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 1

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 1:

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 1 ZOD comments by Co-Chairs and TSU	Has this been addressed in the SOD?	Author response
1.Z1	This is generally well constructed, structured and written in a consistent style. It obviously represents much work already by the authors. Most sections read well (especially that on Agriculture). Marine and terrestrial biology is especially strong but freshwater biology is sadly lacking (essentially not there). Most have summaries, though some are weak (e.g. Cryosphere) and most reference recent research.	(a) Freshwater biology now one page plus a table. (b) Summaries improved	Added more on Freshwater. Strengthened summaries.
1.Z2	Length is a problem: The current draft needs reducing by 22 pages (see Doc 3 of Blue Book for basis of this calculation). Text will need condensing throughout, and recommendations are made below about which sections could be reduced more than others. In addition, we suggest you save space by summarising in tables [the tables in the terrestrial eco. section are excellent examples of dense reporting that would have otherwise taken many pages]. Suggest this approach for all sectors.	See under FOD comments regarding length and tables	Shortened text.
1.Z3	A general issue that needs discussion at Cairns is how to achieve balance in the assessment. There are obvious difficulties in assessing literature where no change is evident, but the corollary of the problem does need assessment: namely, where are there few or no reports of climate-related change, even where you might expect them to occur. Perhaps a way of doing this in by an inductive approach: Where there is clear evidence of climate-related effects, and the processes are understood, then this can provide a basis for predicting that effects will have occurred in 'similar' environments where 'similar' climate change has been observed. Two types of environment where the processes are well understood are the ecotones in mountains and in cold marginal/high latitudes. Are effects found in (all) these cases, and if not why not (maybe the environments are not analogous; maybe there are disguised buffering effects; maybe the data have not been examined). And on this last point, you could examine the data [not the literature that reports a connection, but the data themselves] on most-sensitive exposure units (e.g. cryosphere, plants, animals) for	Authors address the issue of negative/inconsistent response. In the SPM, there is a map showing where the inconsistent and negative response cells lie. It is not yet in the chapter, but needs to appear there in the FGD. No specific reference to mountains or cold marginal/high latitudes	Evidence of no change addressed explicitly, in 1.2, 1.5, and in ES.

	these analogous hypersensitive environments to confirm or reject the hypothesised connection.		
1.Z4	To save space, we suggest : a) S1.1.2 be shortened to one page of bullet points; b) S1.2.1 be summarised in about half the current length; c) Sections on ice, water and coasts could be effectively summarised in tables (as done in the terrestrial ecosystem section) and use much less space; d) 1.3.4 suggest this be condensed to half current length.	(a) Done. (b) Now very much shorter (c) Done for coasts, water. Not done for cryosphere. (d) 6 pages in ZOD; 10 pages in FOD; 7 in SOD. These should be reduced further.	Text shortened to within page limits.
1.Z5	Treatment of the different exposure units differs, at present, considerably in style. Some sections are weaker. a) 1.2 is generally weaker than the rest. b) 1.2.1 is confusing when it mixes discussion of methods and indicators. c) Cryosphere concentrates too much on glaciers. d) Phenology is well written but significantly too long, though with excellent tables. e) Fisheries by comparison is weak and f) livestock is essentially missing. g) 1.4 has a narrow reference base. h) couldn't much of every exposure unit/topic be summarised in tables?	(a) Still weak. The chapter takes a long time to get going, which dilutes its message. (b) Drivers of change (1.2.1) are still considered under Methods (1.2) and it is still confusing. (c) This section remains weak and 'bitty', with lots of very short sub-sections. It opens by saying that it will concentrate on non-WGI aspects (i.e., downstream impacts of ice-melt and SLR) but in fact strays often into WG1-related aspects of the cryosphere. (d) Still long, at 3-4 pages text 2 tables and a figure; still well-written and interesting. (e) Text has been shortened but otherwise not materially changed; figure and table added. (f) Livestock are not mentioned in the main text. (g)Better now (h) more tables are now provided	Addressed.
1.Z6	Obviously, sections on hazards and socio-economic are currently missing and need priority attention. Maybe boxes could be used for case studies: e.g.50 % of the severity of the 2003 heat wave in NW Europe has been attributed to global warming and there is extensive literature on socio-economic effects of this. Also, Finnish economic foresters now plan their planting, harvesting, production, marketing all on assumptions of a yield c. 10% higher than in 1970s (ACACIA report, 1999)	• Disasters and hazards section has grown substantially and is much improved. • Chapter 1 no longer has a box on the 2003 heat wave. It has boxes on (1) the Chacaltaya glacier (2) wine (3) a COST project. Some wasted opportunities. • Nothing could be found on Finland or forests.	Addressed.
1.Z7	Could Figure 1.6 have a European inset? We suspect that plots on this map will increase massively as the literature is	Map has changed substantially and is probably approaching OK in its SPM	Map redone.

	searched during your assessment (e.g. only 1 currently for Australia?)	version.	
1.Z8	In the health section: how much are the emerging diseases due to changing political and economic structures, rather than regional climate change?	This is discussed at the beginning of Section 1.3.7, but not really carried through to the discussion of individual diseases. Thus the question is asked in the Introduction, but not really answered in the main text.	Health rewritten.
1.Z9	The figures on hazardous events and on insured losses (identified for S 1.3.8) when used in the TAR were heavily criticised, we think deservedly so, because they reflect higher levels of exposed risk in the latter (e.g. building on coasts) and better records of the former.	Fig. 1.5 should be OK – might be worth adding correlation coefficient – authors say it is significant but not what it is. There are many caveats in the text, so should be OK.	Fig. removed.
1.Z10	We suggest three key next steps: a) a priority is to quarry all ZOD chapters for information on current sensitivity both to use in your chapter and to make connections with the other chapters; b) identify which CAs could be asked to develop tables for each of the topics. These would need to be contacted before or at Cairns; c) consider as a group the emerging key conclusions: NB. the message may be not so much, as in TAR, is there a detectable effect, but rather more detail on this: i. where is the effect stronger than elsewhere, where weaker (and does this indicate resilience in systems); ii. where is it not detectable but might be expected (is this due to lag effect or adaptation); iii. what lessons does all this tell us about sensitivity of systems to climate change, about rates of response, and about adaptation, etc. Begin to focus on your key messages, then identify the remaining work to be done on these.		Checked all regional and sector chapters for relevant references. Worked extensively with CAs. Reworked key messages.
	Chapter 1 FOD comments by Co-Chairs and TSU	**Has this been addressed in the SOD?**	**Author response**
1.F1	**Comment 1.F1: Osvaldo Canziani** This is an organized and well written chapter; however it is mainly referenced on developed countries bibliography. This skewness should be attributed to the lack of access to developing county's authors and no to the lack of studies and research. In this sense, the cryosphere section looks better balanced. Because of the above, coordination with other chapters, in	Still heavily weighted towards developed countries. No recommendations regarding future research (and nothing on the need for improved observational systems).	Need for studies in developing studies is in 1.1 and 1.5.

	particular the regional chapters would enable a reduction of extensive citation from developed country's authors and the improvement of information from developing country's studies. It is particularly outstanding the lack of references on the urgency to improve the observation networks and the enhancement of data sets through the inclusion of biographical as well as socio-economic information.		
1.F2	**Comments1.F2 – 1.F15: Martin Parry** The TAR concluded that: " climate changes have already affected a diverse set of physical and biological systems". Can you draft a sentence that summarises the advance made in the AR4 assessment: a) is the link made to GHG warming made (or still tenuous)? b) is the TAR message the same but much (how much?) stronger? or c) are there entirely new systems/fields for which there is evidence of effects now? It is not made clear in the Exec Summary what is really new, as compared with the TAR.	This has now been done in the ES, which is closely related to the text that has gone into the SPM	Done.
1.F3	Length: current draft is about one-third too long. 84-page text needs reducing to 60 pages.	Still too long – see below.	Shortened.
1.F4	All sections asked for in PAO are covered, but those on hazards and on s-economic are relatively thin (more on this below).	See below. Section on hazards now much improved, and new paper has appeared (Miller et al.).	PAO followed as much as possible.
1.F5	p 35 Should you consider ocean acidification and its effects on marine life. I know you have already a huge range of material; but o-a is one of the best observed/recorded effects of inc T, with very many ramifications. See: UK Royal Soc report : Policy Document 12/05, June 2005 Ocean acidifcation due to increasing atmospheric carbon dioxide.	Statement appears in ES, but is not properly substantiated in text – there is just a weak reference on Page 46.	Line-of-sight worked on.
1.F6	Tables are effectively used to summarise evidence in many systems; but these are missing from: cryosphere, coastal, health, hazards and socio-economic (although the last probably does not warrant one). Suggest tables in these sections very much needed.	Cryosphere, health and hazards: no table Coasts: table added	Added tables for each section.
1.F7	Additions of tables (see above) could enable text in each subsection of 1.3 to be reduced by about 30% Suggest you ask Jose Moreno (Eur chapter CLA) for info on forest fire frequency in Spain/Potrugal, which very revealing.	Nothing on European fire; no Moreno reference used	European fires added.

1.F8	Table 1.12 Forestry: see Kellomacki et al. (ACACIA project report p[. 140 ASSESSMENT OF EFFECTS CLIMATE CHANGE IN EUROPE, 2000, Ed Parry, M) regarding evidence for inceased biomass of boreal forest in N. Europe in late 20thC	Table has been removed. No reference to Kellomaki or ACACIA report.	We tried, but reference was not explicit enough for chapter.
1.F9	Disasters/Hazards: This section too discursive, lacks detail. Suggest reduce to half length of current text plus one solid table	Much better but still room for improvement.	Rewritten.
1.F10	p. 61 Should not the much greater extent of information now available, as evidenced here (as compared with TAR) be made more evident in the conclusions and exec summary?	This is never said 'up-front'. ES in particular is much more up-beat.	Added.
1.F11	S 1.4.4: Is there only one study of joint attribution? In which case summarise this in about half the current text space.	Two studies assessed, plus the study which was done for the chapter.	Found two more studies.
1.F12	S 1.4.3 Relation to anthropogenic climate change. Cannot you identify regions where relations between regional warming and global warming have been established; and for these regions, then infer that effects related to regional warming are thus related to the global trend? Isn't this a key part of the assessment? But it seems to be missing.	Now done (Figs. 1.8 and 1.9)	Done.
1.F13	S 1.4.5 and 1.4.6 Very weak in comparison with the rest of the chapter. Seems here to be treated as an after-thought. There is, in fact, extensve literature on current adaptation, but I wonder whether you have the space to assess its significance. The questions most relevant here are: Are there lessons we can learn about current adaptation, to address adaptation in the future; is adaptation possibly obscuring observed effects, i.e.leaving no observable residual impact in some situations. Where are these, and are they of a particular kind i.e. more adaptation in some situations than in others. This is a large topic. Decision needs to be made either to address this more thoroughly or more cursorily. Current half-way house seems unsatisfactory	Adaptation now reduced to one para. What about natural as opposed to social adaptation?	Conservation measures added in 1.5.
1.F14	Case study on 2003 European summer: suggest you add graph/ statistics from Nature vol 427 analysis Next step: I suggest read all sector and regional chapters in FoD to extract new information on observed effects.	This case study removed. The figure is in Chapter 12 at present.	Removed.
1.F15	General comments made by Martin Parry in Jan 2005 on	Generally these comments appeared in	Balance of evidence noted. Text

	ZERO-ORDER DRAFT [response in brackets] : 1) Strive for balance by also assessing where effects are NOT observed, but where they ARE expected (eg at some ecotones?). Does this indicate resilience of the system? Or imperfect observation or knowledge of system sensivity? [more now in FoD] 2) Length of text is a problem, given the breadth of yor subject. Suggest a) you avoid lengthy text by summarising in tables [the tables in the terrest ecos section are excellent examples of dense reporting that would have otherwise taken many pages]. Suggest this appro sectorsach for all sectors [FoD text still needs to be cut by about the same amount as in ZoD, i.e. by a third] 3) Suggest S1.1.2 be shortend to one page of bullet points [done] 4) Suggest S1.2.1 be summarised in half the current length [done] 5) Sections on ice, water and coasts could be effectively summarised in tables (as done in the terr ecos section) and use much less space). [water done, but not ice, coast, health, hazards] 6) 1.3.4 suggest this be condensed to half current length [still needs reduction] 7) Obviously, sections on hazards and socio-economic need priority attention [still weak] 8) Figure 1.6 cold have a European inset? Suspect that plots on this map will increase massively as the literature is searched during your assessment (eg only 1 in Australia, etc?) 9) In the health section: how mch are the emerging diseases due to changing pol/econ structures? 10) Suggest next priority is to quarry all ZoD chapters for info on crrent sensitivity. 11) And then consider as a group the emerging key conclusions: NB the message may be not so much, as in TAR, is there a detectable effect but more detail on this: where is it stronger than elsewhere, where weaker (and does this indicate resilience in systems), where is it not detectable but might be expected (is this due to lag effect or adaptation), what lessons does all this tell us about sensitivity of systems to climate change, about rates of response, and about adaptation, etc etc [not precisely clear how conclusion from this assessment differs from TAR? is it the same but stronger; or is it different?]	the ZOD comments, and are addressed there. They are included here for completeness. 6) 10 pages in FOD, 7 pages in SOD	shortened to page limits. Other comments addressed.
	Chapter 1 SOD comments by Co-Chairs and TSU		**Author response**

1.S1	**LENGTH:**	Still over-length. There are 69 text pages, and the target is 55.	Shortened ES by 1 page;
1.S2	**ARE PAO HEADINGS PRESENT?**	Broadly present. The long PAO heading for Systems and Sectors has been shortened to just 'Systems and Sectors'. Not all sub-headings are followed. Importantly, all the sectors are covered under 'Systems and Sectors'. Under 'Larger-scale aggregation and attribution' there are only three sub-headings (PAO has 6).	Broadly yes.
1.S3	**HAVE MOST GENERAL COMMENTS OF ERs FROM ZOD AND FOD BEEN COVERED?**	Comments have been observed in two very important sections – attribution and disasters and hazards. As a result these sections are much improved. Elsewhere the authors haven't paid that much attention to comments, and the chapter still has some problems – see below	All comments addressed.
1.S4	**ARE REFERENCES BROADLY COMPLETE?**	Yes	References corrected and amplified extensively.
1.S5	**IS THERE LINE-OF-SIGHT TEXT ➔ ES AND TEXT+ES ➔ TS+SPM?**	Line-of-sight weak between text and ES	Line-of-sight strengthened.
1.S6	The chapter takes a long time to get going. The Introduction starts on Page 6, and the description of changes in the cryosphere starts on page 15. The chapter lacks emphasis as a result. It has an important message to get across, but this is diluted by taking to so long to get to the points. Even once the description of observed changes starts properly, there is still considerable extra material in some of the sections, which means that the message doesn't come across strongly. The best sections come towards the end, on phenology, and on joint attribution. The section of Disasters and Hazards (also towards the end) will attract much interest and needs to be strengthened. The authors need to consider how to get the readers to these sections whilst still maintaining their interest. The chapter can still be improved by shortening and intensifying the focus. There's still the question - is the section on the NAO needed. [Line-by-line comments are made on the spreadsheet]		Introductory sections shortened greatly so as to get to 'main' points asap.
1.S7	**In summary, the authors need to:** • Really get to grips with the problem of length. The chapter is too long and it loses impact because it takes a very long time to get going. Therefore the authors need to cut length and think especially about achieving cuts in the early text to achieve greater impact. o Can you justify Fig. 1.1? o Maybe Section 1.4.1.2 should be removed.		Section 1.4 has been revised expensively to address these issues, in response to these comments and those from expert reviewers. In particular, issues of scale addressed specifically, description of Tables

	o Maybe stuff on cryosphere can be shortened and better-focussed – some/much of this must be in WG1. Talk to Ch 13 about the Chacaltaya glacier material – they are under-length. • Consider putting the 'Scope and Goals of the Chapter' before 'Summary of Observed Changes in TAR'. • The chapter must not appear to (or actually) set out to make a point. Careful consideration needs to be given to the whole question of balance: Critics will see the chapter as too much a list of where trends can be discerned. There should be equal attention given to where effects might be expected but are not discerned; • Use definitions of causal relationship with regional temp change or anthropogenic forcing that do not cause confusion with readers because of the use of the term 'attribution' in WG1. This term has an established use in WG1 terms since Santer in the SAR. Develop your own terms to define the causal relationships; and make it clear exactly a) what the levels of proof are; and b) how these differ from WG1. Make sure that section (1.4) has no loose wording or unintended statements which could lead to criticism from the scientific community. This would include: o Distinguishing between anthropogenic climate change and climate change arising from natural causes o Distinguishing between trends which the authors wish to attribute to climate change and those which are 'just happening' and for which no causal connection can (yet) be made. o Thinking through issues of scale – are you drawing conclusions about causal relationships at continental, sub-continental or regional scales? o Make sure the figures and tables in this section are precise and accurate. For examples, in Table 1.12, what is a cell? Is it helpful for policymakers to have Types 1, 2, and 3 or could the message be conveyed in a more straightforward fashion? • Improve the quality of the figures. At present they are too complex to be helpful to the reader. In the SPM, there is a map showing where the inconsistent and negative response cells lie. It is not yet in the chapter, but needs to appear there in the FGD. • Cut out all cryosphere material that is covered by WG1. • Get the chapter going quicker. • It is imperative that the chapter be cut to its limit. This is the <u>last</u> opportunity to do this. • Make sure that the Executive Summary (ES) contains section references back to main text (in [1.x] format). Sometimes statements in ES seem more positive than those in main text – make sure that the two are well-related.	improved, studies broken down into only two Types. Done. These very helpful comments were all addressed.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-1-1	A	0	0			References format needs to be unified. In some cases it is indicated "Parmesan and Yohe (Parmesan and Yohe 2003) found that…" In other cases it is indicated "Parmesan (Parmesan and Yohe 2003) found that ...". I suggest choosing a more economic format, in terms of spaces, and always cite Parmesan and Yohe (2003). It is not necessary to indicate the author/authors twice. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Reference format unified.
E-1-2	A	0	0			In section 1.3 there is a great deal of evidence presented that there have been changes observed in many systems and sectors over the last 30 years. There is clearly a wealth of important evidence for observed changes in many effects. Attribution of these changes in natural and managed systems to anthropogenic causes requires (cf page 8 lines 41-51) that the observed changes are unlikely to be due entirely to natural internal climate variability or natural variability of the system and that the observed changes are not consistent with alternative physically plausible explanations of the observed changes that exclude anthropogenic climate change. My opinion is that this chapter does not provide the evidence to support attribution (as defined above) to anthropogenic climate change. This is because there is an absence of end to end attribution studies which quantify how much of the variance of the systems is attributable to the anthropogenic forcings. Without this we do not know whether a significant amount of variance of effects explained by temperature and a significant amount of variance of temperature explained by anthropogenic forcings translates into a small and not significant amount of variance of effects being explained by anthropogenic forcings. So we cannot rule out the observed changes being consistent with natural internal climate variability or natural variability of the system. In addtion there is no evidence that rules out confounding effects of other prime candidates for processes that could have caused the observed changes such as habitat changes for example; therefore the evidence does not support the observed changes not being consistent with alternative physically plausible explanations that exclude anthropogenic climate change. However the evidence presented in the Root study discussed in 1.4 could support the statement of a discernible human influence on spring phenology since there is evidence of a statistically significant link between phenology and spring temperatures (although the study uses mean spring temperatures rather than meteorological parameters, eg exceedance of thresholds that would be more closely aligned to the phenology change) and evidence that the correlations are stronger with anthropogenically forced model runs than naturally	Partially accepted. Do not accept the comment that attribution is not possible, as the definition of joint attribution is satisfied at global and continental scales, just not at some individual grid boxes. The global synthesis of studies in the chapter, as well as the end-to-end joint attribution studies, provide very strong evidence that human-caused warming over the last three decades has had a significant influence on many physical and biological systems. Section 1.4 has been substantially revised to strengthen the conclusion above. References to WGI statements were correct at the time of preparation of the SOD. Following revisions to the WGI text, appropriate revisions have been made to ensure consistency with the relevant WGI statements.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						forced model runs. Even so there are still many caveats that go with an indirect link of this sort (discussed on page 70 lines 4-10) that need to be borne in mind when assessing the possiblity of this statement being in error. For example the wealth of non climate drivers that can effect systems, and the possibility (not ruled out in the absence of a true joint and end to end attribution approach that quantifies the joint relationships between emissions, climate and effects) that there are other pathways between anthropogenic forcings and effects that act differently to temperature. Therefore the confidence in such a statement (linking effects to human influence) must be lower than it would be of a discernible association between regional temperature changes and effects. There is also an important issue of WGII's summary of WGI's conclusions on regional attribution. WGII summarises the WGI conclusions as regional climate trends are attributed to anthropogenic causes which is not a correct statemet of the WGI conclusions. Consistency of language in WGII with WGI will ensure a consistent message of the conclusions of WGI relating to attribution of temperature changes. As part of this it should be noted that WGI concludes that anthropogenic influence has now likely been detected in all continents except Antarctica (which has insufficient observational coverage to make an assessment), and has also been detected in some sub-continental land areas - note on what spatial scales WGI is able to make assessments of this sort and also the likelihood attached to this statement. Presumably the next draft of the WGII report will be made fully consistent with the final draft of the WGI chapter 9 in this regard. (Peter Stott, Met Office)	
E-1-3	A	0	0			In general, and considering that tables imply a lot of space, I suggest the authors may significantly reduce the number of tables in this chapter, without risking the strength of evidences. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Reduced number of tables.
E-1-4	A	0	0			Check use of parenthesis. In many places, principally related to references and information together, parenthesis are opened and never closed. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	References format fixed.
E-1-5	A	0	0			Check references to El Niño Southern Oscillation, sometimes it is referred as ENSO, and sometimes as El Niño, please unify. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Done. The two terms are used correctly.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-1-6	A	0				Tourism is substantially dealt with in CH 1,4,6,7,91112,13,14,16 . This is a significant change compared to TAR. Overall this is done in a satisfactory manner , in particular since the regional chapters do focus on regional issues without losing space on general aspects. What is missing though, is a critical assessment of the literature quoted (even though this literature is peer reviewed), not an individual assessment of papers but a critical overview of the mainstreams of methods that have been used these last years (though it must be recognised that these works have shed some light on what is a very important issue). This concerns both qualitative and speculative approaches and quantitative research. As regards the former, these confront current tourism behaviour and requirements regarding climate to the futures envisaged by scenarios. What is the degree of reliability of this kind of work knowing that the expectations of tourists regarding climates can evolve significantly, as they already have done in the past? There is at least a need for research to explore the range of possible evolutions in behaviours and introduce that into the analyses. Also, to what extent are econometric analyses concerning modifications in tourist flows (the more seducing as they yield figures…) robust and reliable? Is it, for example, acceptable to use a unique climate for the US as it is done in a paper quoted in several chapters? If it is, the coarseness of the results should be mentioned. In short, I believe that there should be in some place in the report, a caveat on the difficulties research on this topic encounters (uncertainties on future behaviours, shortcomings regarding statistics etc.) and their consequences on the results. (Jean-Paul Ceron, CRIDEAU (Université de Limoges-CNRS-INRA))	Not relevant to chapter on observed changes.
E-1-7	A	0				This is a treasure of a chapter. Very well written and an accurate summary of the state of the science. I am sure decision makers will find it very valuable. I have only a few comments on details. In the areas I know well it provides a fair and balanced overview. (Neville Nicholls, Monash University)	Thank you.
E-1-8	A	0				This chapter would benefit from more references to the WG 1 report. In particular, when changes in climate themselves are discussed, it would be much better if the authors cited the relevant sections of the WG 1 report, rather than assessing the literature themselves. There are several references to chapter 9 of WG 1 on regional detection and attribution, but, for example the sections on extratropical cyclones and tropical cyclones make no reference to the relevant sections of the WG 1 report, even though these topics are all discussed in considerable detail in chapters 3 and 9. (Nathan Gillett, University of East Anglia)	Accepted. Revisions made to ensure cross references to WGI.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-1-9	A	0				This chapter states repeatedly that regional climate change has been attributed to anthropogenic forcing. This wording goes beyond that used in WG1, Ch 9, where we say that anthropogenic influence has been detected in regional surface temperatures (in all continents except Antarctica, and in some continental sub-areas). The WG2 message should presumably be made consistent with that of WG1. Note also that we would have less confidence in detection results at the grid box (5 x 5 degree) scale than at larger continental scales. There are several reasons why this might be the case, including uncertainty in the representation of local land-surface processes, uncertainty in the coupling between that the atmosphere and the surface, and because atmospheric variability may be less well simulated by climate models at grid box scales (variability at any scale is partially determined by the cascade of energy from smaller to larger scales via non-linear interaction; this source of small scale atmospheric variability is missing, or at least not well represented, at the smallest resolved scales). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Accepted. References to WGI statements were correct at the time of preparation of the SOD. Following revisions to the WGI text, appropriate revisions have been made to ensure consistency with the relevant WGI statements.
E-1-10	A	0				This chapter defines three types of attribution: (a) attribution of observed climate change to external causes, (b) attribution of changes in systems to changes in climate, and (c) joint attribution (i.e. both (a) and (b)). Definitions (a) and (b) follow the definition of attribution used in WG1 and lay out strong requirements for attributing cause. One would anticipate that attribution would eventually allow answers to the questions "how much of the observed climate change is due to external forcing?" (Ch 9, WG1 provides estimates for global surface air temperature), or "how much of the observed change in a given system characteristic is due to climate change?". Ideally, one would also like to eventually respond to the question "how much of the observed change in a given system characteristic is due to external forcing?". However, meeting the requirements of definition (c) does not necessarily imply that this would be possible (even if we could quantify the size of response in both (a) and (b)), and does not necessarily mean that the response to external forcing would be detectable in the observed system characteristic. Nevertheless, I think I could go along with the notion of "joint attribution" if there were some discussion immediately after the introduction of this definition pointing out that "joint attribution" would not necessarily imply that a substantial part of the change in the impacted system is due to anthropogenic forcing (indeed, the anthropogenic response might not even be detectable in an end-to-end approach despite achieving joint detection). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Accepted. Definition of joint attribution does not require quantification of the fraction of variance in the response due to a specific forcing. Additional text explaining this point has been inserted.

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E-1-11	A	0				There are many references to results of particular studies which rely on a citation of a report by Munich Re. The authors should read the original papers and cite them here. (Nathan Gillett, University of East Anglia)	Corrected.
E-1-12	A	0				The terminology used is different from WG1 terminology, and I worry about the different requirements used for the term "attribution". Ch9WG1 only confidently attributes temperature changes on continental temperature scales, there are isolated results on smaller scales (size of Giorgi regions) and some suggestive results on smaller eg gridbox scales that are an area of active research and that we dont make strong statements about at this point (see table 1 chapter 9). Therefore I worry about WG2 using similar terminology for different methods, and more confident statements about similar things. I suggest taht this should get resolved. Details will follow. (Gabi Hegerl, Duke University)	Accepted. References to WGI statements were correct at the time of preparation of the SOD. Following revisions to the WGI text, appropriate revisions have been made to ensure consistency with the relevant WGI statements.
E-1-13	A	0				The SOD has further improved over the FOD and ZOD, which I also reviewed. In particular, the text has less redundancy and the idea of "joint attribution" is better explained and used consistently throughout the chapter. Also, the synthesis at the end of the chapter is elaborated. This is an important part of the chapter and I feel the additional space and figure panels used now are fully justified. Congratulations to the authors! (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Thank you.
E-1-14	A	0				The overall discusssion is much better organized and evidences of the matter generally well described and documented. References are now consistent. (Giampiero Maracchi, Institute of Biometeorology)	Thank you.
E-1-15	A	0				The chapter reads very interesting and informative. My review focuses on areas of overlap and inconsistency with WG1, particularly with chapter 9 there. (Gabi Hegerl, Duke University)	No response needed.
E-1-16	A	0				The chapter is well writing in general. In the marine systems, in which I was specialist, the major topics and the more recent information are considered. Only three specific questions, but important and nor well understood could be introduced in the text. First, few information exists in the literature about changes in seasonality and recurrence of some "normal" events, like coastal currents, upwelling, eddy production, and others. As changes in these aspects could be important to the dynamic of the marine ecosystems and for some ecosystem services and human resources (aquaculture, fisheries), I suggest in the next lines to introduce this topic. Second, another aspect that I consider important to understand the possible effects, or demonstrated effect, related to climate change, are the	Chapter 1 does not include future projections, only observed changes. It focuses on evidence of change, rather than processes. Chapter focuses on ecosystem changes rather on fisheries yields. Reviewer papers were sent to CA for assessment.

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						cascading effect; obviously, the more important question related to this are the significance on exploited populations, and our ecosystemic analysis of the exploited populations. In Spain these aspect is transcendent, as the drastic drop of anchovy yield in the last two years shows. Third, the sea level rise is considered in a relative conservative point of view. Al least in some part, the increased sea level, excluding isostatic or climatic compensations could be higher that the data presented shows. Clearly the geographical variability in sea level rise is very important, but some areas would be more affected. Beach changes, docks and other engineering structures are predicted to be changed in the NW Spain, with a high economic cost in the next decades (Spanish Strategy of Fight against Weather Change, page now in construction http://www.mma.es/portal/secciones/cambio_climatico/documentacion_cc/estrategia_cc/index.htm). Probably this high regional increased sea level rise could influence other world regions. (Ricardo Anadon, Universidad de Oviedo)	
E-1-17	A	0				The authors have done a splendid job in this fourth assessment. The one egregious omission in Section 1.3.7 on Human Health is the lack of information on the effect of climate change on air pollution and the resulting health effects. I strongly urge that the following statements be added to Section 1.3.7 (Mary Gant, Environmental Protection Agency)	Air quality effects of warming temperatures to-date are not well documented.
E-1-18	A	0				The assessment of the space relation between the pre-warming areas of the physical systems, the biological systems etc. and the effects of the warming on the areas seems to be missing in the whole chapter.The explanation of this assessment can be found in comments given on page 11. (Annick Douguédroit, University de Provence)	The proposal of classification by the reviewer could be interesting, but impossible to enter here as far as it has not been discussed in a scientific paper in an international journal. We suggest that A.Douguedroit uses the opportunity of her careful review for writing this paper which could incorporate the elements of the report for supporting her theory.
E-1-19	A	0				Section B. This section includes many changes as "impacts" which are also considered as "changes in the physical climate system (atmosphere, ocean, cryosphere)" by WG1. This does not mean that chapter 1 should not refer to them again, but perhaps it would be useful to place more emphasis on impacts resulting from those changes rather than those changes themselves. (Andy Reisinger, IPCC SYR TSU)	Overlap with WGI reduced and emphasis placed more on impacts.

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E-1-20	A	0				Overall I think this chapter is much improved from the first order draft, and the authors should be commended for this improvement. (Nathan Gillett, University of East Anglia)	Thank you
E-1-21	A	0				Overall Comments:1. The SOD in the present form is not acceptable for publication. It uses highly selective bibliography to portray the present climate change as primarily human-induced while ignoring several peer-reviewed studies which have provided an alternative view of climate change. There is a definite need to include several recent references now available in published literature and re-write the SOD to provide a balanced view of the persent climate change.2. Most of the climate change studies cited here appear to be associated with Europe (Western Europe in particular) and North America. It is realized that very few studies are available on climate change assessment in tropical regions of Asia, Africa and South and Central America. However, good weather & climate datasets are available over most of these "under-studied" regions and it is important to analyze these datasets to assess climate change and its impacts on regions where more than 70% of world population lives. (Madhav Khandekar, Retired)	Rejected. Analysis of the causes of observed climate change is done in WGI and the chapter uses relevant conclusions from WGI. The literature assessed is as comprehensive as possible given space limitations.
E-1-22	A	0				Overall Comments: 3. The SOD for Ch 1 (and by extension other chapters) of WGII present a gloomy picture of present and future climate change impact by repeated claims of deleterious impact while completely ignoring beneficial impacts of a warmer future world. This is unscientific and unjustified. Global Warming can be beneficial to many high-latitude countries of Northern Hemisphere in terms of milder winters, less house heating cost, longer agricultural season and improved forestry due to increased levels of carbon dioxide. It is important to highlight these beneficial impacts in the WGII Document. 4. Present climate change appears to have minimal impact on most tropical countries of Asia, Africa and Central America. Tropical countries of Asia and Africa are primarily dominated by Monsoonal climate while over Central America, climate is dominated primarliy by the seasonal migration of ITCZ (Intertropical Covergence Zone). More studies and analyses of available data of tropical countries are needed to fully assess climate change impact on a global scale. (Madhav Khandekar, Retired)	Rejected. The chapter only assesses published studies of current observed impacts and does not consider future impacts. The chapter describes the advance of spring and the extendd growing season at length, as well as the increased NPP on land.
E-1-23	A	0				note that I really like the phrase attribution to warming, which seems what your research supports quite a bit more than attribution to greenhouse gases. In some cases, it would be good to check the phrasing to avoid sounding like all change is attributed to warming (if I understand right, you detect that warming has a significant impact on changes).	Accepted. Text revised where appropriate to tighten language.

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						(Gabi Hegerl, Duke University)	
E-1-24	A	0				My impression is that the large scale aggregation results presented in 1.4, based largely on Root et al (2005) and a follow-on analysis in the chapter, do not meet the requirements for joint attribution that are laid out in the definitions on page 8. However, I do think the evidence that is presented would allow a statement that there is a discernible human influence on the date of spring phenology traits. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Partially accepted. The definition of joint attribution is satisfied at global and continental scales, just not at some individual grid boxes. The global synthesis of studies in the chapter, as well as the end-to-end joint attribution studies, provide very strong evidence that human-caused warming over the last three decades has had a significant influence on many physical and biological systems.
E-1-25	A	0				Munich Re's comment: On the other hand side, for periods characterized by abnormal warm sea surface temperatures (1926 – 1970 and 1995 – present day) and by abnormal cool sea surface temperatures (1903 – 1925 and 1971 – 1994) in the 20th century we find different distributions of normalized annual losses (losses normalized in the sense of Pielke and Landsea for changes in wealth, population and for inflation). So mean and median of the warm phase loss distribution are much higher than of the cold phase loss distribution and the difference between both distributions is statistically significant ($\alpha = 1\%$). In addition we find much higher percentages of losses exceeding specified thresholds in the warm phase distribution compared to the cold phase distribution (see the table below). Hence the value of loss expectancy is higher for the warm phase loss distribution than for the cold phase distribution. Given a long-term increase in atlantic sea surface temperatures caused by global warming (Agudelo, P. A., Curry, J. A. (2004), GRL 31), we will consequently see higher annual (normalized) losses averaged over periods with higher sea surface temperatures – as already can be observed for the 20th century. Table: Percentages of years exceeding specified annual loss thresholds in warm and cold phases of the 20th century. cold phase years warm phase years > US 1 bn 19 (of 47) 40% 37 (of 56) 66% > US 5 bn 10 (of 47) 21% 25 (of 56) 45% > US 10 bn 8 (of 47) 17% 17 (of 56) 30% (Peter Hoeppe, Munich Re)	Disasters and Hazards section rewritten.
E-1-26	A	0				It seems at times hard to track down what period has been analyzed and how far	Accepted. Time period being considered has

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						back the data go. This is very useful information that helps to understand context, so some more of it may be good. (Gabi Hegerl, Duke University)	been included.
E-1-27	A	0				It is unclear why this chapter presents long summaries of information that is covered in other chapters. For example, Section 1.3.7, Human Health, devotes 3 1/2 pages to summarizing material covered in Chapter 8, which has already been summarized in both the SPM and TS. Shortening this chapter would strengthen the report (Lenny Bernstein, L.S. Bernstein & Associate, L.L.C.)	Chapter One provides an overview of observed changes across natural, managed, and human systems. The other WGII chapters cover the systems in process-level detail.
E-1-28	A	0				In this new version, more papers published prior 1999 are considered and cited, it does not seem to be in agreement with the guidance notes. A lot of citations are still incomplete in the references section. The chapter is too long according to the recommandations. Some propositions for shortening the chapter are proposed in the following. (Isabelle Chuine, CNRS)	References checked; TAR references removed; and text shortened.
E-1-29	A	0				In summary, the small section relating to Human Health should be rewritten with special effort at balance and objective review of the literature. Interestingly, there is no mention of West Nile virus, nor of Chikungunya no Leishmaniasis. All have appeared in the press in recent years as expanding "due to climate change". (Paul Reiter, Institut Pasteur)	Checked diseases included with Chapter 8 authors.
E-1-30	A	0				In some places, there is overlap in assessment between WG2 and WG1, sometimes just overlap and sometimes even differences in assessment. Particularly the latter is quite problematic, but any overlap will have the danger of differing assessments. I recognize that you need to reiterate changes in physical systems when discussing impacts. However, I suggest to refer to WG1 chapters, and not recite literature, and not reassess. This is how WG1 solves overlaps within the WG, and I think it works quite well. In many instances, this is already done and I thank you for the citations of chapters. In some cases, it should be done now, a notable case are changes in tropical cyclones, where your assessment of contributors to damages is very interesting and clearly WG2 turf, but where the assessment of changes in hurricanes should reside in WG1, chs 3 and (for attribution) 9.. (Gabi Hegerl, Duke University)	Accepted. Appropriate revisions have been made reduce overlap and to ensure consistency with the relevant WGI statements, as well as including references to the relevant WGI sections.
E-1-31	A	0				I think the authors should try to be clearer about which effects have been linked to climate change and which affects have not. In many parts of the chapter observed changes in systems are reported, and there is an implicit suggestion that these effects are all linked to anthropogenic climate change. For example the Sahel drought is mentioned several times in the chapter, but there is no discussion of	Rejected. The IPCC definition of climate change is used throughout, so that linking impacts to climate change just means linking them to significant changes in climate, not that the climate change is anthropogenic.

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						whether this is likely an anthropogenic affect (the evidence on this is equivocal), and the unwary reader might be left with the impression that it is anthropogenic. I think the authors should try to make clearer assessments of what is and what is not linked to anthropogenic climate change, rather than leaving this to the reader. (Nathan Gillett, University of East Anglia)	Section 1.3 links observed impacts to climate change, while section 1.4 links observed changes in physical and biological systems to anthropogenic climate change.
E-1-32	A	0				I think the authors should try to assess literature more rather than just reporting the results of individual studies. I think considerable progress has been made in this direction since the previous draft, but parts of the chapter would benefit from more synthesis and assessment. In particular, in some cases a particular affect is reported as having been linked or attributed to climate change by a particular study, with no overall assessment of whether this is just speculation by one study's authors, or the result of quantitative analysis where multiple studies are in agreement. (Nathan Gillett, University of East Anglia)	Accepted. Revised to address this point where possible.
E-1-33	A	0				I have three overarching suggestions to improve the clarity and robustness of this chapter. Firstly, the current draft tends to blur the distinction between observed effects that are attributed to temperature changes per se, and those changes that are explicitly attributed to temperature changes that are due to GHG emissions. This is partly due to wording, and partly due to the structure which frequently refers to attribution to gobal warming in section 1.3, but the actual attribution studies are only introduced in section 1.4. It might help to restructure this chapter to maintain this important distinction, e.g. by first describing all the effects that are attributed to temperature change per se, without attribution to causes of the temperature changes. The final section could then focus on describing the method for attributing changes to GHG emissions and listing the effects that can on that basis be attributed to GHG emissions. The second issue is that the method for attribution used in the underlying chapter is significantly different from the method used by WG1, and used in previous assessments. In particular, chapter 1 does not generally demonstrate that there is no alternative physically plausible explanation for the detected temperature changes. The studies chapter 1 relies on only do a statistical comparison between forced and unforced model runs, but don't look at the role of specific modes of oscillation and unforced variability that may not have been captured, especially at the relevant local scales, by the GCM runs analysed. Many more model runs, statistical and physical tests would be required to establish this. In addition, the methodology of this chapter does not quantify the amount of variability that can be explained by GHG forcing. It would improve consistency between Working Group reports and increase the defensibility of the findings of this chapter if the word "attribution" were used only where a comprehensive and	The chapter uses the IPCC definition of climate change throughout. Global warming does not mean anthropogenically-forced climate change, although the reviewer appears to interpret it that way. Section 1.3 has been revised to refer to regional warming, as this is what is affecting the systems. It is intended to do exactly what the reviewer suggests, concentrate on significant observed changes in systems and the3ir links to climate change, whatever the cause. As noted, section 1.4 considers attribution of observed changes to anthopogenically-forced climate change. The elimination of alternative explanations for the observed changes is described in more detail now. Consistency with WGI conclusions has been a primary factor in revisions. The joint attribution definitiona does not require that greenhouse gas forcing is the dominant cause of the response, only that there is a significant detectable contribution.

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						quantitative assessment has been carried out, including testing for regional or local modes of unforced variability. This would generally apply where a direct reference can be made to findings of WG1 (which has carried out these types of more comprehensive tests). For the other very important findings of this chapter, I would suggest employing a phrase such as "changes that are consistent with the expected global warming signal and cannot readily explained by natural variability alone as simulated by current models" (or hopefully something shorter that retains the important distinction to quantified and pattern-tested attribution). The third overarching issue is that the language is often open to mis (=over) intepretation. For example, stating that something has been attributed to GHG would imply that GHGs are the dominant, if not sole, cause for the change. However, the methodology of this chapter provides no evidence that GHGs are the sole causes of the observed changes, because no such quantification has been undertaken. It would therefore improve robustness of these findings if suitable qualifiers were inserted in all major statements, along the lines of "GHGs are likely to have contributed to", or "part of the observed changes can be attributed to", or perhaps "have produced a discernible human influence". (Andy Reisinger, IPCC SYR TSU)	Text has been revised to make this clearer.
E-1-34	A	0				I am confused that no one contributor from Northern Eurasian states (former USSR and Mongolia) was invited to contribute to Chapters 1 and 2. The environmental changes there (in particular, in Siberia and Central Asia) are among the largest in the world, are overlapping with socioeconomic changes, and ... are not assessed by specialists from this region. I realise that this mishap is now impossible to fix but foresee gaps, biases, and low quality statements when the contributions of outsiders unfamiliar with the region are used as substitutes. Sorry to start with such negative comment but this is the case. (Pavel Groisman, University Corp. for Atmospheric Research)	We invited Alla Tysban and Caarel Orviku to be Lead Authors, but both declined to participate.
E-1-35	A	0				Generally I felt the chapter read very well, followed a consistent structure throughout the sections, and was more concise than the First Order Draft, and yet retained an appropriate and complete content. (Robert Wilson, Universidad Rey Juan Carlos)	Thank you.
E-1-36	A	0				General- this chapter is an impressive compilation and synthesis of the literature and obviously great efforts have been made to reduce its length and structure it since earlier iterations. One caveat, we are aware in the UK, that the recent past is not necessarily defienitive on climate variability, due to our historic datasets.These data sets will not be available in all countires but some caveats should be made in the appropriate places.One suggestion- perhaps the authors could consider what the	New section 1.5 discusses further research.

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						gaps are for further research and ensure that all possible implications (not stated but for policy-makers) have been extracted. (Merylyn Hedger, Environment Agency)	
E-1-37	A	0				First, I would like to complement the authors of this Chapter on having produced a very much improved version of the original. I enjoyed reading the chapter and appreciate the enormous challenge that was faced by authors in attempting to synthesize such a diversity of information. I hope that you will find the remainder of my comments to be useful. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Thank you.
E-1-38	A	0				Final Overall comment: The present SOD for ch 1 is still too long and must be shortened. Also other chapters of WGII are rather long and can be and must be shortened. Citing large number of references does not necessarily make a good and readable Document. Attempts must be made to shorten the Document by at least 10%. (Madhav Khandekar, Retired)	Chapter revised and shortened.
E-1-39	A	0				Comment 4 continued … Given the concerns about the reliability of grid-point-scale model results (see comment 3), it would be appropriate to add some additional caveats to the discussion of Figures 1.18 and 1.19. I think these are useful diagrams (although perhaps only one is needed), but the text should make it clear to readers that they should not read too much into agreement between the direction of temperature change and that of a phenological change at a given location on the map. It should be made clear that the main purpose of the map would be to illustrate that there is similarity between the broad pattern of temperature change and that of phenological change. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Accepted. One figure deleted and additional caveats included in text, as suggested. Language revised to be consistent with WGI.
E-1-40	A	0				Comment 4 continued ... The evidence supporting such a "discernable human influence" statement would be (a) the WG1 assessment that anthropogenic influence is detectable in regional temperatures, (b) correlation between hemispheric mean simulated temperature (sampled where there are phenological studies), and hemispheric mean phenological date, where the correlation is better if ANT forcing is included than if NAT forcing only is used, (c) association between the spatial pattern of observed or simulated temperature change (not sure which is actually presented) and that of phenological change (Figs 1.18, 1.19, Table 1.12), and (d) supporting process understanding that indicates the direction of response to expect. However, I don't think this is sufficient to claim attribution because of the possibility of confounding effects, lack of estimates of the natural internal variability of phenology trait changes in the absence of forcing, and insufficient	Agreed. The global synthesis of studies in the chapter, as well as the end-to-end joint attribution studies, provide very strong evidence that human-caused warming over the last three decades has had a significant influence on many physical and biological systems.

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						information from the process models that would link forcing to response in an end-to-end manner. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-41	A	0				climate change' is used many times in this chapter, apparently to mean 'anthropogenic climate change'. According to the standard IPCC definition, climate change can also be caused by natural forcing, and therefore the authors should use the qualifier 'anthropogenic' if this is what they mean. (,)	Diagree. The IPCC definition of climate change is used throughout the chapter and anthropogenic climate change is used where that is intended.
E-1-42	A	0				Chapter 1 represents a substantial improvement on the documentation of observed changes in the 2005 FAR. The writters are to be commended on achieving a more coherent and consistent overview of a range of complex issues. Particularly welcome is the call for improvement in observation networks across much of the globe. This is the key to obtaining better reconciliation between model outputs and signals emerging from hydrometeorological monitoring programmes.The IPCC's focus on peer-reviewed material is understandable but in indexing change, greater engagement is needed with institutions responsible for national/international hydrometeorological monitoring programmes. This would increase the chances of identifying representative long time series and allow interpretation of apparent trends to be undertaken with greater confidence.The complete absence of time series illustrating representative trends in precipitation, runoff, extreme river flows and groundwater levels invites scepticism regarding the robustness of 'emerging evidence in hydrology and water resources…….' whilst several of the featured Tables are very thin in terms of coverage and, often, include apparently contradictory evidence.The impact of major floods and droughts on lives and livelihoods is such that they merit greater attention than they are currently afforded in Chapter 1.The balance of the report would be improved if (where appropriate) greater attention was given to factors which help to ameliorate risk as well as those that increase vulnerability to climate-driven threats. For example, whilst tidal flood risk is increasing in a warming world, the decreasing role of snowmelt, frozen ground and ice-jamming has been generally beneficial in the 200 years since the end of the Little Ice Age (across many mid-latitude regions). A stronger distinction should be drawn between those observed trends which are sufficiently compelling to drive policy-making (e.g. the cryosphere's response to warming) and those where, as yet, the observational evidence is inconclusive (e.g. trends in the magnitude of extreme fluvial floods). Little is said on climatic variables (e.g. rainfall) for which many trend analyses have been undertaken. (Terry Marsh, Centre for Ecology and Hydrology)	Thank you. Issues discussed are not the topic of Chapter One. Some are covered by WGI.

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E-1-43	A	0				Boxes in the chapter is now much more organized and less confusing. (Giampiero Maracchi, Institute of Biometeorology)	Good.
E-1-44	A	0				An 'intensification of the hydrological cycle' is mentioned several times in the chapter, with no description of what this means, no references, and no discussion of whether such an affect has indeed been observed. Global precipitation has not increased significantly, but there may be some evidence of anthropogenic influence on regional precipitation. See sections 9.5.4 and 3.3 in WG1. (Nathan Gillett, University of East Anglia)	Intensification comment removed.
E-1-45	A	1	4	1	4	It would be better to use the word 'Human' instead of 'Managed' in the title for this chapter. Certainly 'human' is used throughout the Executive Summary (page 3, line 5; page 4, line 51), Introduction section (page 6, lines 5, 7, 25, and 48; page 7, lines 38, 41, and 42) and no less than 92 times in the chapter as a whole. 'Managed' appears only once in the Executive Summary and Introduction (page 3, line 29) and only 13 times in the chapter as a whole. Managed implies a sense of human control which may not apply to all/many human systems. (Paul Beggs, Macquarie University)	Rejected. Chapter title is specified by Plenary Approved Outline.
E-1-46	A	3	0	5		I think it would be easier to verify the contents presented in the sentences in bold if they included a reference to where this content can be found in the report. For example, Page 3, Line 21: "Many of the climate-driven observed changes..." could have a reference to the chapter, section, page and/or line where this affirmation is extracted from. (Pedro Ribera, Universidad Pablo de Olavide)	Done.
E-1-47	A	3	0	5		Executive Summary: 1. The present summary is essentially same as before with very little substantive changes. As suggested in my FOD review, specific statements on beneficial impacts of climate change must be included 2. Non-climatic drivers must include solar variability and its impact on earth's climate as documented in many recent peer-reviewed studies. 3. Reference to cryosphere reduction is too general and vague with NO specific mention about the fact that NOT all glaciers are shrinking uniformly and that the Antarctic sea ice has increased in extent and concentration in the last 25 years. 4. Observed increase in forest productivity due to lengthening of the growing season and also due due increased concentration of carbon dioxide must be highligted as beneficial. 5. Global losse are primarily due to societal changes as documented in several published studies by Changnon. Referring to unpublished study to document increased global losses while ignoring published studies which document otherwise, is unscientific and unacceptable. (Madhav Khandekar, Retired)	Rejected mainly, accepted in part. 2. Solar variability is a climate driver. 3. Description of observed changes in cryosphere is based on WGI assessment 4. Increase in NPP over land now shown in Figure in Ag and Forestry section. 5. Global losses were normalized for social and economic changes and reveal a small residual trend, although dominant trend us due to societal changes..

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E-1-48	A	3	1	72		Following are citations not provided within individual comments. Most of these can be obtained at http://members.cox.net/igoklany/ Goklany, IM. 1995. Strategies to Enhance Adaptability: Technological Change, Economic Growth and Free Trade. Climatic Change 30: 427-449. Goklany, IM. 1998. Saving Habitat and Conserving Biodiversity on a Crowded Planet. BioScience 48 (1998): 941-953. 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: Global and U.S. Trends, 1900-2004, Climate Change and Disaster Losses Workshop, 25-26 May 2006, Hohenkammer, Germany. (Indur Goklany, US Department of the Interior)	References suggested are mostly not applicable to the chapter. Please provide Workshop Report.
E-1-49	A	3	1			It should be noted up-front that the causes of "anthropogenic climate change" include greenhouse gas forcing as well as changes due to land use and land cover (including the urban heat island effect). (Indur Goklany, US Department of the Interior)	Noted.
E-1-50	A	3	12	3	12	systems and sectors' should be defined somewhere. Is this different from 'natural and managed systems' as in the chapter title? Does it just mean 'things', or is the definition more specific? This was a point I raised in my previous review, which I don't think has been completely addressed. (Nathan Gillett, University of East Anglia)	Sectors removed. Systems defined.
E-1-51	A	3	12	3	14	I found this sentence confusing. The first point made by the sentence is simple - non-cliamatic drivers can influence sectors and systems. However, the second point seems to be that non-climatic drivers can influence the climate (presumably things such as sulphate aerosol or CO2). This seems to be confusing two different aspects of the problem, and I think it would be better to focus on the first of these here. (Nathan Gillett, University of East Anglia)	Rewritten
E-1-52	A	3	18	3	18	Change "to dampen observed impacts" to "to dampen (or perhaps enhance, in cases of unintended consequences) observed impacts". (Claire Parkinson, NASA Goddard Space Flight Center)	Sentence rewritten.
E-1-53	A	3	21	3	26	To my knowledge, no study has yet demostrated the attribution of post-1970 regional temperature changes to greenhouse gas emissions.	Disagree. Karoly and Wu (2005) and Karoly and Stott (2006) detect anthropogenic

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						(Daithi Stone, University of Oxford)	warming at regional scales over last 30 years.
E-1-54	A	3	21	3	26	This statement seems overconfident because of the double step method used. Just because some observed change can be attributed to a regional warming and the regional warming can be attributed to the emissions does not automatically imply that that observed change has been attributed to the emissions. Both of the attribution statements are probabilistic in nature (whether stated quantitatively or qualitatively), meaning power is lost when they are linked together. For instance, even if you put a 70% likelihood on both attribution statements, you would end up with a less than 50% likelihood on the linked statement. (Daithi Stone, University of Oxford)	Disagree. We agree with the description of the method but our assessment of the probability of the individual steps is much higher than suggested based on elimination of alternative explanations from a global assessment.
E-1-55	A	3	21	3	22	This should be rewritten as follows: "Many of the climate-driven observed changes in physical and biological systems are consistent with greenhouse gas induced warming." (Indur Goklany, US Department of the Interior)	Rejected. Text has been revised but not exactly as suggested.
E-1-56	A	3	21	3	22	As discussed in my general comment on this chapter, I don't think use of the word "attributed" is correct here because attribution without qualifiers would imply that GHGs are the sole cause of the observed changes (ie it would require that GHGs have been found to be responsible for close to 100% of the observed change, and there is no physically plausible alternative reason for the change, including unforced variability. The studies on which chapter 1 relies don't produce these findings). But a modification in wording towards "discernible human influence", or "many changes are consistent with the signal expected under global warming and have no ready alternative explanation in natural climate variability" would appear to be entirely justfied on the basis of this assessment. (Andy Reisinger, IPCC SYR TSU)	Partially accepted. Attribution definition does not require that ghg increases are the sole or even dominant cause of the observed changes. However, text has been revised along lines suggested.
E-1-57	A	3	21		26	The assessment that changes can be attributed to temperature increase is using different methods (called joint attribution in the non-bold text, which suggests stronger results than "simple" attribution but actually is quite a bit weaker). I suggest to change the terminology here or at the minimum, caveat.Also, your assessment sounds to me like you attribute ALL changes to anthropogenic drivers, at most (and I am not sure I am fully convinced at this point for the smaller scales),. you can attribute a part of the changes to greenhouse gases. (Gabi Hegerl, Duke University)	Rejected. The assessment of joint attribution in the chapter is based on the definition in the chapter, which does not imply stronger results than simple attribution. The method and results are described more carefully in section 1.4 now. Text has been revised to take account of these issues.
E-1-58	A	3	24	3	26	The sentence says that "Observed responses..." have been attributed. This would mean that ALL observed responses have been attributed - which is clearly incorrect. Please choose an appropriate qualifying word or phrase, such as "Several large scale observed responses and statistical changes ..." Please also note my	Accepted. Text has been revised.

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						general comment that using the word "attribution" implies a quantification of the role of GHG forcing, and use of the word without this meaning leads to an inconsistency between Working Groups. Please find an alternative word or phrase that accurately captures the approach taken by chapter 1 but does not imply quantification. "Discernible human influence" would be useful in many instances (and consistent with the description by WG1); alternatively or in addition, the authors could also state that "many observed responses cannot readily be explained by natural climate variability as simulated by a number of GCMs, but are qualitatively consistent with changes expected when forcing by GHG emissions is taken into account". These statements would be perfectly justifiable, but given that we don't generally have a priori quantitative relatinships between effects and GHG forcing, it is very hard to argue that a quantitative causal relationship (as required for the word "attribution") has been demonstrated. (Andy Reisinger, IPCC SYR TSU)	
E-1-59	A	3	24	3	26	I don't think that the requirements for joint attribution, as set out in the definitions, have been met. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Disagree. More information is provided in section 1.4. Text has also been revised here.
E-1-60	A	3	24			Is the “jointly attributed” referring to the two step attribution process or to a collective attribution study, ie that the collective responses have been attributed. It sounds like the latter, but I think you mean the former. If it is the former then perhaps “each sequentially attributed” would be better. (Daithi Stone, University of Oxford)	Rejected. Joint attribution is based on the two-step process, as defined in the chapter, and a global assessment of the studies considered in teh chapter and the end-to-end joint attribution studies.
E-1-61	A	3	25	3	26	I do not think that regional warming can be attributed to anthropogenic climate change, partly because we are still uncertain as to exactly what that anthropogenic climate change is, but also because regional warming is a manifestation of climate change. I think the attribution statement would have to be between the warming and the greenhouse gas emissions. (Daithi Stone, University of Oxford)	Agreed. Text revised.
E-1-62	A	3	27	3	27	Add: .Systematic space distribution of the impacts of the warming on the areas of physical and biological systems in the extra-tropical zones and in mountains. . Relations between impacts of the warming on physical and biological systems and between physical or biological systems and human socio-economic systems . (Annick Douguédroit, University de Provence)	Rejected. Unclear what the reviewer wants changed.
E-1-63	A	3	28	3	42	Regarding impacts of climate change on permafrost-based infrastructure - It is probably not fair to say there is clear evidence that climate change is having an impact and concluding this with high confidence is probably not appropriate. Chapter 15 does not make these statements and it includes a review of a larger body	Infrastructure statements removed.

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						of literature than Chapter 1 (futher details in other comments on ch 1). If the statement is meant to only apply to changes in permafrost conditions that is appropriate (for some regions there is evidence of this) but not appropriate to apply the statement to impacts on infrastructure. (Sharon Smith, Natural Resources Canada)	
E-1-64	A	3	28	3	28	Insert 'anthropogenic' before 'climate change'. (Nathan Gillett, University of East Anglia)	Difference between regional and anthropogenic climate change clarified throughout chapter.
E-1-65	A	3	28			Insert at the end of this line just after "climate change", the following; "… ,whether induced by GHG emissions or not, ..." (Indur Goklany, US Department of the Interior)	Difference between regional and anthropogenic climate change clarified throughout chapter.
E-1-66	A	3	34			It is a bit strange to make sea level rise part of the cryosphere. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Sea level change is a direct impact of cryospheric changes.
E-1-67	A	3	40	3	40	Add before "and changes in" at the end of the line: "shipping routes in the Arctic opened by lessened sea ice cover,". (Claire Parkinson, NASA Goddard Space Flight Center)	OK, added.
E-1-68	A	3	44	3	46	This statement seems strong compared to the conclusions of WG1, chapter 4. (Nathan Gillett, University of East Anglia)	OK, statement has now been changed, in accord with WG1, Ch4.
E-1-69	A	3	44	3	46	I suggest that the authors reconsider the wording of "changing in response to global warming". In most instances, global warming caused by GHG emissions may have contributed to changes, but is certainly not the only driver for the observed changes (which is what most readers would interpret the phrase to mean). The wording should avoid the misinterpretation that GHG emissions are THE SOLE reason for the observed change. The phrase "have contributed to the observed changes", or "have had a discernible human influence" (useful language from the SAR) will be more correct in most instances than "in response to global warming". Note that WG1 only finds a "discernible human influence" on changes other than temperature (eg on sea-ice), it does not say that changes in sea-ice etc can be attributed to anthropogenic forcing because this would imply that all of the observed change is entirely due to anthropogenic forcing. Current models and observational constraints do not allow such a quantification to be made at present. We also need to be careful about other confounding factors; eg the collapse of ice shelves, as a general statement, cannot be attributed because at least for the Antarctic Peninsula we know that ozone depletion and related circulation changes significantly contributed to the warming. Neither is it clear that the retreat of parts of the Antarctic ice sheet can be attributed, given that Antarctica is projected to grow over the 21st century; we don't sufficiently understand the source of the current dynamic imbalance in the WAIS.	OK, changed to “atmospheric warming”.

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						(Andy Reisinger, IPCC SYR TSU)	
E-1-70	A	3	46	3	46	Expand "is undergoing melting" to "is undergoing melting in many (although not all) areas". (Claire Parkinson, NASA Goddard Space Flight Center)	Agreed, added.
E-1-71	A	3	47	3	49	the hydrological cycle is intensifying' - it's not clear to me what this means. There is no evidence for a global increase in precip, which is what I think this implies. (Nathan Gillett, University of East Anglia)	Text revised.
E-1-72	A	3	48			We (LA's of WG I, Ch 3) have had a discussion on the "intensifying hydrological cycle" and concluded that this wording is better not used. Instead we say that "hydrologic conditions have become more extreme". (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Text revised.
E-1-73	A	3	48		49	Should WG1 talk about droughts and fllods? Surely about impacts, droughts themselves are more discussed in WG1 (Gabi Hegerl, Duke University)	Droughts and floods are very important for WGII.
E-1-74	A	3	50	3	51	Peterson et al. simply did not show that the trend was anything different than natural variability. Rigorous detection and attribution of runoff change simply requires a larger data set. (Christopher Milly, U.S. Geological Survey)	Text removed in ES.
E-1-75	A	3	51	3	51	Trends in runoff and precipitation in Eurasian Arctic are not "simply" agree but require sophysticated hydrological modeling to be reconsiled. For example, runoff is increasing but precipitation during the past 40- 50 years is NOT increasing and summer precipitation over most of northern Eurasia goes down. (Pavel Groisman, University Corp. for Atmospheric Research)	Text removed in ES.
E-1-76	A	3				Executive summary. Few over-arching take-home messages have been distilled from the main body of Chapter 1. Such synthesis is challenging but policy makers will need some preliminary guidance - particularly given the potential costs of amelioration/mitigation of climate-driven threats.The runoff and drought evidence referred to is important but relates primarily to very thinly peopled parts of the globe. The ES would have greater impact if some statements were included which addressed the possible impacts of global warming in the Tropics and mid–latitudes. Where data or knowledge gaps are a substantial stumbling block it is important to emphasis the need for progress in these areas. If the evidence, as yet, suggests little divergence from the variability captured in long term records, this should be acknowledged. (Terry Marsh, Centre for Ecology and Hydrology)	Hydrolgy text rewritten in the ES.
E-1-77	A	4	1	4	2	The literature can justify a stronger statement than this. Please read Milly et al. (2005, Nature, Global pattern of trends...).	Hydrolgy text rewritten in the ES.

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						(Christopher Milly, U.S. Geological Survey)	
E-1-78	A	4	1	4	2	It is almost obvious that areas affected by droughts are located in drier regions. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	No. Disagree.
E-1-79	A	4	1		3	There is some material in this paragraph that is clearly WG2 material, but also some, such as droughts and drying in dry regions / nonconsistent changes in wet regions which sounds like WG. This should be rephrased to clarify what is assessed in this chapter. (Gabi Hegerl, Duke University)	WGII is very concerned about droughts and floods.
E-1-80	A	4	9	4	10	This seems to imply that the Sahel rainfall reduction is anthropogenic. The authors should be more clear about whether or not they are attributing this to anthropogenic climate change or natural variability, or whether this is not known. (Nathan Gillett, University of East Anglia)	Sahel text removed.
E-1-81	A	4	14			This bullet should be modified to note that storm intensities have not increased everywhere over the past few decades. More importantly, one cannot talk intelligently about climate change if one looks at only 30-40 years worth of data. Climate change should be viewed in the context of a much longer based on long term records (including paleo records) indicate that storms in many areas were more equally intense at otehr times. See, e.g., Nott and Hayne (2001); Keim et al. (2004) and Balling and Cerveny (2003) for the North Atlantic; Boose et al (2001) for hurricanes in New England. References: (1) Keim, B.D., Muller, R.A. and Stone, G.W. 2004. Spatial and temporal variability of coastal storms in the North Atlantic Basin. Marine Geology 210: 7-15. (2) Balling Jr., R.C. and Cerveny, R.S. 2003. Analysis of the duration, seasonal timing, and location of North Atlantic tropical cyclones: 1950-2002. Geophysical Research Letters 30: 10.1029/2003GL018404. (3) Boose, E.R., Chamberlin, K.E. and Foster, D.R. 2001. Landscape and regional impacts of hurricanes in New England. Ecological Monographs 71: 27-48. (Indur Goklany, US Department of the Interior)	Hydrology text in ES rewritten.
E-1-82	A	4	14		16	Please keep in touch with WG1 extremes table for executive summary and chapter material. (Gabi Hegerl, Duke University)	Done
E-1-83	A	4	14			It should also be noted that in some areas wave heights have apparently declined. [See text on page 31, lines 49-50; and Table 1.5, last row. See also, Gulev and Grigorioeva (2004), which indicates that wave heights in other places might have been just as high if not higher in earlier decades of the 20th century. They relate this to the NAO, ENSO and the NPO. See also Dawson et al (2002). For a longer term view, which would be appropriate given that this report is on climate change,	Review comment forwarded to CA.

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						look at Nott and Hayne (2001). References: (1) Gulev, S.K. and Grigorieva, V. 2004. Last century changes in ocean wind wave height from global visual wave data. Geophysical Research Letters 31: 10.1029/2004GL021040. (2) Nott, J. and Hayne, M. 2001. High frequency of 'super-cyclones' along the Great Barrier Reef over the past 5,000 years. Nature 413: 508-512. (3) Dawson, A.G., Hickey, K., Holt, T., Elliott, L., Dawson, S., Foster, I.D.L., Wadhams, P., Jonsdottir, I., Wilkinson, J., McKenna, J., Davis, N.R. and Smith, D.E. 2002. Complex North Atlantic Oscillation (NAO) Index signal of historic North Atlantic storm-track changes. The Holocene 12: 363-369. (Indur Goklany, US Department of the Interior)	
E-1-84	A	4	20	4	21	Thermal expansion and meltwater cannot explain why some regions experience larger increases in sea level than the global mean - this just explains the global mean rise. (Nathan Gillett, University of East Anglia)	Coastal section of ES rewritten.
E-1-85	A	4	20	4	21	I would consider sea level rise to be a manifestation of climate change, rather than a result. (Daithi Stone, University of Oxford)	Coastal section of ES rewritten.
E-1-86	A	4	20			Is this figure of 1.7mm/yr for the period since 1970? Is this true for all trends that are specified without period indication? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Clarified in text.
E-1-87	A	4	32	4	34	This statement seems to be almost true by definition. It states that studies which show a significant warming impact on terrestrial species show consistent responses. What is of more interest is whether there are more studies showing a significant warming impact on terrestrial species than studies showing a significant cooling impact. (Nathan Gillett, University of East Anglia)	ES TBS text rewritten.
E-1-88	A	4	39	4	39	Please be clearer when you use the phrase "climate change": is this climate change in the IPCC definition, ie any change in climate regardless of cause, or is this in the sense of the more common perception and interpretation, ie in response to GHG emissions? This is a critical issue and it would be helpful if the text were explicit, otherwise there is too much room for misinterpretation and misquotation. (Andy Reisinger, IPCC SYR TSU)	IPCC definition of climate change used. Section 1.3 assesses observed responses of systems to all regional climate changes, while section 1.4 considers anthropogenic climate change.
E-1-89	A	4	39	4	39	It's not clear to me what "disappearance" means in this line. Are you referring to butterfly extirpation or extinction? I'd suggest using a more specific term in place of "disappearance." (Sarah Shafer, U.S. Geological Survey)	LAs stand by this term.
E-1-90	A	4	39	4	39	….key examples of the disappearance of butterflies.	Butterflies removed.

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						(Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	
E-1-91	A	4	39			ones should probably read "extinction" instead of "dispapearance"? (Isabelle Chuine, CNRS)	LAs stand by this term.
E-1-92	A	4	44	4	44	insert of between degrees and latitude (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Disagree.
E-1-93	A	4	46	4	47	I found this wording a little confusing. It does not separate the effects of climate change and climate variability - the reader is left wondering whether climate variability has always caused significant damage to coral reefs. (Nathan Gillett, University of East Anglia)	Text rewritten.
E-1-94	A	4	48	4	48	insert s after lake (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.
E-1-95	A	4	51			The word "responses" in this sentence can only be understood if the definition on page 7, line 38 is known. I prefer the word "changes", which is also used for the other systems. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	The term 'responses' has mainly been removed from chapter.
E-1-96	A	5	7			Insert after "lengthening of the growing season" the following: "combined with higher CO2 levels and nitrogen emissions most likely…" (Indur Goklany, US Department of the Interior)	Disagree. Chapter focuses on temperature changes.
E-1-97	A	5	11	5	14	Because of its scale and importance some reference should be made to the increases in forest fires as well as the generalities. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Forest fires added.
E-1-98	A	5	13	5	14	Are the authors attributing recent drought and floods in wine-growing regions to anthropogenic influence? I would be surprised if they could be attributed to human influence. (Nathan Gillett, University of East Anglia)	Regional and anthropogenic climate change differentiated throughout chapter.
E-1-99	A	5	13			Staret the new sentence beginning on line 13 with the following: "Against a background of higher productivity, however, …" (Indur Goklany, US Department of the Interior)	Disagree. Focus is on changes related to temperature.
E-1-100	A	5	16	5	23	No reference is made to tropospheric ozone nor to any other contaminant. (Pedro Ribera, Universidad Pablo de Olavide)	Pollution not a topic in chapter.
E-1-101	A	5	17	5	17	Please be clearer when you use the phrase "climate change": is this climate change in the IPCC definition, ie any change in climate regardless of cause, or is this in the sense of the more common perception and interpretation, ie in response to GHG emissions? This is a critical issue and it would be helpful if the text were explicit, otherwise there is too much room for misinterpretation and misquotation. (Andy Reisinger, IPCC SYR TSU)	IPCC definition of climate change used. As this is an IPCC assessment, we always use the IPCC definition.
E-1-	A	5	17			I think it should be stressed that there have been few, if any, empirical studies to	We do stress that there are very few health

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102						attempt to identify whether there has been a health impact of climate change. This is because the studies needed to do this are very difficult, because of the problems with collecting long, historical, consistent time series of health outcomes. Even for a simple aspect such as heat wave impacts it has been difficult to do this. So the lack of "evidence" mentioned here is more about the problems with doing the appropriate studies (this contrasts with agriculture and other impacts where long time series do exist). This statement, as currently written, suggests that there have bee attempts to determine these health impacts and that these have been unsuccessful - I think this is misleading. (Neville Nicholls, Monash University)	studies.
E-1-103	A	5	21	5	23	Add the following at the end of this bullet; "However, many effective adaptation measures are available to cope with such heat waves, and there is evidence that populations can acclimitize to current climate." See Chapter 8, page 12, lines 1-10, and Chapter 14, page 13, lines 18 to 23. (Indur Goklany, US Department of the Interior)	OK Text rewritten.
E-1-104	A	5	22	5	23	Health effects due to heatwaves have been demonstrated in Asia, and there is text in Chapters 8 (8.2.1.1) and 10 (Jean Palutikof, IPCC WGII TSU)	Consulted with Chapter 8.
E-1-105	A	5	22	5	22	Are there also other examples, e.g., Chicago? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Consulted with Chapter 8.
E-1-106	A	5	25	5	36	There should be a parallel bullet devoted to global deaths and death rates due to extreme weather events because that is just as, if not more, important than economic losses. This bullet should be 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, 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 in earlier decades. 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.” References: Goklany (2006b, 2005b). (Indur Goklany, US Department of the Interior)	See Chapter 8.

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E-1-107	A	5	30	5	31	This sentence is incomplete: It says "For specific regions and perils … there is evidence for an increase in occurrence." Of what? (Andy Reisinger, IPCC SYR TSU)	Disasters and Hazards removed from ES.
E-1-108	A	5	30	5	31	This bullet should be modified to note that studies examining large numbers of rivers suggest that in many, if not most, other cases maximum runoff rates have either not changed or declined . See, e.g., (1) Cluis, D. and Laberge, C. 2001. Climate change and trend detection in selected rivers within the Asia-Pacific region. Water International 26: 411-424. (2) Svensson, C., Kundzewicz, Z.W. and Maurer, T. 2005. Trend detection in river flow series: 2. Flood and low-flow index series. Hydrological Sciences Journal 50: 811-824. (3) Lindstrom, G. and Bergstrom, S. 2004. Runoff trends in Sweden 1807-2002. Hydrological Sciences Journal 49: 69-83. (4) Mudelsee, M., Borngen, M., Tetzlaff, G. and Grunewald, U. 2003. No upward trends in the occurrence of extreme floods in central Europe. Nature 425: 166-169. (5) Mudelsee, M., Borngen, M., Tetzlaff, G. and Grunewald, U. 2004. Extreme floods in central Europe over the past 500 years: Role of cyclone pathway "Zugstrasse Vb." Journal of Geophysical Research 109: 10.1029/2004JD005034. (6) Kundzewicz, Z.W., et al. 2004. Detection of change in world-wide hydrological time series of maximum annual flow. GRDC Report Series, Report 32. (9) Walling, D.E. and Fang, D. 2003. Recent trends in the suspended sediment loads of the world's rivers. Global and Planetary Change 39: 111-126. (Indur Goklany, US Department of the Interior)	References checked.
E-1-109	A	5	30			The specific reference to 'extreme floods on some of the largest rivers' is supported by few studies and very scant observational evidence (it appears to reflect a single study which speculatively noted an in increase in 100-yr RP events). Other studies contradict this assertion. Arguably, a stronger case could be made for increasing urban vulnerability to higher intensity convective storms. (Terry Marsh, Centre for Ecology and Hydrology)	Statement removed in ES.
E-1-110	A	5	32	5	36	This bullet should be modified to note that storm intensities have not increased everywhere over the past few decades. More importantly, one cannot talk intelligently about climate change if one looks at only 30-40 years worth of data. Climate change should be viewed in the context of a much longer time frame and should be based on long term records (including paleo records). There are data that indicate that storms in many areas were more or equally intense at other times. See, e.g., Nott and Hayne (2001); Keim et al. (2004) and Balling and Cerveny (2003) for the North Atlantic; Boose et al (2001) for hurricanes in New England. References: (1) Keim, B.D., Muller, R.A. and Stone, G.W. 2004. Spatial and temporal	ES statement revised.

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						variability of coastal storms in the North Atlantic Basin. Marine Geology 210: 7-15. (2) Balling Jr., R.C. and Cerveny, R.S. 2003. Analysis of the duration, seasonal timing, and location of North Atlantic tropical cyclones: 1950-2002. Geophysical Research Letters 30: 10.1029/2003GL018404. (3) Boose, E.R., Chamberlin, K.E. and Foster, D.R. 2001. Landscape and regional impacts of hurricanes in New England. Ecological Monographs 71: 27-48 (Indur Goklany, US Department of the Interior)	
E-1-111	A	5	32	5	36	Include the word hurricane somewhere as the more extreme form of cyclone in the Atlantic as many readers will be aware of this word rather than cyclone especially in the light of the 2005 Hurricane Katrina event and the records broken last year. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	ES statement on D&H removed.
E-1-112	A	5	32	5	36	I think a caveat is needed on the tropical cyclones bullet. The Ch 9 WG1 assessment is that it is more likely than not that there has been an anthropogenic influence, but we say that with low confidence (see Table 9.7.1 and supporting text in 9.5.3.6). This will be revised a bit in the 3rd order draft, but our assessment will remain conservative because of problems with putting the observed changes over the past 3 decades in a longer historical context, some lingering doubts about the data, and because process knowledge is incomplete (e.g., as indicated by the fact that the modelling community has not been able to simulate changes that agree in magnitude with those that have been observed). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Revised to be consistent with WGI.
E-1-113	A	5	32		36	This is a clear overlap with WG1 assessments, and I think it should not be discussed here. The assessment of storm damages in your body of chapter sounds more relevant for your ES to me than the physical storm assessment. (Gabi Hegerl, Duke University)	Statements checked with WGI.
E-1-114	A	5	33			"significant" implies that a change has been "detected". In fact there appears to be a trend, but it is not known if this is consistent with natural internally generated variability or with natural variability. (Daithi Stone, University of Oxford)	D&H statement removed from ES.
E-1-115	A	5	34			"power dissipation" may be replaced by "potential destructiveness", which is more clear. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	D&H statement removed from ES.
E-1-116	A	5	36	5	36	…regions but in the North Atlantic ocean, tropical cyclones of the same categories were as much frequent during the 50's (1,3,8,3). (Annick Douguédroit, University de Provence)	D&H statement removed from ES.
E-1-117	A	6	1	7		Sections 1.1, 1.1.1 &1.1.2: summary of observed changes in TAR is again in general terms and rather vague. Damage due to recent droughts and extreme	Findings in TAR were general and vague. Text adequately reflects those findings.

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						precipitation events is NOT unusual when compraed to such events of the past and these droughts and extreme precipitaion events appear to be part of natural climate varibility and may have nothing to do with present climate change. A notable lack of reported studies on observed changes in developing countries (Asia & Africa in particular) makes the present assessment very inadequate. These two continents represent about 70% of the total world population which has not been significantly impacted by climate change in the last 25 years or more. This aspect of climate change must be highlighted here. (Madhav Khandekar, Retired)	Lack of studies in developing countries and possible reasons for that lack noted in text.
E-1-118	A	6	3	6	15	It would be helpful to have a definition of regions, systems and sectors as a footnote, in an annex or in supplementary material for the reader who is coming at the report cold. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Usage explained in text.
E-1-119	A	6	8	6	8	amend in to read with (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.
E-1-120	A	6	11			"Joint attribution" is introduced here, but the definition given here seems incomplete and differs from that on page 8, line 49. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Phrase removed from Introductory paragraph.
E-1-121	A	6	18	7	5	The order of Sections 1.1.1 and 1.1.2 could usefully be reversed for clarity - do the Scope and Goals first (Jean Palutikof, IPCC WGII TSU)	Done.
E-1-122	A	6	22	6	22	Insert 'in natural and managed systems' after 'detecting observed changes'. Detecting observed changes in climate was covered by WG1. (Nathan Gillett, University of East Anglia)	Done.
E-1-123	A	6	30	6	31	Are ice caps melting in Antarctica? My reading of WG1 Ch 4 is that overall mass balance for Antarctica could be positive to within the known uncertainties. (Nathan Gillett, University of East Anglia)	This is a summary of findings of what was said in WGII from the TAR, Chapter 19.2, not AR4. Removed 'ice caps' from text. Point taken. "outside of Greenland and Antarctica" has been added.
E-1-124	A	6	34	6	34	California - too localised? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Changed to 'southwestern N. America'.
E-1-125	A	6	34			ones should probably read "snowmelt and runoff had occured increasingly earlier…" ? (Isabelle Chuine, CNRS)	Done. Agreed, replaced by "both" in the text.
E-1-126	A	6	34			I still don't like "California" in this statement (too local), but probably it is said that way in the TAR. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Changed to 'southwestern N. America'.

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E-1-127	A	6	47	6	47	Add: Preminary evidence for a systematic space distribution of the impacts of the warming on the areas of physical and biological systems in the extra-tropical zones and in mountains. (Annick Douguédroit, University de Provence)	Not added because this section summarizes observed changes in TAR, which did not find preliminary evidence for a systematic spatial distribution.
E-1-128	A	6	49	6	50	Is this anthropogenic? (Nathan Gillett, University of East Anglia)	No – it is not anthropogenic. TAR focused on observed changes related to regional changes in climate, without designating whether the change was anthropogenic.
E-1-129	A	6	49	6	49	Amend damages to read Impacts x 2 damages on one line (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Changed to 'changes in some socio-economic systems.'
E-1-130	A	6	51	6	51	Add: Preminary evidence for relations between the impacts of warming on physical and biological systems and on physical or biological systems and human socio-economic systems . . (Annick Douguédroit, University de Provence)	Changed to 'changes in some socio-economic systems.'
E-1-131	A	7	5			The chapter focuses on almost only the responses in natural and managed systems to air temperature, however, there are also plenty of examples showing the biological responses to precipitation, such as in arid and semi-arid areas (grassland ecosystem, desert ecosystem, etc.), which should be strengthened. (Xiaoqiu Chen, Peking University)	Rejected. Most of such changes are not outside the range of natural variability, except in a few limited areas. Temperature change is the focus because it has been used to detect an anthropogenic signal in recent climate, while it is much harder to find an anthropogenic signal in precipitation.
E-1-132	A	7	11			Questions 2 and 4 are important and challenging. In relation to rainfall and, particularly, river flow, no real answer can be discerned from the material assembled in Chapter 1. Accepting the limited availability of appropriate hydrological time series, there still appears to be a limited consensus between modelled predictions and observational evidence (e.g. in relation to trends in fluvial flood magnitude). These should be more fully acknowledged - with an associated recognition that process representation in the models need to be improved. (Terry Marsh, Centre for Ecology and Hydrology)	Questions eliminated to focus the chapter on assessing changes related to regional climate change and then assessing aggregate changes to anthropogenic warming. They were also eliminated here and in the concluding section in order to shorten the chapter.
E-1-133	A	7	31	7	32	Define "ENSO" and "NAO" at first use. These are designed later (page 9, lines 45-46), but you should inform readers when the terms are first used. (Knute Nadelhoffer, University of Michigan)	Defined ENSO and NAO at first use.
E-1-134	A	7	32	7	36	It is difficult to follow what authors mean with this sentence ("From the studies... ...climate forcing"). Possibly it could be rewritten in a more clear way. (Pedro Ribera, Universidad Pablo de Olavide)	Sentence rewritten. Section 1.3 and Section 1.4 have been described more clearly and explicitly.
E-1-135	A	7	44	7	46	Is there no information from aboriginal groups in warmer latitudes, Australia for example?	Emailed Barrie Pittock from Australia to search for documentation of aboriginal

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						(Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	responses. Website references only, so not included.
E-1-136	A	7	44	7	46	All sources of indigenous knowledge of observed changes are limited to the Arctic when in fact there are other sources of information. For example, see WWF's webpage: http://powerswitch.panda.org/the_problem/climate_witnesses.cfm (Lara Hansen, WWF)	Search web and couldn't find the website cited by reviewer.
E-1-137	A	7	49	7	49	…developing countries, even concerning the extremes which impacts are only partly known. (Annick Douguédroit, University de Provence)	Rewrote sentence: There is a notable lack of geographic balance in data and literature on observed changes in natural and managed systems, with marked paucity in developing countries.
E-1-138	A	8	2	8	5	This is a research recommendation - not appropriate for an IPCC report. (Nathan Gillett, University of East Anglia)	Disagree. Left in. Some reviewers called for this.
E-1-139	A	8	8	11		Sections 1.2 & 1.2.1: Methods of detection etc-: Natural climate variability can be discussed only with longer data, e.g. 100 years or more. Many examples provided here are based on much shorter data sets (30 years or so) and are thus inadequate. Climate & non-climate drivers etc.: here changes in regional climate due to large-scale oscillations like PDO, ENSO, NAO are not too be confused with present climate change impact, since these large-scale oscillations have been known to vary in their magnitude and amplitude without being forced by anthopogenic changes. In the non-climatic drivers, solar variability and its impact on earth's climate and on specific weather elements like rainfall and droughts must be included. Land-use change and urbanization are now identified as having a significant impact on recent global mean temperature increase (ex.De Laat & Maurellis, 2006, International J of Climatology, 26, p. 897-913). In view of this, climate change on regional scale may be due to anthropogenic surface processes and NOT due to increasing concentrations of greenhouse gases. (Madhav Khandekar, Retired)	Rejected. Changes over three decades may be outside the range of natural variability if the change is large enough, as assessed in published studies. WGI assesses that the dominant cause of global-scale temeprature increase is increasing greenhouse gases, and not land use change.
E-1-140	A	8	8	15		I feel that it takes too long to get to the meat of the chapter from 1.3 onwards. The document is not reader friendly in its organisation. It would be much better if the methods section was in an annex and only summarised in section 1.2 with reference to the annex. I realise that it is probably too late to make changes of this kind, but thought should still be given to making the document more reader friendly. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Agreed. Sections 1.1 and 1.2 reduced in length.
E-1-141	A	8	29	8	30	The distinction with WGI, Ch 9 is vague given that climate models are becoming increasingly complex, including chemistry, biology and hydrology. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Disagree. This distinction is still valid as very few end-to-end studies of attribution of impacts are assessed in WGI, chapter 9

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E-1-142	A	8	32	8	33	attributing these as probably response to changes in climate' - does this mean responses to anthropogenic climate change? If so, say so. (Nathan Gillett, University of East Anglia)	No, it means to observed climate changedue to any cause.
E-1-143	A	8	39			Mention here the attribution of a significant part of changes to anthropogenic causes? (Gabi Hegerl, Duke University)	Regional and anthropogenic climate change now clearly differentiated in chapter.
E-1-144	A	8	41	8	43	Repetition of earlier text in 1.2 (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Removed.
E-1-145	A	8	44	8	48	Insert 'anthropogenic' before 'regional climate change'. 'climate change' does not necessarily mean anthropogenic according to the standard IPCC definition, so this needs to be qualified. (Nathan Gillett, University of East Anglia)	Rejected.
E-1-146	A	8	49	8	51	Attribution appears 3 times in one sentence. Redraft. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Rejected. Language is needed as used based on WGI use and use in this chapter.
E-1-147	A	8	49			The term joint attribution should be discussed between WGs - we have worries about it. (Gabi Hegerl, Duke University)	Done.
E-1-148	A	8	49			See #4. "Joint attribution" sounds to me (without seeing your definition) to be the attribution of multiple observed changes to a single cause. I would prefer "sequential attribution" or something similar. (Daithi Stone, University of Oxford)	Rejcted. The term is defined in the chapter and used as defined. It is also used in teh published literature.
E-1-149	A	9	3	9	4	Note the key statement here - confidence in joint attribution must be lower than the confidence in either of the individual atribution steps. Yet the overall confidence assessment of joint attribution is high confidence which is not supported by the evidence in the chapter. The attribution of effects to temperature cannot be rated as high because of the risk of confounding influences causing the observed changes.In addition WGI does not report attribution at the grid point scale. So both attribution steps are not supported. In addition joint attribution implies a quantification of the joint relationships between emissions, climate and effects, whereas what is described here is a sequential process. (Peter Stott, Met Office)	Disagree. The global assessment of the confidence of the temeprature changes i svery high, and the same applies to the global assessment of the confidence of the link between impacts on some physical and biological systems and regional temeprature changes. Definition of joint attribution does not require quantification, as suggested in the comment.
E-1-150	A	9	4	9	4	See comment 2. I think I could go along with the notion of "joint attribution" if there were some discussion immediately after the introduction of this definition pointing out that "joint attribution" would not necessarily imply that a substantial part of the change in the impacted system is due to anthropogenic forcing (indeed, the anthropogenic response might not even be detectable in an end-to-end approach despite achieving joint detection).	Text on JA revised.

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						(Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-151	A	9	7	9	17	No mention of the key role that the oceans play. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Please provide specific references.
E-1-152	A	9	9	9	17	The introductory paragraph rightly discusses the influence of non-climatic drivers on changes in sectors and systems. It would be helpful to complement this with a short discussion of what constitutes 'change' and the difficulties of establishing whether observed trends should be regarded as compelling (issues include perturbations, natural variability et al). See, for example: Svensson, C., Hannaford, J., Kunderwicz, Z. W. and Marsh, T. J. 2006. Trends in river floods: why is there no clear signal in observations? IAHS/UNESCO Kovacs Colloquium - Frontiers in Flood Research. IAHS Publication 305, 1-18. (Terry Marsh, Centre for Ecology and Hydrology)	Paper checked but did not seem to focus on the issue across systems.
E-1-153	A	9	10	9	11	Delete 'observed responses of' on line 10 and 'to climate variations' on line 11. The main influence of the non-climatic drivers on systems is a direct one - influencing how they respond to climate seems to be a second-order effect. (Nathan Gillett, University of East Anglia)	Text rewritten. No longer relevant.
E-1-154	A	9	16	9	16	affecting change in multiple systems or sectors' - I think this should either be 'affecting multiple systems or sectors' or 'effecting change in multiple systems or sectors'. (Nathan Gillett, University of East Anglia)	Text rewritten. No longer relevant.
E-1-155	A	9	27	9	27	Replace 'geographical distribution' with 'abundance'. It makes no sense to say that the geographical distribution can vary over short distances. (Nathan Gillett, University of East Anglia)	Text rewritten. No longer relevant.
E-1-156	A	9	41	9	42	It is not the larger variability of precipitation on spatial and temporal scales that make its anthropogenic response harder to detect - it is the fact that the anthropogenic signal is weaker compared to the variability, when compared with temperature. (Nathan Gillett, University of East Anglia)	Done.
E-1-157	A	9	41	9	41	I suggest to add a comment on the space distribution of impacts of warming on the areas of systems which could consists in a sentence : …. (IPCC 2001). They drive systematic space distribution of the impacts of the warming on the areas of physical and biological systems in the extra-tropical zones and in mountains (see Box 1.1). And I propose a new box (Box 1.1): As during the pre-warming period of the 20th century the general space distribution of temperature in the extra-tropical zones of the northern (NH) and of the southern hemisphere (SH) presents a decrease from each tropic towards each pole, a global warming has induced a common pattern of the shifting of the the areas which can	The proposal of classification by the reviewer could be interesting, but impossible to enter here as far as it has not been discussed in a scientific paper in an international journal. We suggest that A. Douguedroit uses the opportunity of her careful review for writing this paper which could incorporate the elements of the report for supporting her theory

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						be found in several studies when the space distribution of their results is mentioned. During the last decades values of the prewarming extr-tropical temperatures have shifted towards north in the NH and south in the SH. And areas included between the maximum and the minimum temperatures or sums of temperatures or GDD which have also shifted in the same directions present now a common pattern dividing their new areas when they are compared with their prewarming space distribution (Figure 1.1). Three types of regions can be defined from the local point of view : the region remains between the extreme thermal limits (a), maximum thermal conditions are exceeded (b) and minimum conditions are reached (c).b means withdrawal except if an adaptation occurs and c new possibilities of extension (Douguédroit 2006). It is symmetrical in the Southern hemisphere. The respective surfaces of a, b and c depend on the ratio between the area surface and the importance of the warming. Many cases corresponding with the last situation (c) have been discribed in the north of Europe and America where the warming has been the highest as for Alaska "greening" (1,3,1,1), for arctic and tundra communities replaced by trees and shrubs (1,3,5,3) or plancton and fishes (1,3,4,2) but as each species presents its own limit, warming leads to mismatch in comparison with the pre-warming situation. The polar bear represents an exemple of a situation b (1,3,5,3). In the a type regions, there is no change of species but only changes of their thermal conditions inducing advances in the phenology well noticed in spring ((1,3,5,2). As temperature in the tropical zone does not present, except at regional scale, any well-ordered distribution, no common pattern can be defined. In mountains, due to a general decrease of temperature with altitude, the influence of the recent warming has a similar pattern in all the globe. It consists in a shifting towards higher altitudes. Then case b which is represented by the lower altitudes of the pre-warming areas is the most studied with the retreating glaciers (1,3,1,1) and the decrease od snow cover(1,3,1,4). The upward migration of trees in subalpine regions is an exemple of case c (1,3,5,3). (Annick Douguédroit, University de Provence)	
E-1-158	A	9	44	9	50	This is very true, but it is unclear how this affects the joint attribution attempts in this chapter (in particular, the last sentence of this section). If I understand it correctly, only the changes related to regional temperature change can be joint attributed. Changes related to other climate factors (such as sea level rise, precipitation change, etc.) cannot be joint attributed, if there is no link with associated regional temperature change. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Text rewritten.

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E-1-159	A	9	44	9	51	Large-scale climate oscillations like ENSO or NAO may themselves respond to global climate change. Global climate change has the potential to alter the frequency of extreme ENSO events or shifts in the phase of the Arctic Oscillation. Most observed changes in large-scale climate oscillation indices have not yet been attributed to climate change, however. I suggest stating in this paragraph that in cases where the response of a regional biophysical indicator to climate change can be attributed to a large-scale climate oscillation phenomenon, it is possible that the oscillation pattern itself undergoes changes that may be attributable to anthropogenic climate change. This implies that not in all cases is it sensible to try and separate the response to a large-scale oscillation from a response to longer-term climate change. If changes in the properties of the oscillation in question can be attributed to climate change then there may be a case to attribute regional biophysical changes to climate change indirectly, provided the changing oscillation patterns can be attributed plausibly and with confidence to climate change. (Heiko Balzter, Centre for Ecology and Hydrology)	Text rewritten.
E-1-160	A	9	44	9	50	It is not just named oscillations which lead to interdecadal climate variability. There is decadal variability in all aspects of climate, much of which is not associated with named oscillations. (Nathan Gillett, University of East Anglia)	Text rewritten.
E-1-161	A	10	2	10	2	Surely increasing carbon dioxide concentration is a non-climatic driver of change when it is influencing plant growth directly. (Nathan Gillett, University of East Anglia)	Carbon dioxide removed from text.
E-1-162	A	10	6			We (WGI, Ch3) say in our draft: global dimming was never global in extent. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Text rewritten. No longer relevant.
E-1-163	A	10	6			Jones et al will be changed into Trenberth et al., but earlier in the text a reference was made to WGI, Chapter … I don't know how we cross reference, but it should be consistent. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Agreed. Consistent cross-referencing to other chapters done now.
E-1-164	A	10	15	10	16	Table 1.1: For land cover change, there might also be some effects on global climate, although we don't yet have detection and attribution studies that quantify this. For pollution, tropospheric ozone could be mentioned - has direct effects on systems and health, and is a greenhouse gas. There could also be aerosol effects on precipitation. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Scales from local to global noted. Tropospheric ozone and effects on precipitation added.
E-1-165	A	10				Table 1.1. I don't think eucalypts are considered as an invasive species in Australia! (Neville Nicholls, Monash University)	Deleted.
E-1-	A	11	4	11	5	I don't think it is correct to say that all of these changes can be quantified in terms	Text rewritten. No longer relevant.

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166						of radiative forcing. Maybe better to say that land use change influences albedo, evaporation and surface roughness, which can be quantified in terms of changes in the energy and moisture budgets. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-167	A	11	7	11	7	remove 'Pielke' (appears twice) (Heiko Balzter, Centre for Ecology and Hydrology)	Done
E-1-168	A	11	18	11	20	Surely 'growing populaions' should be 'large populations'? Isn't the absolute population more important than its rate of change? (Nathan Gillett, University of East Anglia)	Text rewritten.
E-1-169	A	11	22	11	22	Replace 'chemicals' with 'fertilisers and pesticides' or something more specific. 'Chemicals' is too vague. (Nathan Gillett, University of East Anglia)	Text rewritten. Word no longer present.
E-1-170	A	11	28	14		Sections 1.2.2 & 1.2.3: Data involving new technolgy (ex. CZCS) are providing improved knowledge of recent changes in marine biological systems; however this does not preclude the possibility of similar changes happening in the past when such data were not available.This aspect of climate change based on modern data collection technolgy must be discussed here. On the analysis of "evidence of no change", there are two (or more) issues here: first, there may not be any real change to report, second, even if there is significant change, it may not get reported in literature due to socio-economic factors of many countries in Asia & Africa and elsewhere. This aspect of climate change assessment must be discussed here. (Madhav Khandekar, Retired)	Text rewritten.
E-1-171	A	11	30	11	31	This statement on record length contradicts page 9, line 48-50. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Section rewritten.
E-1-172	A	11	30	11	31	This sentence seems in contradiction with statement on p9 line 48-50 (Isabelle Chuine, CNRS)	Sentance removed.
E-1-173	A	11	33	12	2	The network observation of hydrography (runoff volume etc.) and glacier (glacial length, movement, etc.) should be added as a data source. (Xiaoqiu Chen, Peking University)	Please provide specific networks you wish to include.
E-1-174	A	11	50	11	50	There is a typo here … "are changing are shifting" ==> "are shifting"? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Corrected.
E-1-175	A	11	50	11	50	Replace 'relative' with 'in response'. (Nathan Gillett, University of East Anglia)	Text rewritten.
E-1-176	A	12	26	12	26	Trishchenko et al. 2002a,b (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Corrected.
E-1-177	A	12	28	12	29	This is a research recommendation - not appropriate here. (Nathan Gillett, University of East Anglia)	Text removed.

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E-1-178	A	12	29	12	29	Insert g in manitude (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Addressed.
E-1-179	A	12	29	12	29	determine the magnitude of changes more precisely and the respective influence of the increase of temperature and CO2 in the atmosphere. (Annick Douguédroit, University de Provence)	Text removed.
E-1-180	A	12	32	12	32	Delete the before marine (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text removed.
E-1-181	A	12	40	12	40	Add: The three regions determined in the areas of the extra-tropical zones after the warming (Box 1,1) can be found on Fig 1,1b along meridional sections from south to north in the Northern hemisphere and from north to south in the Southern hemisphere. Shiftings in the ocean are easier than on the ground because of general lack of obstacles. (Annick Douguédroit, University de Provence)	Section removed.
E-1-182	A	13	3			I'm not sure how necessary this subsection is - it could probably be dropped if space is short. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Section removed.
E-1-183	A	13	16	13	51	Why is the Fig 1.1 put here? The context should be given. (Xiaoqiu Chen, Peking University)	Figure removed.
E-1-184	A	13	16	13	51	The units given on the panel for figure (a) do not agree with those in the caption. I would recommend using the same units and colour scale for both plots. (Nathan Gillett, University of East Anglia)	Figure removed.
E-1-185	A	13	16	13	51	In Figure 1.1 there is no mention of confidence intervals. In the text (p.13 l.6) the importance of stating a signficance level is rightly pointed out. I strongly recommend to colour non-significant trends in Figure 1.1 in white or grey. It is currently impossible to assess the confidence in these trends. Also state whether the trends assume linearity and whether linearity is a reasonable assumption (e.g. provide an example of a q-q plot or other linearity test). Where in the text is the reference to Figure 1.1? (Heiko Balzter, Centre for Ecology and Hydrology)	Figure removed.
E-1-186	A	13	16	13	51	If these figures are only included to demonstrate methods, rather than results, the chapter simply doesn't have space for them - they should be deleted. (Jean Palutikof, IPCC WGII TSU)	Figure removed.
E-1-187	A	14	6	14	7	Phrasing problem: the link with the preceding sentences is not clear. (Isabelle Chuine, CNRS)	Text rewritten.
E-1-188	A	14	12	14	16	I suggest that these two sentences be deleted as they are not appropriate here. (Isabelle Chuine, CNRS)	Text rewritten.
E-1-	A	14	18	15	5	This section could be shorten a bit	Shortened.

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189						(Isabelle Chuine, CNRS)	
E-1-190	A	14	18	15	5	The techniques in this section belongs to the "Statistical techniques" in section 1.2.3.1. So, the two sections could be merged and significantly shortened. (Xiaoqiu Chen, Peking University)	Shortened.
E-1-191	A	14	18			Section 1.2.3.2 only covers biological systems; isn't a similar analysis needed for physical systems? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Section removed in order to shorten chapter to required length.
E-1-192	A	14	22	14	22	…are incomplete. They do not replace their location in the whole area of the species in comparison with the prewarming situation. An exception are … (Annick Douguédroit, University de Provence)	Section rewritten.
E-1-193	A	14	32			Root et al. 2003 is not exactely a "recent" study anymore (Isabelle Chuine, CNRS)	Section rewritten.
E-1-194	A	14	33			What is meant by: "directions of species"? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Section rewritten.
E-1-195	A	15	7	15	7	The unequal sensitivity of species to warming leads to a mismatch of systems and makes appear new systems consisting in species being in situation a, b and c (see Box 1,1). (Annick Douguédroit, University de Provence)	No space to add new Box due to length; also need reference from reviewer.
E-1-196	A	15	8			Please double check to make sure that 1.3.1 is consistent with WG1 (Chapter 4, and Section 9.5.5) (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Done.
E-1-197	A	15	10	15	18	The Cryosphere is well covered but there is little synthesis relating to temperate or tropical. Would some cross referencing to material in the regional chapters be useful here? (Terry Marsh, Centre for Ecology and Hydrology)	OK, references to WG1 Ch4 are added.
E-1-198	A	15	13	15	15	The way this is formulated implies that irrelevant studies that were disregarded in TAR are now included. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Don't agree, I believe the original sentence reads well.
E-1-199	A	15	16	15	17	Amend assesses to read assess, highlights to read highlight and considers to read consider (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, changed.
E-1-200	A	15	21	15	21	The cryosphere section should include more discussion of results from WG1, chapter 4. (Nathan Gillett, University of East Anglia)	OK, more references to WG1 added.
E-1-201	A	15	21	18		Sections 1.3.1,1.3.1.1 & 1.3.1.2: Cryosphere reduction and causes are more complex than what has been presented here. Glacier shrinking is not uniform everywhere. There are several glaciers which have changed very little while some	Text has been modified, adding more references to WG1 Ch4, which confirm the generalised cryosphere reduction, although

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						have even advanced in recent years. Changes in solar varibility has also been suggested as a possible reason for some mountain glacier shrinkage. Box 1.1 should either include discussion on other glaciers OR it should be deleted. Reference Sicart et al, 2003 given in Box 1.1 is not included in the list of references. Global glacier volume losses and sea-levl rise is a complex issue and there are no consistent increasing trends in sea-level rise everywhere. In a recent paper by Raper & Braithwaite (Nature, 439,January 2006,p.311-313), future sea-level rise due to mountain glacier and icecap melting is projected to be about 4.6 to 5.1 cm by 2100, just about half the present projection. Changes in Antarctic ice shelves are not at all consistent with those in the Arctic, this fact must be clearly stated here. (Madhav Khandekar, Retired)	recognizing the complexities in behaviour and driving forces. Chacaltaya represents a typical small disappearing glacier in a mountain area and as such Box 1.1 has been preserved. Reference Sicart et al., 2003 has been added in Box 1.1. References in sea level rise and changes in Antarctic ice shelves are made to WG1 Ch4. Sea level rise due to cryosphere reduction during the period 1961-2003 is now an established fact (see FGD WG1 Ch4).
E-1-202	A	15	21	22	16	According to the Green Book Doc 4b, which is guidance note to authors, cryosphere changes will be dealt with primarily by WG1. But this receives major treatment over 5-6 pages here. This needs shortening, given the excessive length of the chapter. Also, there is no summarizing table of impacts for this sector, which is odd and unfortunate - this would be a strategy which would save a lot of space. (Jean Palutikof, IPCC WGII TSU)	OK, text is shortened and tables added.
E-1-203	A	15	39			Here each subparagraphs starts with "Effects of". This is not repeated in 1.3.2 etc. which describe the changes themselves rather than the "effects of". Because 1.3.1 describes the "effects of", there is redundancy with later sections. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Of all the systems described in 1.3, the cryosphere is the only one dealt specifically in WG1 (Ch4), and therefore we use the term “effects of”.
E-1-204	A	15	41		51	This would be a good place to refer to WG1Ch4"s assessment of changes in glaciers, ensure its consistent and avoid reassessment. The case study of the Andean glacier melt and its impact next page is very interesting. (Gabi Hegerl, Duke University)	OK, reference to CH4 WG1 added.
E-1-205	A	15	42	15	45	The statement and its wording should be cross checked with the findings og WG1 Ch 6 "Palaeoclimate" (Georg Kaser, Geo and Atmospheric Sciences)	OK, Chapter 6, WG1 was reviewed and the text changed accordingly. Glaciers have not experienced such a strong retreat during at least the past 5,000 years.
E-1-206	A	15	42	15	43	change "...last 2 decades..." to "last two decades" and "...20th century..." (Pedro Ribera, Universidad Pablo de Olavide)	OK, done
E-1-207	A	15	44	15	44	Amend second of to read over (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, done
E-1-208	A	15	45	15	48	All measured Norvegian glaciers had predominantely negative mass balances since 2000 and glacier lenght variations were negative respectively. See: Kjøllmoen, B.E., 2005. Glaciological investigations in Norway in 2004., Norwegian Water	OK, reference added and text modified.

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						Resources and Energy Directorate, Oslo. www.nve.no/FileArchive/176/Glac_invest2004.pdf (Georg Kaser, Geo and Atmospheric Sciences)	
E-1-209	A	15	47	15	47	Insert is between glaciers and still; change increases to read increasing (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	According to new evidence cited from CH4 WG1, Norwegian glaciers no longer advance so the corresponding text was deleted.
E-1-210	A	15	48	15	50	Two out of three references are pre-TAR dated, one is about the same data. Are the glaciers menioned still advancing after 2001 (TAR)? Id so, provide recent references. (Georg Kaser, Geo and Atmospheric Sciences)	Glaciers were still advancing until the early 2000s. Reference of 1997 was replaced by one from 200? And one personal communication from 2003 was added.
E-1-211	A	16	0			Box1.1. This is a very useful, providing convincing observational evidence of the effect of recent warming. An updating of the final sentence of the first para. would help to avoid misinterpretation. The quoted reference is 15 years old; given the rapid recent shrinkage, many of the smallest Bolivian glaciers must, surely, no longer exist. (Terry Marsh, Centre for Ecology and Hydrology)	Unfortunately this is the only inventory available. A new inventory being performed in Bolivia is not yet ready.
E-1-212	A	16	1	16	50	Box 1.1 could be shortened (Isabelle Chuine, CNRS)	The text of the Box was maintained, considering the relevance of the case study which is not mentioned elsewhere in WG1 or WG2. However, the text of the cryosphere section was reduced.
E-1-213	A	16	3	16	4	This statement needs more carefull formulation. Refere to the respective paragraph in WG1 Chapter 4.5 is suggested. (Georg Kaser, Geo and Atmospheric Sciences)	OK, statement has been modified.
E-1-214	A	16	3	16	3	Delete the last, insert recent (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, replaced
E-1-215	A	16	4	16	4	Delete the between for and small-sized (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, deleted
E-1-216	A	16	7	16	7	Delete final the and insert a (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, replaced
E-1-217	A	16	9	16	9	Amend located now to read now located (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, modified
E-1-218	A	16	10	16	11	It is correct that around 1990 about 80% of the glaciers in Bolivia had a surface area smaller than 0.5 km^2 and that they may dissappear soon same as probably Chacaltaya glacier. The statement, however, gives a very biased impression since the big number of small glaciers make up only a small portion of total ice coverage in the Bolivian Andes. See, e.g., shrinkage rates for the entire Cordillera Real in	OK, sentence modified, although no data for Cordillera Real could be foind in Ch4 WG1.

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						WG1 Chapter 4.5. (Georg Kaser, Geo and Atmospheric Sciences)	
E-1-219	A	16	13	16	17	Refer to a much more comprehensive summary given in WG1 Ch 4.5 (Georg Kaser, Geo and Atmospheric Sciences)	OK, reference added
E-1-220	A	16	16	16	16	Change "an upward rise" to "an ascent up mountain slopes". (Claire Parkinson, NASA Goddard Space Flight Center)	OK, but only “ascent” added
E-1-221	A	16	17	16	17	Add at the end of the line." It has the character of the location of the lower extent of the tropical glaciers in the c region of mountain areas as defined in Box 1,1. (Annick Douguédroit, University de Provence)	This suggested text at the end of line 17 makes no sense and was not added.
E-1-222	A	16	19	16	30	Box 1.1: This is a very interesting example, but the third and the fouth paragraphs could be reduced. Additionally, in the map (Fig. 1.2) scale is not indicated, and it is important. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	The text was not shortened because this is the only place in WG1 and WG2 where such a case is described in detail. The figure was changed and a scale was added.
E-1-223	A	16	24	16	25	Janssens et al. 2003 seems not an appropraite reference for glacier shrincage and melt-water runoff: Janssens, I.A., A. Freibauer, et al. 2003: Europe’s terrestrial biosphere absorbs 7 to 12% of European anthropogenic CO2 emissions. Science 300(5625): 1538. [Europe; terrestrial biological systems] Probably it was intended to cite: Jansson, P., Hock, R. and Schneider, T., 2003. The concept of glacier storage: a review. Journal of Hydrology, 282: 116–129. (Georg Kaser, Geo and Atmospheric Sciences)	Yes, the reference was wrong, it has been replaced by the mentioned reference of Jansson.
E-1-224	A	16	27	16	51	Giving Chacaltaya glacier such a prominent position produces a very biased picture. This glacier, such as several other easy accessible and thus best observed ones in the high Andes, has lost its equilibrium line since several decades. The same is true for many hundreds of very small glaciers in the Alps, the Himalayas, the Rocky Mountains etc. and many have already gone since years. This has about the same meaning as the retreat of glacier tongues all around the world. Still, Chacaltaya glacier and all other small glaciers make only up a minor portion of the entire glaciated area e.g. in the Cordillera Real. If Box 1.1 keeps the Chacaltaya example, it should at least refer to the neighbouring Zongo glacier that has a mass equilibrium since 1998 (Francou et al., 2005): Francou, B., P. Ribstein, P. Wagnon, E. Ramirez and B. Pouyaud (2005), Glaciers of the tropical Andes: indicators of global climate variability, in Huber, U.M., Bugmann, H.M. and Reasoner, M.A., eds., Global Change and Mountain Regions,	Don’t agree with this review. It is well known that glaciers worldwide have shown a general tendency for retreat during the past century (e.g. Oerlemans, 2005) and that this retreat is probably due to anthropogenic forcing (Chapther 6, WG1 and Reichert et al., 2002). Furthermore, based on ample evidence from other mountain regions (central Asia, North America, South America, Alps, etc.), small glaciers have not only been retreating but also disappearing during the last century. Chacaltaya is a typical case of a small glacier which has practically disappeared in recent

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						197-204. Springer, New York. (Georg Kaser, Geo and Atmospheric Sciences)	years, before our eyes, with well documented recession, and, most importantly to Working Group 2, is has produced effects in skiing, and also illustrates the case for effects in water resources, albeit to a minor degree due to the small size of Chacaltaya. The near-equilibrium mass balance of neighbouring Glaciar Zongo in the period 1998-2002 has been interpreted to be due to colder La Niña years. After 2002, the annual mass balances of Zongo have had a negative trend based on recent references from Francou et al., now cited in the text.
E-1-225	A	16	29	16	29	Insert comma after area (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, added
E-1-226	A	16	31	16	51	A horizontal scale would be helpful on this plot. (Nathan Gillett, University of East Anglia)	OK, added
E-1-227	A	16	48	16	48	Insert 'Bolivia' into the legend of Fig 1.2. (Knute Nadelhoffer, University of Michigan)	OK, added
E-1-228	A	16	48	16	48	In the caption to Figure 1.2, after "Chacaltaya Glacier", add ", Bolivia,". (Claire Parkinson, NASA Goddard Space Flight Center)	OK, added
E-1-229	A	16	48			What is the spatial scale for the figure? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Spatial scale added
E-1-230	A	17	2	17	2	The first sentence implies that wastage of glaciers is only happening in small glaciers, that there are no high latitude small glaciers and that only small glaciers have consequences for water resources. Redraft. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, modified
E-1-231	A	17	5	17	5	Hock 2005 mentioned twice. Hock 2005 Hock et al. 2005? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, fixed, these are in fact two different references.
E-1-232	A	17	7	17	8	It is incongruous to assert something "is" as on line 7, and then note on line 8 that " there is no significant evidence of this." Accordingly, replace "is" on line 7 with "may be". (Indur Goklany, US Department of the Interior)	There is a robust theoretical background for asserting this, as quoted from the literature, so that “should” and “expected” have been added in the text.
E-1-233	A	17	8	17	8	Janssens et al. 2003 seems not an appropriate reference for glacier shrincage and melt-water runoff: Janssens, I.A., A. Freibauer, et al. 2003: Europe’s terrestrial biosphere absorbs 7 to 12% of European anthropogenic CO2 emissions. Science 300(5625): 1538.	Absolutely right, it was a mistake, the reference was replaced by Jansson et al. 2003.

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						[Europe; terrestrial biological systems] Probably it was intended to cite: Jansson, P., Hock, R. and Schneider, T., 2003. The concept of glacier storage: a review. Journal of Hydrology, 282: 116–129. (Georg Kaser, Geo and Atmospheric Sciences)	
E-1-234	A	17	19	17	21	reword to: Continued vigilance is needed as it is estimated that 20 potentially dangerous glacial lakes still exist in Nepal, 24 in Bhutan (Yamada 1998) and several in the Cordillera Blanca and other Peruvian mountains. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, changed according to suggestion.
E-1-235	A	17	23	17	24	Amend part of sentence to read: ….is increasingly being used to observe, anticipate and assess……. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, changed
E-1-236	A	17	27	17	43	Please coordinate with WG1, Ch 9 also. Quite a bit of the material on sea-level rise in WG1 Ch5, particularly that which deals with attribution, now appears in Ch 9 (Section 9.5.2). This section will be revised for the 3rd order draft to correct inconsistencies with Ch 5, and to improve readability. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK, sea level rise data has been referenced to WG1.
E-1-237	A	17	27	17	43	Note that numbers on sea level contributions from the cryosphere have been updated from SOD in WG1 (Ch 4 and 5) (Georg Kaser, Geo and Atmospheric Sciences)	Done
E-1-238	A	17	27	17	27	Amend part of sentence to read: …losses have shown significant acceleration since the end of the 1980s…. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, changed.
E-1-239	A	17	36	17	41	It should be noted that since the Rignot and Kanagaratnam (2006) study spans 9 years while the Velicogna and Wahr (2006) spans 34 months, neither can provide robust (quantitative or qualitative) estimates of long term trends in melting. In fact, given the length of their records, these studies are little more than anecdotes. (Indur Goklany, US Department of the Interior)	OK, this section has been redrafted, citing the main conclusions of WG1 Ch4
E-1-240	A	17	45	17	50	This section is confusing, and not particularly informative or useful. What are the degrees of changes in obliquity and rotation acceleration. Are these really important impacts? I suggest either deleting this section or modifying to clarify and address these questions. Deleting would be better. Crustal uplift due to glacial melting is worth discussing, perhaps in a bit more detail. (Knute Nadelhoffer, University of Michigan)	OK, the gravitational field section has been deleted, and the crustal uplift expanded.
E-1-241	A	18	2	18	4	This needs rewriting to clarify. I suspect the authors mean that 18O:16O ratios of ice formed since 1800 have increased, indicating warming. The text needs to specify which particular oxygen isotope(s) has(have) become enriched (18O?).	Sentence deleted following suggestion in E1-242.

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						(Knute Nadelhoffer, University of Michigan)	
E-1-242	A	18	2	18	4	The sentence "Stable isotope … last 1000 years" is out of place here. Replace it by: "As glaciers disappear, the records preserved in their ice disappear as well. Furthermore, even before the disappearance of the glaciers, their decay can destroy their climate records. For instance, in the Quelccaya Ice Cap, Peru, ..." (continuing with the sentence on lines 4-6). (Claire Parkinson, NASA Goddard Space Flight Center)	OK, sentence deleted and replaced accordingly.
E-1-243	A	18	3	18	4	I don't know how categorically to make statements comparing the climate of the past 50-years with the past millenium given the attention and scrutiny that has recently been given to paleo-climate issues (e.g., the NAS report, the very recent Barton hearings, etc.). This particular statement seems pretty confident. It would be a good idea to co-ordinate this assessment with WG1, Ch 6. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	This sentence has now been deleted.
E-1-244	A	18	11	18	11	single inverts (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, done
E-1-245	A	18	24	18	24	For referencing the decay of the ice shelves of the Antarctic Peninsula, change "Scambos 2004" to "Scambos et al. 2004; Domack et al. 2005". (Full reference to be added on p. 79: Domack, E., D. Duran, A. Leventer, S. Ishman, S. Doane, et al., 2005, Stability of the Larsen B ice shelf on the Antarctic Peninsula during the Holocene epoch, Nature, 436(7051), 681-685.) (Claire Parkinson, NASA Goddard Space Flight Center)	OK, changed
E-1-246	A	18	25	18	25	For referencing the decay of the ice shelves in the Canadian Arctic, add Vincent et al. 2001. (Full reference to be added on p. 101: Vincent, W. F., J. A. E. Gibson, and M. O. Jeffries, 2001: Ice-shelf collapse, climate change, and habitat loss in the Canadian high Arctic, Polar Record, 37(201), 133-142.) (Claire Parkinson, NASA Goddard Space Flight Center)	OK, added
E-1-247	A	18	30	22		Sections 1.3.1.3 thru 1.3.1.9: These sections once again describe changes in ice, snow frozen ground etc in very general terms and with only selected (cherry-picked) references which support the warming of the climate in recent years. A number of studies reported in recent literature document opposing change and these studies must be cited as well. Among the studies to be referred: 1. Zwally et al, 2002, J of Geophysical Research, 107, C2, p. 9-1/9-21-this study clearly shows Anatrctic sea ice increased between 1979-1998 period: 2. Soderkvist & Bjork, Climate Dynamics, 2004,p.57-68; this papaer analyzes significant reduction of Arctic sea ice thickness due to poleward energy flux and increased winter cloudiness and NOT due to GHG increase: 3. Davis/Hanna et al, Science online,DOI:10.1126/science,1110662(2005), this paper documents increased	The section has been redrafted and shortened with more references to the main findings of Chapter 4, WG1, thus concentrating more on the effects of changes in the cryosphere rather than the changes per se which are discussed in detail in Chapter 4, WG1.

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						snowfall on Antarctic and also increased ice sheets:4. Doran et al, 2002, Nature online, DOI:10.1038/nature 710,this paper documents Antarctic cooling. It is important to discuss both sides of cryosphere changes and present a more balanced view. (Madhav Khandekar, Retired)	
E-1-248	A	18	32	18	44	See comment 12. The data from Johennesen et al (2005) and Davis et al (2005), based on 10-11 yrears worth of data, are only slightly more robust. All this should be noted so that the reader is provided perspective. (Indur Goklany, US Department of the Interior)	The ice sheets section has been shortened and redrafted and makes now more references to the main findings of Chapter 4, WG1 in order to provide the correct perspective to the reader.
E-1-249	A	18	32		42	Another good place to link to WG1ch4, synchronize, and limit overlap. (Gabi Hegerl, Duke University)	OK, the ice sheets section has been shortened and redrafted and makes now more references to the main findings of Chapter 4, WG1.
E-1-250	A	18	34	18	34	Amend to read: overall Greenland is (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, done
E-1-251	A	18	44	18	44	Insert as between exists and yet (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, inserted
E-1-252	A	19	0			The effects of frozen ground are here discussed in the context of the cryosphere. There appears to be no corresponding discussion focussing on the temperate zone where the role of frozen ground (and snowmelt) may be a factor in explaining the limited observational evidence for long term increases in flood magnitude. Given the major socio-economic implications, this issue merits greater attention. (Terry Marsh, Centre for Ecology and Hydrology)	Subsection "Impacts on surface runoff" in 1.3.1.5 discusses the limited observational evidence linking frozen ground to enhanced runoff. By inspecting Chapter 4, WG1 (Cryosphere) and Chapter 15, WG2 (Polar Regions), only one more reference could be found regarding observed increases in flood magnitude: Kokelj, S.V., and C.R. Burn, 2005: Near-surface ground ice in sediments of the Mackenzie Delta, Northwest Territories, Canada. *Permafrost and Periglacial Processes*, **16**(3), pp. 291-303..
E-1-253	A	19	1			Use of the word "few" is perhaps inappropriate given how large an area of the north temperate zone is covered by former soviet union (Chris Thomas, University of York)	Changed to "some"
E-1-254	A	19	4			I didnt report a decline in snow cover in Australia in the paper cited here. Just snow depth (mainly in spring) (Neville Nicholls, Monash University)	OK, changed
E-1-255	A	19	6	19	6	2005). Only few areas has been found with a reduced snow cover depth in early winter as in the French Alps( )	No references could be found for observed changes in snow cover in the French Alps.

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						(Annick Douguédroit, University de Provence)	
E-1-256	A	19	9	19	9	Insert the between for and Western (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-257	A	19	12	19	12	Insert an between for and increase (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-258	A	19	23	19	33	The paragraph fails to comment on the great deal of spatial variability and the fact that in some regions there is no apparent trend in permafrost temperatures (discussed in Ch 4 WG2). The comments regarding acceleration of degradation are based only on information from a number of peatland sites in northern Manitoba and this acceleration may have more to do with peatland hydrology dynamics than an accelerated warming in climate (also may be responding to changes in climate that happend over the last century). The comment regarding increases in active layer thickness is based on observations in Russia and this should be indicated (does not necessarily apply to the entire Northern Hemisphere). As stated in WG2 Ch 4, active layer thickness shows a great deal of inter annual variability and for most regions there is not enough data to comment on longer term trends. (Sharon Smith, Natural Resources Canada)	Text was changed according to the comments and more references have been made to Chapter 4, WG1.
E-1-259	A	19	27	19	27	Insert a between by and emperature (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, done
E-1-260	A	19	36	19	36	Insert an between in and increased (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, done
E-1-261	A	19	43	19	43	single inverts (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, done
E-1-262	A	19	44	19	44	Carbon in what form … as methane, CO2? A recent article by Zimov et al (Science, June, 2006) might be relevant here as well. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK, Zimov reference now added. Yes, melting of frozen ground would result in release of methane to the atmosphere.
E-1-263	A	19	51	19	51	Change of to that (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-264	A	19	51	20	6	While there may be abundant evidence of structures in North American and Siberia being affected by permafrost thaw, there is not abundant evidence to clearly show that thawing and subsequent damage is due to climate warming. In many cases (rather than a few as stated) the thaw is due to a micro climate change related to disturbance of the ground surface during construction and also to disturbance of the ground thermal regime due to operation of some structures. Failure to consider the presence of frozen ground (and changes in its thermal regime due to disturbance etc.) in engineering design can lead to damage to structure. Much of the observed damage is due to innapropriate design (failure to accomodate thaw of ice-rich soil,	Text changed to present more balanced view, reference made to Chapter 15, WG2.

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						creep of frozen ground etc.) and localized effects of development. Chapter 15 discusses this and mentions that it is a challenge to attribute the impacts on infrastructure to climate warming. Chapter 15 would appear to have reviewed a larger body of literature and presents a more balanced view. (Sharon Smith, Natural Resources Canada)	
E-1-265	A	19	51	20	6	A few comments on the references provide in this paragraph regarding the interpretation of the information etc: Allard et al. (2002) is a conference abstract and to some extent could be considered a weak reference which provides no analysis to support the statements regarding impacts on infrastructure and whether they can unequivocally be attributed to climate change. USARC (2003) describes impacts of permafrost thaw on infrastructure but does not attribute the observed damage to climate change (it comments on surface disturbance, engineering design). Nelson et al (2002) attributes much of the observed impacts on infrastructure to effects of disturbance on permafrost and innappropriate engineering design. While there is a brief comment related to attribution of some infrastructure damage in Russia to climate change, this is based on an anonymous reference and there is no real analysis etc. presented. (Sharon Smith, Natural Resources Canada)	Text has been changed, and more references added to Chapter 15, WG2, consistent with the view that no conclusive evidence exists for climate change attribution of infrastrutrure damage caused by permafrost thawing.
E-1-266	A	20	1	20	1	Delete that (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-267	A	20	5	20	5	Insert ; after), lower case t in this and delete described in (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-268	A	20	11	20	12	I found 'from 30% to 100% of the depth measured before the heat wave' confusing. I suggest replacing 'from' with 'by'. (Nathan Gillett, University of East Anglia)	Done
E-1-269	A	20	12	20	12	Make reference to the 20 million cu feet rockfall on the east face of the Eiger on 20 July 2006. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	The Eiger rockfall has not been linked to warming. This is now referenced aprorpiately in the Supporting Material.
E-1-270	A	20	16			This subsection could have more cross-links with WG1. Please make sure message is consistent with WG1 Chapter 5 and relevant parts of Section 9.5. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Text changed and links made to WG1
E-1-271	A	20	19	20	19	For referencing the approximately 3% per decade decline in Arctic sea ice, add Parkinson and Cavalieri 2002. (Full reference to be added on p. 93: Parkinson, C. L., and D. J. Cavalieri, 2002: A 21 year record of Arctic sea-ice extents and their regional, seasonal and monthly variability and trends, Annals of Glaciology, 34, 441-446.) (Claire Parkinson, NASA Goddard Space Flight Center)	Reference added

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E-1-272	A	20	22	20	22	For referencing the increase in Antarctic sea ice cover since the late 1970s, add Zwally et al. 2002. (Full reference to be added on p. 103: Zwally, H. J., J. C. Comiso, C. L. Parkinson, D. J. Cavalieri, and P. Gloersen, 2002: Variability of Antarctic sea ice 1979-1998, Journal of Geophysical Research, 107(C5), 10.1029/2000JC000733.) (Claire Parkinson, NASA Goddard Space Flight Center)	Based on Chapter 4, WG1, the text now indicates that no significant change in Antarctic sea ice cover can be demonstrated, so this reference was not added
E-1-273	A	20	24			It should also be noted that increased navigation should lead to greater trade, and falling prices for imported goods in the Arctic, which probably means, most goods (including fresh fruits and vegetables, and medicines, among other things). In turn, that should advance public health, all else being equal. (Indur Goklany, US Department of the Interior)	Text has been modified to include some of these possible changes, and references made to Chapter 15, WG1. No evidence for these changes have been found in the literature
E-1-274	A	20	29	20	29	unchartered' should be 'uncharted'. (Nathan Gillett, University of East Anglia)	Done
E-1-275	A	20	29	20	29	Change "unchartered" to "uncharted". This is not a spelling error. These two words have different meanings and I suspect "uncharted" (= never before mapped) is what was intended. (Knute Nadelhoffer, University of Michigan)	Done
E-1-276	A	20	29	20	29	"uncharted" instead of "unchartered"? (Heiko Balzter, Centre for Ecology and Hydrology)	Done
E-1-277	A	20	35	20	35	Amend have died to read either declined or disappeared (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-278	A	20	38	20	38	Delete in insert are (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-279	A	20	40	20	40	Amend line to read: …….There is evidence for freshening at both polar ends of the Atlantic ….. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-280	A	20	41	20	41	Delete probably a combination; insert likely a consequence (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-281	A	20	44	20	46	This finding is controversial, and Bryden himself reports that the one standard deviation uncertainty on the change is the same size as the change itself. Chapter 5 of WG 1 concludes: 'Indirect evidence suggests that Atlantic meridional overturning circulation has considerable decadal variability, but there is low confidence in its long-term trend.' (Nathan Gillett, University of East Anglia)	Text deleted and changed to indicate the finding of large interannual and decadal variability, stating that there is no evidence for significant long-term trends in Atlantic meridional ocean circulation, proper quote made to Chapter 5, WG1
E-1-282	A	20	44	20	49	The Bryden study has been criticised in some quarters because it is based on a very limited sampling of MOC estimates. See the assessment in WG1 Ch 5 (see SOD, page 8, lines 35-51) which assigns low confidence to heat transport changes, and	Bryden no longer referenced. Text deleted and changed to indicate the finding of large interannual and decadal variability, stating

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						very low confidence to freshwater transport changes. The assessment in WG1 Ch 9 (see SOD, page 41, lines 17-27, which may be revised further for the TOD) is also relevant here. Discussion of MOC change, and its implications for European climate in WG1 Ch 10 are also relevant (the MOC reduces in many models, but this doesn't seem to result in a markedly colder European climate - radiative forcing change dominates over ocean circulation change). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	that there is no evidence for significant long-term trends in Atlantic meridional ocean circulation, proper quote made to Chapter 5, WG1
E-1-283	A	20	44	20	46	It appears biased for this assessment to cite only one paper, and on the basis of this one paper make the unconditional statement that the MOC "has weakened" by 30%. There are plenty of other studies that also look at changes in MOC and find much less or no weakening, but a lot of interannual to decadal variability. There are also questions about the sampling rate of this particular study (5 snapshots in time). Please note that WG1 assessed a very substantial body of literature and concludes that there is only a low level of confidence that the deep limb of the MOC has decreased, and no quantitative statement as to the possible amount of decrease is given. Please reconsider this statement and ensure that it comes with appropriate qualifiers and uncertainty language. (Andy Reisinger, IPCC SYR TSU)	Bryden no longer referenced. Text deleted and changed to indicate the finding of large interannual and decadal variability, stating that there is no evidence for significant long-term trends in Atlantic meridional ocean circulation, proper quote made to Chapter 5, WG1
E-1-284	A	20	45	20	45	Delete in insert between; delete also (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Whole section on Atlantic meridional ocean circulation has been deleted and replaced with new one based on E1-281 and 282
E-1-285	A	20	47	20	47	Delete reduced insert possible future reduction in the; insert a between in and colder (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Whole section on Atlantic meridional ocean circulation has been deleted and replaced with new one based on E1-281 and 282
E-1-286	A	20	48	20	49	Do you mean a negative feedback? If so, need to say so otherwise this text is confusing. (Jean Palutikof, IPCC WGII TSU)	Whole section on Atlantic meridional ocean circulation has been deleted and replaced with new one based on E1-281 and 282
E-1-287	A	20	49			Replace the sentence starting with "However," on line 48, with the followingl: "BRYDEN ET AL. (2005) ALSO NOTES THAT THE 30% MEASURED INCREASE IS 'UNCOMFORTABLY CLOSE' TO THE UNCERTAINTIES IN THE OBSERVATIONS. MOREOVER, MOST OF THE WINTER WARMING OF WESTERN EUROPE SEEMS TO BE DUE TO ATMOSPHERIC CIRCULATION AND SEASONAL RELEASE OF HEAT STORED IN THE OCEAN RATHER THAN HEAT TRANSPORTED BY THE OCEAN CIRCULATION (SEAGER ET AL. 2002; SEE ALSO WEAVER AND HILAIRE-MARCEL 2004, AND WUNSCH 2004). ALSO, the slow-down of the THC might also be expected to give rise to an increase in salinity difference, thus enhancing the	Bryden no longer referenced. Text deleted and changed to indicate the finding of large interannual and decadal variability, stating that there is no evidence for significant long-term trends in Atlantic meridional ocean circulation, proper quote made to Chapter 5, WG1

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						THC (Wu et al. 2004). FINALLY, GREGORY ET AL. (2005) COMPARED THE RESPONSE OF THE THC TO A QUADRUPLING OF CO2 CONCENTRATIONS OVER 140 YEARS USING 11 DIFFERENT MODELS. ALL THE MODELS INDICATED A GRADUAL DECLINE IN THE STRENGTH OF THE THC BY BETWEEN 10 AND 50 PERCENT. HOWEVER, NONE SHOWED A RAPID OR COMPLETE COLLAPSE. NONE OF THE MODELS SHOWS A COOLING ANYWHERE THAT WOULD MORE THAN OFFSET ANY RESULTING WARMING." [New material is in BOLD.] References: (1) Gregory, J. M., et al. 2005. A model intercomparison of changes in the Atlantic thermohaline circulation in response to increasing atmospheric CO2 concentration. Geophysical Research Letters 32: L12703, doi:10.1029/2005GL023209. (2) Seager, R., et al. 2002. Is the Gulf Stream responsible for Europe's mild winters?: Quarterly Journal of the Royal Meteorological Society 128: 2563-2586. (3) Weaver, A. J., and C. Hillaire-Marcel. 2004. Ice growth in the greenhouse: A seductive paradox but unrealistic scenario. Geoscience Canada 31: 77-85. (4) Wunsch, C. 2004. Gulf Stream safe if wind blows and Earth turns. Nature 428): 601. (Indur Goklany, US Department of the Interior)	
E-1-288	A	21	6	21	6	Change Prowse 2002 to Prowse and Beltaos 2002; and make the same change on p. 21, lines 24 and 27. (Claire Parkinson, NASA Goddard Space Flight Center)	Done
E-1-289	A	21	11	21	11	Insert a between of and reduction (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-290	A	21	14	21	16	Too many references? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	References reduced
E-1-291	A	21	14	21	14	Add s onto rate (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	"Rate" not found
E-1-292	A	21	18	21	19	The first part of this sentence (up to the comma) didn't make sense to me. (Nathan Gillett, University of East Anglia)	Sentence changed
E-1-293	A	21	20	21	20	changes in' is not needed here. (Nathan Gillett, University of East Anglia)	Done
E-1-294	A	21	22	21	22	Delete comma (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-295	A	21	23	21	23	Why should the melt be more intense? If there is no evidence that this is happening, don't mention this. (Nathan Gillett, University of East Anglia)	'more intense' deleted
E-1-	A	21	23	21	25	The reference to earlier and more intense melt conditions leading to ice-jam	Could not find a proper reference. Dr. Terry

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296						flooding is reasonable but, globally, surely the long term reduction in the risk of ice-jam floods is of greater significance (in Europe, for example, the risk is substantially diminished relative to the 19th century). (Terry Marsh, Centre for Ecology and Hydrology)	Marsh was contacted but nor reply was obtained.
E-1-297	A	21	30	21	31	The various impacts of Climate Change, including the warming, are often linked together in lines which leads to socio-economic activities. Glacier shrinkage and shortening of the snow cover results in modifications of the hydrological regimes which modify water ressources of local and regional populations for their private needs, agricultural productions and industries. Reduction of snow cover inflences directly skiing activities. Modes of human.... (Annick Douguédroit, University de Provence)	Suggested text edited and added
E-1-298	A	21	35	21	35	Change "animals, plants and birds" to "animals and plants" or to "plants and animals (including birds)". (Claire Parkinson, NASA Goddard Space Flight Center)	Done, birds deleted
E-1-299	A	21	40	21	40	Insert that are between Communities and particularly (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-300	A	21	43	21	44	I don't think the advertisement for the IPY is really necessary. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	IPY sentence deleted
E-1-301	A	21	46	21	47	What are "agent-based simulation models"? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	This concept is now explained in the text.
E-1-302	A	22	1	22	1	1,3,1,9 Cryosphere: Summary. I suggest that each title "Summary" should be preceded by the topic concerned (Annick Douguédroit, University de Provence)	Ok, changed.
E-1-303	A	22	3	22	5	I suggest inverting the order of this sentence to give more emphasis to the generalized shrinkage, something like “Although there are a few cases of both shrinkage and growth related mainly to changes in precipitation, there is abundant evidence that most of the cryospheric components in polar regions and in mountains are undergoing generalized shrinkage in response to warming”. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Done
E-1-304	A	22	6	22	7	snowmelt advance and, due to enhanced snow melt, increased water base flow and modified river regimes influencing water ressources of the population locally and regionaly. Formation of thermokarst.. (Annick Douguédroit, University de Provence)	Text suggested was edited and added
E-1-305	A	22	6	22	15	Include in this list of impacts of cryospheric reduction the increased ease of ship transportation in the Arctic. (Claire Parkinson, NASA Goddard Space Flight Center)	Done

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E-1-306	A	22	19	28		Section 1.3.2 (including 1.3.2.1 thru 1.3.2.4): These sections present changes in Arctic runoff, droughts/floods etc. which can be interpreted as due to natural climate variability than due to anthropogenic climate change. Three recent papers question many of the examples given here: 1. Andreadis & Lettenmaier, Geophysical Research Letters (GRL) 33, L10403,Doi;10.1029/2006GL025711: 2. Rawlins et al, GRL,33,10.1029/2005GL025231: 3. Berezovskaya et al, GRL, 31, 10.1029/2004GL021277. These studies show respectively that droughts over USA have become shorter and less frequent, rainfall over Eurasian drainage basins have significantly decreased leading to less than predicted runoff in Arctic ocean. For North America, droughts on the Great American Plains were more severe and more frequent during the Dust Bowl years of 1920s and 1930s than the recent years. The discussion here must be suitably modifiied to reflect most recent literature and must also include drought/flood cycles during the 1920s and 1930s when earth's temperature was increasing steeply. (Madhav Khandekar, Retired)	*The following abbreviations were often used by Chunzhen Lui in the hydrology section.* *A: Addressed* *NA: Not applicable* *TR: Text removed* *NA did not mean Not Accepted but No Applicable.* NA
E-1-307	A	22	27	22	27	Amend atmosphere to read atmospheric; delete s in circulations (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-308	A	22	33			Suggested addition: 'The interactions of rainfall, evaporation and soil moisture patterns are complex. At the catchment scale, river flows (and groundwater recharge) integrate the many hydrometeorological variables and provide the best index of water resources and river regime variability. Trend identification and evaluation can be particularly challenging where artificial influences (abstractions, diversions, impoundments etc) impact significantly on natural regimes. Therefore it is important to identify benchmark (or pristine) catchment networks to provide a focus for trend analyses. (Terry Marsh, Centre for Ecology and Hydrology)	TR
E-1-309	A	22	35	22	35	This section should highlight Milly et al. (2005, Nature, Global pattern of trends...), who showed that the historical global pattern of streamflow trends was much more readily explained by models of forced climate change than by natural variability. (Christopher Milly, U.S. Geological Survey)	A
E-1-310	A	22	35	23	30	The description on surface water and groundwater in part repeats with those in sections 1.3.1.1, 1.3.1.4 and 1.3.1.5. These parts on the same topic should be merged. (Xiaoqiu Chen, Peking University)	NA
E-1-311	A	22	35			How large are the increases in 'large basin' and 'continental river' runoff referred to in this section? Are they considered significant in the context of the annual variability in regional runoff?	Continental and regional scale

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						(Terry Marsh, Centre for Ecology and Hydrology)	
E-1-312	A	22	37	22	42	Are these the best citations available? The first study simply did not show that the trend was anything different than natural variability, and a quick look at model outputs shows that natural variability alone could have produced their results. Rigorous detection and attribution of runoff change simply requires a larger data set. The authors, at most, speculated on permafrost melting as a cause. They did no quantitative analysis of that explanation; models suggest the trend could easily have been generated by natural variability in precipitation. The Gedney et al. result is based on a questionable runoff reconstruction and has conclusions that fly in the face of much of the literature on the subject, which is not surprising, as it ignores the physical feedback of boundary-layer drying and heating and the biological feedback of increased plant growth. (Christopher Milly, U.S. Geological Survey)	NA
E-1-313	A	22	37			It should be noted that while some studies suggest runoff rates might have increased for some rivers, other studies indicate that for many, if not most, other cases maximum runoff rates have either not changed or declined . For example, Kundzewizc et al (2004) undertook a study of world-wide hydrological time series of maximum annual river flow obtained from the Global Runoff Data Centre (GRDC) in Koblenz, Germany. Their analysis of 195 long time series of annual maximum flows drawn from this database does not support the hypothesis of general growth of flood flows. They note that "Even if 27 cases of strong, statistically significant increase have been identified by Mann-Kendall's test, there are 31 decreases as well, and most (137) time series do not show any significant changes. Some regional patterns have been observed. However, a caution is needed, that in case of strong natural variability, a weak trend, even if it exists, cannot be detected by statistical testing." See my comment for p5 line 30 for this reference, and others. (Indur Goklany, US Department of the Interior)	A
E-1-314	A	22	40	22	40	Insert a between in and decrease; insert an after and (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-315	A	22	40			(The same also in SPM, p. 19, line 8) Reduction of transpiration from plants at increasing CO2 concentration and consequently increase of runoff - this statement need to be expalined more in detail. Most of plants will produce more biomass at increasing CO2 concentration and available soil moisture. This is not possible at decreasing transpiration. There are information on the plant stress due to high CO2 concentration, these results must be considered more complex and need further research. On the other hand increased air temperature at no change of relative	TR

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						humidity will cause increase of potential evaporation. I would like to mention only that such simplified statement might be confusing a litlle. (Milan Lapin, Faculty of Mathematics, Physics and Informatics, Comenius University)	
E-1-316	A	22	41	22	42	This sentence seems to be not neccesary "However, other evidence for such a CO2-runoff relationship is difficult to find". (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	A
E-1-317	A	22	42	22	42	Delete: of the, insert: for an (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-318	A	22	51	23	4	The cited decrease of pan evaporation is interesting because of increasing potential evaporation in most of regions since 1951. The pan evaporation data belong among measurements with very low reliability, especially at the beginning of regular measurements (about 1960), so calculated increased actual and potential evapotranspiration by physically correct methods are more reliable than the pan measurements. (Published paper from Slovakia: HRVOL, J., LAPIN, M., TOMLAIN, J. (2001): Changes and variability in solar radiation and evapotranspiration in Slovakia in 1951-2000. Acta Meteorol. Univ. Comen., XXX (2001), 31-58. (results up to 2005 are prepared for publication) showed continual increčase of potential evapotranspiration in Slovakia for summer and spering seasons) (Milan Lapin, Faculty of Mathematics, Physics and Informatics, Comenius University)	TR
E-1-319	A	23	0	23	0	I think a table showing no consistent trends is not indispensable, particularly considering the need of reducing the chapter in length. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Listed in SM
E-1-320	A	23	0			Groundwater: Across much of the globe, groundwater is a major water supply and irrigation source; it also serves to sustain river flows and wetlands. Given the importance of groundwater, this short paragraph provides an inadequate overview of current observational evidence (doubtless reflecting its paucity in the peer-reviewed literature). There is strong evidence (e.g. in Spain, USA and China) for the impact of heavy abstraction regimes on groundwater resources (and supported wetlands) but, very little observational evidence appears to have been examined to help determine what climate-driven trends (in relatively undisturbed aquifers) may exist. The references on pages 20 & 21 to increasing runoff and a lack of trend in ET (and a possible decline in AE), provide little support for any assumption that a	There is lack of refrences

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						decline in overall aquifer recharge is occurring. (Terry Marsh, Centre for Ecology and Hydrology)	
E-1-321	A	23	1	23	10	Vegetation type and land use significantly affect evapotranspiration. Water transpired through leaves and needles is pumped up from the roots, so deep-rooting plants and plants with high fine root biomass can have higher transpiration acitivty. E.g. conifer forests have higher evapotranspiration rates than deciduous forests. This may explain the observed increase in ET but decrease in ETpan in some regions. A brief discussion of the role of vegetation in ET should at least be mentioned. (Heiko Balzter, Centre for Ecology and Hydrology)	TR
E-1-322	A	23	8	23	8	It appears that the reference to Milly et al (2002) does not belong here. The relevant citation is PCD Milly and KA Dunne, 2001, Trends in evaporation and surface cooling in the Mississippi River Basin, GRL, 28(7), 1219-1222. (Christopher Milly, U.S. Geological Survey)	TR
E-1-323	A	23	9			We (WGI, Ch3) conclude that in determining evapotranspiration trends, there is a trade-off between less solar radiation and increased surface wetness, with the latter generally dominant. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	TR
E-1-324	A	23	14	23	14	Delete water (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-325	A	23	14	23	14	delete "water" (Heiko Balzter, Centre for Ecology and Hydrology)	A
E-1-326	A	23	19	23	19	Insert s at end of zone (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-327	A	23	19	23	19	"coastal zones" (Heiko Balzter, Centre for Ecology and Hydrology)	A
E-1-328	A	23	32			This table should be modified to include results from studies noted in my comment for p5 line 30. (Indur Goklany, US Department of the Interior)	Some results listed in text, others in SM
E-1-329	A	23	32			Table 1.2: The Table would have greater impact (and the apparent contradictions would be less potentially damaging) if it was complemented by representative time series plots illustrating some of the trends referred to. The target audience would then have a greater appreciation of the complexity of the trends, the importance of medium and long term perturbations, and the sensitivity of trend assessments to changed in the timespan examined. Suggest replacing 'due to' to 'attributed to' in the short 'observed change' commentaries. 3rd box down: United Kingdom, Last 50 years, "no trend in annual runoff", "Hannaford & Marsh, 2005". This is	Modified and move to SM

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						misleading. We would favour "Last 40 years". "Increasing annual runoff in northern and western areas; runoff patterns associated with NAO. No runoff trends in hydrometric records longer than 60 years". The 'comment' should be revised (probably by tightening the geographical focus) to provide a better reconciliation between Lins & Slack, et al (26% increase in runoff in the USA) and Stewart et al (30% decline in annual discharge from North America). Clarification is needed of the 'decreased rainfall (41.3%)' for the Yellow River Basin. The percentage presumeably refers to the contribution the rainfall decrease makes to the overall decline in runoff. (Terry Marsh, Centre for Ecology and Hydrology)	
E-1-330	A	23	32			Table 1.2. The table needs a "global" location entry, with reference to Milly et al. (2005, Nature, Global pattern of trends...). (Christopher Milly, U.S. Geological Survey)	A
E-1-331	A	23	32			Table 1.2. The citation of Milly et al (2005) under United States does not belong there. Rather, the relevant citation here is PCD Milly and KA Dunne, 2001, Trends in evaporation and surface cooling in the Mississippi River Basin, GRL, 28(7), 1219-1222. (Christopher Milly, U.S. Geological Survey)	A
E-1-332	A	23	32			Table 1.2. The entry for the U.S. needs to be modified to properly reflect the findings. First, Lins and Slack (1999) data go back to 1914 in many cases. Their analysis of streamflow data for relatively undisturbed watersheds shows that while there may be an increase in the discharges due to lower and middle streamflow regimes, there seems to be no general upward trend in the highest flows due to hydrological events alone, suggesting that the likelihood of floods due to weather or climate may have actually declined. Second, add information from McCabe and Wolock (2002). Reference: McCabe, G.J. and Wolock, D.M. 2002. Trends and temperature sensitivity of moisture conditions in the conterminous United States. Climate Research 20: 19-29. (Indur Goklany, US Department of the Interior)	Modified and move to SM .
E-1-333	A	23	32			Here and elsewhere I like the use of tables summarizing the available studies and the changes found. Consider to have them for all systems/sections (there are a few missing now). (Albert Klein Tank, Royal Netherlands Meteorological Institute)	A
E-1-334	A	23	33	23	33	United States trends. Reference (Groisman et al. 2004) is cited as Parel (2004) Last names and first names of the authors were incorrectly misplaced. The correct list of the authors is: Groisman, P.Ya., R.W. Knight, T.R. Karl, D.R. Easterling, B.Sun, and J.M.Lawrimore, 2004: Contemporary Changes of the Hydrological Cycle over	A

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						the Contiguous United States: Trends Derived from In-Situ Observations, J. Hydrometeorol. ,5, 64-85. (Pavel Groisman, University Corp. for Atmospheric Research)	
E-1-335	A	24	0	24		Table 1.2a. Bottom row right column. Too many references. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-336	A	24				Table 1.2 I think the decline in inflow into reservoirs in southwest Australia reported by IOCI (2002) is bigger than 5% - more like 40% (Neville Nicholls, Monash University)	A
E-1-337	A	25	0	25		Table 1.2a. Devils Lake, USA not North Dakota (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-338	A	25	3	26	3	Floods are studied in 1,3,2,2 : Hydrology and water ressources and also in 1,3,8 Disasters and hazards.I suggest that 1,3,2,2 and 1,3,8,1 (page 56) would be merged in 1,3,8 into 1,3,8,1 for floods and 1,3,8,4 for droughts and Economic and insurances losses becomes 1,3,8,5 . (Annick Douguédroit, University de Provence)	NA
E-1-339	A	25	3			This section will attract considerable attention from policy makers, and merits a critical review. As it stands Tables 1.3a and 1.3b provide inadequate support for the statements in the summary para. The Chapter really requires tables presenting 'Observed changes in flood magnitude/frequency and drought magnitude/frequency'. The terse titles actually featured ('Floods' and 'Droughts') betray the paucity of observed evidence available or reviewed as part of the FAR process. It is accepted that the peer-reviewed literature may be unable to furnish such information but such fundamental limitations will need to be addressed by the IPCC so that future assessments will have greater utility for policy makers. The 'Observed change' comment on the River Elbe should be extended to read: '….much larger than the 100-yr flood but there is no increasing trend in flood magnitude in a record from 1827.' (Terry Marsh, Centre for Ecology and Hydrology)	Modified and move to SM
E-1-340	A	25	3			Cross-link with WG1, Ch 3. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	A
E-1-341	A	25	5		7	Good place to refer to Ch3 WG1 (Gabi Hegerl, Duke University)	A
E-1-342	A	25	8	25	9	ongoing intensification of the water cycle'. What exactly does this mean? There has been no significant trend in global precipitation. Compare with WG 1, chapter 3 conclusions. (Nathan Gillett, University of East Anglia)	A
E-1-	A	25	8			We (LA's of WG I, Ch 3) have had a discussion on the "intensifying hydrological	A

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343						cycle" and concluded that this wording is better not used. Instead we say that "hydrologic conditions have become more extreme". (Albert Klein Tank, Royal Netherlands Meteorological Institute)	
E-1-344	A	25	10	25	15	PDSI referrred to on line 10, but not spelt out until line 15. Redraft with PDSI in brackets after full definition. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	(Palmer Drought Severity Index)
E-1-345	A	25	15			I think there are concerns about the effectiveness of PDSI as an indicator of drought in mid-latitude areas also. The PDSI, I believe, parameterises evaporation based on temperature, and this has been attacked as unrealistic. I think there remain doubts about drought trends, because of this. (Neville Nicholls, Monash University)	NA
E-1-346	A	25	19	25	20	last not least. This sentence contradicts comment in first row of Table 1.3a below. See also line 7-8 on page 28. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-347	A	25	19	25	20	Final statement is inconsistent with Table 1.3a, which has flood increases/decreases in some areas/studies. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	A
E-1-348	A	25	19	25	19	"...least few decades..." to "..last few decades..." (Pedro Ribera, Universidad Pablo de Olavide)	A
E-1-349	A	25	23			Table 1.3a: Regarding the entry on the Elbe, It should also be noted that Mudelsee et al. (2003, 2004) examined floods on the Oder and Elbe Rivers and didn't find any increase in flood frequency in recent years (compared to the long term record going back to 1021 AD for the Elbe and 1269 AD for the Odere). To quote from their 2003 paper: "For the past few decades, however, observations ... do not show a clear increase in flood occurrence rate. ...For the past 80 to 150 yr, we find a decrease in winter flood occurrence in both ... rivers, while summer floods show no trend, consistent with trends in extreme precipitation occurrence. The reduction in winter flood occurrence can partly be attributed to fewer ... [ice jams] ... at the end of the winter." This entry should be modified to reflectMee et al's work. (Indur Goklany, US Department of the Interior)	A
E-1-350	A	25	23			Table 1.3a: Regarding the entry on global trends, we note that Kundzewizc et al (2004) undertook a study of world-wide hydrological time series of maximum annual river flow obtained from the Global Runoff Data Centre (GRDC) in Koblenz, Germany. Their analysis of 195 long time series of annual maximum flows drawn from this database does not support the hypothesis of general growth of flood flows. They note that "Even if 27 cases of strong, statistically significant increase have been identified by Mann-Kendall's test, there are 31 decreases as	A

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						well, and most (137) time series do not show any significant changes. Some regional patterns have been observed. However, a caution is needed, that in case of strong natural variability, a weak trend, even if it exists, cannot be detected by statistical testing." This entry shoul be revised to account for these results, as well as the results of Cluis and Laberge (2001). See Comment 9 for additional references. (Indur Goklany, US Department of the Interior)	
E-1-351	A	25	23			Some of the table entries (e.g. lines 2,4,5) do not describe any change under "Observed change". Instead there is only a statement on whether or not an event has occurred. These lines should be removed for clarity. This also happens in other tables. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Moved to SM
E-1-352	A	26	0			Table 1.3a: The information on the Yangtze should be augmented with information from Jiang, T., Zhang, Q., Blender, R. and Fraedrich, K., " Yangtze Delta floods and droughts of the last millennium: Abrupt changes and long term memory," Theoretical and Applied Climatology 82: 131-141 (2005). (Indur Goklany, US Department of the Interior)	A
E-1-353	A	26	2			Table 1.3b: In the entry for the Eastern U.S., it should be noted that 1941-1999 is a relatively short record. As noted by Quiring (2004), the droughts that occurred during the 16th century tended to be both longer and more severe than they are today. This was based on a 800-year tree ring based reconstruction of the Palmer Hydrological Drought Index to document the frequency, severity, and duration of growing-season moisture anomalies (both wet and dry anomalies) in the southern mid-Atlantic region of the US. Reference: Quiring, S.M. 2004. Growing-season moisture variability in the eastern USA during the last 800 years. Climate Research 27: 9-17. (Indur Goklany, US Department of the Interior)	NA
E-1-354	A	26	2			Table 1.3b: The entry for the Western U.S. should be put into its long term context, namely, the current drought is not unusual. We recommend using Cook, E.R., Woodhouse, C., Eakin, C.M., Meko, D.M. and Stahle, D.W., "Long-term aridity changes in the western United States," Sciencexpress.org / 7 October 2004. To quote from their abstract, "The western United States is experiencing a severe multiyear drought that is unprecedented in some hydroclimatic records. Using gridded drought reconstructions that cover most of the western United States over the past 1200 years, we show that this drought pales in comparison to an earlier period of elevated aridity and epic drought in AD 900 to 1300, an interval broadly consistent with the Medieval Warm Period..."	NA

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						(Indur Goklany, US Department of the Interior)	
E-1-355	A	26	11	26	11	the hemisphere' - which one? (Nathan Gillett, University of East Anglia)	North
E-1-356	A	26	11			which hemisphere? (Chris Thomas, University of York)	North
E-1-357	A	26				Table 1.3b - Much of Europe and UK - no evidence of significant increased droughts? - The drought in 1990-1993, in 2000 and 2003 was in Slovakia and also in other parts of central and SE Europe the most serious since 1901 at least (also at consideration of runoff and soil moisture). There are many references on this problem. (Milan Lapin, Faculty of Mathematics, Physics and Informatics, Comenius University)	Lack of references
E-1-358	A	26				Table 1.3a - Central Europe - no evidence of increased summer floods? - In spite of significant flood protection constructions and measures the summer floods in 1997, 1998, 2002, 2005 and 2006 (locally also in the other years since 1994) is possible to count among the most devastating since 1951 at least. There are many references on this problem. (Milan Lapin, Faculty of Mathematics, Physics and Informatics, Comenius University)	Lack of references
E-1-359	A	27	18	27	26	There is no consistency in the use of references in the text that are also in the relevant table. E.g. why give a reference on line 24, but not give a reference after Lake Baikal Russia. If the references are in the table do they need to be recited in the text? This comment applies to all text associated with tables. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-360	A	27	18	27	18	Insert (Table 1.3c) after processes (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-361	A	27	26	27	26	Insert s after increase (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-362	A	27	27	27	27	Delete (Table 1.3c) (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-363	A	27	30	27		Table 1.3c First row. There must be a more up-to-date reference than Gertn and Adrian 2002. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	No references
E-1-364	A	27	30			Tabel 1.3c Insert more recent references (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Lack of references
E-1-365	A	28	3	28	3	This summary should show the influence of Milly et al. (2005, Nature, Global pattern of trends...), who showed that the historical global pattern of streamflow	A in text

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						trends was much more readily explained by models of forced climate change than by natural variability. (Christopher Milly, U.S. Geological Survey)	
E-1-366	A	28	5	28	7	This seems a bit uncritical … shouldn't this acknowledge that there might also be other causes contributing to the observed changes? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	A
E-1-367	A	28	5	28	20	The opening sentence in the Summary does not reflect the complexity of the observational evidence presented in Chapter 1. Changes in runoff (quoted in Chapter 1) can be largely attributed to increasing temperatures (e.g. increased glacial wasting, thickening of the active layer), rather than an intensification of the water cycle. The apparent lack of trend in evaporation losses also seems inconsistent with the concept of 'intensification'. The only 'drier zone' mentioned in the context of intensifying drought is the Sahel. This constitutes an unconvincing case study: inter-decadal variability (and longer term perturbations) in rainfall are large and rainfall has shown no decline over the last 20 years. In addition, some model studies (e.g. Haarsma, R. J., F. M. Selten, S. L. Weber, and M. Kliphuis. 2005. Sahel rainfall variability and response to greenhouse warming, Geophys. Res. Lett., 32), suggest that increasing rainfall over the next 80 years will reduce the probability of prolonged drought. (Terry Marsh, Centre for Ecology and Hydrology)	Addressed in SM
E-1-368	A	28	5	28	8	Is it consistent to assess that "Changes runoff and stream flow, as well as in droughts and floods, indicate that here is an ongoing intensification of the water cycle…"and "Significant trends in floods ..have not been found"? Is the ongoing intensification of the water cycle significant? (Annick Douguédroit, University de Provence)	A
E-1-369	A	28	5	28	5	Insert in between changes and runoff (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-370	A	28	5	28	5	Insert 'in' before 'runoff'. (Nathan Gillett, University of East Anglia)	Adressed
E-1-371	A	28	6	28	6	What does 'intensification of the water cycle' mean? (Nathan Gillett, University of East Anglia)	A
E-1-372	A	28	7	28	8	The statement concerning global non-detection of flood trend is inconsistent with the first line in Table 1.3a and hence apparently an incorrect summary of the literature. (Christopher Milly, U.S. Geological Survey)	NA
E-1-373	A	28	7	28	8	see comment on lines 19-20 page 25 (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A

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E-1-374	A	28	9	28	9	Insert 'directly' before 'due'. (Nathan Gillett, University of East Anglia)	A
E-1-375	A	28	16	28	16	Insert s after temperature; delete environments (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-376	A	28	19	28	20	Insert a between and decrease; Change last sentence to Temporary lake formation on an Arctic ice shelf has been reported. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	A
E-1-377	A	28	23	32		Section 1.3.3: Coastal region changes may be more due to human activity on ground than due to local/regional sea-level rise. Causes of regional sea-level rise are complex and not necessarily due to increasing GHG or warming of the earth's temperature. Storm surges & storm intensity are again part of natural variability and not a result of anthropogenic climate change. The entire discussion here is too general and does not present a convincing case for human-induced climate change impact. This section can be shortened. (Madhav Khandekar, Retired)	The text points out these complexities. Furthermore, as pointed out in section 1.2.1, non-climate, as well as climate drivers of change, can affect systems & sectors directly and indirectly through their effect on climate (p. 9, lines 22-25). We do discuss these, and section 1.3.3.1 has been rewritten to clarify the issues. Therefore this section should not be shortened.
E-1-378	A	28	23			Section 1.3.3. I have not reviewed Chapter 1 previously but thought I should look at it very quickly prior to submitting comments at the deadline. It is an impressive compilation of a vast amount of information and the following comments are not intended to be a criticism of the author team. However, in the context of reviewing Chapter 6 (Coastal) and being one of the Canadian LAs for Chapter 14 (NA), I would like to suggest more balance in Table 1.4, including more Canadian examples and perhaps a reduction of the 7 US examples. Overall, I think that Table 1.4 could be greatly improved with little effort by making column 3 the first column and including a wider geographic range with more examples. There is room in the table as it now stands to almost double the number of references. I would be happy to work with the Chapter 1 team in Cape Town to provide suggestions of additional coastal examples from Canada and elsewhere. Likewise for Table 1.5, where the international mix is much better. (Donald L. Forbes, Bedford Institute of Oceanography)	OK We were asked to shorten this chapter! Please do. See also comment E-1-384
E-1-379	A	28	26			"climate-induced" and "land subsidence" may also be "anthropogenic" (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Changed wording
E-1-380	A	28	34	28	35	In my opinion the indicated rate of rising sea level for the last century does not reflect the actual rate of change. In the north Spain Marcos et al (2005) detect and clearly accelerated trend that can arrive to 2.87 - 6.08 mm/yr during the 90s. As an increased trend in sea level rise can affect the vulnerability of coastal areas, I think must be stated. Reference: Marcos M, Gomis D, Monserrat S, Álvarez E, Pérez	This is a local change over a short (decadal) period—thus does not reflect a global change. Leave text as is.

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						B, García-Lafuente J (2005) Consistency of long sea-level time series in the Northern coast of Spain. Journal of Geophysical Research 110:1-13 (Ricardo Anadon, Universidad de Oviedo)	
E-1-381	A	28	34		36	Good place to refer to Ch5 WG, to make sure to be consisten, particularly with their assessment of acceleration (Gabi Hegerl, Duke University)	Added reference to Chap. 5, section 5.5
E-1-382	A	29	2	29	6	Is the global trend in extreme high water levels really due to all these indices? This seems unlikely to me, since they don't all have significant trends. More assessment is needed here, rather than just reporting the results of a single study. (Nathan Gillett, University of East Anglia)	Changed wording
E-1-383	A	29	4	29	6	This section could be shortened by referring to some of the indices of climate variability simply using the initials already used for them earlier in the chapter (ENSO, NAO etc), or by listing only some of the indices. (Robert Wilson, Universidad Rey Juan Carlos)	Abbreviated sentence
E-1-384	A	29	10			Table 1.4 Too many examples for USA - combine into one box? There must also be SE Asian examples and Australian. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	See also comment E-1-378. Perhaps Reid can provide examples?
E-1-385	A	30	3			Section 1.3.3.1. It is useful where possible to include more precise quantitative information rather than qualitative statements on percentages of coasts 'eroding'. One might report 'Over the last century, 67% of the eastern coastline of the UK has experienced a landward retreat of the low-water mark (Taylor et al. 2004)'. Taylor JA, Murdock AP and Pontee NI 2004 A macro-scale analysis of coastal steepening around the coast of England. Geographical Journal 170: 177-188. (Thomas Spencer, University of Cambridge)	Added sentence and reference
E-1-386	A	30	5	30	5	Isn't beach erosion a natural process in many regions? Or does this text mean that beach erosion has been enhanced by anthropogenic climate change on 70% of sandy beaches? (Nathan Gillett, University of East Anglia)	That 70% of the world's beaches are eroding is a fact, regardless of cause. The causes are many—including SLR, natural, and anthropogenic factors, some of which are discussed in this section. The section has been rewritten for clarity.
E-1-387	A	30	18	30	28	To what extent is the beach erosion discussed a natural process, and to what extent is it due to anthropogenic climate change? This needs to be assessed. (Nathan Gillett, University of East Anglia)	The paragraph has been rewritten to clarify the issue.
E-1-388	A	30	18			What is meant by "historic" sea level rise? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Last century. Changed wording.
E-1-389	A	30	24	30	24	Insert s after rate (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done

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E-1-390	A	30	28	30	28	Where in the tropics? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	SE Asia
E-1-391	A	30	34	30	34	Delete curtaillment, insert curtailment (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-392	A	30	41			Section 1.3.3.2. It would be helpful to provide some non-US statistics here. 'Saltmarsh inventories of SE England show a net areal loss of 1000 ha (1973 - 1998), or 33% of saltmarsh extent in 1973, although more recent (post-1988) loss rates appeared to have slowed (Cooper et al. 2001)'. Cooper NJ, Cooper T and Burd F 2001 25 years of salt marsh erosion in Essex: implications for coastal defence and nature conservation. Journal of Coastal Conservation 9: 31-40. In many cases, such as in estuaries for example, the loss of coastal wetlands is related to wider hydrodynamics and sediment budget contexts, themselves often determined by human interventions such as channel dredging and land claim. (Thomas Spencer, University of Cambridge)	Added
E-1-393	A	31	3	31	3	Insert space after estuaries (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.
E-1-394	A	31	8	31	8	The research of Hughes and Paramor (2004) has been contested. A recent review by Wolters et al. (2004), for example, concludes that although laboratory evidence shows that bioturbation and herbivory by the ragworm Nereis can lead to sediment instability and loss of pioneer plant species the field evidence is more equivocal. [Wolters M, Bakker JP, Bertness MD, Jefferies RL and Moller I 2005 Saltmarsh erosion and restoration in south-east England: squezing the evidence requires realignment. Journal of applied Ecology 42: 844-851]. For a more secure example of non- sea level rise control, perhaps spatial variations in the erodibility of marsh clays (Crooks and Pye 2000) might be better. [Crooks S and Pye K 2000 Sedimentological controls on the erosion and morphology of saltmarshes: implications for flood defence and habitat restoration. In: Pye K and Allen JRL (eds) Coastal and estuarine environments: sedimentology, geomorphology and geoarchaeology. Geological Society of London, Special Publication 175: 207-222. (Thomas Spencer, University of Cambridge)	Deleted. Added.
E-1-395	A	31	8	31	8	Delete first (; insert space after) (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Changed.
E-1-396	A	31	13	31	13	Old references. There must be a more recent review paper on this key topic that has been widely discussed. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Deleted first two references; the last one is still relevant. Can Reid supply a more recent one?
E-1-397	A	31	13	33	13	In their revison of the Impacts of marine ecosystems, fisheries and aquaculture in the coastof Spain, Anadón et al (2005) has presented evidences of changes in the	Irrelevant here. Discuss in section 1.3.4

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						recurrence of harmful algal blooms and the presence of pathogens (like Perkinsius) that could be related to increased temperatures or hydroclimatic oscillations. The geographical expansion of some species, or the increased recurrence of algal blooms is actually affecting some productive activities; In the North West coast of Spain the mussel raft production is affected by this problems. I suggest to introduce a small phrase related with these problem as "The expansion of range distribution affect also to harmful species of microalgae, affecting the aquaculture of mollusc in some coastal areas(Anadón et al, 2005). Reference: Anadón R, Duarte CM, Fariña C (2005) Impactos sobre los Ecosistemas Marinos y el Sector Pesquero. In: Moreno JM (ed) Evaluación Preliminar de los Impactos en España por efecto del Cambio Climático. Ministerio Medio Ambiente, Madrid, p 147-182 (Ricardo Anadon, Universidad de Oviedo)	
E-1-398	A	31	13	33	13	In relation with the above problem of changes in seasonality and recurrence of hydrographic and biological events, some evidences exist about their effect on fisheries. Platt et al (2003) shows changes in cod recruitment when spring algal blooms occur before or after the mean time. As the time for blooming algae is changing in relation with Climate change, this effect could affect directly the abundance of some species objective with high economic importance in the world. This process probably occurs Reference: Platt T, Fuentes-Yaco C, Frank KT (2003) Spring algal bloom and larval fish survival. Nature 423:398-399 (Ricardo Anadon, Universidad de Oviedo)	Irrelevant here. Discuss in section 1.3.4
E-1-399	A	31	24	31	28	This is simplistic because hurricanes can also introduce sediment into areas that are remote or isolated from riverine sources of sediment, thus aiding coastal wetland survival in the face of sea level rise. Thus Hurricane Andrew (1992) generated 2 - 6 cm more vertical marsh accretion in the coastal marshes of southern Louisiana than occurred in the year before or the year after the storm [Cahoon DR, Reed DJ, Day JW Jr, Steyer GD, Boumans RM, Lynch JC, McNally D and Latif N 1995 The influence of Hurricane Andrew on sediment distribution in Louisiana coastal marshes. Journal of Coastal Research Special Issue 21: 280-294. See also section 6.4.1.4. (Thomas Spencer, University of Cambridge)	Added reference to Cahoon et al., 2003 (from Chap. 6, 6.4.1.4).
E-1-400	A	31	24	31	24	Insert initial K after second Williams as assume it is another Williams? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	It's the same Williams for both.
E-1-401	A	31	24	31	28	Are the authors attributing increased landfalling hurricanes to anthropogenic influence? WG 1 concludes that there is some evidence for an increase in tropical	Not at all. The paragraph described coastal vegetation changes due to storms and other

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						cyclone intensity worlwide, but that we cannot attribute this to anthropogenic forcing. For landfalling hurricanes in the US the chances of attributing to anthropogenic forcing are lower. (Nathan Gillett, University of East Anglia)	perturbations—no specific anthropogenic attribution was given.
E-1-402	A	31	30	31	47	There is any evidence of coastal erosion along the Mediterranean coasts (Spain, France, Italy, former Yugoslavia) or this part of the Mediterranean is not affected by it? What is the effect of sediments at the mouth of Po river? (Giampiero Maracchi, Institute of Biometeorology)	This paragraph does not discuss coastal erosion at all. Question is not relevant here.
E-1-403	A	31	30	31	47	Some discussion on the findings of P. Lionello must be introduced at this point on surges in the Adriatic sea (Giampiero Maracchi, Institute of Biometeorology)	Lionello is referred to below. Also, too many references to add—let's pick the most useful one.
E-1-404	A	31	30	31	47	I insist to discuss more in details the situation in the Mediterranean Sea and discuss occurrence of surges in the Adriatic based on results by Lionello. (Giampiero Maracchi, Institute of Biometeorology)	Ditto
E-1-405	A	31	30	31	43	Again no pattern in relation to citation in text and in Table 1.5 (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	This was left to authors.
E-1-406	A	31	30	31	47	Add references and their discussion: Fenoglio-Marc L., Long-term sea level change in the Mediterranean Sea from multi-satellite altimetry and tide gauges. Physics and Chemistry of the Earth, 27, 1419-1431 (2002). Samuel, S., K. Haines, S. Josey and P.G. Myers, Response of the Mediterranean Sea thermohaline circulation to observed changes in the winter wind stress field in the period 1980-1993. J. Geophys. Res., 104, 7771-7784, 1999. Tsimplis M.N. and S.A. Josey, Forcing of the Mediterranean Sea by atmospheric oscillations over the North Atlantic. Geophys. Res. Let., 28(5) 803-806,2001 Vignudelli S., G.P. Gasparini, M. Astraldi and M.E. Schiano, A possible influence of the North Atlantic Oscillation on the circulation of the Western Mediterranean Sea. Geophys. Research Let., 26, 623-626, 1999. Lionello P., E.Elvini, A.Nizzero (2003): Ocean waves and storm surges in the Adriatic Sea: intercomparison between the present and doubled CO2 climate scenarios, Clim. Research., 23, 217-231 (Giampiero Maracchi, Institute of Biometeorology)	Ditto
E-1-407	A	31	37			After "relative sea level rise" add the following: "due to non-climate change related causes." (Indur Goklany, US Department of the Interior)	Done
E-1-408	A	31	40	31	40	change "...surges in Venice has averaged..." to "...surges has averaged..." (Pedro Ribera, Universidad Pablo de Olavide)	Done
E-1-409	A	31	40	31	40	"in Venice" is repeated from the previous line and should be removed. (Robert Wilson, Universidad Rey Juan Carlos)	Done

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E-1-410	A	31	44	31	44	Amend beginning of sentence to read: Surges have shown…… (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-411	A	32	3	28	3	1,3,2,4 Hydrology and water ressources: Summary (Annick Douguédroit, University de Provence)	Summary titles changed.
E-1-412	A	32	12	32	22	Too much emphasis on USA examples. Suggest that good data should be available from the Netherlands. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Can Reid provide examples? *The authors looked for published papers on observed coastal changes in the Netherlands, but did find any. The impacts there are very confounded by other driving forces.*
E-1-413	A	32	17	32	17	reference is missing (Heiko Balzter, Centre for Ecology and Hydrology)	Reference added
E-1-414	A	32	22			For your information, the Netherlands spends Euro 44 million annually only on sand supply restoring its shoreline. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	A. Klein—Please supply reference. *Authors looked for published citation with this information but couldn't find one. The reviewer needs to supply a published paper for consideration.*
E-1-415	A	32	24	32	24	1,3,3,4 Coastal processes and zones: Summary (Annick Douguédroit, University de Provence)	Summary titles changed to be specific.
E-1-416	A	32	37	39		Section 1.3.4: Once again, the discussion is too general with no in-depth analysis. Coral reef changes may have been accelerated due to strong El Nino of 1997/98; also changes in North Atlantic phytoplankton may be more due to NAO variations and not due to human-induced climate change. Rising (sea)water temperature and associated changes need not be linked to human activity only. Solar variability can produce changes in SST (Sea Surface Temperature), see Nature, 329, p.142-143 & Space Science Review, 94,p.1-11(author G C Reid). This section can be and should be shortened and several references deleted as these are of minor importance. (Madhav Khandekar, Retired)	Text rewritten.
E-1-417	A	32	37			Marine and freshwater should be seperated into two sytems as they are so different. This would make little difference to the length as the same relevant text as so far produced could be put under the freshwater heading. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	They are in different sections.
E-1-418	A	32	37			Considering the importance of mangroves little text is given to them compared to corals, which have half a page. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Corals were requested by earlier expert reviewers. Mangroves also covered in 1.3.3.
E-1-419	A	32	39	33	7	This introductory paragraph needs redrafting. The pelagic realm is not the only one in the oceans and is not just found at the surface. The deep ocean fauna may also	Text rewritten.

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						be crucial to climate regulation. The deep and intertidal benthos are also important. If the present structure is to be maintained then a sentence introducting freshwater systems needs adding after the reference on line 5. Line 6. Pollution does not just derive from terrestrial runoff. An important element with respect to climate change has been missed out, the role that the biological pump plays in carbon cycling. There is strong evidence in the North Atlantic at least for a decline in diatoms associated with warming (Edwards et al.). Diatoms because of their opaline composition act as ballast in sinking particles carrying carbon to the deep ocean. If the marked decline in these organisms seen in the North Atlantic proves to be more widespread it could indicate that the oceans are now less able to take up CO2 so that atmospheric levels might increase even more rapidly. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	
E-1-420	A	33	5	33	5	Needs sentence on importance of fresh water biology (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text rewritten.
E-1-421	A	33	9	33	17	This is generally true for all systems and could be moved to the introduction (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Text rewritten.
E-1-422	A	33	11	33	11	Insert comma after Recently (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text rewritten.
E-1-423	A	33	19	33	19	Delete the (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text rewritten.
E-1-424	A	33	23	33	24	SCUBA diving is NOT a major source of stress for the vast majority of coral reefs (in comparison to climate change, pollution, overfishing and destructive fishing, etc), except perhaps in highly localised settings where tourism use is very intense. This reference to non-climate stresses should be consistent with more informed treatments of stresses such as Figure 16.1. (Paul Marshall, Great Barrier Reef Marine Park Authority)	Text rewritten.
E-1-425	A	33	28	33	28	...regional climate effects, including on plankton, from direct... (Annick Douguédroit, University de Provence)	Text rewritten.
E-1-426	A	33	39	34	19	Coral reefs may be iconic, but they are only part of marine systems and this section should be moved to just before 1.3.4.4. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Section rewritten.
E-1-427	A	33	39			This is not a very comprehensive or well-structured review/overview of the topic. It omits key issues relating to non-climate stresses (such as the many important stresses listed in Figure 16.1). It should make clear that coral bleaching is triggered by unusually high water temperatures early on, to provide context and relevance of the issue. It should also cite primary data papers in preference to overview/policy comment papers to support specific claims.	Section rewritten.

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						(Paul Marshall, Great Barrier Reef Marine Park Authority)	
E-1-428	A	33	41	33	43	This reference to non-climate stresses should be consistent with more informed treatments of stresses such as Figure 16.1. (Paul Marshall, Great Barrier Reef Marine Park Authority)	Section rewritten.
E-1-429	A	33	47	33	51	The statistic from Wilkinson 2004 is NOT that 16% of the world's corals were killed; rather, it is that 16% of the world's coral reefs were "seriously damaged". This is an important distinction and this statement needs to be corrected. (Paul Marshall, Great Barrier Reef Marine Park Authority)	Section rewritten.
E-1-430	A	33	48	33	49	This sentence is not clear. Bleaching occurs within 2-4 weeks of what kind of event? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Section rewritten.
E-1-431	A	33	48	33	49	Occurs within 2-4 weeks of what? - high temperatures? Move lines 9 to 15 from page 34 to after this point. Line 49 Start a new paragraph with Regional-scale (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Section rewritten.
E-1-432	A	33	49	33	49	Insert new paragraph of Lines 9-15 on Page 34 after weeks. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Section rewritten.
E-1-433	A	34	1	34	1	Insert and was between Ocean and linked (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Section rewritten.
E-1-434	A	34	14	34	14	Hughes et al 2003 is not a primary source of knowledge on this issue. Suggest Coles and Brown 2003, as cited in previous paragraph. (Paul Marshall, Great Barrier Reef Marine Park Authority)	Section rewritten.
E-1-435	A	34	16			Add a new para on this line to note that some studies have also measured a net increase in the growth rate of coral at higher temperatures [see, e.g., (1) Lough, J.B. and Barnes, D.J. 2000. Environmental controls on growth of the massive coral Porites. Journal of Experimental Marine Biology and Ecology 245: 225-243. (2) Bessat, F. and Buigues, D. 2001. Two centuries of variation in coral growth in a massive Porites colony from Moorea (French Polynesia): a response of ocean-atmosphere variability from south central Pacific. Palaeogeography, Palaeoclimatology, Palaeoecology 175: 381-392. (3) Carricart-Ganivet, J.P., 2004. Sea surface temperature and the growth of the West Atlantic reef-building coral Montastraea annularis. Journal of Experimental Marine Biology & Ecology 302: 249-260].. A modeling study undertaken by McNeil et al. (2004) to account for changes in the coral calcification rate due to both the saturation state of CaCO3 in seawater and the sea surface temperature suggests that annual average coral reef calcification rates will increase with future ocean warming and eventually exceed pre-industrial rates by about 35% by 2100. Their results suggest that present coral reef calcification rates are equivalent to levels in the late 19th century and is at odds	Section rewritten.

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						with suggestions of large and potentially catastrophic decreases in the future. [Ref: McNeil, B.I., R.J. Matear & D.J. Barnes, 2004. Coral reef calcification and climate change: the effect of ocean warming. Geophysical Research Letters 31 L22309, doi: 10.1029/2004GL021541.} [See also Riebesell, U. 2004. Effects of CO2 enrichment on marine phytoplankton. Journal of Oceanography 60: 719-729.] (Indur Goklany, US Department of the Interior)	
E-1-436	A	34	17	34	19	The inferential logic in this sentence is not well warranted by the literature cited, and its relevance to the topic is unclear. (Paul Marshall, Great Barrier Reef Marine Park Authority)	Section rewritten.
E-1-437	A	34	21	35	23	In 1,3,4,2 which successively deals with the Pacific ocean and the Atlantic, the 2 paragraphs concerning the Atlantic could be reversed. It would begin by "In the North Atlantic recent macroscale...Richardson 2004" followed by "Both changes... et al. 1998)" which could be reduced as it resumes ideas countained in the previous paragraph, including its reference. There are only few clear information regarding the direction of the numerous shifts presented in those pages. Page 35, lines 13-17 present a perfect example of the pattern shown in Box 1,1."This geographical movement corresponding with the pattren describes in Box 1,1 is much more pronounced than any documental terrestrial study", presumly because there are no physical obstacles in the ocean towards north.. (Annick Douguédroit, University de Provence)	No room to add the new box in chapter; plus need reference from author. It is considered that this is due to advective processes which is mentioned in the paragraph. EXPANDED SENTENCE
E-1-438	A	34	21	37	1	Same remark: There are only few clear information regarding the direction of the numerous shifts presented in those pages. Page 35, lines 31-43 present a perfect example of the pattern shown in Box 1,1. (Annick Douguédroit, University de Provence)	? NO CHANGES
E-1-439	A	34	26	34	26	In the draft the references to climate change effects on marine ecosystems and fisheries are related to SST or general climatic oscillations. In my feeling, changes in the seasonality and recurrence in the coastal circulation could be the driven forces for changes on distribution and abundance of organisms. This problem is not properly stated in the draft. Now, evidences of changes in seasonality of some coastal processes, like upwellings, poleward currents and others, has been obtained. These changes can affect the pelagic ecosystems, and probably the benthic, because some regional areas are modified from retention to dispersion sites. These changes can occur during the spawning period of commercial fisheries, like sardine and anchovy. This mechanism could be responsible of some of the observed changes in fisheries and species distribution. In the north Spain these problem has been studied with data of a long term series; over the increased SST changes in seasonality and	Advective processes have been mentioned in the draft. I think this reference is a rather localised study over 10 years and does not provide evidence of climate change impacts on ecosystems. The reference can be added under the editor's descretion. NO CHANGES

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						intensity of upwelling, as well as on recurrence and intensity of the Iberian Poleward current were detected. I suggest to introduce a phrase like "Also changes in seasonality, recurrence and intensity of hydrographic processes (upwelling, coastal currents,) can occurs as response to climate change (Llope et al, in press) Reference: Llope M, Anadón R, Viesca L, Quevedo M, González-Quirós R, Stenseth NC (in press) Hydrographic dynamics in the Southern Bay of Biscay: integrating multi-scale physical variability over the last decade (1993-2003). Journal of Geophysical Research (Ricardo Anadon, Universidad de Oviedo)	
E-1-440	A	34	26	34	26	Change to: by changes in salinity, wind patterns and ocean circulation (Table 1.6). (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I agree, this should be changed. CHANGED
E-1-441	A	34	29	34	29	Insert Royal before Society (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I agree, this should read the Royal Society CHANGED
E-1-442	A	34	31	34	31	Recently McClain et al (2004) has described, using satellite imaginary analysis, the expansion of the oligotrophic subtropical gyres, both the North Atlantic and the North Pacific; whereas southern oceans subtropical gyres not. In my feeling, this demonstration is one of the more evident influences at of climate change at basin scale. This change could affect the whole ecosystem of subtropical and south temperate areas of the oceans, including both ecosystem dynamic and fisheries. I suggest to introduce a small phrase like "Changes in the extension of the subtropical oligotrophic gyres of the Atlantic and Pacific during the last years (McClain et al, 2004), could influence the productivity and ecosystems of the area, including fisheries" Reference: McClain CR, Signorini SR, Christian JR (2004) Subtropical gyre variability observed by ocean-color satellites. Deep-Sea Research II 51:281-301 (Ricardo Anadon, Universidad de Oviedo)	I feel this satellite study is over a too short time-period to provide evidence of climate change. The reference can be added under the editor's descretion. NO CHANGES *Martin Edwards, our Marine Biological Systems CA, felt that this satellite study was over a too short time-period to provide evidence of climate change but left the final decision up to the CLA, who sided with Martin Edwards and reference wasn't added..*
E-1-443	A	34	31	34	31	At end of line insert: whereas chlorophyll in the NE Atlantic has increased since the mid 1980s (Raitsos et al… 2006). Raitsos, D. E., P. C. Reid, et al. (2005). "Extending the SeaWiFS chlorophyll data set back 50 years in the northeast Atlantic ." Geophysical Research Letters 32(6): art. no.-L06603. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I agree, this reference and line should be added: .....whereas chlorophyll in the NE Atlantic has increased since the mid 1980s (Raitsos et al. 2005). Raitsos DE, Reid PC, Lavender SJ, Edwards

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							M, Richardson AJ (2005) Extending the SeaWiFS chlorophyll data set back 50 years in the northeast Atlantic. Geophys Res Lett 32 CHANGED
E-1-444	A	34	33	34	33	Insert and UK after Pacific; delete to the, insert in (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Yes I agree with this, in my original document this was mentioned, however, since editorial changes it seems to have disappeared. CHANGED
E-1-445	A	34	34	34	34	Insert reference: Southward et al. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	(see comment above) add this reference which has was in my original document: (Southward et al. 2005) Southward AJ, Langmead O, Hardman-Mountford NJ, Aiken J, Boalch GT, Dando PR, Genner MJ, Joint I, Kendall MA, Halliday NC, Harris RP, Leaper R, Mieszkowska N, Pingree RD, Richardson AJ, Sims DW, Smith T, Walne AW, Hawkins SJ (2005) Long-term oceanographic and ecological research in the western English Channel. In: Advances in Marine Biology, Vol 47, Vol 47, p 1-105 This reference should also be included in table 1.6. under Rocky shore/intertidal communities (it was included in the original table) CHANGED
E-1-446	A	34	35	34	35	Amend beginning of sentence to read: Similar shifts were also noted in fish communities of kelp forests in California……; delete off the southern Californian coast	This can be done under the editor's descretion. NO CHANGES *Again, Martin Edwards felt this change was*

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	*unnecessary, but left the final decision up the CLA, who agreed with the more specific "off the coast southern Californian Coast."*
E-1-447	A	34	37	34	37	Delete the, insert a (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Should be the. NO CHANGES
E-1-448	A	34	38	34	38	This is the first time the North Atlantic has been referred to (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	NO CHANGES
E-1-449	A	34	45	34	45	Delete The, insert A; amend to read a decline in krill (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	CHANGED
E-1-450	A	34	46	34	46	Delete decline (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	CHANGED
E-1-451	A	34	47	34	47	Insert will between and and an (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	CHANGED
E-1-452	A	34	51	34	51	Delete Temerate insert Temperature (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	CHANGED
E-1-453	A	35	1	35	1	After index insert: with the strongest correlation with NHT (Beaugrand and Reid 2003) (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I don't think this is correct. (do not change) NO CHANGES
E-1-454	A	35	3	35	3	Delete North Sea; after productivity insert: of the North Sea (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	CHANGED
E-1-455	A	35	5	35	7	Perry et al 2005 Science paper should also be cited here (it is already in the bibliography) (Chris Thomas, University of York)	The Perry reference has been cited earlier in the paragraph. NO CHANGES
E-1-456	A	35	13	35	17	This description of the work is OK, but I would not describe it in quite those terms - the problem is that it is a community-based analysis (nothing wrong with that) which means that expansions of warm-water communites and retraction of cold-water ones are, by definition, symmetrical. It is therefore difficult to claim both warm water species expanding and cold water species retreating as though it was independent evidence. I would reword the second sentence to read "During the last 40 years, there has been a northerly shift by 10deg of plankton communities in the north-east Atlantic". i.e., circuvent the problem (Chris Thomas, University of York)	Not sure about this comment, it is obvious that if the warm-water community has moved 10 degrees northward then a similar response has happened to the cold-water community i.e. retreated 10 degrees northward. I suggest that we do not change this as it is written based on the original manuscript. NO CHANGES
E-1-457	A	35	13	35	23	These large shifts in marine species, much greater than observed amongst terrestrial flora and fauna, should receive much greater emphasis in the Executive Summary, and be carried through more with more emphasis to the TS and SPM. It seems to	I agree this is an important statement, however, it must be written clearly that this difference is probably related to advective

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						me this is one of teh major results of the WG2 AR4. (Jean Palutikof, IPCC WGII TSU)	processes that obviously do not occur in terrestrial systems. For example, it is thought that the shelf-edge current running along the european continental shelf has accelerated over the last decade and so you would expect that warm-water species would be carried further and faster north, so we have to be careful about this statement. *The statement suggested (but rejected by Martin Edwards) was not brought forward the SPM.*
E-1-458	A	35	20	35	20	there' should be 'their'. (Nathan Gillett, University of East Anglia)	I agree, this should be changed to 'their' CHANGED
E-1-459	A	35	25	35	25	Amend title to read Changes in marine fisheries (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I agree, this should be changed to 'changes in marine fisheries' as this section only discusses marine fisheries. CHANGED
E-1-460	A	35	33	35	33	Insert reference: Quero et al. 1998. Quero, JC; Buit, MHDu; Vayne, JJ. Les observations de poissons tropicaux et le rechauffement des eaux de l'Atlantique europeen. Oceanol. Acta. Vol. 21, no. 2, pp. 345-351. 1998. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	This can be done under the editor's descretion *The CLA agreed with Martin Edward and saw no need to include this older reference.*
E-1-461	A	35	34	35	36	Perry et al give some evidence of retreatv of northern species as well as southern expansion. (Chris Thomas, University of York)	OK, no change NO CHANGES
E-1-462	A	35	37	35	37	Amend sentence to read: …..ancovies northward in the North Sea and the extension of the ranges of red mullet and bass northward to …. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	NO CHANGES
E-1-463	A	35	43	35	43	At end of line insert: in the same way as the plankton (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, this should be added. CHANGED
E-1-464	A	35	43	35	43	Add: Those changes can be linked with the plankton changes (1,3,4,3). (Annick Douguédroit, University de Provence)	OK, this should be added. (see comment above) CHANGED
E-1-465	A	35	49	35	49	Insert 'an' between hence and eventual (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, this should be added. CHANGED
E-1-	A	36	8	36	33	What does 'per association' mean in this context? This should be explained in the	You should use the original figure legend

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466						caption to fiture 1.3. (Nathan Gillett, University of East Anglia)	from the paper, I agree with this comment as it is not explained what species per association is. CHECK with original author. Done. Author gave permission for figure to be used. *The legend has been rewritten, in accordance with the paper the figure was taken from.*
E-1-467	A	36	30	36	30	Delete: Mean number of species per association. Enlarge maps with a much smaller space between. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I suggest you contact Gregory Beaugrand for the original figure. CHECK with original author – Done. Author gave permission for figure to be used. *The figure was resized and the legend has been rewritten, in accordance with the paper the figure was taken from.*
E-1-468	A	36	32			Fig1.3 Text on figures too big (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I suggest you contact Gregory Beaugrand for the original figure. CHECK with original author – Done. Author gave permission for figure to be used. Figure was resized.
E-1-469	A	36	32			Fig 1.3 Move degrees N to right hand side of graphic (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	I suggest you contact Gregory Beaugrand for the original figure. CHECK with original author – Done. Author gave permission for figure to be used. Degrees N removed from figure.
E-1-470	A	36	36	36	36	Trophic mismatch has been demostrated for phytoplankton blooms and recruitment of cod by Platt et al (2003) in the Newfoundland area. I suggest to introduce this reference: Platt T, Fuentes-Yaco C, Frank KT (2003) Spring algal bloom and larval fish survival. Nature 423:398-399 (Ricardo Anadon, Universidad de Oviedo)	I do not aggree with this, the platt study is only over a few years and decribes inter-annual variability of recruitment. It does not provide any evidence on climate change impacts. NO CHANGES
E-1-471	A	36	36	36	36	I suggest to introduce the reference of Llope et al (in press) mentioned above. Is a clear reference for changes in upwelling intensity ans seasonality, and coastal currents in the last 30 years. Could be included in the North Atlantic in the first	I think this reference is a rather localised study and does not provide evidence of climate change impacts on ecosystems. (do not

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						line. Another possibility is the introduce the reference of a submitted paper (Llope et al, submited) related to changes in nutrient concentration, stoichiometry at long term scale in the North of Spain. These authors demonstrated the influence of changes in hidrogrpahy on fitoplankton community. Thear are few demonstrations in the literature about climate change driven modifications on phytoplankton communities. Reference: Llope M, Anadón R, Viesca L, Quevedo M, González-Quirós R, Stenseth NC (in press) Hydrography of the southern Bay of Biscay shelf-break region: integrating the multi-scale physical variability over the period 1993-2003. Journal of Geophysical Research Llope M, Anadón R, Sostres JÁ, Viesca L (submitted) Nutrients and stoichiometry of the southern Bay of Biscay shelf-break region (1993-2003): winter supply, seasonality, long-term dynamics and the structuring effect on the phytoplankton community. (Ricardo Anadon, Universidad de Oviedo)	include) NO CHANGES
E-1-472	A	36	36	36	36	I suggest to introduce the reference of Anadón et al (2005) that recopilate the presence of toxic species and the recurrence of blooms in the northwest coast of Spain, with implications to mussel and other mollusc culture in raft. Reference: Anadón R, Duarte CM, Fariña C (2005) Impactos sobre los Ecosistemas Marinos y el Sector Pesquero. In: Moreno JM (ed) Evaluación Preliminar de los Impactos en España por efecto del Cambio Climático. Ministerio Medio Ambiente, Madrid, p 147-182 (Ricardo Anadon, Universidad de Oviedo)	I think this reference is a rather localised study and does not provide evidence of climate change impacts on ecosystems. (do not include) NO CHANGES
E-1-473	A	37	0			Table 1.6. row three. Insert. Southerly movement of boreal plankton in the western North Atlantic due to lower salinities in column 2. insert eastern before North Atlantic in column 3. Add below western North Atlantic. In column 4 insert Johns et al. 2001. Johns, D. G., M. Edwards and S. D. Batten (2001). "Arctic boreal plankton species in the North West Atlantic." Canadian Journal of Fisheries and Aquatic Sciences 58(11): 2121-2124. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK CHANGED
E-1-474	A	37	4	37	4	Insert heading: Changes in lakes (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Addressed. The section heading 'Section 1.3.4.4 Changes in freshwater ecosystems' was changed to 'Section 1.3.4.4 Changes in lakes', given that there is a subsequent 'Section 1.3.4.5 Changes in rivers'. This new heading provides better clarity for the contents of Section 1.3.4.4.

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E-1-475	A	38	19	38	19	Insert 'dragonflies' and delete Odonata (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Addressed. Change was made in conjuction with the change addressed in the comment below
E-1-476	A	38	19	38	20	Hickling et al reference is correct. There is an additional Hickling et al paper (2006), which shown northwards movements of freshwater fish and aquatic bugs, as well as Odonata in Britain. Thus, the sentence can be re-written: "Climate warming has also produced northwards shifts in the distributions of freshwater fish, Odonata and aquatic bugs in Britain (Hickling et al. 2005, 2006)" The full reference is: Hickling, R., D.B.Roy, J.K.Hill, R.Fox & C.D.Thomas. 2006. The distributions of a wide range of taxonomic groups are expanding polewards. Global Change Biology 12:450-455. (Chris Thomas, University of York)	Addressed. Word change addressed, and the reference was added.
E-1-477	A	38	22	38	33	Section contains only vague statements about change without saying where and for what time period. Don't like the words: some species (line 22), several large lakes (line 25), many cases (line 26), some wild species (line 31) (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Not applicable. These phrases, while appearing to be vague out of context, are adequately supported by the references cited at the end of each sentence. These terms were chosen to save space and the references at the end of each sentence provide details on specific locations and species. I am reluctant to use numbers, as the numbers only reflect the actual number of lakes where published reports exist – it is likely that similar changes are happening in other lakes, they just haven't been reported.
E-1-478	A	38	22	38	33	Redraft paragraph - 4 references to Winder and Schindler (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Not applicable. The references to Winder and Schindler are appropriately used. Winder and Schindler investigate multiple aspects of change in their paper, whereas the other papers cited typically only reported one aspect of change. Hence the Winder and Schindler study is independently applicable for each sentences where it is used.
E-1-479	A	39	8	39	8	1,3,4,6 Marine and fresh water biological systems (Annick Douguédroit, University de Provence)	Not applicable. The heading for the summary for all sections is just 'Summary'.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-1-480	A	39	11	39	11	…water temperatures representing typical examples of the common impact pattern of warming in the extra-tropical zones (see Box 1,1). (Annick Douguédroit, University de Provence)	Unclear comment/Not applicable. I think it is just a comment and not a request for changes, therefore not applicable.
E-1-481	A	39	12			Why "already"? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Not applicable to O'REILLY. This sentence refers to impact on coral reefs and should be addressed by the marine eocsystems author(s).
E-1-482	A	39	14	39	14	Insert 'and altitudinal' between northward and range (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Addressed. Deleted 'northward' so that the phrase is just 'range expansions', which is more general and refers to both altitudinal and poleward shifts.
E-1-483	A	39				Table 1.7. Is there no information on Lake Baikal? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Addressed. Added a reference for Lake Baikal (Shimaraev and Domysheva 2004). Am not sure how to geographically catagorize Lake Baikal. Have added 'Eastern Europe' to the locations column, but this can be changed to be consistent with location names in other sections. CLA: Eastern Europe changed to Russia in Table 1.3.
E-1-484	A	40	2	48		Section 1.3.5: As suggested in my FOD review, this section can be and should be further shortened and a more balanced analysis of changes in phenology and ornithology must be presented. Most of the examples of change are from Europe, whose climate in last fifty years or more has been strongly influenced by NAO and related large-scale circulation patterns (see Climate Dynamics, 2001,p.701-715: Int'l J of Climatology, 2002, p.743-750 & 2003, p. 987-1010). Changes in European phenology may be due more to changes in NAO and related circulation patterns and NOT due to anthropogenic climate change. Further, there is a conspicuous lack of studies from tropical countries in Asia and Africa and this makes the issue more regional than global. This aspect must be discussed here in the context of global warming & climate change. (Madhav Khandekar, Retired)	The obvious unbalance of studies in general, not only in 1.3.5, is discussed in section 1.1.1 a tthe very first beginning. We believe that there is an urgent need to improve the observation networks of observed changes and to enhance data sets for physical and biological systems and socio-economic sectors, particularly in developing countries
E-1-	A	40	2	48		Section 1.3.5. Overall I thought this section included a good, comprehensive set of	Great Idea. We did just that.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
485						recent studies of climate-related responses in terrestrial biological systems - I have suggested additional references where appropriate. I think the structure of this section is rather more concise and effective than in the First Order Draft. Potentially the authors could still shorten the section by limiting citations more strictly to those that fit the framework of the chapter, and referring to additional references where appropriate in the Tables or Supplementary Material. (Robert Wilson, Universidad Rey Juan Carlos)	
E-1-486	A	40	2			section 1.3.5 could be shortened a bit especailly sub-section 1.3.5.2 (Isabelle Chuine, CNRS)	Is done
E-1-487	A	40	2			Section 1.3.5 COMMENT: Need to include some of the literature on observed browning in the boreal forests eg D'Arrigo, R. D., R. K. Kaufmann, N. Davi, G. C. Jacoby, C. Laskowski, R. B. Myneni, and P. Cherubini (2004). "Thresholds for warming-induced growth decline at elevational tree line in the Yukon Territory, Canada." Global Biogeochemical Cycles 18(3). 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). Wilmking, M., and G. P. Juday "Longitudinal variation of radial growth at Alaska's northern treeline--recent changes and possible scenarios for the 21st century." Global and Planetary Change In Press, Corrected Proof. Wilmking, M., G. P. Juday, V. A. Barber, and H. S. J. Zald (2004). "Recent climate warming forces contrasting growth responses of white spruce at treeline in Alaska through temperature thresholds." Global Change Biology 10(10): 1724-1736. (William Hare, Potsdam Institute for Climate Impact Research (PIK))	Will be adressed in section 1.3.6
E-1-488	A	40	5	40	8	Phrasing problem. Also, genetic evolution is a response to changing climate that is omitted here, and ecosystems functioning (and not only communities compostion) is also affected by the responses of the species to climate change. (Isabelle Chuine, CNRS)	We added genetics and ecosysteme functioning in the new version.
E-1-489	A	40	5	40	5	"(e.g., soil nutrients, roosting sites)" could be omitted to save space. (Robert Wilson, Universidad Rey Juan Carlos)	Is deleted and space is saved.
E-1-490	A	40	6	40	6	Amend to read: change beyond the tolerances of species…. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-491	A	40	7	40	8	"which could influence species compositions in ecosystems" could be reduced, both to save space and to improve the flow between this sentence and the subsequent sentence (beginning "If none of these responses is possible" that links directly to the phrase preceding that which I suggest removing). (Robert Wilson, Universidad Rey Juan Carlos)	Section rephrased and shortened completely.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-1-492	A	40	11	40	13	Way of doubling references is odd; takes more space than needed. Why not: ... are by Hughes (2000), ... (Albert Klein Tank, Royal Netherlands Meteorological Institute)	List of references is adjusted now, problem arose from endnote programme
E-1-493	A	40	17	40	28	I wonder if this second introductory paragraph to section 1.3.5 could be shortened or clarified by omitting some of the examples and changing the structure. One approach would be to retain the first sentence (lines 17-18), remove the second sentence (lines 18-19), and move straight to the current third sentence (current lines 20-22), but now omitting the word "also" (currently on line 22). It might then be possible to finish the paragraph with something like "Many populations of animals and plants have been under pressure from agricultural intensification and land-use change for the past 50 years and many species are in decline. Land-use change and habitat fragmentation directly threaten species with extinction but also reduce the capacity of species to respond to climate change by shifting their distributions (Hill et al. 1999, Warren et al. 2001)." (Robert Wilson, Universidad Rey Juan Carlos)	Paragraph has been shortened and the suggested sentence has been omitted.
E-1-494	A	40	19	40	19	The term "soluble-nitrogen" is a misnomer. Inert N2 gas is soluble. A better term is "reactive nitrogen", often abbreviated as "Nr". This term refers to inorganic and organic N species which are biologically and geochemically reactive". A useful reference is "Galloway, J. N., and E. B. Cowling. 2002. Reactive nitrogen and the world: 200 years of change. Ambio 31:64-71." (Knute Nadelhoffer, University of Michigan)	The term has been suggested by another reviewer.
E-1-495	A	40	24	40	24	After elevations, insert: or in tropical forest canopies. Cannot recall the reference, but am aware of work on tree kangaroos in Queensland that have been dying because of high temperatures, which are at the limit of their tolerance. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Reference should be provided
E-1-496	A	40	31			Recent study in NE Spain mountains (Jump et al. 2006) show significantly lower growth of mature beech trees at the lower limit of this species compared with those at higher altitudes. Growth at the lower Fagus limit is characterised by a rapid recent decline starting in approximately 1975. By 2003, growth of mature trees had fallen by 49% when compared with pre-decline levels. Analysis of climate-growth relationships suggests that the observed decline in growth is a result of warming temperatures. ALISTAIR JUMP, JENNY HUNT, JOSEP PEÑUELAS 2006. Rapid climate change-related growth decline at the southern range-edge of Fagus sylvatica. Global Change Biology in press (Josep Penuelas, CSIC-CREAF Barcelona)	Thank you for the reference. It has been suggested for inclusion in 1.3.6
E-1-497	A	40	35	40	36	Delete reproduction (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	We have included more inforkmation about reproduction in the section, justifying

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							"reproduction" in the title.
E-1-498	A	40	35	40	35	After in insert: the reproduction of the (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Reworded entire paragraph.
E-1-499	A	40	36	40	39	Sentence " There have been reported instances of … period have been reported (Tryjanwski et al. 2004)" is not absolutely clear. Try rephrasing. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	We rewrote that sentence. Thank you for pointing it out.
E-1-500	A	40	38	40	38	Insert "an" between as and evolutionary; delete of; delete in, insert 'over a' (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Sentence has been entirely rewritten
E-1-501	A	40	41	40	44	Are these changes significant and attributable to anthropogenic climate change? (Nathan Gillett, University of East Anglia)	They are significant and track the regional warming.
E-1-502	A	40	50			the bracket "(increased…" does not get closed again. (Chris Thomas, University of York)	Thanks. Fixed
E-1-503	A	41	1	41	31	I'm surprised not to see reference to the following paper in this section or in Table 1.9 Fitter AH, Fitter RSR Rapid changes in flowering time in British plants SCIENCE 296 (5573): 1689-1691 MAY 31 2002 (Chris Thomas, University of York)	Reference is included now.
E-1-504	A	41	3	41	3	"Phenology …… and plants -" - Term already cited in earlier text, but not then defined. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Term is now mentionned here the first time.
E-1-505	A	41	12	41	12	Add: Changes in phenology are typical impacts of warming on terrestrial ecological systems located in the a region of the extra-tropical areas (see Box 1,1). (Annick Douguédroit, University de Provence)	Box concept can't be implemented due to space limitations.
E-1-506	A	41	14			The figure of 2.3 and 5.1 days/decade seems too high compared to the 2 weeks in the 2nd half of the 20th century in line 24, when taking into account that the latter includes begin and end of period, and that higher latitude changes are greater than elsewhere. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Numbers are correct. 5.1 days / decade is higher due to the fact that only significant time series were included in this meta-analysis.
E-1-507	A	41	18	41	18	Zhou et al only includes information to 1999. Cite a more recent reference (e.g.?Delbart Table 1.9) on NDVI and possibly cross reference to earlier text on this subject. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	In this table studies of the length of the growing season are included whereas Delbart is only on spring changes.
E-1-508	A	41	27		28	Do you trust the study with a 32 lengthening of growing season? This seems a bit suspicious to me (but out of my turf clearly!) (Gabi Hegerl, Duke University)	Actually, there was a typo, only 29 days. Due to space limitations, example is moved to supplementary material.
E-1-	A	41	28	41	28	The detected earlier leaf fall in Birch trees in Russia (Kozlov and Berlina 2002)	Sentences is shortened now, however example

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509						seems to contradict the statement in line 24 of the same page that data show "a clear extension of the growing season by up to two weeks…". Does this statement hold for the Russian territory, or id there no (or weak) evidence for an increasing season length in Russia? Please clarify. (Heiko Balzter, Centre for Ecology and Hydrology)	should demonstrate that there is – at least one – exception from this finding.
E-1-510	A	41	28	41	28	earlier birch leaf fall'. This sounds like a shorter growing season to me. (Nathan Gillett, University of East Anglia)	Correct (see above)
E-1-511	A	41	30	41	31	In Tab. 1.8, some works on growing season trends should be supplemented, for example, Chmielewski & Roetzer (2001, Agricultural and Forest Meteorology, 108: 101-112) for Europe and Chen et,al (2005, Global Change Biology, 11: 1118-1130) for China. (Xiaoqiu Chen, Peking University)	Thank you reference included, the period of the other is too short (1982-1993) and it is unclear why the other years were not included.
E-1-512	A	42	0			Insert plus (+) signs into positive values in column 4 cells. (Knute Nadelhoffer, University of Michigan)	+ are inserted.
E-1-513	A	42	1	42	2	Table 1.9 is biased towards the US, Australia and Europe. Are there no long-term data available for Africa, South America or Russia? If this is the case the authors should discuss the possible bias in the main text, and modify their conclusions where appropriate. (Heiko Balzter, Centre for Ecology and Hydrology)	This unintended bias due to lack of studies is addressed now.
E-1-514	A	42	2			Northern Eurasia Entry. Spring onset in Northern Eurasia was reported by the change of stable passsage of mean daily temperatures through thresholds of 5 deg C (and several others, e.g., 0C) for the past 50 years at least. References: (1) Arctic Climate Impact Assessment 2004, Ch. 1; (2) Groisman, P.Ya., B. Sun, R. S. Vose, J. H. Lawrimore, P. H. Whitfield, E. Førland, I. Hanssen-Bauer, M. C. Serreze, V. N. Razuvaev, and G. V. Alekseev, 2003: Contemporary climate changes in high latitudes of the Northern Hemisphere: Daily time resolution. WMO/TD - 1172, [Proceedings of the International Symposium on Climate Change, Beijing, China ,31 March-3 April, 2003], World Meteorol. Organ. Publ. # 1172 , 51-55; .and (3) Groisman, P.Ya., B.G. Sherstyukov, V.N. Razuvaev, R.W. Knight, J.G. Enloe, N.S. Stroumentova, P. H. Whitfield, E. Førland, I. Hannsen-Bauer, H. Tuomenvirta, H. Aleksandersson, A. V. Mescherskaya, and T.R. Karl, 2006: Potential forest fire danger over Northern Eurasia: Changes during the 20th century. Global and Planetary Change (in press). Please, select any one of the above. (Pavel Groisman, University Corp. for Atmospheric Research)	Link to frost free season and climatological growing season which are adressed in 1.3.6.1 is now more punctuated.
E-1-515	A	43	14	43	14	Add s to trend (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Sentence completely rewritten
E-1-	A	43	15	43	18	Does the delay in hunting by Hudson bay polar bears constitute a mammal	The impact is on the bear, hence we decided

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516						example, where they are waiting for the ice to form before being able to set off again? I'm not sure what the best reference would be. (Chris Thomas, University of York)	to include it here.
E-1-517	A	43	17	43	17	Correct the reference Post () (Heiko Balzter, Centre for Ecology and Hydrology)	Thanks. Done
E-1-518	A	43	26			Hickling et al reference should be deleted from this line - this paper does not contain any information on phenology (the Hickling paper is cited elsewhere, so don't remove from the bibliography!) (Chris Thomas, University of York)	Thanks---done
E-1-519	A	43	29	43	29	Delete s in plant; amend stronger to strongest (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-520	A	43	36	43	36	Raunkiaer life forms or different origins'. What does this mean? More explanation is needed. (Nathan Gillett, University of East Anglia)	Section shortened.
E-1-521	A	43	39	33	42	I don't quite follow this sentence. (Chris Thomas, University of York)	Section rewritten
E-1-522	A	43	39	43	44	Geographical differences are also evidence for the growing season trend in Northern China (Chen et,al. 2005, Global Change Biology, 11: 1118-1130). (Xiaoqiu Chen, Peking University)	Thank you !!
E-1-523	A	43	46		49	The length of timeseries analyzed is clearly important, I have trouble understanding what is said here (Gabi Hegerl, Duke University)	Rewritten
E-1-524	A	43	50	43	50	Insert 'the' between matches and turning (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-525	A	44	1	40	14	this paragraph is not necessary. Studies reported here are already reported elsewhere (Isabelle Chuine, CNRS)	Here the link to responses to climate change is done, as also requested by G-1-205. Paragraph is moved ahead.
E-1-526	A	44	1	44	1	What does the italic "signal." mean? (Heiko Balzter, Centre for Ecology and Hydrology)	Rewritten
E-1-527	A	44	2	44	3	What does it mean for the spring climate change signal to be fairly well understood? Does this mean that there is a good understanding of the physiological processes that cause the dates of spring phenological traits to advance when the climate warms? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Yes,
E-1-528	A	44	15	44	42	Fig 1.4 is difficult to follow because (a) the thickish lines obliterate each other, and (b) it is counter-intuitive having temperature that way around. Can it be updated because the time series finishes before TAR	Figure is updated

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						(Chris Thomas, University of York)	
E-1-529	A	44	16	44	31	Would it be possible to update this figure to 2005 or 2006? I think this would be important given that the tendancy of the NAO/NAM index appears to have reversed in recent years (see WG1, 9.5.3.2 and Ch 3). It would be interesting, and important for your assessment, to know whether phenological traits have continued on the same trajectory despite apparent change in the trajectory of the NAO/NAM. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Figure is updated
E-1-530	A	44	44	44	49	Section 1.3.5.3 "Changes in reproduction". This section is important because changes in fecundity can be one of the mechanisms for the changes in species distributions referred to in the following section (1.3.5.4). A relevant reference is Davies, Z.G., Wilson, R.J., et al. (2006) Changing habitat associations of a thermally constrained species, the silver-spotted skipper butterfly, in response to climate warming. Journal of Animal Ecology 75: 247-256, that shows how egg-laying rate and microhabitat selection depend on temperature, and that as a result the area of habitat used for egg-laying in a butterfly species has increased over 20 years, increasing population size. (Robert Wilson, Universidad Rey Juan Carlos)	Thank you---included 1.3.5.4
E-1-531	A	44	44			The "changes in reproduction" section need to be expanded. I suspect references are missing, for example Birds and Climate Change, Volume 35 (Advances in Ecological Research) by A. P. Moller, W. Fiedler, P. Berthold; Berteaux, D., D. Réale, A. G. McAdam, and also S. Boutin. 2004. Keeping pace with fast climate change: can artic life count on evolution? Integrative and Comparative Biology 44:140-151. (Isabelle Chuine, CNRS)	Thank you—we have expanded the section. But space doesn't allow us to inclde all refs in the text.
E-1-532	A	44	49	44	49	Add: It is a clear example of a "c" region as defined in Box 1,1. (Annick Douguédroit, University de Provence)	Unfortunately there is not enough space to follow the Box concept.
E-1-533	A	44	51	47	45	Changes in species distribution could be divided into Climate-linked extension from page 45-line 1 to page 46-line 1 and page 46-line 17 to page 47-line6 and climate-linked extinction page 46-line3-15. (Annick Douguédroit, University de Provence)	We now have distribution (just neutral, can be extension or shift or reduction) and then follow by the special cases of extinction and invasion in a separate subchapter.
E-1-534	A	45	1	45	1	Add: Changes in climate-linked extension. They represent examples of shifts located in the" c" regions of textra-tropical or mountainous areas with poleward or upward migrations (see Box 1,1). Indeed many studies focusing… (Annick Douguédroit, University de Provence)	Unfortunately there is not enough space to follow the Box concept.
E-1-535	A	45	5	45	6	Why not other broad scale influences (precipitation, air quality changes, …)? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Other broad scale influences are not in the same direction (increase, decrease) everywhere where those systemetic shifts in

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							areas occur.
E-1-536	A	45	5	45	5	Insert full stop after shifts; Upper case H However (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-537	A	45	6	45	6	Insert commas after areas and regions (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-538	A	45	6	45	9	" comment on the sentence: 'A certain inherent resilience of treeline forests is reported and the magnitude of elevation shifts of alpine plants lags behind the isothermal shift (Walther et al. 2005), whereas some butterflies appear to track decadal warming quickly (Parmesan et al. 1999).': A citation for the 'resilience of forests' should be added and the statement of the time lag of alpine plants originates from Grabherr et al. 1995: Suggestion for a revision: 'The sensitivity of treeline forests to climate warming varies with topography and the treeline history (e.g. human impacts) and thus differs in extent, intensity and the process of change (Holtmeier and Broll 2005). Elevation shifts of alpine plants may lag behind the isothermal shift (Grabherr et al. 1995), but more recent observations indicated increased moving rates of alpine plant species (Walther et al. 2005) and some butterflies appear to track decadal warming rapidly (Parmesan et al. 1999).' full references: Holtmeier, F.-K. and G. Broll 2005: Sensitivity and response of northern hemisphere altitudinal and polar treelines to environmental change at landscape and local scales. Global Ecology and Biogeography, 14: 395-410. Grabherr, G., M. Gottfried, A. Gruber, and H. Pauli 1995: Patterns and current changes in alpine plant diversity. In: Arctic and Alpine Biodiversity: Patterns, Causes and Ecosystem Consequences. Ecological Studies, Vol. 113 (eds Chapin, F.S. and C. Körner), pp. 167-181. Springer, Berlin. Walther, G.-R., S. Beißner, and C.A. Burga 2005: Trends in upward shift of alpine plants. Journal of Vegetation Science, 16, 541-548. (the citation 'Walther et al. 2005' is ambiguous - compare the references; the above one is relevant in this context; please check throughout chapter 1 and add an 'a' / 'b' accordingly;" (Harald Pauli, University of Vienna)	Done and shortenend
E-1-539	A	45	8	45	9	Whilst some butterflies have indeed responded by shifting their distributions, changes to the distributions of most individual species (Warren et al. 2001) and of species richness as a whole (Menendez et al. 2006) are lagging behind climate change. New Reference: Menéndez, R., A.González-Megías, J.K.Hill,	Added.

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						B.Braschler, S.G.Willis, Y.Collingham, R.Fox, D.B.Roy & C.D.Thomas. 2006. Species richness changes lag behind climate change. Proceedings of the Royal Society, B. 273:1465-1470. (Chris Thomas, University of York)	
E-1-540	A	45	26	45	41	"This paragraph should be revised, because cause some sentences are incorrect and/or inadequately cited (e.g. the second sentence), some are a bit imprecise, some citations should be added: Suggestion for a revision: 'Altitudinal shifts of plant species were documented repeatedly (Grabherr et al. 2001; Dobbertin et al. 2005; Walter et al. 2005) (see Tab. 1.10). In several northern hemiphere mountain systems, treelines have markedly shifted to higher elevations during the 20th century such as in the Urals (Moiseev and Shiyatov 2003), in Bulgaria (Meshinev et al. 2000), in the Scandes (Kullman 2002), and in Alaska (Sturm et al. 2001). In some places, the position of treeline has not extended up in elevation in the last half-century (Cullen et al. 2001; Masek 2001; Klasner and Fagre 2002), which may be due to time-lag effects owing to poor seed production/dispersal, to the presence of "surrogate habitats" with special microclimate, or to topographical factors (cf. Holtmeier and Broll 2005). In mountainous regions, climate is a main driver of species composition, but in some areas, grazing, logging, or firewood collection can be, in addition, of considerable relevance for the current vegetation patterns. In parts of the European Alps, for example, the treeline is influenced by past and present land use impacts (Theurillat and Guisan 2001; Carnelli et al. 2004). In high mountain areas above the climatic treeline, however, human influence is commonly much less pronounced compared to lower elevations (Körner 1994; Pauli et al. 2004). A climate warming-induced upward migration of alpine plants in the high Alps, documented in the 1980s and 1990s (Hofer 1992; Grabherr et al. 1994, 2001; Pauli et al. 2001), was observed to have accelerated towards the beginning of the 21th century (Walther et al. 2005). Species ranges of alpine plants also have extended to higher altitudes in the Norwegian Scandes (Klanderud and Birks 2003). Species in alpine regions that are often endemic and of high importance for plant diversity (Väre et al. 2003) are vulnerable to climate warming, most likely because of their commonly small climatic ranges, habitats determined by low termperature, small isolated populations and through the absence of suitable areas at higher elevations in which to migrate (Pauli et al. 2003).' Additional references for this paragraph (all original refs. remained but the position may have changed):	Included - thanks

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						Moiseev, P.A. and S. G. Shiyatov 2003: Vegetation dynamics at the treeline ecotone in the Ural highlands, Russia. In: Alpine Biodiversity in Europe - A Europe-wide Assessment of Biological Richness and Change. Ecological Studies, Vol. 167, pp. 423-435. Springer, Berlin. Meshinev, T., I. Apostolova, and E. Koleva 2000: Influence of warming on timberline rising: A case study on Pinus peuce Griseb. in Bulgaria. Phytocoenologia, 30, 431-438. Kullman, L. 2002: Rapid recent range-margin rise of tree and shrub species in the Swedish Scandes. Journal of Ecology, 90, 68-77. Sturm, M., C. Racine, and K. Tape 2001: Climate change - increasing shrub abundance in the Arctic. Nature, 411, 546-547. Holtmeier, F.-K. and G. Broll 2005: Sensitivity and response of northern hemisphere altitudinal and polar treelines to environmental change at landscape and local scales. Global Ecology and Biogeography, 14: 395-410. Carnelli, A.L., J.-P. Theurillat, M. Thion, G. Vadi, and B. Talon 2004: Past uppermost tree limit in the Central European Alps (Switzerland) based on soil and soil charcoal. Holocene 14: 393-405. Körner, C. 1994: Impacts of atmospheric changes on high mountain vegetation. In: Mountain Environments in Changing Climates (ed Beniston, M.) . Routledge, London, pp. 155-166. Pauli, H., M. Gottfried, D. Hohenwallner, K. Reiter, R. Casale, G. Grabherr 2004: The GLORIA field manual - Multi-Summit approach. European Commission DG Research, EUR 21213, Office for Official Publications of the European Communities, Luxembourg. Hofer, H.R. 1992: Veränderungen in der Vegetation von 14 Gipfeln des Berninagebietes zwischen 1905 und 1985. Bericht des Geobotanischen Institutes ETH, Stiftung Rübel, 58, 39-54. Grabherr, G., M. Gottfried, and H. Pauli 1994: Climate effects on mountain plants. Nature, 369, 448-448. Grabherr, G., M. Gottfried, and H. Pauli 2001: Long-term monitoring of mountain peaks in the Alps. In: Biomonitoring: General and Applied Aspects on Regional and Global Scales. Tasks for Vegetation Science, Vol. 35 (eds Burga, C.A. and A. Kratochwil), pp. 153-177. Kluwer, Dordrecht. Walther, G.-R., S. Beißner, and C.A. Burga 2005: Trends in upward shift of alpine plants. Journal of Vegetation Science, 16, 541-548. Klanderud, K. and H.J.B. Birks 2003: Recent increases in species richness and shifts in altitudinal distributions of Norwegian mountain plants. The Holocene, 13,	

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						1-6." (Harald Pauli, University of Vienna)	
E-1-541	A	45	37	45	41	The text should note whether this sentence is based on observation or hypothesis. (Indur Goklany, US Department of the Interior)	There is little doubt about severe climatic conditions and other facts.
E-1-542	A	45	37	45	41	I found this setence hard to understand. (Nathan Gillett, University of East Anglia)	Sentence is modified now.
E-1-543	A	45	43	46		Table 1.10. Two important additional examples of range contractions at lower latitudinal margins are Wilson, R.J., Gutiérrez, D. et al. (2005) Ecology Letters 8, 1138-1146 that shows average upward shifts of 210 m in the lower elevational limits of 16 butterfly species in Central Spain between 1967-73 and 2004, associated with a 1.3ºC increase in annual mean temperature (already in the reference list). Also, FRANCO, A.M.A., HILL, J.K. et al. (2006) Impacts of climate warming and habitat loss on extinctions at species' low-latitude range boundaries. Global Change Biology 12: 1545-1553. It would also be worth citing an additional paper by Hickling et al that shows northward range expansions in a wide range of taxonomic groups in the UK: the reference is Hickling, R., Roy, D.B. et al. (2006) The distributions of a wide range of taxonomic groups are expanding polewards. Global Change Biology 12:450-455. (Robert Wilson, Universidad Rey Juan Carlos)	References Wilson and Franco both included in Table.
E-1-544	A	45	43	46	1	Table 1.10. Britain, 4 northern butterflies line. This should be updated because a new paper is just coming out - already available online. The third column (obs changes) should read "3 of 4 species showing significant shifts between 1970 and 2004-05; two retreating 73 and 80 km north, and the third retreating 149 m uphill" "Franco et al. 2006" can be added to reference box. Add to bibliography: Franco, A.M.A, J.K.Hill, C.Kitschke, Y.C.Collingham, D.B.Roy, R.Fox, B.Huntley & C.D.Thomas. Impacts of climate warming and habitat loss on extinctions at species' low-latitude range boundaries. Global Change Biology Online publication date: 19-May-2006 doi: 10.1111/j.1365-2486.2006.01180.x (Chris Thomas, University of York)	Franco et al. 2006 added to Table
E-1-545	A	45	43	46	1	Table 1.10. Add new line. Spain / 16 montane butterfly species / Average uphill retreat of low-elevation boundaries by 212 m between 1967-73 and 2004. / Reference Wilson et al 2005. already in bibliography. (Chris Thomas, University of York)	Done
E-1-546	A	45	43	46	1	Table 1.10. Add new line. Britain / 329 species of Grasshoppers, Butterflies, Longhorn beetles, Freshwater fish, Carabid beetles, Aquatic bugs, Woodlice, Spiders, Soldier beetles, Dragonflies and damselflies, Herptiles, Harvestmen, Mammals, Birds, Lacewings and Millipedes. / Average shift northwards of 31-64	Done

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						km, and upwards of 25 m, in 25 years. Significant northwards shifts in 12 of the 16 taxonomic groups. / Reference Hickling et al. (2006).... Hickling, R., D.B.Roy, J.K.Hill, R.Fox & C.D.Thomas. 2006. The distributions of a wide range of taxonomic groups are expanding polewards. Global Change Biology 12:450-455. (Chris Thomas, University of York)	
E-1-547	A	45	43	46	1	"Table 1.10: in line 13 within the table starting with Sweden, Spain..., column Location, add: 'Russia, Bulgaria'; in column Author add: 'Moiseev and Shiyatov 2003'; full reference: Moiseev, P.A. and S.G. Shiyatov 2003: Vegetation dynamics at the treeline ecotone in the Ural highlands, Russia. In: Alpine Biodiversity in Europe - A Europe-wide Assessment of Biological Richness and Change. Ecological Studies, Vol. 167, pp. 423-435. Springer, Berlin. Table 1.10: in line 16 within the table starting with European Alps, column Author, add : 'Grabherr et al. 1994, 2001; Pauli et al. 2001;' full references: Grabherr, G., M. Gottfried, and H. Pauli 1994: Climate effects on mountain plants. Nature, 369, 448-448. Grabherr, G., M. Gottfried, and H. Pauli 2001: Long-term monitoring of mountain peaks in the Alps. In: Biomonitoring: General and applied aspects on regional and Global Scales. Tasks for Vegetation Science, Vol. 35 (eds Burga, C.A. and A. Kratochwil), pp. 153-177. Kluwer, Dordrecht. Pauli, H., M. Gottfried, and G. Grabherr 2001: High summits of the Alps in a changing climate. The oldest observation series on high mountain plant diversity in Europe. In: "Fingerprints" of Climate Change - Adapted Behaviour and Shifting Species Ranges (eds Walther, G.-R., C.A. Burga, and P. J. Edwards), pp. 139-149. Kluwer Academic Publisher, New York." (Harald Pauli, University of Vienna)	Done
E-1-548	A	46	1			Table1.10: often, but not always the period analyzed is given, completion would be helpful (Gabi Hegerl, Duke University)	We try to include this, when time is left.
E-1-549	A	46	3	46	15	Make reference to sensitivity of tree kangaroos to high temperatures and vertical limits in tree canopy. Ref to Kanowski CSIRO? Also refer to his work on changing leaf composition and thus diatary impacts with higher CO2. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	No peer reviewed literature on observed effects found, CO2 effects are experimental.
E-1-550	A	46	3	46	15	Extinctions evidence. Could add something like this: "Butterfly populations have been disproportionately prone to extinction near low latitude and low elevation	**Thank you—We included the new Thomas**

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						boundaries (Parmesan 1996, Wilson et al. 2005, Franco et al. 2006). Population-level studies of butterflies have detected average latitudinal retreats of ca. 29 km/decade and elevation retreats of ca. 63 m/decade (Thomas et al., 2006). " New reference: Thomas, C.D., A.M.A.Franco & J.K.Hill. (2006) Range retractions and extinction in the face of climate warming. Trends in Ecology & Evolution doi.org/10.1016/j.tree.2006.05.012 (Chris Thomas, University of York)	**ref. 1.3.5.3**
E-1-551	A	46	3	46	15	Dr. Erik Beever has shown that climate change has lead to local extinctions of American Pika populations in the Great Basin region of the U.S. (see Beever, E.A., P.F. Brussard, and J. Berger. PATTERNS OF APPARENT EXTIRPATION AMONG ISOLATED POPULATIONS OF PIKAS (OCHOTONA PRINCEPS) IN THE GREAT BASIN. Journal of Mammalogy 84(1): 37–54). (Lara Hansen, WWF)	Thank you—We added this information 1.3.5.3
E-1-552	A	46	3	46	4	Change "more extinctions are likely to occur" to "it has been suggested that more extinctions would occur". The current formulation is a claim that needs to be proven. One might be able to shed some light on this claim by looking at paleo evidence from eras when temperature changes have changed dramatically. For example, did the warming subsequent to the Last Glacial Maximum lead to temperature/climate related extinctions, what fraction of species went extinct, did it decrease biological diversity, and how much did biological productivity change? However, absent some exploration of these issues -- and a critical evaluation of the methodology used by Thomas et al. (2004) -- this statement is little more than an assertion, and this ought to be noted. We should also note that this study did not take into consideration the role of CO2 in changing moisture requirements for plants or on plant productivity (and, therefore, other species dependent on them). Nor did it consider the effect of CO2 on the temperature tolerance on plants. In light of all these effects, how would these impacts of CO2 interact with changes in temperature and water availability to affect interspecies competition, and extinction potential of each species? Moreover, climate change isn't the only pressure on species and biodiversity. Primary threats are conversion of habitat to human uses, but these threats will themselves be modified by climate change and CO2 effects. In particular, if temperature changes are low to moderate, productivity of agriculture and forestry will increase which – all else being equal -- should relieve some of these pressures (because less land would need to be cultivated or harvested for timber). [Of course, if temperature change is high enough, we would expect that productivity would decline, and pressures would increase.] Also, the study does not consider that if a species moves out, something else might take its place – and	We rewrote the entire paragraph to help address your point here. 1.3.5.4

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						probably will, since nature abhors a vacuum. All these shortcomings are in addition to the fact that at regional and, especially local, scales the basic climatic inputs used by the studies employed by Thomas et al, are highly uncertain. For all these resons, the term "likely" on line 4 is inappropriate. (Indur Goklany, US Department of the Interior)	
E-1-553	A	46	3	46	3	…can be difficult but some of them could present "c" region cases (see Box 1,1). (Annick Douguédroit, University de Provence)	We believe that including it in section 1.3.5.3 will get the point across.
E-1-554	A	46	6	46	6	Delete 'e' in inbreed (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Thanks—corrected.
E-1-555	A	46	9	46	9	A small explanation could be added for the extinction events in amphibians, for example "found in amphibians around the world, apparently because of increased fungal disease outbreaks owing to increased minimum temperatures (Pounds et al. 2006)" (Robert Wilson, Universidad Rey Juan Carlos)	We did do just what you suggested. Thanks
E-1-556	A	46	10			The Pounds et al 2006 reference is relevant here too… thus…. "Burrowes et al., 2004; Pounds et al. 2006)" (Chris Thomas, University of York)	Agree—and we added it, as suggested.
E-1-557	A	46	15	46	15	This last line can be removed as it refers to the same example cited on line 14 (Robert Wilson, Universidad Rey Juan Carlos)	References adjusted
E-1-558	A	46	15	46	15	May be pertinent to mention the polar bear and Ian Sterling's work at this point, although this work is well referenced to in chap. 4, p. 30. (Pierre Bernier, Natural Resources Canada)	**We decided to mention polar bears but not in great detail.**
E-1-559	A	46	17	47	6	Another interesting example is of introduced Collembola which have invaded relatively warm locations on Marion Island in the southern ocean; reference: Gabriel AGA, Chown SL, Barendse J, Marshall DJ, Mercer RD, Pugh PJA, Smith VR (2001) Biological invasions of Southern Ocean islands: the Collembola of Marion Island as a test of generalities. ECOGRAPHY 24 (4): 421-430 AUG 2001 (Chris Thomas, University of York)	Thanks—included
E-1-560	A	46				Table 1-10 add evidence from Russia (Moiseev and Shiyatov, 2003. Ecological Studies 167: 423-435); in Canada from field based observations (Gamache and Payette 2005. J. of Biogepgraphy 32(5): 849-862) (Pierre Bernier, Natural Resources Canada)	Moiseev reference included. The example of Canada mentions that besides the milder 20th century climate, local topographic factors appear to have influenced the rise in tree lines and recent establishment by seed.
E-1-561	A	46				Table 1.10. Typographical error: For the entry "Senegal, West Africa," the third column should begin "25-30 km…" (Patrick Gonzalez, The Nature Conservancy)	Correct values is given now.
E-1-	A	46				Tab. 1.10 – table row "European Alps": The reported statement "Elevational shift,	Our statement only holds for alpine summit

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562						increased species richness on mountain tops" is in contrast with the statement (reported in Cap. 4, pag. 45, lines 30-31) "While species richness has increase in some places, it has declined in others". (Bruno Petriccione, National Forest Service)	vegetation (column 2).
E-1-563	A	47	8	47	27	"Average species richness of butterflies per 20 km grid cell in Britain increased between 1970-82 and 1995-99, but less rapidly than would have been expected had all species been able to keep up with climate change (Menendez et al 2006)." Reference: Menéndez, R., A.González-Megías, J.K.Hill, B.Braschler, S.G.Willis, Y.Collingham, R.Fox, D.B.Roy & C.D.Thomas. 2006. Species richness changes lag behind climate change. Proceedings of the Royal Society, B. 273:1465-1470. (Chris Thomas, University of York)	**Thank you—we added it.**
E-1-564	A	47	10	47	27	It would be useful to cite in this section a recent article showing that species richness is increasing more slowly than predicted by climate warming, because species range expansions are constrained by a lack of habitat availability. Menéndez, R., González Megías, A. et al. (2006) Species richness changes lag behind climate change. Proc. Roy. Soc. Lond. B, 273:1465-1470. (Robert Wilson, Universidad Rey Juan Carlos)	Thank you—we did just that.
E-1-565	A	47	15			Insert "have" at end of line? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	We rewrote the entire sentence.
E-1-566	A	47	16	47	16	Delete 'd' in increased (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	We rewrote the entire sentence
E-1-567	A	47	20			Add the following new material after the period (full stop) on line 20: "Recent data from Amazonia spanning 25 years (1976-2001) indicate that: (i) trees 10 cm or more in diameter recruit and die twice as fast on the richer soils of southern and western Amazonia than on the poorer soils of eastern and central Amazonia; (ii) turnover rates have increased throughout Amazonia over the past two decades; (iii) mortality and recruitment rates have both increased significantly in every region and environmental zone, with the exception of mortality in eastern Amazonia; (iv) recruitment rates have consistently exceeded mortality rates; (v) absolute increases in recruitment and mortality rates are greatest in western Amazonian sites; and (vi) mortality appears to be lagging recruitment at regional scales. Analysis indicates that these spatial patterns and temporal trends are neither caused by obvious artefacts in the data or the analyses, nor can trends be directly driven by a mortality driver (such as increased drought or fragmentation-related death) because the biomass in these forests has simultaneously increased. In summary, these findings therefore indicate that long-acting and widespread environmental changes are stimulating the growth and productivity of Amazon forests [O. L. Phillips, T. R.	I wish we had the room to add all of this information. Our page length is severly limited.

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						Baker, L. Arroyo, et al., Pattern and process in Amazon tree turnover, 1976-2001, Philosophical Transactions of the Royal Society of London Series B - Biological Sciences 359: 381 – 407 (2004); see also: Y. Malhi and O.L. Phillips, Tropical forests and global atmospheric change: a synthesis, Philosophical Transactions of the Royal Society of London Series B - Biological Sciences 359: 549-556 (2004).]" (Indur Goklany, US Department of the Interior)	
E-1-568	A	47	29			The most recent publication in this context: Bradshaw W.E. & Holzapfel C.M. (2006) Evolutionary response to rapid climate change. Science 312, 1477-1478. (Gian-Reto Walther, Institute of Geobotany, University of Hannover)	OK, done
E-1-569	A	47	31	47	32	The Thomas reference, not currently in the bibliography, is Thomas, C.D. (2005) Recent evolutionary effects of climate change. Pp 75-88 in Climate Change and Biodiversity, Eds T.E. Lovejoy and L. Hannah, Yale University Press, New Haven and London. (Robert Wilson, Universidad Rey Juan Carlos)	OK, agree
E-1-570	A	47	31	47	45	References are missing see for review Pulido & Berthod 2004 In Birds and Climate Change, Volume 35 (Advances in Ecological Research) by A. P. Moller, W. Fiedler, P. Berthold (Isabelle Chuine, CNRS)	That is group of papers, and some of them are cited already
E-1-571	A	47	31			Add reference for the review of Jump and Peñuelas: JUMP A., PEÑUELAS J. 2005. Running to stand still: adaptation and the response of plants to rapid climate change. Ecology Letters R8: 1010–1020 (Josep Penuelas, CSIC-CREAF Barcelona)	It isn't exactly on eveloutionary response
E-1-572	A	47	32			Jump et al (2006b) used a genome scan to identify temperature-related adaptive differentiation of individuals of the tree species Fagus sylvatica. By combining molecular marker and dendrochronological data they assessed spatial and temporal variation in marker frequency at the locus identified as under selection. They show that marker frequency at this locus varies predictably with temperature. Probability of marker presence shows a declining trend over the latter half of the 20th Century in parallel with rising temperatures in the region. Our results show that beech populations may show some capacity for an in situ adaptive response to climate change. However as reported ongoing distributional changes demonstrate (Peñuelas and Boada 2003), this response is not enough to allow all populations of this species to persist in all of its current locations.ALISTAIR S. JUMP, JENNY M. HUNT, JOSE-ANTONIO MARTÍNEZ IZQUIERDO, JOSEP PEÑUELAS 206. Natural selection and climate change: temperature-linked spatial and temporal trends in gene frequency in Fagus sylvatica Molecular Ecology in press (Josep Penuelas, CSIC-CREAF Barcelona)	Not published yet, but I decide to add it

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E-1-573	A	47	37	47	37	Is the change in migration direction a behavioural response or a genetic response? (Nathan Gillett, University of East Anglia)	Probably both, but it is really difficult to detect. Mentionned study and discuss details on it.
E-1-574	A	47	44	47	45	The meaning of the sentence is unclear and does not seem appropriate here. (Isabelle Chuine, CNRS)	Improved
E-1-575	A	47	47	47	47	1,3,5,7 Terrestrial biological systems (Annick Douguédroit, University de Provence)	Unclear what this means
E-1-576	A	47	49	47	49	Given that anthropogenic influence is not yet detectable at many individual locations, there must be many species which have not yet been significantly influence by climate change. Couldn't the fact that the overwhelming majority of studies reveal notable impacts of climate change be due to the publication bias discussed in the introduction? I think the finding that even some species have been significantly influence by climate change is an important conclusion, and the authors should make sure that they are not overselling their case here. (Nathan Gillett, University of East Anglia)	We changed the term overwhelming into vast
E-1-577	A	48	1	48	1	expansions at higher latitudes in the extra-tropical zones and altitude , some evidence of population declines at lower latitudinal limits in the same zones or elevational limits to species ranges, and vulnerability of species with restricted ranges leading to local extinctions according to the modification of the distribution in space of the species areas during a warming (see Box 1,1). (Annick Douguédroit, University de Provence)	Box concept can't be included due to space restrictions
E-1-578	A	48	10			How are they "predicted"; cross reference to other chapter needed? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	No special subchapter to cross reference identified
E-1-579	A	48	14	52		Section 1.3.6: Most areas of agriculture and forestry have benefited by the general global warming & climate change and this must be highlighted here. Grain yields have increased substantially in North America, Europe, India and Australia. Forestry has in general benefited by climate warming and by increased levels of carbon dioxide (see, Global Change Biology, 2006, p.848-861 & p.862-882) and this aspect must be highlighted here. (Madhav Khandekar, Retired)	This is already said in the text NA
E-1-580	A	48	17	48	17	this likely due to the fact that all the reports studies concern the temperate regions of Europe and North America which correspond with "a" regions (see Box 1,1) of the species's areas described and to a strong influence…. (Annick Douguédroit, University de Provence)	The proposal of classification by the reviewer could be interesting, but impossible to enter here as far as it has not been discussed in a scientific paper in an international journal. We suggest that A. Douguedroit uses the opportunity of her careful review for writing this paper which could incorporate the

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							elements of the report for supporting her theory
E-1-581	A	48	17	48	19	Delete 's from changes x 2 (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK
E-1-582	A	48	19	48	20	Replace "especially management practices … related to subsidies (Easterling 2003)" with the following new sentence: "These factors include economic development policies leading to improved infrastructure, greater trade in inputs and outputs, science-based management practices and technological change, and extension services to disseminate information on these practices and technologies (Easterling 2003, Goklany 1998)." (Indur Goklany, US Department of the Interior)	NA for length reasons
E-1-583	A	48	27	48	29	Would it not be possible that some changes are nevertheless confounded with changes in management practises, and perhaps with systematic genetic development of these types of crops (e.g., replacement of fruit trees with faster maturing, higher yielding, easier to harvest varieties, and replacement or introduction of hybrid vinifera vines in wine producing regions, etc)? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Yes for agriculture in the field, not for most of the observations in experiment plots. In any case, this aspect is mentioned within the contributing factors other than climate warming
E-1-584	A	48	32	48	32	Germany spelling (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-585	A	48	32	48	32	"Germany" (Heiko Balzter, Centre for Ecology and Hydrology)	
E-1-586	A	48	40			In Tab. 1.11, the trends of cherry and apple flowering dates during a similar time period from Menzel (2003) and Chmielewski et al (2004) are very different. The possible explanation should be given or one of the two articles should be neglected. (Xiaoqiu Chen, Peking University)	OK, adressed
E-1-587	A	48		48		Tabel 1.1a. According to the table the advance in wheat flowering computed for the European Union is a model result, and therefore it doesn't belong in a table of observed changes. (Nathan Gillett, University of East Anglia)	OK, adressed
E-1-588	A	49	2			Delete (Europe) in Table 1.11 column 1 under Yields. Column three penultimate row text appears garbled. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-589	A	49	3	49	3	The box describing the observed change for Mongolia states "Decline of NDVI of the third decade of July […]." Is "third decade of July" a typographical error? If so, it should be corrected. (Sarah Shafer, U.S. Geological Survey)	OK, adressed
E-1-	A	49	19	49	19	Are we sure this wan't simply a result of non climate factors ? Eg a growth in	OK, adressed

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590						number of monasteries ? (Peter Stott, Met Office)	
E-1-591	A	49	19	49	19	After open bracket insert: e.g. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-592	A	50	1	50	3	I'm wondering if there is a typo here - "Using 30 years" on line 1 does not seem to accord with "in the last 70 years" on line 3. Should "30" be "130"? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK, adressed
E-1-593	A	50	3	50	3	List the years in brackets (xxxx-xxxx) (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-594	A	50	5	50	5	Delete 'to', insert 'with' (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-595	A	50	7	50	7	Delete comma before Jones (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-596	A	50	12	50	12	Delete the (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-597	A	50	13	50	13	Insert 'to' betwixt due and the (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-598	A	50	14	50	14	… 30 years. Alsace situated on the northern limit of quality wine production has, due to the warming, shifted within the limits ("c" region: see Box 1,1). On a … (Annick Douguédroit, University de Provence)	Idem 580 (see above)
E-1-599	A	50	16	50	16	Insert "in" after warmer (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-600	A	50	19	50	43	The text states that "no detectable change in crop yield directly attributable to climate has been reported for Europe...". This statement contradicts the paragraph in the same chapter on p. 51, l. 48 to p. 52, l.2 that the heat wave in 2003 has led to a 30% reduction in crop yield in several European countries. (Heiko Balzter, Centre for Ecology and Hydrology)	NA: it is not a contradiction. There is no change, but the variability is here
E-1-601	A	50	19	50	19	Does not follow on from previous text on page 49 (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-602	A	50	30	50	30	US yields of what? (Nathan Gillett, University of East Anglia)	OK, adressed
E-1-603	A	50	45	51	14	Several articles did not list in literatures e.g. Fang et al (2003), Zheng et al (2003). And Zheng's result may be put in the section 1.3.5.2. (Xiaoqiu Chen, Peking University)	We have not these references. Please supply them with the details (but it could be too late). We looked for them, but the reviewer did not supply enough information for us to find these references. However, we do include more recent material from authors with the same

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							names, who we believe may be the same authors.
E-1-604	A	50	49	50	49	Using "nitrogen fertilization" is inappropriate here. Although atmospheric N deposition may have a fertilization effect, it can also have a detrimental effect… acidifying forest soils, depleting cations (Ca, Mg), increasing Al solubility and decreasing forest CO2 uptake (e.g. Aber, J. D., C. L. Goodale, S. V. Ollinger, M. L. Smith, A. H. Magill, M. E. Martin, R. A. Hallett, and J. L. Stoddard. 2003. Is nitrogen deposition altering the nitrogen status of northeastern forests? Bioscience 53:375-389. AND Nadelhoffer, K. J., B. A. Emmett, P. Gundersen, O. J. Kjonaas, C. J. Koopmans, P. Schleppi, A. Tietema, and R. F. Wright. 1999. Nitrogen deposition makes a minor contribution to carbon sequestration in temperate forests. Nature 398:145-148. (Knute Nadelhoffer, University of Michigan)	OK, adressed
E-1-605	A	50	51	50	51	Amend to read: … which has led…… (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-606	A	51	8	51	8	The text states that Schwartz and Cheng (2002) detect a decrease in the frost-free period in Northern China by 10 days. State over what time period this trend was detected. If it was really a decrease in the frost-free period, then this means an increase of the frosty period, which is a different trend from the other references discussed in this section, which all find increases in the length of the growing season. The trend of a decreasing frost-free period goes in the opposite direction. (Heiko Balzter, Centre for Ecology and Hydrology)	OK, adressed
E-1-607	A	51	8			Should we read "increase" instead of "decrease"? (Pierre Bernier, Natural Resources Canada)	OK, adressed
E-1-608	A	51	12	51	12	reference bad cited "Clark and et al 2003". Rererence is also badly written in the reference section (page 77, line 4) (Pedro Ribera, Universidad Pablo de Olavide)	OK, adressed
E-1-609	A	51	14			Insert at the end of this para, the following: "Based on data from 1976-2001, long-acting and widespread environmental changes are stimulating the growth and productivity of Amazon forests (Phillips et al. 2004; Malhi and Phillips 2004)." See Comment 28 for further details. (Indur Goklany, US Department of the Interior)	OK, but no more space for adding new references when they do not introduce new important facts. *Iincluding these references is not necessary for improved regional balance.*
E-1-610	A	51	17	51	17	…extension northward in the Northern hemisphere and upward in altitude (type "c" region: Box 1,1) of some damaging …. (Annick Douguédroit, University de Provence)	Idem 580 (see above).
E-1-611	A	51	18			The same insect is also dramatically expanding in Canada. See "Logan, J. A., J. Regniere, J. A. Powell 2003. Assessing the impacts of global warming on forest	OK, but no more space for adding new references when they do not introduce new

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						pest dynamics. Frontiers in Ecology and the Environment 1:130–137". Also, see work of Fleming, R.A. and Candau, J.N. (1998). Influences of climate change on some ecological principles of an insect outbreak system in Canada's boreal forests and the implications for biodiversity. Environmental Monitoring and Assessment, 49(2-3):235-249 on interaction between spruce budworm and climate change. (Pierre Bernier, Natural Resources Canada)	important facts *Including these references is necessary for improved regional balance.*
E-1-612	A	51	21	51	13	an opposite trend has been detected in relation due the observed rainfall decrease'. This doesn't make sense to me. (Nathan Gillett, University of East Anglia)	OK, adressed
E-1-613	A	51	22	51	26	Sentence "While the increase of … affecting large wildfires" is not absolutely clear. Try rephrasing. The information about "in spite of management acting to reduce fuel load in forests" may not be neccesary. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	OK, adressed
E-1-614	A	51	22	51	27	It is stated that climate variability is the dominant factor affecting large wildfires. It should at least be mentioned that the presence of an ignition source is required to start a fire, and often this ignition source is linked to anthropogenic influences (deliberate and accidental fires). (Heiko Balzter, Centre for Ecology and Hydrology)	OK, adressed
E-1-615	A	51	26	51	29	The previous sentence is about the effect of climate on forest fires. This sentence starts with 'This is confirmed by', but then goes on to talk about the effects of forest fire on climate. (Nathan Gillett, University of East Anglia)	OK, adressed
E-1-616	A	51	27	51	29	Changes in forest fire frequency and area consumed by forest fires over entire Russia (including Siberia) were documented since 1961 by Korovin and Zukkert (2003). They confirm the general notion of the paragraph but we cannot infer ANYTHING using only a ten year-long period analyzed by Conard et al. 2002. Ref.: Korovin, G.N. and N.V. Zukkert, 2003: Climatic change impact of forest fires in Russia. In: V.I. Danilov-Danilyan (Ed.), Climatic Change: View from Russia., Moscow, TEIS Publ., 416 pp., 69-98. (Pavel Groisman, University Corp. for Atmospheric Research)	OK, but maybe too late for inclusion in the final version: we will try. The study of Conard et al is only said to display the variability, and not any trend
E-1-617	A	51	31	51	37	New study should be incorporated: Westerling, A.L., H.G. Hidalgo, D.R. Cayan, and T.W. Swetnam. 2006. Warming and Earlier Spring Increases Western U.S. Forest Wildfire Activity. Science DOI: 10.1126/science.1128834 (Lara Hansen, WWF)	OK, adressed
E-1-	A	51	33	51	33	Insert 'and the correlation between observed interannual variations in area burnt and	OK, adressed

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618						temperature'. (Nathan Gillett, University of East Anglia)	
E-1-619	A	51	35	51	37	I don't think there is an inconsistency between Gillett et al (2004) and Bergeron et al (2004). The latter looked at fire frequency, while the former considered area burnt. A decline in fire frequency is not necessarily inconsistent with an increase of area burnt - this simply indicates that the size distribution of fires has been changing over time. Note that both studies used the same large-fire datebase. The recent article by Westerling et al (Science Express, 6 July 2006) might also be relevant here. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK, adressed
E-1-620	A	51	35			I don't see the disagreement. If, after 1970, the number of fires over entire Canada was higher, than there is no disagreement. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	OK, adressed
E-1-621	A	51	38	51	38	Should refer to Indonesian fires. Even though they are largely started by land clearance they are exacerbated by El Nino. ?/ also Amazonia and Australian fires linked to El Nino.? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Chapter deals with trends in effects with trends in climate, not effects of major climate variability systems per se.
E-1-622	A	51	38			Insert on line 21, the following new paragraph: "However, despite pest infestations and forest fires, net primary productivity has increased worldwide (as noted)." (Indur Goklany, US Department of the Interior)	NA: not everywhere
E-1-623	A	51	41	51	42	Part of the technological changes discussed earlier may also be regarded adaptive measures. Is this taken into account? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	yes
E-1-624	A	51	41	51	46	British Columbia is using more southern grown seeds (genotypes) in some of its reforestation programs because of the projected warming in the region (see The Mackenzie Basin Impact Study, published in 1997 and the ongoing case studies of the Okanagan and Columbia regions and the Georgia Basin (including the Greater Vancouver Regional District). (Lara Hansen, WWF)	OK,,thanks for the info, but we cannot introduce it in the text (no more space and impossibility to get the studies in the short time for the final version)
E-1-625	A	51	48	52	2	There are some well identified aspects of vulnerability to climate change in the case of forestry in Canada. Althoug climate-driven disturbances are an obvious one, one of the most commonly discussed aspect is the decreased length of winter road operations, something that some areas are feeling already. I could, however, not find published information on this point. (Pierre Bernier, Natural Resources Canada)	OK, thanks for the information, but difficult to use it
E-1-626	A	52	6			After the period (full stop) on line 6, add the following: "However, these projections do not fully account for adaptation options that should become available	NA : we focus on observed changes. For future, refer to chapter 5

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						as today's developing countries become wealthier and technologically more advanced, as assumed in the SRES scenarios, nor do they account for new technologies that should be developed in the future to cope with climate change or for increases ion human capital that generally accompany increasing wealth (Goklany 2005c, 2006a)." (Indur Goklany, US Department of the Interior)	
E-1-627	A	52	10	52	11	The drought in the Sahel is not necessarily anthropogenic. See WG 1, chapter 11. (Nathan Gillett, University of East Anglia)	OK, adressed
E-1-628	A	52	11	52	11	Delete the (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-629	A	52	12	52	12	Amend to read: …experiments in the Philippines (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK, adressed
E-1-630	A	52	15	52	15	1,3,6,4 Agriculture and forestry: Summary (Annick Douguédroit, University de Provence)	OK, adressed
E-1-631	A	52	21	52	22	This seems contrary to Bergeron et al (2004), whom you noted discuss a decrease in fire frequency. Also, you should be clear here about whether this assessment is about fire frequency, or area burnt. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK, adressed
E-1-632	A	52	28	55		Section 1.3.7: In general human health has improved world-wide due to improved socio-economic conditions and improved health care. There are NO significant adverse impact on human health as a result of recent climate change. ENSO related human health impact are not to be confused with climate change impact, since ENSO events have been occurring for hundreds of years without being forced by anthropgenic changes. Unusually large number of deaths during the European heat wave of 2003 was probably due to improper/inadequate response and other logistical problems and need not be highlighted. Last five winters in Northern Hemisphere have been significantly colder in Europe and parts of Asia with more fatalities than normal especially in tropical areas like Bangladesh & Vietnam (during 2002/2003 winter). Climate change impact on heat & cold stress must include some reference to recent colder winters. (Madhav Khandekar, Retired)	Disagree. There are impacts of CC on Human Health. We agree that ENSO effects are different from CC effects. To avoid confusion, we have removed the previous unit, 1.3.7.1 on ENSO and human health.
E-1-633	A	52	30	52	30	Should it be '...human health associated with regional climate change' instead of '...human health and regional climate change'? (Paul Beggs, Macquarie University)	Agree; Done
E-1-634	A	52	33	52	33	Delete "or" and add at the end of the sentence ",or increased air pollution. In addition, we acknowledge the benefits to human health from reduced GHG emissions."	OK; Done

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						(Mary Gant, Environmental Protection Agency)	
E-1-635	A	52	35	52	36	Should it be 'The superposition of greenhouse gas-induced warming on natural climate variability ...' instead of 'The superposition of natural climate variability on greenhouse gas-induced warming ...'? In line 36, 'fluctuations' of what, and 'interannual variability' in what? (Paul Beggs, Macquarie University)	OK; Modified
E-1-636	A	52	38	52	40	Is 'Climate' really required at the beginning of this sentence (could it simply start with 'Extremes in temperature ...')? Would the end of this sentence be better written as '... such as the well-described impacts of heat and cold extremes on mortality'? (Paul Beggs, Macquarie University)	OK; Modified
E-1-637	A	52	38	52	38	Delete first 'certain' in the line (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-638	A	52	41	52	41	change "...can be an important derterminants..." to "...can be important derterminants..." (Pedro Ribera, Universidad Pablo de Olavide)	OK; Done
E-1-639	A	52	48	53	9	The three bulleted points are interesting, and make a good introduction to this section. However, they are largely forgotten in the following text. They should be revisited in the Summary (1.3.7.6) (Jean Palutikof, IPCC WGII TSU)	OK; Changes are real, and are due to climate and non-climate factors. Text of entire section, including Summary was rewritten.
E-1-640	A	53	1	53	1	Delete: use as well as, insert: and (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-641	A	53	2	53	9	In lines 2 and 3, delete the authors' surnames appearing outside the parentheses. In the second point, delete the 'a' before 'climate factors' and insert a second close bracket after Purse et al. 2006. In line 9, the start of the point should be consistent with the previous two points, i.e. 'That the change in disease incidence is ...'. Towards the end of this third point, use the word 'changed' instead of 'increased' (one might be examining a DECREASE in disease incidence rather than an increase). (Paul Beggs, Macquarie University)	OK; Modified
E-1-642	A	53	4	53	4	Delete 'a' (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-643	A	53	7	53	7	Effects of land use on ecosystems are non-climate effects. (Nathan Gillett, University of East Anglia)	OK
E-1-644	A	53	11			What "evidence" (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Section removed
E-1-	A	53	11			The title of this subsection suggests that it contains an assessment of the links	OK: Section removed

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645						between ENSO and health, but my reading is that it simply reports on some studies. A more critical assessment would certainly be welcome. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-646	A	53	20	53	28	It the material in these lines being presented as a health effect of observed climate change? If so, be more explicit about this. (Paul Beggs, Macquarie University)	OK; Section removed
E-1-647	A	53	30	53	30	Trends is here an ambiguous word because of its statistical sense. There is no statistical trends studied below. (Annick Douguédroit, University de Provence)	OK; "Trends" replaced by "patterns"
E-1-648	A	53	30			I don't know anything about the literature on vector borne diseases and climate, but I get the impression (perhaps incorrect) that the overview here could be more comprehensive. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK. But we had to reduce the volume of the section; Reference made to Chap. 8
E-1-649	A	53	32	53	38	In line 32, is the word 'factors' really required? Would the sentence read better if it was deleted? Towards the beginning of the second sentence of this paragraph, would 'relationships' be a better word to use instead of 'interactions'? Interaction is defined as action on each other; reciprocal action. While vector-borne diseases are sensitive to temperature and rainfall, temperature and rainfall are not sensitive to vector-borne diseases. In lines 35 and 36, while insect and tick vectors would be expected to respond to changes in climate, the remainder of the sentence, which seems to base this statement on the assertion that other cold-blooded terrestrial species would be expected to respond to changes in climate, seems a little weak and unnecessary. I think there is plenty of published research which supports the statement that insect and tick vectors would be expected to respond to changes in climate. (Paul Beggs, Macquarie University)	OK; Done
E-1-650	A	53	40	53	43	Suggest some reference to the connection between human proximity to deer population which is known to be a key factor in Lyme disease in the USA, and is not due to changes in climate. (Haroon Kheshgi, ExxonMobil Research and Engineering Company)	OK;
E-1-651	A	53	40	53	40	latitudinal spread (extension of the area of "c" type: see Box 1,1) and abundance…. (Annick Douguédroit, University de Provence)	OK
E-1-652	A	53	40	53	43	In the peer-reviewed literature, on Lyme disease and Tick-borne encephalitis, nearly all studies emphasise that changes in vegetation, wildlife and human behaviour are considered the major factors that have resulted in changes in the prevalence and incidence of ticks and tick-borne infections. Perhaps the best example is Lyme disease in North America. The prevalence of ticks, deer, and	Substantial. Reference made to Randolph (2001), made in Lyme Dis and TBE. Indicating a vast array of other pertinent factors which may affect TBE spread other than climate change!

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						white-footed mice has increased exponentially since the reforestation of vast areas of the north-eastern states of the US.The statement "Tick distribution … has been observed to have moved northward in Sweden" BECAUSE of the current warming trend has been hotly contested by Europe's foremost specialists in this field. For example, Sarah Randolph, Professor of Parasite Ecology at Oxford University, has repeatedly challenged the authors of these articles. In two popular articles (misquoted in chapter 12), she writes: "Because climate has changed globally over the same decades, the common assumption is that climate change is the cause of increased incidence of these, and many insect-borne infections... Clearly, levels of tick-borne diseases the world over are dependent on so vast an array of pertinent factors that it is the height of simple-minded folly to attribute their ups and downs to a singular overriding factor such as global climate change. In fact, it is likely counter-productive, encouraging the implementation of ineffective measures to combat climate change that result in a siphoning of funds from more promising programs that without a doubt could do a much better job of both identifying the true agents of change in the levels of various infectious diseases and ameliorating their negative consequences". Professor Randolph has published more than 60 articles in the peer-reviewed press in the past 15 years alone, and many other specialists have discussed the issue, yet there is no mention of these in the discussion. This is a major problem in this chapter, Chapter 8 (Human Health) and Chapter 12 (Europe): the selection of articles that suggest negative consequences of climate change, whilst ignoring, or at best giving minor mention after the negative impact has been stated. (Paul Reiter, Institut Pasteur)	
E-1-653	A	53	40	53	43	In line 40, is 'spread' the best word to use? Would 'distribution' or 'range' be better? In lines 42 and 43, in what way may results have been influenced by changes due to reporting and changes in human behaviour, i.e. how do changes in human behaviour, for example, influence the finding of changes in the disease VECTOR? (Paul Beggs, Macquarie University)	OK; Modified
E-1-654	A	53	48	54	14	It is misleading to state that "malaria transmission in highland areas remains controversial". The controversy is between specialists in malaria transmission, and authors, many of them previously unknown, who seek evidence of climate-related changes. There is a wealth of carefully documented studies on the subject, none of which are cited in this paragraph, and all of which do not support the view that malaria is ascending to higher altitudes because of the current warming trend. For this reason, in 2004, a consortium of the world's top specialists in vector-borne diseases published an appeal for greater accuracy in this debate (Reiter P, Thomas	Substantial. However, the treatment given to malaria is a balanced one, showing both sides of the discussion!

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						CJ, Atkinson P, Hay SI, Randolph SE, Rogers DJ, Shanks GD, Snow RW, and Spielman AJ. 2004. "Global warming and malaria: a call for accuracy". The Lancet, Infectious Diseases (4): 323-324. We were serious in this call; we, and nearly all our colleagues, feel strongly that this debate is being driven, for various ends, by persons who have little knowledge of our field. The Chen et al. article is obscure, but is correct in stating that malaria has recently appeared in areas that were malaria-free 20 years ago. What they do not mention is that from the 1920s until the era of DDT, malaria was endemic in areas that are nearly 1000 m higher! Here again, at the end of the paragraph, a few articles are quoted that outline other factors in the presence/absence of the disease (though none of the studies in the Highlands of East Africa. One aspect this question that is never mentioned is that the African continent has very few highlands. The area involved in the dispute is tiny compared to vast regions in sub-Saharan Africa at lower altitude where malaria is stable and endemic. THE TOTAL AREA ABOVE 2000m IN ALL OF AFRICA is approximately 315,558 km2, about the size of Poland. This is 0.4% of the total surface of the continent. Many of these highlands are arid and inhospitable. In other words, although the debate will undoubtedly continue, the truth is that, even if malaria does move to higher altitudes, the incidence of cases will hardly be relevant in the context of the terrible toll due to the disease in the rest of the continent. (Paul Reiter, Institut Pasteur)	
E-1-655	A	53	51	53	51	Insert "the" between since and 1970s (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK;
E-1-656	A	54	6	54	6	Delete second "a" (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-657	A	54	12	54	12	Amend to read:….movement of people… (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-658	A	54	16	54	21	This chapter should have more of these types of assessments (i.e., considerating the quality and quantity of information available from which to make assessments of the links between external influences, climate, and impacts). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	OK;
E-1-659	A	54	16	54	18	The statement that there is a shortage of long-term historical observations of climate and malaria is incorrect. The meticulous records of the tea estates in Kenya, for example, are among the longest and best such records in the world.. The quotes of Hay (2002), Hay (2002b) and Shanks (2002) are, as usual, at the end of the paragraph, although the authors are recognized as world authorities on the subject. The reference to Patz, 2002 does not appear in the bibliography. It is also misleading to state that Pascal (2006) has "confirmed" warming trends at these	Ssusbstantial. However, we indicate that there are differences of opinion on this topic. Reference to Patz et al (2002) to be added.

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						sites; other investigators (e.g. Shanks 2002) have been unable to demonstrate any long-term change, and Pascual's method is hotly disputed. (Paul Reiter, Institut Pasteur)	
E-1-660	A	54	23	54	23	Trends is here an ambiguous word because of its statistical sense. There is no statistical trends studied below. (Annick Douguédroit, University de Provence)	OK; Patterns
E-1-661	A	54	23	54	41	Discussion is needed here on the results reported in the WHO report on Climate Change and Human Health (2004) (Giampiero Maracchi, Institute of Biometeorology)	OK
E-1-662	A	54	25	54	33	The meaning of the end of the first sentence 'independent of ... populations' is not very clear. In the second sentence, it is not really relevant to mention mean surface temperatures, so the sentence could be simplified by restricting its discussion to the number of days with high temperatures. (Paul Beggs, Macquarie University)	OK
E-1-663	A	54	31			At least one reference is missing on 2003 heat wave: Chuine, I., P. Yiou, N. Viovy, B. Seguin, V. Daux, and E. L. R. Ladurie. 2004. Grape ripening as an indicator of past climate. Nature 432:289-290. (Isabelle Chuine, CNRS)	Add this at end of refs
E-1-664	A	54	32	54	32	15000 not 14000 (Annick Douguédroit, University de Provence)	OK;
E-1-665	A	54	35	54	35	remove 'become' (Heiko Balzter, Centre for Ecology and Hydrology)	OK
E-1-666	A	54	35	54	41	Is 'sensitive' the correct term to use in the first sentence? By itself, the first sentence could be misinterpreted to mean that the human body of people in high-income populations is becoming less sensitive (this might be referred to as acclimatisation). This is not the case, as lines 37-39 list what could be considered adaptive responses. Indeed, the summary of this section on page 55 line 43 states this. Could 'vulnerable' be used instead? The same applies to the latter part of the last sentence. Winter mortality is not declining due to decreased human response to cold. I think the author again is referring to adaptive measures such as heaters. (Paul Beggs, Macquarie University)	OK; Modified
E-1-667	A	54	35	54	35	Delete 'become'. (Nathan Gillett, University of East Anglia)	OK; Done
E-1-668	A	54	35	54	35	"...populations are become..." to "populations have become..." (Pedro Ribera, Universidad Pablo de Olavide)	OK; Done
E-1-669	A	54	39	54	39	temperatures except during the heatwave in 2003 in France (Davis... (Annick Douguédroit, University de Provence)	OK; Modified

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E-1-670	A	55	3	55	4	Use of the word 'Harmful' in the title for this paragraph, and use of an acronym (HABs) for harmful algal blooms, seem somewhat unjustified. In the WGII chapter dedicated to Human Health (Chapter 8) the term 'harmful algal blooms' is only used once (page 34, line 41). Indeed, algal blooms themselves are only mentioned once in the Human Health chapter (again page 34, line 41). The acronym is only used once (page 55 line 5). (Paul Beggs, Macquarie University)	OK, but for clarity, leave as is
E-1-671	A	55	3	55	4	Reword first sentence to : There is an apparent increasing trend in the occurrence of harmful algal blooms (HABs) in coastal watrs. Part of the increase may be due to increased reporting and climate change may also be implicated (Edwards et al. 2006). Edwards, M., D. G. Johns, S. C. Leterme, E. Svendsen, and A. J. Richardson, (2006). 'Regional Climate Change and Harmful Algal Blooms in the Northeast Atlantic', Limnology and Oceanography, 51(2), 820-829. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-672	A	55	8	55	8	Delete suitability. Insert after conditions: suitable (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-673	A	55	9	55	9	Delete the initials of the authors and insert a year for the first citation. Also, the associated references seem to be missing from the References section. (Paul Beggs, Macquarie University)	OK; Need to insert ref yr
E-1-674	A	55	9	55	11	Delete such as for and all latin names to end of sentence (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK; Done
E-1-675	A	55	11	55	15	Sentence "The variability an near-concurrence… stimulating blooms." is very long and not abslotely clear. Try cutting it in two, and rephrasing. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	OK; Done
E-1-676	A	55	14	55	14	Mudie not in refs. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Ref removed.
E-1-677	A	55	14	55	14	Is there a reference that supports a climate change effect? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	See references given in 1.3.4.
E-1-678	A	55	17	55	28	The recent article by Beggs and Bambrick (2005), which examines the global rise of asthma as an early impact of climate change, should be mentioned somewhere in this section. Beggs PJ, Bambrick HJ. Is the global rise of asthma an early impact of anthropogenic climate change? Environmental Health Perspectives 2005; 113(8):915-919. (Paul Beggs, Macquarie University)	OK. Add this to refs
E-1-679	A	55	17	55	33	The increased dust storms in northern China in recent years should be mentioned. (Xiaoqiu Chen, Peking University)	Please provide reference.

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E-1-680	A	55	20	55	20	The WHO 2003 citation should be Huynen and Menne 2003 (see Chapter 8, page 56, lines 7-9). (Paul Beggs, Macquarie University)	OK: Add this
E-1-681	A	55	20	55	23	The Teranishi et al. 2000 citation should be deleted at the end of the sentence in these lines, because the sentence states that the studies are mostly from Europe, and Teranishi et al. 2000 is a Japanese study. It is out of place when there are other European studies that have not been cited, and when there is a North American study that has not been cited. (Paul Beggs, Macquarie University)	OK; Done
E-1-682	A	55	23	55	24	The statement that 'there is no good evidence that temperature changes have increased pollen abundance' is debatable. The review of past and future impacts of climate change on aeroallergens by Beggs (2004) certainly presents several studies in which temperature changes seem to have increased pollen abundance (see the second and third paragraphs of the 'Pollen amount' section in this review). The paper by Wan et al. (2002, see Chapter 8 for full reference) also shows warming caused increased total pollen production. (Paul Beggs, Macquarie University)	OK: Modified
E-1-683	A	55	23	55	24	The statement that 'there is no good evidence that temperature changes have increased pollen ... allergenicity' is debatable. The review of past and future impacts of climate change on aeroallergens by Beggs (2004) certainly presents two studies in which temperature changes seem to have increased pollen allergenicity. (Paul Beggs, Macquarie University)	OK; removed
E-1-684	A	55	24	55	25	The words 'and allergenicity' could be inserted at the end of this sentence, given that there are at least two recent studies that suggest that increased CO2 may facilitate increased pollen/plant allergenicity. The full references are as follows: Mohan, J.E., L.H. Ziska, W.H. Schlesinger, R.B. Thomas, R.C. Sicher, K. George, and J.S. Clark, 2006: Biomass and toxicity responses of poison ivy (Toxicodendron radicans) to elevated atmospheric CO2. Proceedings of the National Academy of Sciences of the United States of America, 103(24), 9086-9. Singer, B.D., L.H. Ziska, D.A. Frenz, D.E. Gebhard, and J.G. Straka, 2005: Increasing Amb a 1 content in common ragweed (Ambrosia artemisiifolia) pollen as a function of rising atmospheric CO2 concentration. Functional Plant Biology, 32(7), 667-70. (Paul Beggs, Macquarie University)	OK; Added to refs
E-1-685	A	55	26	55	28	These two sentences require supporting citations. (Paul Beggs, Macquarie University)	OK
E-1-686	A	55	28	55	28	Should maybe state how the conversion to silage production affects pollen production (by harvesting before the crop blooms?)	OK

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						(Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-687	A	55	29	55	30	The Prospero and Lamb reference does not appear to be in the References section, and the citation needs 'and Lamb' inserted and 'African dust' deleted. (Paul Beggs, Macquarie University)	OK: Add ref.
E-1-688	A	55	32	55	33	Is the increase in Saharan dust really attributable to anthropogenic climate change? Is this based on an assessment of the literature, or just speculation in one study? (Nathan Gillett, University of East Anglia)	Not attribution climate changes to anthropogenic causes in this statement.
E-1-689	A	55	32	55	32	Insert s after increase. Delete first has (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	OK
E-1-690	A	55	34	55	34	Add two new sections. Section 1.3.7.6 Health Effects Associated with Increased Air Pollution. Models have recently been developed that can assess the impact of climate change on ozone concentrations and the resulting health effects. Knowlton, Rosenthal et alia used such a modeling framework to simulate hourly regional weather conditions and ozone in five summers in the 2050s across the New York metro area. They assessed changes in ozone-related impacts on summer mortality resulting from climate change alone and from climate change combined with changes in ozone precursor emissions. Climate change alone caused a 4.5% increase in ozone-related acute mortality. Adding ozone precursor emission increases to climate change yielded similar results. When population growth was included, absolute impacts increased substantially. In a second study, Bell, Goldberg, et alia studied the effect of climate change on ambient ozone concentrations and the subsequent human health impacts. They estimated hourly concentrations in 50 eastern U.S. cities for five summers each in the 1990s and 2050s, reflecting current and projected future climates, respectively. The study did not consider future changes in anthropogenic emissions, but only the impact of altered climate on ozone and health. Ozone levels were estimated to increase under future climatic conditions, with the largest increases in cities currently with high pollution levels. On average across the 50 cities, the summertime daily one-hour maximum increased 4.8 ppb, with the largest increase at 9.6 ppb. The average number of days per summer exceeding the 8-hour regulatory standard increased by 68%. Elevated ozone levels correspond to approximately a .11 to .27% increase in daily total mortality. Section 1.3.7.7 Health Benefits of Greenhouse Gas Mitigation The World Health Organization has ranked mortality and morbidity from air pollution as one of the top ten causes of disability (WHO 1997). Reduction in	This will increase our already over-sized chapter. Best suited for Chap. 8. Discussed in 8.4

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						fossil fuel combustion brings immediate and long-term benefits. During the Olympic Games in Atlanta in 1996, residents were asked to avoid using their automobiles which reduced peak daily ozone concentrations by approximately 28%, the number of asthma acute care events decreased 42% in the Georgia Medicaid claims file while the number of non-asthma acute care events decreased by 3% (Friedman). Cifuentes, Borja-Aburto, et alia studied the health benefits of reducing greenhouse gas emissions by developing scenarios for Mexico City, Santiago, Sao Paulo, and New York City. Adopting available technology to reduce fossil fuel emissions over the next twenty years in these four cities will reduce particulate matter and ozone and avoid approximately 64,000 deaths, 65,000 chronic bronchitis cases, and 37 million person-days of work loss or other restricted activity. Bell, M., R. Goldberg, et alia 2006: Climatic Change, Ambient Ozone, and Health in 50 U.S. Cities. Accepted for publication in Climatic Change. Cifuentes, L., V. Borja-Aburto et alia 2001: Assessing the Health Benefits of Urban Air Pollution Reductions Associated with Climate Change Mitigation (2000-2020): Santiago, Sao Paulo, Mexico City, and New York City. Environmental Health Perspectives 109(Supp 3): 419-425. Friedman, M.S., et alia 2001: Imapct of Changes in Transportation and Commuting Behaviors During the 1996 Summer Olympic Games in Atlanta on Air Quality and Childhood Asthma. JAMA 285: 897. Knowlton, K., J. Rosenthal, et alia 2004: Assessing Ozone-Related Health Impacts Under a Changing Climate. Environmental Health Perspectives 112(15): 1557-1563. WHO, Health and Environment in Sustainable Development: Five Years after the Earth Summit (World Health Organization, Geneva, 1997) (Mary Gant, Environmental Protection Agency)	
E-1-691	A	55	35	55	35	Change "1.3.7.6" to "1.3.7.8". (Mary Gant, Environmental Protection Agency)	Done
E-1-692	A	55	35	55	35	1,3,7,6, Human health: Summary (Annick Douguédroit, University de Provence)	OK
E-1-693	A	55	37	55	44	The discussion of potential health effects is rather selective. Thus there is usually a maximum mortality from cardio-vascular disease in winter. So winter warming should lead to decreased mortality. Have any studies discussed this? It would seem to be a health impact of climate change that could be more easily studied than some others such as malaria. (Neville Nicholls, Monash University)	Pl see Chap. 8 for fuller discussion

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E-1-694	A	55	38	55	38	…and range towards north of some … (Annick Douguédroit, University de Provence)	OK; done
E-1-695	A	55	40	55	40	changes in the El Nino-Southern Oscillation'. But there is no evidence of anthropogenic influence on ENSO to date. See WG 1, ch 9 for an assessment. (Nathan Gillett, University of East Anglia)	OK
E-1-696	A	55	41	55	42	I don't understand the statement linking an increase in variability and the 2003 heat wave. Are you trying to say that the heat wave was attributed to a change in variability? If so, that doesn't come out in the supporting sub-section. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	No. There was no attempt to link increased variability and the heat wave of 2003.
E-1-697	A	55	41			Remove "An increase in variability of", because high temperatures themselves have caused the health effects; not some increase in variability, which is also left undefined here. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	OK; Done
E-1-698	A	55	47	58		Section 1.3.8: There are NO consistent increasing trends on world-wide extreme weather events. Careful analysis (Khandekar et al, Pure & Applied Geophysics, 2005, 162,p.1557-1586) of extreme weather shows no increase in recent years. Some of the extreme events like winter blizzards on the Canadian Prairies have certainly declined (Lawson, Natural Hazards, 29(2003),p.123-138). Tropical cyclone & hurricane impacts do not show increasing trends in deaths or property losses when adjusted to societal change. Excellent studies by Changnon (Natural Hazards, 18, p. 287-300 & 29, p. 273-290) must be referenced. Referring to a study (Miller et al, 2006) which is not published yet while ignoring published studies by Changnon is unacceptable. Figure 1.5 from Miller et al, 2006 is very poorly drawn showing a linear increase in losses which appears highly suspect. This Figure is not acceptable and must be deleted. (Madhav Khandekar, Retired)	This is a sensitive subject! The Changnon papers are by now fairly old and do not include the recent increase in hurricanes activities and losses. They also only cover the US. Some extremes have almost certainly declined – which is why it is important to look at the evidence case by case. A better figure provided.
E-1-699	A	55	47			I think it would be useful to include additional cross links to WG1 in this sub-section. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	These added – and in particular the WGI material has now been extended for a number of these topics. Will also clarify that the extremes considered by WG1 are not always the same as the extremes most relevant for considering catastrophic impacts.
E-1-700	A	55	49	55	49	Delete The Capital R (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	???
E-1-701	A	56	1	56	2	The evidence of change does not concern all the high energy events; see tropical cyclones in 1,3,8,3. (Annick Douguédroit, University de Provence)	Clarified.

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E-1-702	A	56	1	56	2	Delete this sentence. WG I has responsibility for the assessment of changes in the frequency, geography and/or severity of high energy weather events. WG II should limit itself to assessment of the impacts of, vulnerabaility to, and potential for adaptation to these events. Discussion of changes should be limited to citing WG I's conclusions. Specific examples of the difficulties that having two separate assessments create are presented in subsequent comments. (Lenny Bernstein, L.S. Bernstein & Associate, L.L.C.)	WG1 does not always consider the same class of extremes as are relevant to catastrophic impacts – see above. Now that WG1 material is clear it is possible to ensure consistency in coverage where the same material is being discussed but also to highlight the differences where the class of extremes under consideration is at longer return periods of occurrence.
E-1-703	A	56	4	56	4	Delete second events (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	??? Maybe the line referencing is wrong?
E-1-704	A	56	6	56	7	Anthropogenic influence likely warmed the climate considerably between 1900 and 1970 as well. (Nathan Gillett, University of East Anglia)	This maybe the case but the data is often poorer over this period and the magnitude of change smaller.
E-1-705	A	56	9	56	14	I don't think we should give the message that IPCC is on a "quest", or a "search" for statistical significance (if you search for the latter, you will find it, at least 5% of the time when conducting tests at the 5% level). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	This is not an IPCC quest but a quest by researchers looking for trends. Clarified.
E-1-706	A	56	10	56	10	Delete full stop, insert comma, lower case data (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Will do. Done
E-1-707	A	56	11	56	11	Insert collectively between search and for; delete from Line 12 (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Will do. Done
E-1-708	A	56	16	56	16	See comments for page 25-line3 to page26-line3. (Annick Douguédroit, University de Provence)	Need to discuss with CLA. Addressed earlier.
E-1-709	A	56	16	56	36	Replace this section with a summary of the WG I's assessment of river flows and floods found in Chapter 3, Pg. 22, line 53 to Pg. 23, line 53 of their SOD. WG I's assessment is based on a much broader selection of the literature -- they cite some 30 references compared with the 3 cited by WG II. WG I also includes extensive literature about stream flows and floods in developing nations. (Lenny Bernstein, L.S. Bernstein & Associate, L.L.C.)	Ensured consistency with WG1 but again the term 'flood' is used to apply to extreme flows at almost all return periods and the focus here is on catastrophic events – ie floods at long return periods.
E-1-710	A	56	16	56	36	Detection of runoff changes is discussed in WG 1 9.5.4.2.1. (Nathan Gillett, University of East Anglia)	Not sure if this is relevant to floods – but checked cross reference.
E-1-711	A	56	16			good place to relate to chapter 3 assessments of heavy rainfall changes (Gabi Hegerl, Duke University)	Cross referenced.
E-1-712	A	56	28	56	28	correct the comma in '200,0000km2'. Should it be 200,000 or 2,000,000 ? (Heiko Balzter, Centre for Ecology and Hydrology)	Should be 200,000km2
E-1-	A	56	38	57	2	See WG 1 3.5.3 and 9.5.3.7, and reference them here. Discussion of changes in	Except that some ETCs are highly damaging.

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713						extratropical cyclones would seem to be primarily the province of WG 1. (Nathan Gillett, University of East Anglia)	
E-1-714	A	56	38	57	2	Replace this section with a summary of the WG I's assessment of extratropical cyclones found in Chapter 3, Pg. 68, line 13 to Pg. 69, line 17 of their SOD. Again, WG I's assessment is based on a much broader selection of the literature -- they cite over 20 references compared with the 2 cited by WG II. WG I also includes literature about extratropical cyclones globally, whereas WG II's assessment is limited to Western Europe. (Lenny Bernstein, L.S. Bernstein & Associate, L.L.C.)	Most extreme ETCs in terms of impact are in Western Europe.
E-1-715	A	56	40	57	2	The discussion on extratropical cyclones seems inclonclusive and rather sparse. Perhaps some more on this could be taken from WG1 Chapter 9? (although that chapter si also rather inconclusive on this topic). (Neville Nicholls, Monash University)	The subject is somewhat inconclusive – and therefore the text needs to reflect this.
E-1-716	A	56	40	57	2	No information for elsewhere in the world? (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Link with what is said in WG1
E-1-717	A	56	44	56	44	Should that be a significant decrease in annual pressure minima, rather than a significant increase? An increase, implying storms are becoming less intense, would be in contradiction with the rest of the sentence. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Correct - this should be decrease in annual pressure minima.
E-1-718	A	56	47	56	48	NAO referred to many times before without definition (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Clarified
E-1-719	A	57	4	57	34	See WG 1 3.8.3 and 9.5.3.6 for a discussion of observed changes in tropical cyclones and their attribution. Reference these sections here. (Nathan Gillett, University of East Anglia)	Referenced these expanded sections
E-1-720	A	57	4	57	34	Replace this section with a summary of the WG I's assessment of extratropical cyclones in Chapter 3, Pg. 63, line 42 to Pg. 67, line 9 of their SOD. WG I's assessment is based on a much broader selection of the literature -- they provide specific assessments for each of the major ocean basins compared with an emphasis on the North Atlantic in WG II's text. (Lenny Bernstein, L.S. Bernstein & Associate, L.L.C.)	Cross referenced but see above – impacts most significant in Western Europe.
E-1-721	A	57	4			This subsection on tropical cyclones needs a careful look to make sure that it is consistent with Ch 3 and Ch9 to ensure that IPCC is giving a consistent message regarding this controversied subject. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Closely linked with the new material in WG1.
E-1-722	A	57	4		34	This paragraph overlaps strongly with WG1's assessment of hurricane changes in chapters 3. This should be reduced and tied to WG1. (Gabi Hegerl, Duke University)	As above.

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E-1-723	A	57	7	57	7	The authoritative reference that should be cited here is Webster et al (2005, Science), not Pielke. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	There are now many references in this debate and will select the most important judiciously.
E-1-724	A	57	14	57	27	Try shrinking this paragraph. Again, considering the need of reduction, information about Emanuel methods may not be indispensable the authors could just informed about his/her findings, the same with Webster (Munich Re Group 2005). (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	This should now be possible given the expanded material in WG1.
E-1-725	A	57	14	57	48	Make reference to heat content and not just sst and shedding of rings from loop current? And heat content relative to 26 degreeC and role in hurricance intensification.? Note under economic losses that more than one cyclone record was broken in the Atlantic in 2005 and refer to impact of Katrina. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Referenced various records broken in 2005 – also noted temperatures relative to TC threshold.
E-1-726	A	57	24	57	24	Webster (Munich Re Group 2005) surveyed tropical cyclone intensities across all basins… Webster should not be quoted as an author of Munich Re Group 2005. In fact his paper is: Webster, P. J. et al. (2005), Changes in Tropical Cyclone Number, Duration, and Intensity in a Warming Environment, Science 309, S. 1844–1846 (Peter Hoeppe, Munich Re)	This reference became garbled and is now clarified.
E-1-727	A	57	24	57	24	Cite the Webster et al. paper here, not the Munich Re report. (Nathan Gillett, University of East Anglia)	Done and see above.
E-1-728	A	57	36	57	36	Why not include (at least mention) figures on people affected? See e.g. The CRED database and analyses and/or Red Cross/Red Cresecent World Disaster Reports. (Maarten van Aalst, Red Cross/Red Crescent Centre on Climate Change and Disaster Preparedness)	'people affected' is not a very easy measure to employ through time and the datasets are likely to be incomplete before 1990s.
E-1-729	A	57	36	58	44	There should be a parallel sub-section dealing with deaths due to extreme events, since deaths are just as, if not more, relevant to human well-being than economic losses. This sub-section should note that, 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 in earlier decades. 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	Deaths are dominated by floods for which dramatic reductions have been achieved through improvements in warnings, mitigation and disaster relief. Therefore it is not really possible to use disaster deaths as indicators of changes in the occurrence of the catastrophes themselves – as is the topic of this chapter.

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						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 (Goklany 2006b, 2005b). (Indur Goklany, US Department of the Interior)	
E-1-730	A	57	38	57	48	The first two sentences of this paragraphs report an increase in economic losses that is then put down in the following paragraphs. I suggest the authors should only inform about the normalized losses, which are the only really meaningful. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Clarified that normalized losses are the only ones that should be considered.
E-1-731	A	57	38	57	39	Are these figures inflation-adjusted? (Nathan Gillett, University of East Anglia)	Yes – and exposure adjusted.
E-1-732	A	57	41	57	41	Instead of the citation "Munich Re Group 2005" it would be better to cite the more actual report "Munich Re Group 2006" (Annual Review: Natural Catastrophes 2005, F-Media Druck GmbH, Munich) published in March 2006. (Peter Hoeppe, Munich Re)	Done.
E-1-733	A	57	46	57	48	These previous national US assessments, as well as those for normalized Cuban hurricane losses (Pielke et al. 2003), did not show an significant upward trend in losses over time, but this was before the remarkable hurricane losses of 2004 and 2005. (Peter Hoeppe, Munich Re)	Correct – the last two years are critical.
E-1-734	A	57	47	57	47	insert y after an (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-735	A	57	48	57	48	I think this is inappropriate. It leads the reader into interpreting recent events in a particular way without providing supporting information. This suggestion, that the losses in 2004and 2005 draw Pielke's results into question, needs to be supported with a reference or a solid in chapter assessment. What does Pielke think about this? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	I believe Pielke agrees that adding 2004 and 2005 has the potential to change his earlier conclusions – at least about the absence of a trend in US Cat losses. .
E-1-736	A	57	50	58	43	Since we are not familiar with the Miller et al. study reported on these lines, and the citation provided is of no help, we do have a number of questions about this study. First, to convert losses from one cuyrreny unit to another, did this study use market exchange rates or PPP-adjusted rates? Second, since many of these countries (India, Philippines, Central America) have relatively large informal sectors) and many people suffereing losses would be from these sectors, how well were their losses tallied. Third, how were changes in assets at risks accounted for? (Indur Goklany, US Department of the Interior)	Market exchange rates have been used, and for meny developing the losses are economic losses as reported by EM-DAT. The changes in assets at risk are based on national GDP assumptions (except for the US where State level data is employed).
E-1-	A	57	50	58	43	I'm wondering if too much space is devoted to Miller, given that the inference one	Clarified that this study is the best available at

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737						draws from Fig 1.5 is senstive to the inclusion of individual outliers (as pointed out in the text) and that it is acknowledged that early data are incomplete. Some additional comment on data quality, beyond just completeness, is probably in order (I'm not expert, but this type of data would presumably be influenced by all kinds of factors, including varying political influences and changes in reporting practices, that might confound any climate signal). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	attempting to normalize the losses and that the conclusions of the study are stated fairly. The study does attempt to choose a period (since the 1970s) during which reporting standards become increasingly comprehensive.
E-1-738	A	58	15	58	17	Is the statistically significant correlation purely a result of the trend in both series? Does the correlation remain statistically significant if both are de-trended? If not, then this merely tells us that both series contain a trend. More fundamentally, why correlate losses with global temperature? Some justification is needed. (Nathan Gillett, University of East Anglia)	Losses can be correlated with year and also with global temperatures. The correlation with T is a function of both series containing trends with time over this period.
E-1-739	A	58	15	58	44	Fig 1,5 is not reliable from a statistical point of view because the significant trend is pulled upward by "outliers" (especially 3 points with losses >100000) which provoque a pseudo-significancy as it is suggested by the authors themselves in lines 18-21 "Removing.....entirely". So I propose "Since 1970 the global normalized results do not show any statically significant correlationn with global temperatures." and to remove the end of the paragraph and the figure 1,5 because it can mislead a reader not familiar with correlation. (Annick Douguédroit, University de Provence)	Figure moved to Supplementary Figure and employed a different plot that smoothes catastrophe losses and shows these alongside temperature. After smoothing (that thereby removes the peaks noted) the correlation remains. The text now provides a balanced commentary on this. .
E-1-740	A	58	19	58	20	What is the reason for swapping Chinese and Indian GDP per capita? (Nathan Gillett, University of East Anglia)	Remove – the point concerns the fact that any global dataset is inevitably biased towards what has been observed in countries with larger economies.
E-1-741	A	58	45	58	45	1,3,8,5 Disasters and hazards: Summary (Annick Douguédroit, University de Provence)	Done
E-1-742	A	58	47	58	47	Replace "Global losses reveal rapidly rising costs…" by "Global losses does not reveal rapidly rising costs…. (Annick Douguédroit, University de Provence)	Clarified language used – but this is replacing one positive statement by an equally positive negative statement – which is not the summarized conclusions of the study.
E-1-743	A	58	49	58	49	there still remains an underlying rising trend' - but this is uncertain according to the previous paragraph. (Nathan Gillett, University of East Anglia)	Text rewritten.
E-1-744	A	58	49	58	49	The conclusion that an underlying trend remains after adjustment for non-climate factors seems inconsistent with with the assessment of the degree of uncertainty in the Miller et al result. This conclusion might be better expressed as "While a recent analysis suggests that there may be a small, underlying, trend ...., the results of this	Agreed – will restate the conclusions of this work judiciously to summarize what is revealed and what is uncertain.

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						study are uncertain and the dominant signal remains ...". (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-745	A	59	3	59	3	increases in' should be removed here. The point is that the PDI correlates with the SSTs even if both are detrended, not just that they have a common increasing trend. (Nathan Gillett, University of East Anglia)	Clarified.
E-1-746	A	59	3	59	3	…. Correlates with increases in sea surface temperatures……A graph would be helpful here. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Do not believe there is space for a figure.
E-1-747	A	59	10	59	11	What is the meaning of "now extant" ? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Text changed.
E-1-748	A	59	23	59	25	This period is almost certainly too short to detect anthropogenic influence on climate in a small region. (Nathan Gillett, University of East Anglia)	Agreed. However, it is one of the few published literaure on observed changes in the 2 key climatic variables (temperature and solar radiation) that directly affect energy use in buildings. Short period notwithstanding, it gives a good indication of the impact of climate change on energy use.
E-1-749	A	59	23	59	38	Most of this material could be taken from WG 1 chapter 3. This would also improve the level of assessment, and reduce the reliance on individual studies. (Nathan Gillett, University of East Anglia)	Not aware of what material is in WG1 Chapter 3. However, I believe it is always a good idea to have individual studies in any review (even a short review like this), rather than simply refering to other reviews/Chapters.
E-1-750	A	59	33	60	31	This section is heavily biassed towards the west and developing countries. It would be good to see some examples drawn from Asia, and there is some text in Chapter 10 which could be helpful. (Jean Palutikof, IPCC WGII TSU)	Agreed. There is very little published work on this topic (i.e. observed climate change and energy use) for Asia. If there is indeed relevant information in Chapter, this could be included. However, we have to consider the space limition.
E-1-751	A	59	35	59	35	Delete the (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Agreed and revised accordingly.
E-1-752	A	59	38	59	38	"Trigo RM, Garcia-Herrera R, Diaz J, Trigo IF, and Valente MA. 2005. How Exceptional Was the Early August 2003 Heatwave in France? Geophysical Research Letters 32:Art. No. L10701" could possibly be included (Pedro Ribera, Universidad Pablo de Olavide)	Agreed. This article was added.
E-1-753	A	59	41	59	41	Replace 'space' with 'air'. Replace 'end-user' with 'use'. (Nathan Gillett, University of East Anglia)	Agreed and revised accordingly.
E-1-	A	59	47			rather inelastic, see chapter 6 WGIII	Not sure what the reviewer meant by

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754						(Geoff Levermore, Manchester University)	"inelastic".
E-1-755	A	59	51	60	12	The climatic impacts on turism repeat with those in page 18, line 15-19. The two parts could be merged and shortened. (Xiaoqiu Chen, Peking University)	Related to 1.3.9.2.
E-1-756	A	59				The 'catch-all' nature of this section is somewhat confusing. I assume that the authors are here interested in how individuals and communities are responding behaviourally and economically to climate change. If so, why have energy use and tourism been selected for detailed coverage and not water use, transport, purchasing trends, perceived risk etc. ? The following references may be of interest Plotnikoff, R.C., Wright, M.-F., Karunamuni, N. 2004. Knowledge, attitudes and behaviours related to climate change in Alberta, Canada: Implications for public health policy and practice. International Journal of Environmental Health Research 14 (3), pp. 223-229 Grothmann, T., Patt, A. 2005. Adaptive capacity and human cognition: The process of individual adaptation to climate change. Global Environmental Change 15 (3), pp. 199-213 (Paul Jeffrey, Cranfield University)	Energy and tourism selected because they are key socio-economic issues; a section on regional adaptation has been added in the FGD as well. The suggested references were checked, but neither of them is directly related to the topic of Section 1.3.9 and therefore were not included. The first one is about a survey of 600 Alberta households on knowledge, attitdues and behaviours related to climate change. The Second one is about the development of a socio-cognitive Model of Private Proactive Adaptation to Climate Change (MPPACC) and its application to two case studies.
E-1-757	A	60	2	60	12	WWF report highlights the potential effects of climate change on the tourism sector in the Mediterranean. (Lara Hansen, WWF)	Chapter is on observed impacts
E-1-758	A	60	16			An example of people adapting to climate change is documented in Power, S., Sadler, B., and Nicholls, N., 2005. The influence of climate science on water management in Western Australia. Bull. Amer. Meteorol. Soc., 86, 839-844. (Neville Nicholls, Monash University)	Added. Thank you.
E-1-759	A	60	19	60	19	Add: …..upon tourism activity except for skiing. (Annick Douguédroit, University de Provence)	Text changed as suggested (PN).
E-1-760	A	60	26	60	31	This paragraph seems selective, it seems to rely on a single study, and more overall assessment is needed. (Nathan Gillett, University of East Anglia)	The study cited was an assessment.
E-1-761	A	60	29	60	29	Delete: alteration of, insert: changing (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.
E-1-762	A	60	32			Add a new para on line 32 noting that over past few decades, death rates due to droughts, floods and windstorms are down globally, suggesting that either such events have not strengthened and/or populations are having better success coping with such events in terms of limiting their death toll. In addition, despite increases in population, mortality for droughts and floods are down, but not necessarily for	Please provided full reference for article.

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						windstorms.. For additional details. See Goklany (2006b), which is based on dfata from EM-DAT from 1900-2004. (Indur Goklany, US Department of the Interior)	
E-1-763	A	60	34	64		Sections 1.4 & 1.4.1: Attribution must include natural variability and changes in large-scale circulation patterns due to solar variability. Several recent studies (J of Climate, 16, p.2067-2077: Geophysical Research Letters, 30, p.1175 & 32,L16712) demonstrate impact of solar variability on Arctic basin temperature and on NAO (North Atlantic Oscillation) spatial structure. Multi-decadal climate variability may be part of natural variability and not induced by anthropogenic climate change. This aspect of climate variability must be discussed. (Madhav Khandekar, Retired)	Rejected. The observed temperature variations over the last three decades are outside the range of natural climate variations in most regions, see WGI Fig 3.9.
E-1-764	A	60	34	70	19	The section 1.4 describes mostly aggregated analyses of changes in phenology, particularly onset of spring. While the material is complementary to the material in section 1.3.5.2 (p.41 ff), I would recommend to merge these two chapters into a larger chapter on phenology. Having read the whole chapter 1 the two sections seem to be artificially separated. Some more general conclusions currently in section 1.4 might still have a place at the end of chapter 1 and should be retained there. But much of the material on phenology would be better placed together with 1.3.5.2. (Heiko Balzter, Centre for Ecology and Hydrology)	Rejected. Section 1.4 links the observed changes in physical and biological systems with observed temeprature changes due to increasing greenhouse gases.
E-1-765	A	60	38	60	41	Is the "jointly attributed" referring to the two step attribution process or to a collective attribution study, ie that the collective responses have been attributed. It sounds like the latter, but I think you mean the former. If it is the former then perhaps "each sequentially attributed" would be better. "Joint attribution" sounds to me (without seeing your definition) to be the attribution of multiple observed changes to a single cause. I would prefer "sequential attribution" or something similar. (Daithi Stone, University of Oxford)	Rejected. Joint attribution is based on the two-step process, as defined in the chapter, and a global assessment of the studies considered in the chapter and the end-to-end joint attribution studies.
E-1-766	A	60	38	60	38	As noted above (comments 4-6), I don't think the evidence presented here meets the standards set out in this chapter's definition of joint attribution. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Disagree. More details of the methods and results, and more end-toend joint attribution studies are included now.
E-1-767	A	60	38	60	41	Actually, 1.2 talks about regional climate change, not regional temperature changes. The latter are only a subset of the former. It also talks about anthropogenic causes, not specifically to increases in greenhouse gases and aerosols in the atmosphere. There are other anthropogenic forcings on regional climate, such as land use change and the emission of chemicals that destroy stratospheric ozone.	Text changed.

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						(Daithi Stone, University of Oxford)	
E-1-768	A	60	51	61	33	The content on meta-analyses repeats with that in the section 1.3.5.2 "Change in phenology" and is too long. The two parts should be shortened significantly and merged either in 1.3.5.2 or in the current section (section 1.4.1.1). (Xiaoqiu Chen, Peking University)	Agreed. Text reduced.
E-1-769	A	60	51			Many of the earlier results also cover large regions or multiple sites, rivers, etc. Why are they not meta-analysis? (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Meta-analysis specifically refers to synthesis and analysis of previoulsy published studies.
E-1-770	A	61	9			"global warming" -> "Northern Hemisphere warming"? (Daithi Stone, University of Oxford)	Agreed. Revised to regional warming.
E-1-771	A	61	13	61	14	"climate change predictions": Does this refer to predictions from observed climate change or to predictions of climate change? (Daithi Stone, University of Oxford)	Text changed.
E-1-772	A	61	15	61	16	…living in the northern hemisphere (examples of regions of "c" type of the ares: see Box 1,1) and documented….in the northern hemisphere (examples of regions of "a" type of the areas: see Box 1,1). (Annick Douguédroit, University de Provence)	Box not included in chapter. Need reference from reviewer.
E-1-773	A	61	19	61	19	Diagnostic fingerprint for 279 species sounds like could be more than the total number of species given as more than 270 on line 13. What proportion of species are affected ? (Peter Stott, Met Office)	Total number of species is 1700.
E-1-774	A	61	24	61	25	"expected with warming": But some areas have been cooling over the last 30 years, so we would not expect a response to warming there (or rather, a negative one). (Daithi Stone, University of Oxford)	Disagree. Very few regions have cooled over the last 30 years where the studies of the biological systems have been carried out.
E-1-775	A	61	28	61	28	…last 30 years (examples of regions of "c" type for the expansions and of regions of "a" type for the advances in timing of the areas: see Box 1,1). This number is … (Annick Douguédroit, University de Provence)	Suggested box not included. Please provide reference.
E-1-776	A	61	40	62	30	The content on phenological trends and their relation to temperature in Box 1.3 repeats obviously with those in the section 1.3.5.2 "Change in phenology" and should be shortened and put in 1.3.5.2. (Xiaoqiu Chen, Peking University)	Box 1.3 is important to demonstrate lack of publication bias, thus should stay in 1.4
E-1-777	A	61	40			Box 1.3 could be shortened. Figures' legend are missing (Isabelle Chuine, CNRS)	Figure legends are inserted.
E-1-778	A	61	47	61	49	Delete species phenology, insert The phenology of species in the analysis; insert the after to; insert and after C; delete fall, insert in the autumn/fall (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done

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E-1-779	A	62	1	62	30	Figures are not indicated and do not have legends as in other boxes. (Natalia Perez Harguindeguy, Instituto Mulitidisciplinario de Biología Vegetal (UNC-CONICET))	Figures do have numbers and legends now.
E-1-780	A	62	20	62	21	Is this a temporal correlation coefficient (ie matching like trends), a spatial correlation coefficient (comparing patterns), or spatio-temporal? (Daithi Stone, University of Oxford)	It is a spatial correlation of trends as described in "spatially consistent"
E-1-781	A	62	23	62	26	I didn't understand this sentence. Do the authors mean that the project found that the chance of the phenological changes having occured by chance was 20%? (Nathan Gillett, University of East Anglia)	No. 20 % of the records did not show advancing onset in the last 30 years.
E-1-782	A	62	33	64	8	I am unconvinced about the relevance of the NAO materila, and given the overrun in terms of length of this chapter it might usefully be cut. (Jean Palutikof, IPCC WGII TSU)	Material is drastically cut.
E-1-783	A	62	33	64	9	Are the studies mentioned here related to multidecadal climate variability? Multidecadal means lasting several decades; or high and low NAO and ENSO indices represent each 1 year only as mention on Fig 1,6. But the purpose of the WGII is related to climate change, not to year by year climate variability. I think that 1,4,1,2 is not included in the topics of WGII. So I propose to remove it. (Annick Douguédroit, University de Provence)	The term mulitdecadal variability is cut and NAO and related material cut and moved to 1.3.5.
E-1-784	A	62	35	62	49	The correlation of the NAO with temperature should be mentioned in this paragraph. Cite WG 1, chapter 3. (Nathan Gillett, University of East Anglia)	Thank you, is done.
E-1-785	A	62	35	63	33	The content on NAO-phenology relationships repeats with those in the section 1.3.5.2 "Change in phenology" and should be shortened and merged either in section 1.3.5.2 or in the current section (section 1.4.1.2). (Xiaoqiu Chen, Peking University)	Both section are merged and cut.
E-1-786	A	62	40	62	40	Insert the after of; delete of, insert on (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Changed
E-1-787	A	62	43	62	49	There may be a high relevance of the NAO index for all regions if the distinct temperature and precipitation anomaly patterns associated with NAO are taken into account. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Link to WG 1 chapter 3 is made (see E-1-784)
E-1-788	A	62				Box 1.3 second figure. Define open and closed circles (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Full legend is given now.
E-1-789	A	63	40	63	40	Insert The before NAO (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done, section however shortened and moved to respective subchapters of 1.3.5.
E-1-790	A	63	49	63	49	Delete: find significantly earlier arrival of, insert: found that (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done, section however shortened and moved to respective subchapters of 1.3.5.

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E-1-791	A	63	50	63	51	After Germany insert: arrived significantly earlier; amend correlation to read correlated (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done, section however shortened and moved to respective subchapters of 1.3.5.
E-1-792	A	64	4	64	7	I didn't understand this sentence. Should the last part of the sentence read 'but do not attribute population trends to anthropogenic climate change'? (Nathan Gillett, University of East Anglia)	Section shortened and modified.
E-1-793	A	64	5	64	5	Delete: study, insert: identification; insert by after America and put the reference after by. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done, section however shortened and moved to respective subchapters of 1.3.5.
E-1-794	A	64	11	64	13	Is the "jointly attributed" referring to the two step attribution process or to a collective attribution study, ie that the collective responses have been attributed. It sounds like the latter, but I think you mean the former. If it is the former then perhaps "each sequentially attributed" would be better. "Joint attribution" sounds to me (without seeing your definition) to be the attribution of multiple observed changes to a single cause. I would prefer "sequential attribution" or something similar. Actually, 1.2 talks about regional climate change, not regional temperature changes. The latter are only a subset of the former. It also talks about anthropogenic causes, not specifically to increases in greenhouse gases and aerosols in the atmosphere. There are other anthropogenic forcings on regional climate, such as land use change and the emission of chemicals that destroy stratospheric ozone. (Daithi Stone, University of Oxford)	Rejected. Joint attribution is based on the two-step process, as defined in the chapter, and a global assessment of the studies considered in the chapter and the end-to-end joint attribution studies. Attribution of temeprature changes is to greenhouse gas forcing.
E-1-795	A	64	11	70		Section 1.4.2: Discussion on joint attribution must include discussion on observed natual variability. Trends in regional temperature changes (Figures 1.8 & 1.9) are definitely shown to be contaminated with anthropogenic surface processes like urbanization etc (see Int'l J of Climatology, 26(2006), p. 897-913) and many regional changes may possibly be due to the urbanization & land-use change impact. This aspect must be discussed. (Madhav Khandekar, Retired)	Accepted. In some locations, land surface changes are likely to have a significant warming impact. However, over the many diverse regions in which observed warming has been identified, including some regions remote from land use change, it is extremely unlikely that the warming is due primarily to land use change.
E-1-796	A	64	15	64	16	The evidence presented in Gillett et al would support statements on the detection of anthropogenic influence on temperature, and the detection of anthropogenic influence on area burnt, but I think we would be uncomfortable making strong attribution statements in either case. One limitation on this study is that Gillett et al were not able to perform their analysis with a full suite of climate change simulations allowing for a range of possible alternative explanations (e.g., no	Accepted. Text revised to take into account these comments.

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						simulations with natural forcing were available). Another is that they used a rather simple empirical model to predict variations in area burnt from variations in temperature. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-797	A	64	28		29	cite chapter 9 here for likelyhood assessments. (Gabi Hegerl, Duke University)	Done. References to WGI assessment included.
E-1-798	A	64	32		38	The interpretation of model gridpoint results is an area of active research by ch9's assessment and should at this point be treated with caution. While the results are very convincing, its presently unclear how much of this comes really from spatially larger scales. Also, the physics of model variability on single gridbox scales is inherently limited. I suggest to keep in touch with WG1CH9's assessment of this topic and caveat accordingly. (Gabi Hegerl, Duke University)	Accepted. Assessment revised to be consistent with WGI chapter 9.
E-1-799	A	64	33	64	33	Amend to read: ….of the order of 500 km….. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Revised
E-1-800	A	64	34	64	37	This sentence implies that natural internal variability and natural external forcing have not contributed at all to observed changes. This is definitely false for the former, very probably false for the latter. The observed changes are generally most consistent with the combined three sources of variability (natural internal, natural external, anthropogenic external). (Daithi Stone, University of Oxford)	Text rewritten.
E-1-801	A	64	34	64	38	This assessment of attribution on the grid-box scale is stronger than we feel comfortable with in WG1, Ch 9, for a number of reasons (see Comment 3), and I think not necessary to interpret the results of the analysis displayed in Figs 1.8, 1.9 and Table 1.12 (see Comments 4-6). (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Accepted. Assessment revised to be consistent with WGI chapter 9.
E-1-802	A	64	35	64	38	The claim that they cannot be explained by natural internal climate variations or the response to changes in natural external forcing assumes that the models incorporate all the factors that contribute to these variations/forcings. Accordingly, insert "current knowledge regarding " between "by" and "natural" on line 36, and eliminate the last sentence of this para. (Indur Goklany, US Department of the Interior)	Text changed.
E-1-803	A	64	37	64	38	I think this is misinterpreting this result. There are significant changes across a greater than expected number of grid points and the consistency is given by the number of grid boxes significantly different from anthro forced runs being within the range expected by chance. So warming trends are significant and consistent on a larger number of grid boxes than would be expected by chance. This does not	Accepted. Assessment revised to be consistent with WGI chapter 9. However, the global scale result is that other forcings, such as land use change or natural external forcing factors cannot explain the significant warming signal

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						mean that, for a particular grid box, we can make an attribution statement in the way that we can do for global mean and continental scale temperatures. It will be important to cross check with WGI chapter 9's conclusions, which are that anthropogenic influence is now detectable on continental scales. Chapter 9 does not say we can attribute temperatures at grid point scales to anthropogenic forcings. (Peter Stott, Met Office)	found at many individual grid boxes.
E-1-804	A	64	40		43	Nice citation, note that the confidence varies for the cited changes (maybe rephrase to reflect this, but I don't feel strongly about this) (Gabi Hegerl, Duke University)	Thank you.
E-1-805	A	64	41			"increases in sea level": global, but I am pretty sure not the spatial pattern (Daithi Stone, University of Oxford)	Directly from WGI statement.
E-1-806	A	64	45			Sub-section 1.4.2.2 could be shortened. R2 of Fig. 1A is missing (Isabelle Chuine, CNRS)	Shortened and figure changed.
E-1-807	A	64	47	65	2	I think that appropriate wording is used here - discernable (or detectable) - but the final statement that this is what is meant by joint attribution does not jive with the definition of joint attribution on page 8. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Joint attribution used here, but not carried forward to SPM.
E-1-808	A	64	49			I am unaware of research that has shown that they can be combined, only that they have been combined. The combination of the two actually follows a logical fallacy, so I doubt very much that research has shown that it can be done. (Daithi Stone, University of Oxford)	Disagree. Based on global synthesis with spatial pattern analysis, there is no other reasonable explanation.
E-1-809	A	64	50	64	51	"Discernible change in species at a regional scale are associated with regional temperatures". This assessment gets it about right. And some (unkown fraction) of some of those temperatures are probably attributable to anthropogenic forcings. But this does not satisfy joint attribution according to the defintion supplied on page 8 . It indicates a discernible influence of regional temperatures on effects.Attribution of these changes in natural and managed systems to anthropogenic causes requires (cf page 8 lines 41-51) that the observed changes are unlikely to be due entirely to natural internal climate variability or natural variability of the system and that the observed changes are not consistent with alternative physically plausible explanations of the observed changes that exclude anthropogenic climate change. However there is an absence of end to end attribution studies which quantify how much of the variance of the systems is attributable to the anthropogenic forcings. Without this we do not know whether a significant amount of variance of effects explained by temperature and a significant amount of variance of temperature explained by anthropogenic forcings translates into a small and not significant amount of variance of effects being explained by anthropogenic forcings. So we	Text revised to address the points rasied by the reviewer, including discussion of why other influences can be rejected. Text revised to be consistent with WGI assessment.

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						cannot rule out the observed changes being consistent with natural internal climate variability or natural variability of the system. In addition there is no evidence that rules out confounding effects of other prime candidates for processes that could have caused the observed changes such as habitat changes for example; therefore the evidence does not support the observed changes not being consistent with alternative physically plausible explanations that exclude anthropogenic climate change. There is also an important issue of WGII's summary of WGI's conclusions on regional attribution. WGII summarises the WGI conclusions as regional climate trends are attributed to anthropogenic causes which is not a correct statemet of the WGI conclusions. Consistency of language in WGII with WGI will ensure a consistent message of the conclusions of WGI relating to attribution of temperature changes. As part of this it should be noted that WGI concludes that anthropogenic influence has now likely been detected in all continents except Antarctica (which has insufficient observational coverage to make an assessment), and has also been detected in some sub-continental land areas - note on what spatial scales WGI is able to make assessments of this sort and also the likelihood attached to this statement. Presumably the next draft of the WGII report will be made fully consistent with the final draft of the WGI chapter 9 in this regard. (Peter Stott, Met Office)	
E-1-810	A	65	1	65	2	Delete the sentence "This is what we are calling joint attribution" - It has been defined at the start of section 1.4.2. (Heiko Balzter, Centre for Ecology and Hydrology)	Done
E-1-811	A	65	4	65	21	It should be pointed out that the connection of these two attribution statements ignores the uncertainty in downscaling the model simulated temperature changes to the areas that would affect the species being examined (which are I would guess are of spatial extents at which the models have damped variability). All you can say is that you get better agreement, not a “discernible” skill because you do not know if your uncertainty is large enough to make the other options plausible. (Daithi Stone, University of Oxford)	Reject. The reviewer asserts that the variability estimates from the climate model simulations are too small, in part becuase the model resolution is coarser than the scale of the areas that affects the species or systems being considered. This is incorrect. First, comparison of model variability at interannual and decadal timescales at individual grid boxes over land indicates that the models either reliably etsimate or overestimate the internal variability of temperature at these time scales (Karoly and

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							Wu, 2005; Karoly and Stott, 2006). Second, the spatial scale of temperature changes on 30 year time scales or longer is typically several thousand km, much larger than a single model grid cell, (as estimated from the HadCRUT2 data set), so the temperature changes affecting the species are relatively large scale and can be relaibly estimated from model simulations.
E-1-812	A	65	4			Is the reference to Munich Re Group the correct one? (Jean Palutikof, IPCC WGII TSU)	Corrected
E-1-813	A	65	4			Cite the Root et al. study itself. (Nathan Gillett, University of East Anglia)	Corrected
E-1-814	A	65	5	65	5	……changes in spring events for wild animals and plants corresponding with the "a" type of regions as defined in Box 1,1 using 145 ... (Annick Douguédroit, University de Provence)	Suggested box not included in chapter.
E-1-815	A	65	7			Root et al 2003 does not appear in the reference list. (Jean Palutikof, IPCC WGII TSU)	Corrected
E-1-816	A	65	10		21	This paragraph reads a bit confusing to me, not clear which study uses HadCM3 results, while towards end of paragraph also other studies are cited. (Gabi Hegerl, Duke University)	Revised
E-1-817	A	65	17	65	21	This seems a strange selection of variables. Surface temperature would be the obvious variable to start with. (Nathan Gillett, University of East Anglia)	Text rewritten.
E-1-818	A	65	18			Replace 'empirical' with 'observed'. (Nathan Gillett, University of East Anglia)	Text changed/reference removed.
E-1-819	A	65	19	65	19	This is misleading I think. Hansen's arguments about the overall consistency of the evidence of changes in the climate system is not of the same nature as the evidence presented in 1.4 which relies heavily on correlations rather than physical models or understanding like Hansen's arguments do. (Peter Stott, Met Office)	Text revised.
E-1-820	A	65	27		31	Note that Gillett et al only considers anthropogenic forcing, so there is a caveat in his attribution that its unclear how much natural forcing may explain (although I suspect it wouldn't be much). (Gabi Hegerl, Duke University)	Accepted. Text revised.
E-1-821	A	65	35	65	45	This introduction is dry, and could be made a more 'punchy' introduction to this important section. It may also be misleading and is certainly contradictory. (1) On line 38 you say that studies are selected that have a 'statistically significant trend'.	'No change' has been removed. "As described by the authors' added.

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						Then, on lines 44-45 you say that studies are categorized according to whether they change in the expected or unexpected direction, or show 'no change'. Presumably the 'no change' category is empty if you have made the pre-selection indicated on line 38. (2) On line 38 you talk about trends 'related to temperature or other climate change variable'. How do you know this relationship exists? Maybe this should be qualified to read 'which has been related by the authors'. (Jean Palutikof, IPCC WGII TSU)	
E-1-822	A	65	35	66	3	Changes in, for example, hydrology or coastal processes can often have little relation to changes in temperature. So examining the spatial pattern of changes in temperature and the spatial pattern of responses to changes in climate does not provide you with material to link up in your "joint" attribution process. Only responses to changes in temperature could be considered. (Daithi Stone, University of Oxford)	Accepted. Focus has been on changes in temeprature, and those related to temperature imapcts, such as cryosphere melting.
E-1-823	A	65	35	70	18	In the very interesting studies presented here is never mentioned any location, as presented in Box 1,1, which could be once again an help to understand the simultaneous presence of cells without significant system/sector change in cells with positive significant temperature change and to make the difference between cells representing expansion in latitude or altitude ("c" type region), modification of the phenology, particularly spring events ("a" type regions) and retreat ("b" type regions: see Box 1,1). (Annick Douguédroit, University de Provence)	Locations are included in Supplementary Material and shown in figure.
E-1-824	A	65	37	65	40	Is "statistically significant trend" (i.e., tendency over time) really what is meant here? I would worry whether this would be perceived as introducing some kind of selection bias that would artificially favour finding a link between rising temperatures and impacts that are consistent with anthropogenic warming. Wouldn't it be better to use a criterion where studies are selected if there is a significant association between (interannual?) temperature variations and (interannual?) variations in the system or sector? Perhaps this is what is meant here? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Disagree. Studies are selected that show are significant change in the systrem, either related to or not consistent with the response to a regional warming. The statistical assessment then assesses the relationship between the pattern of observed changes and the pattern of warming.
E-1-825	A	65	38	65	38	This is related to my previous comment 51 … the wording here seems awkward - what is a "trend in change"? (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Agreed. Text revised.
E-1-826	A	65	40	65	40	Delete: in, insert: into (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text revised.
E-1-827	A	65	42	65	42	single inverts (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.

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E-1-828	A	65	48			It should be noted that Karoly and Wu (as described) does not comprise an attribution study, because the option of all natural effects (internal and external) have not been examined. Or if they have been examined then this should be noted (I have yet to read the paper). (Daithi Stone, University of Oxford)	Agreed. Text revised.
E-1-829	A	65	49	65	49	Should list the three models here (Peter Stott, Met Office)	Text revised.
E-1-830	A	65	49			"HadCM2" is just one GCM. (Daithi Stone, University of Oxford)	Text revised.
E-1-831	A	65	50	65	51	I think it too strong to use the "attributable" word here because of the concerns that we have about grid point scale detection and attribution discussed in previous comments. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Agreed. Text revised.
E-1-832	A	66	2	66	2	Supp info - Explain why the numbers differ from table 1.12 (Peter Stott, Met Office)	Table corrected and updated.
E-1-833	A	66	5	66	29	This would be so much more convincing if there was any evidence of spatial correlations between effects and the relevant meteorological parameters. One would hope to find correlations between the spatial patterns of spring events and the exceedance of relevant thresholds, but Root et al did not do this, looking instead at mean spring temperatures. (Peter Stott, Met Office)	The statistical testing assesses the spatial match between the patterns of observed significant changes in the systems and the pattern of observed warming. Other studies have shown that the exceedence of a spring temeprature threshold for growth is highly related to the mean spring temeprature eg Christides et al, 2006.
E-1-834	A	66	34	66	34	Missing r squared value (Peter Stott, Met Office)	Figure changed.
E-1-835	A	66	44			Which dataset is meant here? Include a reference. The Hadley Centre data is HadCRUT3, which is from the Hadley Centre and the Climatic Research Unit, but doesn't come from NOAA. (Nathan Gillett, University of East Anglia)	Text and figure revised.
E-1-836	A	67	1	67	51	I think Fig 1.8 needs to be described in substantially more detail, as does the statistical analysis in the supplementary information. My guess is that the coloured cells indicate locations where observed trends in surface air temperature are significant, and that the colours indicate the simulated response to anthropogenic forcing in those cells. WG1, Ch 9 feels that the latter should not be described as attributed changes. We have only quanitified the contribution from external forcing globally, and only say that there is likely a detectable anthropogenic influence on regional (continental and some sub-continental) scales (see Comment 3).	Text and figure revised.

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						(Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-837	A	67	1	67	51	Comment 53 continued ... My guess also is that the chi-square statistics given in the supplementary information measures the spatial association between the sign of the observed temperature trend and the direction of the phenological change, at locations where system data are available and where observed temperature trends were found to be consistent with simulated temperature trends. I tried to reproduce the chi-squared statistic from the first table (line 7, page 4 of the supp. inf.) but without success. I realize that this should be straight forward, so I was likely making some silly mistake - but this indicates that formulae and a more detailed description of what has been done should be included. I agree that monte-carlo assessment of the chi-square statistic is appropriate because outcomes for the experimental subjects being classified (grid squares) are not completely statistically independent of each other. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	More details of the statistical testing are included in the Supplementary Material now. Tables have been updated and corrected.
E-1-838	A	67	1	68	35	The temperatures shown on fig 1.8 are not the temperature warming attributable to anthropogenic forcing. The figure shows detectable observed temperature changes that are consistent with anthropogenically forced runs. There has been no quantification made of the amount of warming attributable to anthropogenic forcings at these grid points. Without this quantification it is not possible to attribute the observed changes in effects to the anthropogenic forcings since we cannot assess the amount of variance of the effects that is caused by the anthropogenic forcings to know whether it is significant or not. (Peter Stott, Met Office)	Agreed. Figure caption corrected.
E-1-839	A	67	1			Figure 1.8: I worry that this figure may be misunderstandable, given that the individual gridbox results are an area of active research at this point, and that its unclear what the physical limits of models are at the gribox scale. I suggest to caveat the map for attributable warming accordingly, saying that continental scale changes can be confidently attributed and that scales below are an area of active research, see ch9WG1. - also. only parts of the warming are attributable, not necessarily all! (Gabi Hegerl, Duke University)	Agreed. Figure caption corrected.
E-1-840	A	67	46			Figure 1.8 - figures are too small, put only 2 to a page (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Figure removed, and replaced by single panel figure.
E-1-841	A	67	46			“Regional temperature warming” or “regional temperature changes”? (Daithi Stone, University of Oxford)	Figure caption and descritpion corrected.
E-1-842	A	68	1	68	36	I suspect that it would be sufficient to include only Fig 1.9 to support the analysis of spatial association between the direction of temperature change and the direction	Agreed. Figure 1.8 removed and only single panel figure retained.

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						of phenological change. It gives visual prominence to observed temperature trends (which would be expected to have a stronger association with phenological change than model simulated trends) while still indicating where model simulated trends are consistent with observations. Discussion of the map should include cautions as suggested in comment 6. A particular concern is that readers should be advised not to read too much into concurrence between observed temperature change, simulated temperature change, and phenological change at individual locations. The message is in the broad patterns - as is also assessed by Karoly and Wu through their use of the Livzey and Chen field significance criterion. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	
E-1-843	A	68	30			Now I see the point of figure 1.9 (and figure 1.8). It is not intended to show the documented changes, which I originally thought. Rather it shows the sites and areas of observations and only in Table 1.12 the information on change in expected direction, etc is provided. This is clear now, and it is an important part of the chapter. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	Thank you. No change required.
E-1-844	A	68	31	68	31	Insert degree symbol correctly (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done
E-1-845	A	68	32			It appears that some are indicated that are significantly smaller too. (Daithi Stone, University of Oxford)	Corrected.
E-1-846	A	68	32			Do you meant he 5% level? (Daithi Stone, University of Oxford)	Corrected.
E-1-847	A	68	38	68	44	This seems a small amount of analysis to devote to this principal synthesis figure. Also many regions have no significant temperature response, but many significant impact responses. This should be commented on. Is this due to publication bias, or do the authors think that the climate impacts have a higher signal-to-noise in their response to anthropogenic forcing than surface temperature? (Nathan Gillett, University of East Anglia)	Accepted. Figure and description revised to note the points raised in the comment.
E-1-848	A	68				Changes in, for example, hydrology or coastal processes can often have little relation to changes in temperature. So examining the spatial pattern of changes in temperature and the spatial pattern of responses to changes in climate does not provide you with material to link up in your "joint" attribution process. Only responses to changes in temperature could be considered. It is inappropriate to include responses in hydrology on a map showing temperature changes. There is no obvious reason why the two should be related, and in fact in climate models they are generally not related. (Daithi Stone, University of Oxford)	Rejected. Some hydrological changes, such as early snow melt, are due to temperature changes. Same for sea level rise. Only significant changes related to warming are shown in the figure.

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E-1-849	A	69	2			Table 1.12 : legend is not clear enough. What is meant by "consistent with warming" ? would not "consistent with climate change" be more appropriate? Some changes can be indeed consistent with a cooling. How can there be cells with significant change consistent with warming when these cells have no temperature data? How do we know this is consitent with a warmign then? some more explanations or corrections are required. (Isabelle Chuine, CNRS)	Table caption revised. Studies reported consistency with warming based on analysis in that study, not necessarily using the global temperature dataset used in this analysis.
E-1-850	A	69	9	69	9	The "Total" box in the second column (under "Cells with significant system/sector change consistent with warming") contains the number 83. However, the total of the cells in the second column sum to 60, not 83. Correct the value in the "Total" box if needed. (Sarah Shafer, U.S. Geological Survey)	Agreed. Errors corrected.
E-1-851	A	69	9	69	10	Talbe 1.12a heading "consistent with warming" should this be warming or cooling since you also have cells with significant negative temperature change to deal with as well as the cells without significant temperature change which could be warming or cooling (Peter Stott, Met Office)	Agreed. Corrected.
E-1-852	A	70	23			Please repeat the question here. (Albert Klein Tank, Royal Netherlands Meteorological Institute)	The questions have been removed from section 1.1, so this section has been rewritten.
E-1-853	A	71	5	71	5	Insert "that" after diseases (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Disagree.
E-1-854	A	71	7	71	9	Where is the evidence for the 'intensification of the hydrological cycle' and increase in floods and tropical cyclones? I think this is overstating the case compared to the conclusions of WG 1. (Nathan Gillett, University of East Anglia)	Intensification of hydrological cycle removed from text.
E-1-855	A	71	7	71	8	Delete: there is spread of ; insert: have spread after vectors; delete "the"; insert: a resulting after and; delete "the"; delete: of an expected, insert: for an (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Edited.
E-1-856	A	71	9			WG1 Chapter 9 says that changes in tropical cyclones cannot yet be detected, let alone attributed. (Daithi Stone, University of Oxford)	Sentence deleted.
E-1-857	A	71	12	71	13	proposal: not all geographical regions even if there is evidence of observed changes in every continent including Antartica . They are spread according a common space distribution into 3 types of regions in the extra-tropical zones, particularly in the Northern hemisphere concerned by most of the studies, and in the high altitudes: expansion towards north or high altitude, maintenance of the areas in temperate latitudes or middle altitudes with modifications related mainly with spring events	Need published reference to respond to this comment.

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						and finally a few cases of endangered species for which warning has reached locations beyong the limit of the acceptable temperature (see Box 1,1). (Annick Douguédroit, University de Provence)	
E-1-858	A	71	14	71	15	Delete: of studies provide evident of; insert: come from studies after vectors; delete: comes, insert derives (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.
E-1-859	A	71	15			The majority of the studies are based on high altitude regions? I didn't get this impression from the rest of the chapter. (Nathan Gillett, University of East Anglia)	This is an assessment statement.
E-1-860	A	71	20	71	20	Delete: the greatest, insert: most pronounced (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Done.
E-1-861	A	71	22			reduced outdoor and tourism acitivities'. Are these covered in the rest of the chapter? I don't remember reading about them. (Nathan Gillett, University of East Anglia)	Now covered in 1.3.1, so OK to recap here.
E-1-862	A	71	24	71	25	"In regard to sea level rise, there are only a ..." to "In regard to sea level rise, only a ... (Pedro Ribera, Universidad Pablo de Olavide)	Sentence deleted.
E-1-863	A	71	25	71	25	Amend to read: …..only a few documented isolated historic cases of island abandonment….. (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Sentence deleted.
E-1-864	A	71	27			climate induced environmental changes'. Does this mean only sea level rise? If so, just say 'climate-induced sea level rise'. (Nathan Gillett, University of East Anglia)	Sentence deleted.
E-1-865	A	71	29	71	31	This paragraph seems to be based on a very narrow view of adaptation. Rewrite the first sentence as follows: " ALTHOUGH there is little documented evidence of adaptation in the CLIMATE CHANGE LITERATURE to regional climate trends in agriculture and forestry, IT SHOULD BE NOTED THAT AGRICULTURE AND FORESTRY ARE INHERENTLY EXERCISES IN ADAPTIVE MANAGEMENT. FOR INSTANCE, DURING THE MEDIEVAL WARM PERIOD VINEYARDS SPRANG UP IN BRITAIN, BUT THEN WERE ABANDONED WHEN COOLER TEMPERATURES PREVAILED. SUCH ADAPTATIONS ARE OFTEN OVERLOOKED BECAUSE THEY ARE INCREMENTAL AND AUTONOMOUS. THE CLIMATE CHANGE COMMUNITY COULD LEARN A LOT BY EXAMINING ADAPTIVE RESPONSES THAT HAVE BEEN UNDERTAKEN OVER THE AGES IN RESPONSE TO CLIMATE IN GENERAL AND TO CLIMATE VARIABILITY IN PARTICULAR (GOKLANY 1995)." [NOTE: New language is in UPPER	Added 'highly managed' to text.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						CASE; deletions are not shown] (Indur Goklany, US Department of the Interior)	
E-1-866	A	71	31			The sentence: "There is even less evidence related to food supply" is incorrect. In fact, there is plenty of evidence of human adaptation to cope with disruptions in food supply extending back millennia. Examples of adaptation measures that have been implemented with some degree of success over the ages include granaries, irrigation and other water management systems, trade in food and crops, aid (e.g., the World Food Program, soup kitchens, subsidized bread), research in and development of different crop varieties, etc. Accordingl;y, replace this sentence with the following: "There is a rich history of adaptations designed to augment and/or stabilize food supplies despite changes in climate, climate variability, or other fcators affecting resource productivity. Such adaptations include the building of granaries, irrigation and other water management systems, trade in food and crops, provision of aid, research and development of different crop varieties such as those that led to the Green Revolution, aquaculture, etc. Adaptation specifically to anthropogenic climate change can be advanced through the study of such adaptations." (Indur Goklany, US Department of the Interior)	Sentence removed.
E-1-867	A	71	33	71	33	Insert: although after :; insert well before documented (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Addressed.
E-1-868	A	71	34	71	36	Replace the sentence beginning with "Vulnerability" on line 34 with the following: "The global decline in aggregate deaths and death rates due to extreme weather events during the 20th century suggest that adaptation measures to cope with some of the worst consequences of such events have been successful. However, the 2003 European heat wave and the 2005 hurricane season in the North Atlantic show that despite possessing considerable adaptive capacity, even the developed nations are vulnerable if they do not mobilize adaptation measures in a timely and efficient manner." (Indur Goklany, US Department of the Interior)	Added sentence.
E-1-869	A	71	36	71	37	The text implies that increased use of air-conditioning is purely a response to warming. Surely use of air conditioning would have increased in the absence of climate change due to incresased wealth, improved technology etc. (Nathan Gillett, University of East Anglia)	Addressed.
E-1-870	A	71	41	71	44	I reiterate that I think this overstates the strength of the evidence a bit. I don't think we can attribute cause according to the definitions given. However, there probably is a basis for a "discernable influence" statement on large scales. (Francis Zwiers, Canadian Centre for Climate Modelling and Analysis)	Text changed to 'discernible anthropogenic influence.'

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
E-1-871	A	71	41	71	44	First sentence important: should be in conclusion (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text rewritten.
E-1-872	A	71	48	71	48	Amend first in to read are (Philip Reid, Sir Alister Hardy Foundation for Ocean Science)	Text rewritten.
E-1-873	A	72	1	103		References: the list of references is still too long and can be curtailed by deleting several references of minor importance. Perhaps a major reference in one area should be cited while other references listed in the major reference may be identified for general readers of this Document. The main purpose of this chapter and the entire WGII Document is to present evidence for climate change and discuss its possible cause and impacts. This can be done by citing important references only. (Madhav Khandekar, Retired)	Task of chapter is to review published literature on observed changes.
E-1-874	A	79	2	79	2	Add: Domack, E., D. Duran, A. Leventer, S. Ishman, S. Doane, et al., 2005, Stability of the Larsen B ice shelf on the Antarctic Peninsula during the Holocene epoch, Nature, 436(7051), 681-685. (Claire Parkinson, NASA Goddard Space Flight Center)	Relevant for WGI.
E-1-875	A	79	16	79	16	.Add: Douguédroit, A. 2006: Le climat comme « état du système climatique » :Apports à la géographie du nouveau paradigme in Actes du Colloque Géopoint 2006: Demain la géographie.Permanences, dynamiques, mutations. P. Martin ed, in press. (Annick Douguédroit, University de Provence)	Please send reference to B. Seguin, LA from France.
E-1-876	A	80	7	80	8	Some details for the Emberlin et al. (2002) reference are missing (see line 8). The issue number is (4) and the last page number is 170. (Paul Beggs, Macquarie University)	Not clear. Is reference 2003?
E-1-877	A	82	13	82	13	Misspelling: The first initial of the author's name is not "J," it is "P": Gonzalez, P. 2001: Desertification and a shift of forest species in the West African Sahel. Climate Research 17: 217-228. [African; desertification]. (Patrick Gonzalez, The Nature Conservancy)	Done.
E-1-878	A	84	0			Add the reference to WHO report on Climate change and human health (2004) for discussion on effects of heat waves on human health. (Giampiero Maracchi, Institute of Biometeorology)	See Chapter 8.
E-1-879	A	90	32	90	33	This article did not show the published journal. (Xiaoqiu Chen, Peking University)	Done.
E-1-880	A	93	1	93	3	Names on the reference are screwed up. The reference should be as follows: Groisman, P.Ya., R.W. Knight, T.R. Karl, D.R. Easterling, B.Sun, and J.M.Lawrimore, 2004: Contemporary Changes of the Hydrological Cycle over the Contiguous United States: Trends Derived from In-Situ Observations, J.	Done.

Chapter-Comment	Batch	From Page	From Line	To Page	To line	Comments	Notes of the writing team
						Hydrometeorol. ,5, 64-85. (Pavel Groisman, University Corp. for Atmospheric Research)	
E-1-881	A	93	4	93	4	Add: Parkinson, C. L., and D. J. Cavalieri, 2002: A 21 year record of Arctic sea-ice extents and their regional, seasonal and monthly variability and trends, Annals of Glaciology, 34, 441-446. (Claire Parkinson, NASA Goddard Space Flight Center)	WGI.
E-1-882	A	95	44	95	44	The citation for Romanovsky et al. is incorrect, i.e. Burgess is missing from list of authors. It should be Romanovsky, V., Burgess, M., Smith, S., Yoshikawa, K., and Brown, J. 2002…….. (Sharon Smith, Natural Resources Canada)	Done.
E-1-883	A	101	13	101	13	Add: Vincent, W. F., J. A. E. Gibson, and M. O. Jeffries, 2001: Ice-shelf collapse, climate change, and habitat loss in the Canadian high Arctic, Polar Record, 37(201), 133-142. (Claire Parkinson, NASA Goddard Space Flight Center)	Added.
E-1-884	A	101	31	101	31	"(Europe, plants)" shoud read "(Global, plants)" (Gian-Reto Walther, Institute of Geobotany, University of Hannover)	Done.
E-1-885	A	103	47	103	47	Add: Zwally, H. J., J. C. Comiso, C. L. Parkinson, D. J. Cavalieri, and P. Gloersen, 2002: Variability of Antarctic sea ice 1979-1998, Journal of Geophysical Research, 107(C5), 10.1029/2000JC000733. (Claire Parkinson, NASA Goddard Space Flight Center)	WGI.