	16			Eliminate the subtitle 19.3.3.1 as unnecessary. (Jouni Paavola, University of Leeds)	Disagree
E-19-233	A	23	19			4AR --> AR4 (Clair Hanson, IPCC TSU)	OK
E-19-234	A	23	35		43	no references (Clair Hanson, IPCC TSU)	Added
E-19-235	A	23	38			combing --> combining (Clair Hanson, IPCC TSU)	OK
E-19-236	A	23	43			Kiribati is indeed vulnerable. Why don't you also quote the global average? (Richard Tol, Hamburg University)	Point is about regional vulnerabilities; point out how they differ. Cite chapter sub-section.
E-19-237	A	23	48		51	no references (Clair Hanson, IPCC TSU)	Added
E-19-238	A	23				There should be a para devoted to the fact that climate change would reduce some vulnerabilities. For example, with respect to water stress, for example, Arnell (2004) analysis indicates that there could be a reduction in the global population under water stress with climate change. This is more likely to be the case, if populations take adaptive actions that are available and affordable (Goklany 2005a, 2005c, 2006a). Similarly, mortality due to cold and cold waves should decline. (Indur Goklany, US Department of the Interior)	We state that some literature shows reduction in vulnerability
E-19-239	A	24	2	24	46	This subsection is based on an overly simplistic view of the threats to biodiversity. In particular, CC will not only affect species directly, it will also indirectly affect biodiversity through changes in: (a) the amount of habitat available or freed up for nature, e.g., by the reduction in cropland that might occur at lower levels of temperature change even as CO2 concentrations increase or (b) biomass production (see, e.g., Levy et al. 2004). In fact, for the next several decades, these indirect avenues may be more critical to biodiversity than the direct CC effects on species. Accordingly, we recommend a substantial rewrite that would address these countervailing factors and how they may be modulated by climate change (see, e.g., Goklany 2005a, 2006a). (Indur Goklany, US Department of the Interior)	Text revised to be consistent with chap 4.
E-19-240	A	24	4	24	11	Good section where the combined effects of climate change and other components of antropogenic global change are highlighted. (Ulf Molau, Göteborg University)	Thanks

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-241	A	24	4		11	no references (Clair Hanson, IPCC TSU)	Added
E-19-242	A	24	5		6	reword 'The loss of diversity … includes … reduction in the diversity' (Clair Hanson, IPCC TSU)	OK
E-19-243	A	24	10	24	11	Maybe change "stresses of pollution" to "stresses such as pollution and overexploitation". (Janice Lough, Australian Institute of Marine Science)	OK
E-19-244	A	24	15	24	15	"diversity of polar and tundra ecosystems" - this phrase I interpret as ecosystem diversity rather than species richness, in which case this is absolutely true. Some rephrasing would help in clarity. (Ulf Molau, Göteborg University)	Paragraph is about species AND ecosystems
E-19-245	A	24	18			after 'scientists' insert 'had previously' and delete 'would happen' (Clair Hanson, IPCC TSU)	No longer in draft
E-19-246	A	24	20			Strike “adverse” because not all impacts are adverse at all levels of climate change. (Indur Goklany, US Department of the Interior)	Net impacts are projected to be adverse. Point is more adverse impacts happen at higher magnitudes of climate change
E-19-247	A	24	28		29	no references (Clair Hanson, IPCC TSU)	Have been added
E-19-248	A	24	30	24	30	This is another example where there is a crtical need to distinguish between 'bleaching' and 'bleaching-induced mortality'. I suggest this be reworded for accuracy and meaningfulness to: "….could result in significant deterioration in condition of four-fifths of coral reefs due to bleaching...." (Paul Marshall, Great Barrier Reef Marine Park Authority)	Have revised.
E-19-249	A	24	30			give the specific reference indicating bleaching of 4/5 of corals with 1 C warming above 1990, and for 10% of global ecosystems losing area. The latter seems to be from Leemans and Eickhout (2004), who say that 1 K warming causes the type of ecosystem to change over 10% of the land area, which is not quite the same as what you say. They also state that only 36% of impacted (I assume) forests can shift in step with a warming of 1 K over 100 years, that is, "adapt" to the change. With regard to bleaching, the critical issue is how frequently the bleaching occurs, and how frequently severe bleaching occurs. Please add some information pertaining to this. Donner et al. (2005) is helpful in this respect: using the HadCM3 andf PCM climate change scenarios in a global assessment, they find that severe bleaching occurs at the majority of the world’s coral reefs in the 2030s and becomes a biannual event by the 2050s, by which time global mean temperature has increased by about 2 K above that of the 1990s. REFERENCE: Donner, S.D., Skirving, W.J., Little, C.M., Oppenheimer, M., Hoegh-Guldberg, O.: 2005, ‘Global assessment of	Language is based on language from Ch 4.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						coral bleaching and required rates of adaptation under climate change', Global Change Biology 11, 2251-2265. (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-250	A	24	31			The statement that 10% of global ecosystems will be lost is essentially meaningless. Believe it or not, for the most part changes in the global area of ecosystems is (virtually) a zero sum game because habitat is converted rather lost. (Think of it as the law of conservation of area. A 10% loss of forest ecosystems means that that area goes into a new, possibly different ecosystem, whether it is grassland or desert or whatever. Even where area is apparently lost, e.g. if coastal wetlands are submerged, it's not clear that it is a total loss.) We recommend dropping this statement about 10% loss in global ecosystem area, and providing estimates in terms of net change in productivity. (Indur Goklany, US Department of the Interior)	Disagree. Ecosystems refers to different assemblages of species. There can be few or many ecosystems in the same space. There is no law of conservation of ecosystems. Consistent with Chapter 4 conclusions.
E-19-251	A	24	33	24	33	This is another example where there is a crtical need to distinguish between 'bleaching' and 'bleaching-induced mortality'. I suggest this be reworded for accuracy and meaningfulness to: "….is estimated to result in serious degradation to 97% of coral reefs due to bleaching...." (Paul Marshall, Great Barrier Reef Marine Park Authority)	Have followed language in Ch 4.
E-19-252	A	24	33			give the specific reference indicating bleaching of 97% of corals with 2 C warming above 1990 (Danny Harvey, Dept of Geography, University of Toronto)	From Ch 4.
E-19-253	A	24	34	24	35	the statement about species extinction seems to be from Thomas et al. (2004). However, they indicate 1/6 extinction (with dispersal) to 1/3 extinction (without dispersal) for 1.8-2.0 K warming, and it would be better to give this range rather than just one number. Their warmings are with respect to the 1961-1990 mean according to an email from Thomas, which in turn is 0.35 K warmer that the 1856-1910 mean in the CRU dataset, so the 1/6 to 1/.3 extinction is for 1.6-1.8 K warmer than 1990. Malcom et al. (2006) gets similar extinction rates averaged over 25 biodiversity hot spots, and their work should be cited here too. REFERENCE: Malcolm, J.R., Liu, C., Neilson, R.P., Hansen, L. and Hannah, L.: 2006, 'Global warming and extinctions of endemic species from biodiversity hotspots', Conservation Biology 20, 538-548. (Danny Harvey, Dept of Geography, University of Toronto)	Have added Malcolm et al.
E-19-254	A	24	34			For the same reasons as outlined above, strike "one-sixth of global ecosystems losing area". (Indur Goklany, US Department of the Interior)	Language consistent with Ch 4
E-19-	A	24	38			For the same reasons as outlined above, strike "one-fifth of global ecosystems	Ditto

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
255						losing area". (Indur Goklany, US Department of the Interior)	
E-19-256	A	24	39			the statement about species extinction is optimistic with regard to risks, as Thomas et al. (2004) indicate up to 52% species extinction for 2.0-2.6 K warming above 1961-1990 (the stated warming comes from the Supplemental Information associated with their paper but without indicating the reference period, so I emailed Thomas to ask him). (Danny Harvey, Dept of Geography, University of Toronto)	Disagree; language is consistent with Ch 4.
E-19-257	A	24	42			An additional bullet should be added here, pertaining to possible complete loss of the Amazon rainforest in associated with about a 3 K global mean warming (relative to pre-industria, achieved by the 2080s) if the mean climate becomes El Nino-like. You can cite White et al. (1999) and Cox et al. (2004). (Danny Harvey, Dept of Geography, University of Toronto)	But, based one GCM. Most models show wettter Amazon
E-19-258	A	24	45			replace 'so' with 'as a result (Clair Hanson, IPCC TSU)	Revised
E-19-259	A	24	49			The whole of section 19.3.5 is about physical changes in the climate system, and I strongly suggest that it is reviewed for consistency with WGI (esprecially WGI Ch 10) (Richard Wood, Hadley Centre)	Done.
E-19-260	A	25	1	25	2	Is the threshold for deglaciation of GIS for partial of complete deglaciation? If partial then this is consistent with entries in Tables 19.1 and 19.2 (Rachel Warren, School of Environmental Sciences)	Point clarified
E-19-261	A	25	17			chapter ref (Clair Hanson, IPCC TSU)	Done.
E-19-262	A	25	25			the missing reference is: Harvey, L.D.D. and Haung, Z. 1995. Evaluation of the potential impact of methane clathrate destabilization on future global warming. Journal of Geophysical Research 100 (D2), 2905-2926.. (Danny Harvey, Dept of Geography, University of Toronto)	Thanks, corrected.
E-19-263	A	25	29	25	32	The following is also relevant: Kleypas JA, RA Feeloy, VJ Fabry, C Langdon, CL Sabine & LL Robbins, 2006. Impacts of Ocean Acidification on Coral Reefs and Other Marine Calcifiers: A Guide for Future Research, report of a workshop held 18-20 April 2005, St Petersburg, FL, sponsored by NSF, NOAA and the US Geological Survay, 88 pp. (http://www.isse.ucar.edu/florida/) (Janice Lough, Australian Institute of Marine Science)	We decline to include this because most of our basic science referencing is through WGI, and this is not an easy-to-get reference.
E-19-264	A	25	32			Turley et al is listed as 2005 in the reference list (Danny Harvey, Dept of Geography, University of Toronto)	Citation fixed and reference added to list.
E-19-	A	25	39			have --> has	Done

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
265						(Clair Hanson, IPCC TSU)	
E-19-266	A	25	46	25	47	Replace "would depend crucially on the rate of deglaciation" with the following: "to such changes is potentially very high since estimates of deglaciation suggest this could take a very long time (centuries to millennia; page 19 of this chapter)". (Indur Goklany, US Department of the Interior)	We are making a more general point, namely, that the ability to adapt is low for short timescales mentioned in the next snetence, higher for longer timescales, and hgihest for millennial scales. This is clear if the two sentences are read, so we leave as is and decline the suggestion. Text ahs been revised for consistency with WG 1 in any case.
E-19-267	A	25	49		50	reword last sentence (Clair Hanson, IPCC TSU)	Suggestion accepted.
E-19-268	A	26	1	26	2	I would not feel particularly bound by what WG1 says in this case, as the WG1 assessment of the vulnerability of the GIS to warming is based on models that are openly admitted to lack processes that would likely make them more sensitive to warming if they were included. You acknowledge this later, so I would insert "using current models" after "Greenland". (Danny Harvey, Dept of Geography, University of Toronto)	Suggestion accepted.
E-19-269	A	26	1		2	replace +/- with ± (Clair Hanson, IPCC TSU)	Done
E-19-270	A	26	7			the cited paper is missing from the reference list (Danny Harvey, Dept of Geography, University of Toronto)	Done
E-19-271	A	26	9	26	9	No Payne and Vieli 2005 in biblio. Also I suspect you mean V&P JGRF 2005 (Richard Hindmarsh, British Antarctic Survey)	Accepted.
E-19-272	A	26	9	26	12	Eliminate the sentence that begins with "Based on output of one AOGCM…" since it is trumped by the following sentence. At the same time change "wider" to "wide". (Indur Goklany, US Department of the Interior)	Aceceted; section updated to be consistent with WGI language.
E-19-273	A	26	9			the cited paper is missing from the reference list (Danny Harvey, Dept of Geography, University of Toronto)	Citation corrected.
E-19-274	A	26	12			insert "that" after "indicates" (Danny Harvey, Dept of Geography, University of Toronto)	Section eliminated.
E-19-275	A	26	13		14	chapter ref (Clair Hanson, IPCC TSU)	Referenece added.
E-19-276	A	26	16			Ott-Bliesner indicate a threshold of 3 C regional summer warming, but do not directly translate this into a global mean annual warming threshold. This obviously depends on the seasonal and spatial pattern of temperature change under of GHG increase scenario, which differs from model to model.	Yes, but the conversion from global to regional and seasonal is discussed and described in the other references given at the same point in the text. Therefore we do not

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Danny Harvey, Dept of Geography, University of Toronto)	think any change is needed.
E-19-277	A	26	19		39	You overlooked Link and Tol (2004, Portuguese Economic Journal, 3, 99-114). Might that be because this study shows positive effects of an MOC shutdown? (Richard Tol, Hamburg University)	The main motivation was to be brief. We added additional citations (including Link and Tol, 2004)..
E-19-278	A	26	19			Since the impacts of MOC shutdown would be felt well beyond Europe, I think it would be helpful to the reader to include material on these impacts (some of which is currently in Ch 12) within this section 19.3.5.3. (Richard Wood, Hadley Centre)	This is addressed.
E-19-279	A	26	21	26	39	No estimate of a threshold is given here - should add estimates given elsewhere e.g. 3C table 19.2 remarks and elsewhere in text (presumably low or very low confidence) for consistency with rest of chapter. (Rachel Warren, School of Environmental Sciences)	The discussion and references have been revised . We also refer to the detailed discussion of this issue in WG1 Chaper 10.
E-19-280	A	26	21	26	22	I think the place to find the discussion of the relationship of MOC and THC is in the WGI Glossary (Richard Wood, Hadley Centre)	The text now refers to the Glossary. Thanks for pointing this out.
E-19-281	A	26	26			Insert ", less possibly in the foreseeable future," prior to "absolute cooling" (Indur Goklany, US Department of the Interior)	The section has been extensively revised and links back repeately to the relevant sections of WG1 (e.g. Chapter 10) where the predictions are discussed in great detail. In addition, the sentence in question is clearly flagged with "potential".
E-19-282	A	26	27			insert "greater" before "warming" (Danny Harvey, Dept of Geography, University of Toronto)	Included.
E-19-283	A	26	28	26	28	For a more complete reference for the experiment where we hosed the MOC in 2049 (showing many more fields) you might want to replace the Wood et al. 2003 reference with [Vellinga, M. and R.A. Wood, 2006: Impacts of thermohaline circulation shutdown in the twenty-first century. Climatic Change (in press)]. Another reference you may want to add here is [Jacob D., H. Goettel, J. Jungclaus, M. Muskulus, R. Podzun, J. Marotzke (2005), Slowdown of the thermohaline circulation causes enhanced maritime climate influence and snow cover over Europe, Geophys. Res. Lett., 32, L21711, doi:10.1029/2005GL023286.] This is the only study to date that uses a regional model to provide more detailed discussion of the impacts of a major MOC reduction on Europe. (Richard Wood, Hadley Centre)	We updated the references.
E-19-284	A	26	31	26	31	Suggest referring to WGI Chapter 8 here (section 8.7) for discussion of MOC thresholds. (Richard Wood, Hadley Centre)	We added this reference.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-285	A	26	37	26	38	Again suggest checking WGI Ch 10 for consistency of confidence levels. I would say that 'moderate confidence' in predictions of MOC slowdown during the 21st Century means that there's about a 50% chance of a slowdown (is 'moderate' the same as 'medium'?). From WGI Ch 10 I would have much higher confidence than that - personally I would be comfortable with 'high confidence' of a slowdown. The second part of the sentence (about the scale of climate change leading to a shutdown) looks consistent with WGI Ch 10 to me. (Richard Wood, Hadley Centre)	We now use "high confidence", consitent with the WG1 Chapter 10 statement that "it is likely that the MOC will reduce".
E-19-286	A	26	39			insert "and/or abrupt" before "shutdown" (Danny Harvey, Dept of Geography, University of Toronto)	The sentence has been changed to be more consistent with the language adopted by WG1 Chapter 10.
E-19-287	A	26	39			Append the following to the end of this sentence: "and even less confidence in the impacts such climate change" (Indur Goklany, US Department of the Interior)	We have added a link to WGII, section 12.6.2, where these impacts are discussed in more detail.
E-19-288	A	26	41	27	21	Should Pacific Decdal Oscillation (PDO/IPO) be mentioned here as it significantly modulates ENSO teleconnections & has been associated with "regime shifts" in Pacific ecosystems? (Janice Lough, Australian Institute of Marine Science)	PDO changes are discussed at several locations in WG1 (e.g., Techical summary, as well as chapters 8 and 10). We refer to these chapters.
E-19-289	A	26	45	26	47	WGI Ch 9 considers possible recent changes in ENSO (see WGI SOD Ch 9 section 9.5.3.1) and does not conclude that there has been a clear anthropogenic influence on ENSO. There is also little consensus on likely future changes in ENSO (see WGI CH 10 Executive Sumary). Also, it is important to make the distinction here between an 'El nino-like' mean climate response (i.e. the East Pacific warms more than the west in the mean) and any changes in the variability. Overall, I think this paragraph needs to be revised for consistency with WGI Ch 9 and 10. (Richard Wood, Hadley Centre)	This section has been revised to be consistent with WG1, chapters 9 and 10. The question whether there has been a detectable anthropogenic change in ENSO properties is a debatable point and some studies arrive at alternative conclusions than Timmerman (1999). We also refer to section 9.5.3.
E-19-290	A	26	45		46	chapter ref (Clair Hanson, IPCC TSU)	This reference been updated and moved
E-19-291	A	27	17			chapter ref (Clair Hanson, IPCC TSU)	The section has been rewritten and the references to the relevant FAR chapters have been updated.
E-19-292	A	27	22			A brief section discussing the possibility of an El Nino-like future mean climate should be added, which is distinct from the issue of changes in the amplitude of ENSO variability. I think that this is a risk at least as large as that of a complete shutdown of the thermohaline overturning. (Danny Harvey, Dept of Geography, University of Toronto)	The section discusses modes of variability. A shift in the mean would not fit this description.
E-19-	A	27	26		46	no references	cross-references now included

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
293						(Clair Hanson, IPCC TSU)	
E-19-294	A	27	26			chapter ref (Clair Hanson, IPCC TSU)	cross-references now included
E-19-295	A	27	30	27	31	"days and nights" - not just "days" (Janice Lough, Australian Institute of Marine Science)	Accepted
E-19-296	A	27	37			chapter ref and give figure number (Clair Hanson, IPCC TSU)	cross-references now in
E-19-297	A	27	48	27	51	The statement about cyclones correctly refers to model simulations, but then inappropriately refers to 'predictions' form them. It is additionally understood that there is some controversy over cyclone patterns (intensity and frequency) and that the WMO recently made a consensus statement that fell short of endorsing totally the conclusions of Emanuel and Webster et. It would be preferable for the text to reflect this disagreement. (Aynsley Kellow, University of Tasmania)	Statements have been made consistent with WGI. We have used "some data reanalyses" which implies not all. Prediction changed to projection.
E-19-298	A	27	50	27	51	Unclear sentence - I think you mean that cyclones have increased more rapidly than models would have predicted? (Rachel Warren, School of Environmental Sciences)	This is now expanded to reflect WGI text.
E-19-299	A	27	50			change "have increased far more rapidly" with "have already increased" (Danny Harvey, Dept of Geography, University of Toronto)	Has been reworded and is accurate.
E-19-300	A	28	0	29	0	I would not only update reasons for concern, but also change the order: like (4) and (2) I would put in front. (Alexander Golub, Environmental Defense)	We follow order from TAR. Seems logical.
E-19-301	A	28	5	28	5	I would include on-peak load of power generation systems that become increasingly vulnerable during extreme weather events. (Alexander Golub, Environmental Defense)	Narrow point
E-19-302	A	28	6	28	12	Mention that disaster preparedness is also an excellent adaptation strategy. This strategy is already pursued in a climate context, by, e.g. The Red Cross/Red Crescent Centre on Climate Change and Disaster Preparedness (e.g. Van Aalst, M.K. and M. Helmer (2003) Preparedness for Climate Change. A study to assess the future impact of climatic changes upon the frequency and severity of disasters and the implications for humanitarian response and preparedness. Red Cross/Red Crescent Centre on Climate Change and Disaster Preparedness, The Hague, The Netherlands (available at www.climatecentre.org; also included as a reference in chapter 17) (Maarten van Aalst, Red Cross/Red Crescent Centre on Climate Change and Disaster Preparedness)	Added.
E-19-	A	28	6	28	12	Discussion here ignores coastal conservation and restoration in the protection	Added.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
303						against extreme weather events, despite the fact that there is evidence that e.g. coastal wetlands and mangroves have this function. The evidence is relevant both for developed and developing countries. See e.g. Adger et al (2005) Socio-ecological recilience to coastal disasters. Science 309: 1036-39. (Jouni Paavola, University of Leeds)	
E-19-304	A	28	6			change "which" to "that" (Danny Harvey, Dept of Geography, University of Toronto)	Changed.
E-19-305	A	28	6		7	BK & W 1993 --> Burton et al., 1993 same for WKB 2001 and the reference Burton 2005-6 should it be 2005 or 2006? (Clair Hanson, IPCC TSU)	Revised.
E-19-306	A	28	19			close brackets (Clair Hanson, IPCC TSU)	Ok
E-19-307	A	28	22	28	34	Add that the 2-4 range has very radical consequences for ecosystems - far worse than <2 which is already very significant. The estimates of 25% species lost and 50% unable to adapt up to 2C and 33% for 2-4C with 66% ecosystems unable to adapt would be useful here unless you intend to keep the pgph qualitative in which case state this qualitatively instead. (Rachel Warren, School of Environmental Sciences)	Ok
E-19-308	A	28	29			Eliminate “adversely”. Indeed effects have been shown to have occurred but whether these effects are in fact adverse for most cases has not yet been demonstrated robustly. We note that the notion that “change” is congruent to “adverse effect” is not acceptable without proof. (Indur Goklany, US Department of the Interior)	See response above
E-19-309	A	28	32	28	33	change "up to 2 C" with "2 C or less" (Danny Harvey, Dept of Geography, University of Toronto)	preferential language
E-19-310	A	28	34			add references to Thomas et al. (2004) and Malcolm et al. (2006) here. At the end of the existing sentence, add: "Indeed, we are already seeing adverse effects on some unique and vulnerable ecosystems". REFERENCE: Malcolm, J.R., Liu, C., Neilson, R.P., Hansen, L. and Hannah, L.: 2006, ‘Global warming and extinctions of endemic species from biodiversity hotspots’, Conservation Biology 20, 538-548. (Danny Harvey, Dept of Geography, University of Toronto)	OK
E-19-311	A	28	36		37	complete the sentence (Clair Hanson, IPCC TSU)	Ok
E-19-312	A	28	39	28	40	Replace this sentence with two sentences addressing mortality and property losses separately. The first sentence should read as follows: “Despite the recent spate of deadly extreme weather events such as the 2003 European heat wave and the hurricanes of 2004 and 2005, aggregate mortality and mortality rates due to	Comment is not relevant—standard baseline issue.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						extreme weather events are generally lower today than they used to be. Globally, mortality and mortality rates have declined by 95 percent or more since the 1920s. Cumulatively, the declines from events such as droughts, windstorms and floods more than compensated for increases due to the 2003 heat wave (Goklany 2006b, 2005b). The second sentence, addressing property losses should read as follows: "In terms of real dollars, property losses from extreme weather events have increased, however, this upward trend vanishes if property losses are normalized in terms of the property at risk (see, eg., Pielke, Jr., R.A., and Landsea, C.W (1998), Normalized Hurricane Damages in the United States: 1925–95. Weather and Forecasting, 13, 621–631); Goklany (2000); Mary W. Downton, J. Zoe Barnard Miller, and Roger A. Pielke Jr. (2005), Reanalysis of U.S. National Weather Service Flood Loss Database. Natural Hazards Review. February 2005: 13-22.) (Indur Goklany, US Department of the Interior)	
E-19-313	A	28	42			Replace "indicates" with "suggests" and "has" with "may have". These issues have not been fully resolved. (Indur Goklany, US Department of the Interior)	Diasagree
E-19-314	A	28	43			change "an" to "and" (Danny Harvey, Dept of Geography, University of Toronto)	Ok
E-19-315	A	28	43			an --> and (Clair Hanson, IPCC TSU)	Ok
E-19-316	A	28	47			Change the "medium confidenecee" to "low confidence." Rationale: Most impact studies do not fully account for adaptive capacity. More importantly, such capacity, while low today in many developing countries, is likely to be enhanced in the future if economic and technological development increase consistent with SRES scenarios (Goklany 2006b). Accordingly, very little confidence can be assigned to the results of currently available analyses of the future impacts of CC. (Indur Goklany, US Department of the Interior)	STRONGLY DISAGREE. This has been a robust finding in the literature. What studies show equitable distribution of impacts? Literature carefullly assessed and summarized.
E-19-317	A	28	50			Change the "high confidence" to "low confidence". Rationale: Most impact studies do not fully account for adaptive capacity. More importantly, such capacity, while low today in many developing countries, is likely to be enhanced in the future if economic and technological development increase consistent with SRES scenarios (Goklany 2006a, 2005c). Accordingly, very little confidence can be assigned to the results of currently available analyses of the future impacts of CC. (Indur Goklany, US Department of the Interior)	Literature carefullly assessed and summarized
E-19-318	A	28	51			chapter ref (Clair Hanson, IPCC TSU)	Ok

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-319	A	29	21			Watson and the Core Writing Team did not come up with the reasons for concern. Smith et al. did. (Richard Tol, Hamburg University)	Topic properly cicted.
E-19-320	A	29	45	30	33	The text should also note that most impact studies still do not fully account for adaptive capacity or, perhaps more importantly, increases in such capacity which is likely to be enhanced in the future if economic and technological development increase consistent with SRES scenarios (Goklany 2006a). Thus, it's not obvious that currently available impact assessments do not, in fact, overestimate the costs of climate change despite the other factors mentioned in this paragraph. Accordingly, we recommend striking the last two sentences of this para, i.e., everything commencing with "On balance…" (Indur Goklany, US Department of the Interior)	And they don't account for extreme events either—we are careful to assess wide range of points of view on this.
E-19-321	A	29	45			Change "a preliminary" to "an inadequate". (Indur Goklany, US Department of the Interior)	In many cases, efficient adaptation is assumed
E-19-322	A	29	49			"neglected aspects" again, selective quotation, while CRM Ltd is probably not peer-reviewed; for example, Hamilton et al.(2005, Climate Research, 29, 255-268) show that including tourism does not matter much (Richard Tol, Hamburg University)	Will look at Hamilton etal.
E-19-323	A	29	50			Roson and Tol (2006, Integration Assessment Journal, 6, 75-82) argue that Kemfert's model is biased; using a different methodology, Fankhauser and Tol (2005, Resource and Energy Economics, 27, 1-17) reach the opposite conclusion. (Richard Tol, Hamburg University)	OK, but point is still valid.
E-19-324	A	29				F19.1 figure caption is incomplete (Clair Hanson, IPCC TSU)	Figure deleted
E-19-325	A	30	4			There is no reason to believe in a positive bias in the cost estimates. (Richard Tol, Hamburg University)	Respectfully disagree based on full range of literature
E-19-326	A	30	4			"lower confidence" I already argued against this in the summary. You ignore that the Tol (2002) and Nordhaus (2006) study confirm sign and size of the previous estimates. You similarly ignore the Maddison and Rehdanz studies that further confirm the message. (Richard Tol, Hamburg University)	Citation to chapters for which such studies are to be assessed in details are given in revised text.
E-19-327	A	30	10	30	13	The sentence beginning with "Parry et al (1999)…" fails to provide a context against which climate change caused increases in the population at risk of coastal flooding, water stress, malnutrition and disease should be viewed. The studies on which Parry et al (1999) are based show that, in fact, many millions more at risk of these hazards in the absence of climate change (coastal flooding is an exception). In other words, by and large, the impact of climate change on the populations at risk	Irrelevant

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						of these climate sensitive hazards is relatively small compared to non-climate change related factors. See Goklany (2003,2005a). This context needs to be provided in this chapter. (Indur Goklany, US Department of the Interior)	
E-19-328	A	30	13	30	15	The McMichael et al (2004) study is scientifically suspect. In Chapter 20 of "Comparative Quantification of Health Risks", McMichael et al. themselves note on page 1547 that "Empirical observation of the health consequences of long-term climate change, followed by formulation, testing and then modification of hypotheses would therefore require long timeseries (probably several decades) of careful monitoring. While this process may accord with the canons of empirical science, it would not provide the timely information needed to inform current policy decisions on GHG emission abatement, so as to offset possible health consequences in the future. Nor would it allow early implementation of policies for adaptation to climate changes, which are inevitable …" So it seems these results are based on a scientific short cut. It would also be useful place the burden of disease ascribed to CC in context of the total global burden of disease. For 2000, it works out to about 0.4%. . (Indur Goklany, US Department of the Interior)	Revised
E-19-329	A	30	19	30	20	The treshold for MOC shutdown quoted in WGII TAR was never supported by WGI. I suspect it is the same for some of the other thresholds. This was an inconsistency between WGi and WGII and I think it is important to correct it this time. Where there is no justification for assigning probabilities to events, I think it is important that probabilities (implicit in the 'confidence' statements) are not assigned at all. See my earlier comment on Table 19.2, MOC line, columns 3 and 4. (Richard Wood, Hadley Centre)	Can only speak for the AR4 and we have tried to be consistent with WG I
E-19-330	A	30	28	30	30	Statement on WAIS deglaciation. I found the language really confusing here. Does 'medium confidence' mean there is a 5/10 chance that for a warming greater than 2 to 4-5 degrees (which of those three numbers?) a WAIS collapse is possible? (and hence a 5/10 chance that it is impossible). Or does it mean a 5/10 chance that it will actually happen? I think the sentence needs rewriting for clarity - and after that I will return to my mantra that any such statements need to be consistent with the assessment in WGI! (Richard Wood, Hadley Centre)	Rewritten to be consistent with relevant WGI chapters.
E-19-331	A	30	29			replace 2 to 4-5 --> 2-5 (Clair Hanson, IPCC TSU)	Revised
E-19-332	A	30	31	30	33	I agree that the literature on MOC slowdown is broadly consistent with the TAR. But I disagree that it is not reporting high confidence conclusions. See WGI Ch 10	Rewritten to be consistent with relevant WGI chapters.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						Exec Summary - I would argue from what they say that there is high confidence that the MOC will slow down (although WGI Ch 10 does not frame it in those terms), and very high confidence that it will not shut down in the 21st Century. (Richard Wood, Hadley Centre)	
E-19-333	A	30	33			from 'but …conclusions' please reword (Clair Hanson, IPCC TSU)	Revised
E-19-334	A	30	48			Change "relative" to "perceived". Because the study of adaptation to CC is in its infancy, we cannot say whether adaptations are truly lacking or only perceived to be lacking. Equally importantly, many view available adaptation options in terms of today's conditions. We need to examine what adaptations might become available in the future as societies get wealthier and technologically more sophisticated, but few studies take increases in adaptive capacity into consideration. A similar change should be made on p. 41, line 8. (Indur Goklany, US Department of the Interior)	We disagree. The proposed change implies nothing at all is known about adaptive potential, which is not true. As is stated here, relative lack of feasible adaptations is an important criterion in our assessment of key vulnerabilities. We now explicitly identify key vulnerabilities for which low adaptive capacity was a key factor in their selection in Table 19.1. This is literature based.
E-19-335	A	30	49			remove 'this' (Clair Hanson, IPCC TSU)	ok
E-19-336	A	31	23	31	28	There is an unjustified pejorative tone in this paragraph: 'claims on the optimistiic side' (about adaptation are contrasted with 'prospects' which seem much worse - supported only by the the questionable reference to Pittock 2005 (see above). There is considerable observational evidence that almost every hazard considered here is moderated by socio-techhnical adaptation - that wealth and technology in the face of hazard translate into lower risks. The pejorative tone here is totally unjustified and the statement that the literature is relatively small is inaccurate. The disaster literature indicates quite clearly much greater mortality from everthing from cyclones to infectious diseaes in locations which lack the economic resources or technological capacity to mitigate hazards. (Aynsley Kellow, University of Tasmania)	Pejorative text deleted. Other points accpeted.
E-19-337	A	31	24	31	24	The reference to Pittock 2006 is not included in the reference list (Jim Hall, University of Newcastle upon Tyne)	Reference deleted.
E-19-338	A	31	24			Add Goklany (2006a) to the list of references. (Indur Goklany, US Department of the Interior)	Not needed.
E-19-339	A	31	34			incomplete references (Clair Hanson, IPCC TSU)	addressed
E-19-340	A	31	40			Strike "On the other hand…" What's "on the other hand" about that sentence? (Indur Goklany, US Department of the Interior)	We disagree.
E-19-	A	31	45	32	7	The statement concerning cyclone losses and adapatation is not supported by the	We agree with the gist of these comments and

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
341						literature. Roger Pielke Jr has shown that damage is not related to the merits of adaptation by by factors such as poor oastal planning. It is not that adaptation does not worlk, but rather that it has not been applied. Indeed, the mortaility trend for hurricanes in the US has been downwards for the past century, and the lesson from Hurricane Katrina is not that adaptation was insufficient, but that a city built below sea level is vulnerable to a direct hit from a mere Category 3 Hurricane. The 'lack of adaptation' in New Orleans occurred in the historical past. (Aynsley Kellow, University of Tasmania)	have ammended the text accordingly. It is what we intended to say but perhaps did not communicate in clearly enough.
E-19-342	A	31	46	31	47	These two sentences are only valid for **economic losses**. They do not apply to deaths due to extreme events. Accordingly, on line 47, insert "economic" prior to "losses", and insert a new sentence noting that despite the recent spate of deadly extreme weather events such as the 2003 European heat wave and the hurricanes of 2004 and 2005, aggregate mortality and mortality rates due to extreme weather events are generally lower today than they used to be. Globally, mortality and mortality rates have declined by 95 percent or more since the 1920s. Cumulatively, the declines from events such as droughts, windstorms and floods more than compensated for increases due to the 2003 heat wave (Goklany 2006b, 2005b). (Indur Goklany, US Department of the Interior)	Have included the reference to economic losses. No detailed response = on baseline issue isappropriate,as noted many times already. Long term trend of declining mortality is added to the text.
E-19-343	A	32	3	32	4	It should be noted that European authorities, despite greater acceptance of climate change as a significant risk, failed to bring their considerable adaptive capacity to bear during the 2003 heat wave, perhaps because they underestimated the usefulness of adaptation as an effective response strategy or viewed adaptation and mitigation as mutually exclusive approaches. {Goklany, DOI] (Indur Goklany, US Department of the Interior)	This is implied in the text and does not need further elaboration
E-19-344	A	32	11	32	11	It appears, for example, that much coastal urban infrastructure can be adaptively protected against sea level rise and storm increases (London Climate Change Partnership, 2002), and upland water systems can be adapted to temperature and precipitation changes in part through system interconnections and changes in operating regimes (Rosenzweig and Solecki, 2001). References: London Climate Change Partnership, 2002, London's Warming: The impacts of climate change on London, Technical Report, November 2002. Rosenzweig, C., & Solecki, W.D. (2001). Climate Change and a Global City: The Potential Consequences of Climate Variability and Change, Metro East Coast. New York, NY: Columbia University Earth Institute, Report for the U.S. Global Change Research Program, National Assessment of the Potential Consequences of Climate Variability and Change for the United States.	This is a debateable refinement of the text and it seems inapropriate to include new evidence or lines of argument at this stage based on one review comment

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(David Major, Columbia University)	
E-19-345	A	32	17	32	17	The reference to the UK Foresight studies is inappropriate as it did not deal with the observed effect of extreme events. A more appropriate reference would be [Hall, J.W., Meadowcroft, I.C., Sayers, P.B. and Bramley, M.E. Integrated flood risk management in England and Wales. Natural Hazards Review, 4(3) (2003) 126-135] which reviews the effect on policy of recent flood events in the UK. (Jim Hall, University of Newcastle upon Tyne)	See above
E-19-346	A	32	20	32	24	It should be noted that market and social systems might be able to adapt to changes in geophysical systems, if the se changes occur over a long enough period or are predictable. (Indur Goklany, US Department of the Interior)	We do state this.
E-19-347	A	32	24	32	27	Good point! (Ulf Molau, Göteborg University)	Thanks!
E-19-348	A	32	24	32	30	Although social and market systems are clearly more adaptable than biological systems, this section downplays the possibilities of adaptation in biological systems. Specifically, CC is one of many threats to biological systems. Other threats include habitat conversion, and loss of corridors. Therefore, one method of reducing the vulnerability of biological systems is to reduce these other pressures (see Goklany 1995, 2000, 2005a, 2006a). (Indur Goklany, US Department of the Interior)	We agree. Text ammended accordingly
E-19-349	A	32	25	32	25	"without a commensurately larger adaptive capacity" suggest delete - this confuses me. (Rachel Warren, School of Environmental Sciences)	Agree.
E-19-350	A	32	26			than --> that (Clair Hanson, IPCC TSU)	ok
E-19-351	A	32	26			change "than" to "that" (Danny Harvey, Dept of Geography, University of Toronto)	ok
E-19-352	A	32	33			Add anew sentence at the end of this para that would read as follows: “On the other hand, adaptive capacity should increase in the future which would increase the ability to cope not only to climate change but all other problems, e.g., water stress, droughts, floods, pests, plant diseases, human health, etc. (Goklany 2006a, 2005c). (Indur Goklany, US Department of the Interior)	See above.
E-19-353	A	32	38			insert 'of' after 'growth' (Clair Hanson, IPCC TSU)	ok
E-19-354	A	32	50	32	51	replace the existing first sentence of this section with: "Climate change assessments and the development of repsonses strategies HAS IN THE PAST been hampered by multiple uncertainties and unknowns (see WG II Ch. 2.2.3 abnd WG III Ch 2.4),	Sentence rephrased. The suggested reference assumes a particular value judgement, namely a risk-averse interpretation of UNFCCC Art.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						ALTHOUGH RECENT WORK BY HARVEY (2006C) REFLECTIVE OF RECENT IMPACT STUDIES AND THE RISK-AVERSE NATURE OF ARTICLE 2 OF THE UNFCCC INDICATES THAT PLAUSIBLE RESOLUTION OF REMAINING SCIENTIFIC UNCERTAINTIES WOULD NOT ELIMINATE THE NEED FOR STRINGENT EMISSION REDUCTIONS (COMPARED TO BUSINESS AS USUAL SCENARIOS) OVER THE NEXT 20-40 YEARS" (added material is in bold). You cite WG III Ch 2.4 in support of your statement that the development of response strategies is STILL hampered by uncertainty. I took strong exception to the whole tone of that section in the FOD, and wrote the cited paper specifically in response to that section of the WG 3 report (the WG3 report as a whole suffers from what I would call "paralysis through analysis" - getting so caught up in the endless discussion and analysis of minor details, while completely missing the key points and failing to grab the problem by the horns, but that is another issue). I am attaching the cited paper. The reference is: Harvey, L.D.D.: 2006c, 'Plausible resolution of uncertainties in global-warming science has no near-term practical implications for climate policy', Climate Policy (submitted). (Danny Harvey, Dept of Geography, University of Toronto)	2.
E-19-355	A	33	9	33	9	The term "likelihood" has a specific meaning in statistics and is used inacurately here. Frequentist probabilities (in the limit) describe the "relative frequency" of a particular outcome of an experiment repeated under known circumstances. (Jim Hall, University of Newcastle upon Tyne)	Reworded
E-19-356	A	33	13	33	13	The term "pseudo-frequentist" is not in general use and should be avoided. I think in this context the authors are refering to the situation in which data from observations of the climate are combined with (often strong) subjective assumptions about climate models or the form of likelihood functions. This approach is Bayesian, and to suggest that the derived probabilities are anything but subjective is incorrect. (Jim Hall, University of Newcastle upon Tyne)	Reworded and reference added
E-19-357	A	33	13			"Pseudo-frequentism" is not a school of probabilistics. This is textbook material, there is no need for the IPCC to start reinventing the axioms of probability and information. (Richard Tol, Hamburg University)	Reworded
E-19-358	A	33	18	33	32	The statement about Bayesian probabilities represents an advance over the FOD, but then the next paragraph refers to Bayesian probabilities describing 'key uncertainties in the natural systyem'. The earlier statement that they reflect 'the degree of belief of experts' is preferable, because they do not describe any properties in the natural system, and do not constitute a 'truth test', but a measn of	In the referred sentence, we explicitly mention that these uncertainties are described by subjective Bayesian probabilities.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						making decisions under uncertainty. If only we could derive probablities FROM the natural system our task would be so much easier! (The use oof such subjectively-derivede probabilities in Least-Cost Utility Planning by electric utilities is a good example of a practical application). (Aynsley Kellow, University of Tasmania)	
E-19-359	A	33	21	33	21	The paper by Hall et al. has now been accepted for publication. (Jim Hall, University of Newcastle upon Tyne)	Update reference
E-19-360	A	33	21	33	21	Helton and Oberkampf (2004) does not, to my knowledge, exist. There is a paper by Helton and Burmaster (2004), which is given in the references, but this is hardly appropriate in the context. Perhaps the authors are thinking of [Helton JC, Oberkampf WL. Alternative representations of epistemic uncertainty. Reliab Eng Syst Saf 2004;85(1–3):1–10.] though this does not deal with climate uncertainties and could be deleted altogether. (Jim Hall, University of Newcastle upon Tyne)	Reference replaced
E-19-361	A	33	31	33	38	I think that this paragraph will leave those who don't already know this material to be confused. Not all the uncertainties listed in this parapraph are equally "key". THE key uncertainty in the climate system is the climate sensitivity; everything else in the climate system is secondary by comparison. This is true for equilibrium as well as transient climate responses. As shown in my own early work with Schneider, variations in climate sensitivty (within the accepted 1.5-4.5 K uncertainty range) are far more important to the transient temperature response than uncertainty in ocean mixing parameters (especially - and much recent work has not done this - when parameters are covaried in such a way as to still replicate observed features such as the vertical variation of temperature, natural radiocarbon, and bomb C14 with depth). Similarly, uncertainty concerning the present radiative forcing by aerosols is not important in determining GHGs concentrations that avoid DAI. This is because, stabilization of CO2 emissions (whatever the chosen level) eventually requires reducing emissions to near zero, so co-occuring emissions of aerosol percursors will also drop to near zero (if they don't indpendently of climate concerns, due to concerns over pollutant effects). Since DAI is in the context of stabilized GHG concentrations, DAI needs to be evaluated under the assumption of vastly reduced aerosol emissions. Thus, their radiative forcing hardly matters to long term GHG stabilization levels. However, anticipating that some would still like to invoke aerosol forcing so as to increase the allowed CO2 concentration, I considered a number of cases with aerosol forcing in Harvey (2006a). Inclusion of such cases does not change the bottom line result, that stringent CO2 emission reductions must begin now. I	Reference added. Text revised.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						suggest replacing this paragraph with the following: "The probabilistic analyses of DAI ... uncertainties in the CLIMATE system, which are dominated and ecnapsulated by uncertainty in the climate sensitivity, and in the vulnerability of natural systems to climatic change. As discussed in Section 19.1.2.3, the computation of GHG concentrations that represent DAI requies three inputs: (I) the probability distribution function (pdf) of climate sensitivity; (ii) the pdf for the threshold of global mean temperature change beyond which unacceptable impacts occur; and (iii) the probability that will be accepted for incurring impacts that have been deemded to be unacceptable. The first input is largely a matter of scientific determination, althought there is no truly objective determination of the climate sensitivity pdf (as discussed in Harvey, 2006a). The second input depends in part on scientific inputs but also has a strong value-based component. The third input is largely value-bsaed and depends as well on the perceived cost of reducing risks." This suggestion wording assumes that my suggestion with regard to Section 19.1.2.3 (page 7 comment) is implemented. REFERENCE: Harvey L D D 2006a Dangerous Anthropogenic Interference, Dangerous Climatic Change, and Harmful Climatic Change: Non-Trivial Distinctions with Significant Policy Implications Clim. Change (accepted) (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-362	A	33	37	33	37	Replace "likelihood" with "probability" (see comments above about the statistical definition of likelihood). (Jim Hall, University of Newcastle upon Tyne)	Accepted
E-19-363	A	33	41	33	41	line is missing here (Rachel Warren, School of Environmental Sciences)	Fixed
E-19-364	A	33	42			Something is missing here, but what is missing might be redundant with or rendered unnecessary by the changes suggested for lines 31-38. (Danny Harvey, Dept of Geography, University of Toronto)	Fixed
E-19-365	A	33	42			Concentrations (Clair Hanson, IPCC TSU)	Fixed
E-19-366	A	33	50			for --> of (Clair Hanson, IPCC TSU)	Fixed
E-19-367	A	34	16	34	21	The term "discrete" on line 16 is not consistent with the subsequent text. "Set-based" might be more appropriate. If deterministic estimates were based upon "range bounding" they would not be deterministic! The words "or range bounding" should be deleted. On line 20 replace "number" with "set". Suggest adding, at the end of line 21, the phrase "without specifying any probability distribution across the members of that set".	Accepted

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Jim Hall, University of Newcastle upon Tyne)	
E-19-368	A	34	27	34	27	It is not true that adaptive analyses "are a sub-category of probabilistic optimising analyses". The adaptive management problem can be framed as a conventional problem of dynamic programming under probabilistic uncertainty but could equally well be framed without reference to probability distributions. How about "Adaptive analyses foresee changing decisions in the light of new information as and when it materialises"? (Jim Hall, University of Newcastle upon Tyne)	Accepted.
E-19-369	A	35	6			CBA is not based on a pre-defined target, by definition; "partly" is quite wrong. (Richard Tol, Hamburg University)	Accepted
E-19-370	A	35	18			after Wigley (2004), add: Harvey (2004). REFERENCE: Harvey, L.D.D. 2004. Declining temporal effectiveness of carbon sequestration: Implications for compliance with the United Nations Framework Convention on Climate Change. Clim. Change 63, 259-290. (Danny Harvey, Dept of Geography, University of Toronto)	Reference added.
E-19-371	A	35	21	36	25	The first two paragraphs in the indicated section deal with probabilistic analyses in which the temperature or CO2 concentration threshold for DAI is SPECIFIED, and the probability of exceeding it is calculated given a pdf for climate senstivity (or, at least, a range of different climate sensitivities is used). The third paragraph of the indicated section deals with a study in which both climate sensitvitiy and the temperature threshold are represented by pdfs. Thus, there is a logical progression from discussing work with one pdf, to work with two pdfs, but this will not at all be evident to the reader who isn;t already very familiar with the cited work. To make this clear, I would replace the first sentence (lines 21-22) with: "An initial set of studies has assessed the likelihood of staying within specified CO2 concentrations or temperature thresholds that are assumed to represent DAI, but accounting for uncertainty in climate sensitivity" [CHECK EXACTLY WHAT O'NEAL AND OPP DID] Next, the paragraph beginning on page 36, line 18, should begin with: "However, the temperature threshold for DAI can itself also be represented by a pdf" The existing first sentence of this paragraph is incorrect, and should be change to: "Wigley (2004) combined pdfs for climate sensitivity and the temperature threshold for DAI in order to construct a pdf for the CO2 level required to avoid DAI. ....' [REREAD WIGLEY'S WORK BEFORE COMMENTING FURTHER - I DISAGREE WITH MUCH OF WHAT IS WRITTEN ABOUT IT] (Danny Harvey, Dept of Geography, University of Toronto)	Suggested changes incorporated.
E-19-372	A	35	22			O'Neill and Oppenheimer indeed argue this, but you fail to mention that they based their results on the outdated paper of Stocker and Schmittner.	This model is not outdated. Please refer to section 19.3 for a discussion of model results

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Richard Tol, Hamburg University)	regarding MOC.
E-19-373	A	35	31			Since you begin this sentence by stating that the result is intuitively obvious, you can delete "(very high confidence)" (Danny Harvey, Dept of Geography, University of Toronto)	Deleted.
E-19-374	A	36	2	36	48	Here the text reports on findings in a non-readable way. There is a need to present clear syntheses and judgements first, after which supporting or qualifying evidence can be presented in greater detail. Now the syntheses and judgements do not emerge and the text does not have enough interpretation to make it meaningful. (Jouni Paavola, University of Leeds)	This section has been significantly revised for clarity. The section reviews and reports results from literature that specifically examines mitigation response strategies with respect to key vulnerabilities and UNFCCC Article 2, and this has been clarified.
E-19-375	A	36	7	36	16	Suggest that the argument advanced in this draft section be replaced with a discussion of the analysis and misinterpretation (which the draft text is) of probability distributions of conditional scenarios. What the draft argument does not consider is that understanding will improve with time, and there is some ability to adjust the direction of future emissions. Figure 19.2 assumes that CO2 is held constant irrespective of the consequences. The range of current uncertainty is an important contributor to the conclusion of 2001 NRC report on climate change that concluded that there was no safe level of GHGs. This type of information supports an adaptive strategy, yet there is little discussion how scenarios might be used to inform such an adaptive strategy. It would seem that reference to the approach of Lempert is appropriate here. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	This is a criticism of the literature and not our reporting of the literature.
E-19-376	A	36	26			Insert an additional paragraph here that reads as follows: "Finally, Harvey (2006a,b) extended the analysis of Wigley (2004) by explicitly considering the acceptable probability for incurring harm that had previously been deemd to be unacceptable (the third of the three inputs to the computation of allowable GHG concentrations listed above). He finds that, for a climate sensitvity pdf with a 5-95% range of 1.5-4.5 C (i.e., reflecting the longstanding scientific concensus), for a pdf for the threshold of global mean temperature change that causes unacceptable with a 5-95% range of 1.7-4.5 C, and for a 10% risk of exceeding this threshold, the allowable CO2 concentration is 390 ppmv if no aerosol offset of GHG forcing is assumed, while the allowable CO2 concentration is 435 ppmv if maximal aerosol offset is assumed. These results assume the present non-CO2 GHG radiative forcing to be reduced to half its present level. If more recent climate sensitivity pdfs (discussed in WG1, Ch 9, Section 9.6.3.1) are adopted, or non-CO2 GHG forcing is larger than assumed, or a smaller probability of incurring unacceptable harm is permitted, then the permitted CO2 concentration is smaller still." I would	Suggested changes incorporated.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						recommend including one or two figures from Harvey (2006a,b), which I can readily provide in electronic form. (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-377	A	36	27	36	48	I think that this paragraph really states the obvious, and so can be greatly reduced in length (by 1/2 to 2/3). The basis point is that if we temporarily overshoot the desired stabilization level, we are going to get a bigger transient warming than if we don't. This is also shown in Harvey (2004), which should be cited as well. (Danny Harvey, Dept of Geography, University of Toronto)	Paragraph has been condensed, and reference added.
E-19-378	A	37	21	37	22	The caption /figure does not define what is meant by exceeding 2C; is this only looking out to a specific year (e.g., to 2100) with the assumption of scenarios or is it looking at equilibrium temperature? Suggest that caption explain the figure more precisely. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	The caption clearly states that the figure is looking at equilibrium temperature.
E-19-379	A	37	21	37	22	Suggest that the assumptions be added to the caption, namely "assuming CO2 equivalent level is maintained constant and neglecting contributions from natural climate variability." (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Clarification has been added.
E-19-380	A	37	32	37	32	This paragraph on geoengineering seems to be in a wrong place - it does not connect well to the surrounding discussion. Either the paragraph has to be relocated or the transfers improved to make the connections. (Jouni Paavola, University of Leeds)	This paragraph has been removed
E-19-381	A	38				F19.3: plots aren't labelled A/B (Clair Hanson, IPCC TSU)	Top and bottom panels are now referred to.
E-19-382	A	39	3			Append after "(HadCM2)", the following: "accuracy of impact models, and assumptions regarding adaptive capacity and adaptation." (Indur Goklany, US Department of the Interior)	Text changed
E-19-383	A	39	7			what is said is fully justified even without the "very", so I would delete it. (Danny Harvey, Dept of Geography, University of Toronto)	Text changed
E-19-384	A	39	29	39	29	Suggest an assessment of the finding of Webster (Energy Journal 23, pp 97-119) be included here. He examined adaptive decisions based on cost benefit, including probabilities and learning, and considered the roles of irreversibility etc. Such an analysis addresses some of the assumptions common to many other cost benefit analyses. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	Reference added.
E-19-385	A	39	29	40	9	This section needs to be completely rewritten. In the previous two sections, you faithfully summarize what the literature says. Here, you fail to mention the crucial cost-benefit studies and to summarise the conclusions. Instead, you devote all the	Cross-ref to WG III Ch. 3 added. Focus of subsection explained.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						space to studies that either critique CBA or pretend to do CBA but really do something different. (Richard Tol, Hamburg University)	
E-19-386	A	39	46			a --> an (Clair Hanson, IPCC TSU)	Changed
E-19-387	A	39	47			After "time discounting" add "adaptation." (Indur Goklany, US Department of the Interior)	Changed
E-19-388	A	40	10	40	10	Conclusion is missing. Probabalistic approach suggests more aggressive mitigation policy compare to deterministic approach. This important results should be acknoledged here with the proper reference. (Alexander Golub, Environmental Defense)	Text added
E-19-389	A	40	15	40	16	This sentence contradicts the opening sentence of the paragraph, which I agree with. Cost-effectiveness does NOT involve evaluating tradeoffs between impacts of climate change and costs of emission reduction because, as you state in the first sentence, CEA begins with a chosen target. Rather, CEA evaluates the lowest-cost way of achieving the target. What you desribe in the second sentence is CBA, not CEA. (Danny Harvey, Dept of Geography, University of Toronto)	Text changed
E-19-390	A	40	16	40	17	I do not consider Wigely et al. (1996) to be either CBA or CEA - so rethink the justification for citing this paper here. It might be considered CEA if one makes the assumption that delayed emission reductions will cost less (which I guess they do assume), but there is no analysis to support this assumption, and many have subsequently published work disputing that assumption. (Danny Harvey, Dept of Geography, University of Toronto)	Reference deleted
E-19-391	A	40	31	40	38	My paper (Harvey, 2006c) should be cited here, and the existing statements modified accordingly. This paper was written specifically as a critique to the general idea in the cited work. REFERENCE: Harvey, L.D.D.: 2006c, 'Plausible resolution of uncertainties in global-warming science has no near-term practical implications for climate policy', Climate Policy (submitted). (Danny Harvey, Dept of Geography, University of Toronto)	See response to E-19-354
E-19-392	A	40	34			Replace "there is general consensus" with: "these studies suggest". (Indur Goklany, US Department of the Interior)	Text changed
E-19-393	A	40	37	40	38	It is worth to mention that Mastrandrea and Schneider proved the opposite: proper addressing of uncertainties calls for immediate and more aggressive actions against climate change. (Alexander Golub, Environmental Defense)	Sentence deleted
E-19-	A	40	40			Add a new para that reads as follows: "Cost-effectiveness analysis has also been	Section 19.4.2 deals with mitigation

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
394						used to explore and compare the relative merits of various adaptation and mitigation approaches through the next several decades. Goklany (2005a), based on global impacts analyses of malaria, hunger, water stress and coastal flooding through the 2080s,concludes that in the in the short-to-medium term the emphasis should be on reducing current vulnerabilities to climate sensitive problems that might be exacerbated by climate change. However, because in the long term mitigation is unavoidable, he recommends implementation of "no-regret" measures (e.g., reduction of subsidies for overuse of energy and land) and an active program to make future mitigation more cost-effective." (Indur Goklany, US Department of the Interior)	specifically. This reference would be more appropriate for Chapter 18.
E-19-395	A	40	45	40	48	This sentence suggests that the level of sophistication in stand-alone carbon cycle and climate models is simpler than in comprehensive IA models, but the carbon cycle and climate modules in IA models are in fact much simpler than the stand-alone models. (Marko Scholze, University of Bristol)	We agree. Rewritten.
E-19-396	A	41	15			insert "the" before "literature" (Danny Harvey, Dept of Geography, University of Toronto)	Inserted.
E-19-397	A	41	16			change "which" to "that" (Danny Harvey, Dept of Geography, University of Toronto)	Changed.
E-19-398	A	41	23			Add "Some" prior to "Risk analyses". (Indur Goklany, US Department of the Interior)	Added.
E-19-399	A	41	25			Append a new sentence at the end of the para that reads as follows: "However, the socioeconomic impacts are unknown. Given the long periods over which these problems might develop, the potential for adaptation could be high.] (Indur Goklany, US Department of the Interior)	The second part of the statement relates to occurrence, not consequence. Added as follows: "Similar conclusions could as well be applied to risks for social systems, though the literature often suggests that any thresholds for these are at least as uncertain.".
E-19-400	A	41	27	41	32	This statement is too vague to be meaningful, and can be easily shown to be much too lenient. First, you have to assign a probability to exceeding 2 C warming ("could" is vague). Second, how large a range of climate sensitivity pdfs do you expect the reader to consider, or are you implicitly considering, when you say "using different pdfs ... for climate sensitivity"? Also, you've said nothing about radiative forcing by non-CO2 GHGs. To illustrate, suppose that one takes one of the more recent climate sensitivity pdfs, with a 5-95% probability range of 2.2-6.8 K (others, cited in WG1, have even higher 95th percentiles) and one is prepared to accept a 5% probability of exceeding 2.6 K warming above pre-industrial times.	We agree, rewritten.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						Then, the allowed radiative forcing is 1.43 W/m2. If the present non-CO2 GHG forcing of 1.2 W/m2 (given in WG1, Ch 2) can be cut in half, the allowable CO2 concentration is 326 ppmv (assuming 280 ppmv pre-industrial CO2 and 3.75 W/m2 forcing for a CO2 doubling). For other defensible assumptions, the allowable CO2 concentration is 280 ppmv - that is, all of the allowed forcing is taken up by non-CO2 GHGs. This is of course contrary to the accepted wisdom, and might be dismissed outright, but the calculations leading to this result are really quite simple. The point that the IPCC should be repeatedly emphasizig is that, for just about any plausible range of assumptions one cares to make, we ALREADY VIOLATE, or are very close to already violating, the conditions set out in Article 2 of the UNFCCC. The point that then needs to be stringently made is that the issue is no longer, can we comply with Article 2, and what levels of CO2 are permitted, but rather, to what extent and how long will we be in non-compliance, and how much damage will be incurred along the way. It then follows that, as one colourful California politician stated, "the time for debate is over". (Danny Harvey, Dept of Geography, University of Toronto)	
E-19-401	A	41	27			this is the first time PDF is used - please define (Clair Hanson, IPCC TSU)	Done.
E-19-402	A	41	39	41	39	I'd be surprised if we can still reach 450 even with overshoot if there is no action for several decades? Of course, I may be wrong here, but suggest check. (Rachel Warren, School of Environmental Sciences)	Checked. It's ok.
E-19-403	A	41	41			Add a new item (number 6) that would read as follows: "An adaptation program that would reduce current vulnerabilities to climate sensitive problems that might be exacerbated by climate change would over the next few decades reduce cumulative risks and damages from climate and climate change more cost-effectively and rapidly than would any mitigation program (Goklany 2005a). In the longer term, however, mitigation may be unavoidable." This should be also added to the list of key conclusions on page 2. (Indur Goklany, US Department of the Interior)	We cite the full range of literature..
E-19-404	A	41	44			insert 'are' after 'there' (Clair Hanson, IPCC TSU)	Revised
E-19-405	A	42	0	42	0	In the priority list for research risk perception should be included. (Alexander Golub, Environmental Defense)	Agreed.
E-19-406	A	46	42	46	43	the paper as published does not have "27" in the title, and the pages are 137-156. (Danny Harvey, Dept of Geography, University of Toronto)	Fixed.

LATE COMMENTS:

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-19-1	LATE	0				The chapter has improved substantially on the FOD. However, in terms of key vulnerabilities the issue of climate change as one of a set of multiple stresses is underdeveloped. To me a major concern is that in many cases, climate change might be 'the straw that breaks the camel's back'. This idea should be developed as much as the literature allows. Also, adaptation will not be to climate change alone – in coastal areas it will be a part of coastal mangement, and this will be true across all sectors. (Robert Nicholls, University of Southampton)	Interactions multiple stressors, and synergisms now explicitly in revised text.
E-19-2	LATE	0				Rapid deglaciation is mentioned as a threat, but issues around the so-called "commitment to sea-level rise" which will progress for centuries even if changes <1 m/century are not mentioned. We grappled with this difficult issue in a recent report to the OECD. [Nicholls, R. J., S. E. Hanson, J. Lowe, D. G. Vaughan, T. Lenton, A. Ganoposki, R. S. J. Tol & A. T. Vafeidis (2006) *Improving methodologies to assess the benefits of policies to address sea-level rise*. Report to the OECD. Organisation for Economic Co-operation and Development (OECD), Paris.] (Robert Nicholls, University of Southampton)	We address commitment
E-19-3	LATE	13		20		I find some of the statements on the issues I understand quite sweeping and even wrong in some cases. It needs careful review and ideally interaction with all the supporting chapters. Detailed remarks below. (Robert Nicholls, University of Southampton)	Review and will consult chapters
E-19-4	LATE	9	25			Nicholls (2004) is the wrong reference – Nicholls et al (2005) (Robert Nicholls, University of Southampton)	OK
E-19-5	LATE	9	48			Good examples of multiple stresses. (Robert Nicholls, University of Southampton)	Thanks
E-19-6	LATE	10	16			Do you mean flooding (which implies a temporary inundation), or submergence (which implies permanent inundation)? (Robert Nicholls, University of Southampton)	Text revised
E-19-7	LATE	10	41			What impacts of sea-level rise? (Robert Nicholls, University of Southampton)	Ok
E-19-8	LATE	13				Coastal Communities -- Vulnerability can be partially reduced through adaptation? In some of the studies that I have been involved with intelligent adaptation integrated with wider coastal management can reduce reduce vulnerability compared to today even with sea-level rise and climate change. I think the message	Ok

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						here is that vulnerability of human systems to flooding and submergence can be greatly reduced by proactive adaptation. The vulnerabiliity comes from those systems that cannot so adapt. (Robert Nicholls, University of Southampton)	
E-19-9	LATE	13				Coastal Communities Why are mangroves distinguished, while saltmarshes and other coastal wetlands are omitted? Note that options for adaptation for coastal ecosystems appear more limited than for human systems. (Robert Nicholls, University of Southampton)	Tables 19.1 and 19.2 have been combined and rewritten with considerable abridgement to conform to space limitations. Comment no longer applicable.
E-19-10	LATE	14				"although SLR will displace many people" – Chapter 6 has numbers for a specific scenario. (Robert Nicholls, University of Southampton)	As above. Entry has been subsumed in "migration and conflict" entry under Global Social systems with further loss of detail.
E-19-11	LATE	15				Antartic Ice Sheet – stress the likely long timescales. And a key issue is do people care about these long timescales. This was explored in the OECD report. [Nicholls, R. J., S. E. Hanson, J. Lowe, D. G. Vaughan, T. Lenton, A. Ganoposki, R. S. J. Tol & A. T. Vafeidis (2006) *Improving methodologies to assess the benefits of policies to address sea-level rise*. Report to the OECD. Organisation for Economic Co-operation and Development (OECD), Paris.] (Robert Nicholls, University of Southampton)	Citation added at section 19.2
E-19-12	LATE	17				Coastal Resources. Unclear what the temperature rise means in terms of sea-level rise – a footnote or reference explaining assumptions or the models that were used is required. (Robert Nicholls, University of Southampton)	Coastal is no longer split out as a KV
E-19-13	LATE	17				Coastal Resources – impacts for temperatures above 2 degrees C does not reflect the uncertainties – benefit-cost analysis would suggest that there is not a problem and developed areas will be protected – would suggest softening and stressing increased risk of …… (Robert Nicholls, University of Southampton)	Coastal is no longer split out as a KV
E-19-14	LATE	17				Coastal Resources – need to remind the reader of the long timescales asscoaited with large sea level rises. (Robert Nicholls, University of Southampton)	We do in 19.3.5
E-19-15	LATE	18				Biological Systems – might link to other profound stresses such as conversion of mangroves to aquaculture in the tropics – some studies suggest that we lose 1% of coastal wetlands per year to direct and indirect human destruction. Overfishing is another important issue, but I know much less about it. (Robert Nicholls, University of Southampton)	Too narrow
E-19-16		19				WAIS – if WAIS contribution is up to 1m/century, complete deglaciation must take $\geq$500 years	Rewritten to be consistent with relevant WGI chapters.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Robert Nicholls, University of Southampton)	
E-19-17	LATE	19				Greenland IS – full deglciation in several centuries? This seems very fast compared to my understnading of the literature. I thought the timescale was really over the next millenia or so. (Robert Nicholls, University of Southampton)	Rewritten to be consistent with relevant WGI chapters.
E-19-18	LATE	23	35		43	A key point is that islands impacts and abandonment may be more complex than suggested here with important socio-ecological thresholds (see Barnett and Adger, 2003; Gibbons and Nicholls, 2006). [Barnett, J. and Adger, N.W., 2003. Climate dangers and atoll nations. Climatic Change, 61, 321-337 Gibbons, S.J.A. and Nicholls, R.J. (2006) Island abandonment and sea-level rise: An historical analog from the Chesapeake Bay, USA. *Global Environmental Change*, 16, 40-47.] (Robert Nicholls, University of Southampton)	Ok
E-19-19	LATE	25	45			Replace Atlantis, 2005 with Tol et al (2006). TOL, R.S.J., M.T. BOHN, T.E. DOWNING, M.-L. GUILLERMINET, E. HIZSNYIK, R.E. KASPERSON, K. LONSDALE, C. MAYS, R.J. NICHOLLS, A.A. OLSTHOORN, G. PFEIFLE, M. POUMADERE, F.L. TOTH, A.T. VAFEIDIS, P.E. VAN DER WERFF and I.H. YETKINER, 2006. Adaptation to Five Metres of Sea Level Rise, *Journal of Risk Analysis*, 9(5), 467-482. (Robert Nicholls, University of Southampton)	Done
E-19-20	LATE	25	46		48	The reported rates here confuse me – if sea-level rise is 1-m/century, then we are looking at more than a milleniafor 12-m rise. Please make this clear and internally consistent to the reader. (Robert Nicholls, University of Southampton)	Section has been clarified on this point.
E-19-21	LATE	31	23		24	Suugest looking at Nicholls and Tol (2006) which considered the optimisitic versus pessimistic views on coastal impacts. [NICHOLLS, R.J., and TOL, R.S.J. (2006), Regional to global implications of sea-level rise: An analysis of the SRES scenarios. *Philosophical Transactions of the Royal Society A.*] (Robert Nicholls, University of Southampton)	No opportunity to bring in new material. In any case does not add to or detract from the argument.