IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Chapter 11

The following compilation of review comments and author responses is supplied by the Working Group I Technical Support Unit as a record of the process used to prepare the Working Group I report. These comments and responses are not to be edited and/ or re-distributed in part or in full to others.

Please note that under IPCC procedures authors are required to take account of all substantive review comments in both review rounds. Thus responses to individual comments may be influenced by comments from other reviewers.

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11-1	A	0:0		In the first portion of this chapter provides a very detailed discussion of the methods used to downscale information from global climate models to various areas of the globe including regional climate models (RCMs) and statistical downscaling (SD). However, much of the information that is presented, especially the figures, is based on averaging the global models over a given region. This seems to defeat the purpose of this chapter (although it may have been necessary since the AR4 output only recently became available and time was not sufficient to downscale the information from the global model results). Thus, either the discussion of the downscaling methods can be reduced and/or the global model averages could be presented in chapter 10. [Michael Alexander Alexander]	Noted and accepted; text changed (CLA's)
11-2	A	0:0		Excellent chapter. I am impressed at the progress made in recent years in regional climate modeling and the ability to simulate present regional climates with ensembles of global climate models. [Richard Anthes]	Noted
11-3	A	0:0		The overall discusssion is well organized and evidences of the matter generally well described and documented. References are generally consistent, but some should be added and discusse din the text. The only negative oint is the length of the chapter, considering also that a very small font size has been used, it is way too long. May be some well organized summary tables can be arranged with main findings for each region. [Marina Baldi]	Noted: summary table will be developed and text adjusted as needed.
11-4	A	0:0		Citation in the text NOT properly reported. In most of the cases years are missing. [Marina Baldi]	References will be corrected
11-5	A	0:0		Chapter 11 reads well from the standpoint of clarity but it's too long. There are several issues redundantly presented, like the comparison between statistical and dynamical downscaling methodologies. [Roxana Bojariu]	Editorial homogenization and shortening will be addressed.
11-6	A	0:0		This chapter offers an impressive coverage of the literature concerning regional climate projections. The breakdown of the discussion into major continental regions of the world is an enormously welcome advance compared to the TAR, as it will enable readers more readily to locate the latest information from their own regions. It also provides a natural stepping stone into WG II, especially the regional chapters which are organised according to the same regional divisions. [Timothy Carter]	Noted.
11-7	A	0:0		The chapter is currently very long. However, the authors should be allowed some latitude here, if regional statements are to be presented because they are very important. Cuts can nevertheless be made in the regional descriptions, and although it is necessary to provide	Noted: Editorial changes made. Summary tables developed.

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				supporting evidence to justify the headline statements about future projections, this needs to be streamlined. Perhaps use of supporting tables with numbered references, could save some space. [Timothy Carter]	
11-8	A	0:0		There is currently no conclusions section. This is needed to remind the reader of the major advances since the TAR in general terms, to describe the key gaps in knowledge and to identify future research needs. [Timothy Carter]	No conclusion section will be included, need met by executive summary.
11-9	A	0:0		TSU NOTE: Please see supplementary review material [Daniel Caya]	LA's taken this into account in editorial actions.
11-10	A	0:0		There is insufficient material (paractically none) on the regional distribution of sea level rise and the frequency/intensity of extreme events. [John Church]	Yes. Information added where available (LA's) – see also coastal box 11.4. (RJ)
11-11	A	0:0		The regional sections are overall a nice idea what are currently unbalanced in terms of length and topics treated. The North Americal section appears to be especially excessive in length [Robert E. Dickinson]	Editorial action taken (CLAs).
11-12	A	0:0		Well describes and documented, which helps to better understand global and regional climate projections. However, assumptions and scenarios should be specified clearly for each region. The uncertainties section should guide experimental observation set-up so that the projections could be validated of at least their limitations defined. [Savitri GARIVAIT]	Context statements included where appropriate. Else, beyond scope of this chapter. (LAs for each section)
11-13	A	0:0		Although the chapter is quite conclusive about the impact of climate change on snow cover and snow water, its message could be more forcefully communicated with a figure showing the change in snow water equivalent (which is what matters most for water resources) for at least one region. [Steven Ghan]	Ch 10 has projected change for northern hemisphere (fig 10.3.13). A figure for N.America may be added, space allowing.
11-14	A	0:0		The latter half of this Chapter contains much information about regional climate, but it is not tabulated or summarised, and is used only to relate to the models. The Chapter shoild begin with a comprehensive summary and tabulation of regional climate, before bringing in the models [Vincent Gray]	Do not agree on begining with such a table, but some summary material added in a table.
11-15	A	0:0		There is no attempt to check any of the projections in the early part against what actually happens in the regional climate. All the many claims of what "is likely" have no scientific basis and I have attempted to change all of them into more qualitative terms.Also simulations are not "experiments" [Vincent Gray]	Noted: addressed through a new figure extending the 20thC time series plots. Disagree that simulations are not experiments. (RJ/IH)

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11-16	A	0:0		The discussions of empirical and dynamical downscaling and their applications are scattered broadly throughout the chapter, sometimes in sections as short as a few sentences. Some re-arrangement to centralise these discussions would improve the cohesiveness of the chapter. [Katharine Hayhoe]	Editorial action taken (CLAs), but some scatter needed as methods can be / are regionally dependent.
11-17	A	0:0		TSU NOTE: Please see supplementary review material [Katherine Hayhoe]	Noted.
11-18	A	0:0		I feel strange that there is no representation on "soot" which can give tremendous effect on regional climate. [Kiminori Itoh]	Noted: some additional discussion added (LAs and CLAs)
11-19	A	0:0		The chapter is written very nicely, taking into account most of the important achievments during tha last years. However the single paragrapgh of the individual regions are unbalances in length and content provided. A harmonizing would be needed. A chapter on changes in the regional hydrological cycle is missing. Water availability is an important issue. There are some important references missing: van den Hurk, B., Hirschi, M., Schär, C., Lenderink, G., van Meijgaard, E., van Ulden, A., Rockel, B., Hagemann, S., Graham, P., Kjellström, E., and Jones, R.: 2004, ¡ÆSoil control on runoff response to climate change in regional climate model simulations¡Ç, J. Climate 18, 3536-3551. Gaertner, M.A., Christensen, O.B., Prego, J.A., Polcher, J., Gallardo, C., and de Castro, M.: 2001: ¡ÆThe impact of deforestation on the hydrologic cycle in the western Mediterranean: An ensemble study with two regional models¡Ç, Clim. Dyn. 17, 857-873. Jones, R.G., Noguer, M., Hassell, D.C., Hudson, D., Wilson, S.S., Jenkins, G.J., and Mitchell, J.F.B.: 2004, ¡ÆGenerating high resolution climate change scenarios using PRECIS¡Ç, Met Office Hadley Centre, Exeter, UK, p. 35. Hagemann , S.; Jacob, D.(2005): Gradient in the climate change signal of European dis-charge predicted by a multi-model ensemble, Climate Change, Pru-dence Special Issue, submitted S.Pfeifer, D. Jacob, 2005: Changes of the Arctic Climate under the SRES B2 scenario con-ditions. Meteorologische Zeitschrift, Vol. 14, No 6, December 2005.include Pfeifer et al p85 l53 Jacob, D., Goettel H., Jungclaus. J., Muskulus, M., Podzun, R. Marotzke, J (2005): Slow-down of the thermohaline circulation causes enhanced maritime cli-mate influence and snow cover over Europe, Geophysical Research Letters, accepted, Nov 05 Arpe, K., Hagemann, S., Jacob, D. Roeckner, E. (2005): The realism of the ECHAM5	Noted, editorial action taken (CLAs). Hydrological concern noted; cross references to Fig 10.3.9 included and ...? (response pending discussions with Ch 10)

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				models to simulate the hydrological cycle in the Artic and North European area, Nordic Hydrology, 36 (4), accepted [Daniela Jacob]	
11-20	A	0:0		A very interesting Chapter that people will delve into for regional information but needs to be much more concise and can be trimmed considerably - also see whether links made with individual regional chapters in WG II can help utilise limited space more efficiently by making information complementary rather than repetitive [Roger Jones]	Editorial action taken (CLAs).
11-21	A	0:0		(quite several things, but time runs out; anyway, the product looks already good and will be good.) [Gottfried Kirchengast]	Noted
11-22	A	0:0		The discussion of possible future regional changes in TC's is rather inconsistent across the regions. See for example, p. 11-67, lines 1-3 versus 11-54, lines 39-50; 11-71, lines 14-24; 11-78, lines 37-45; 11-93, lines 45-55; and 11-94, lines 1-9. Perhaps some effort could go into making these assessment more consistent with each other. [Thomas Knutson]	Editorial action taken (CLAs, Penny)
11-23	A	0:0		Our paper (Knutson and Tuleya 2004) is quoted in some sections. These are idealized results, relating SST warming and simulated CO2-induced changes in atmospheric temperature and moisture profiles to the behavior of hurricane intensities and precipitation. Possible changes in dynamical influences such as vertical wind shear are neglected. Such influences could prove to be quite important, especially as one considers regional-specific future changes in tropical cyclone activity as opposed to more vague statements about future tropical cyclone intensity behavior in "some regions". Region-specific projections have much more uncertainty than the latter due to the fact that there is uncertainty in the future evolution of certain climate features that have known impacts on regional tropical cyclone activity. Specifically, how will ENSO change in the future, and will the future mean state of the tropical Pacific become more or less El Nino-like? As another example, will future anthropogenic changes in the Atlantic project onto the Atlantic Multi-decadal Oscillation patterns (either positive or negative phase?) or onto the NAO? Until we have more confident answers to these types of questions, our confidence in regional-specific tropical cyclones projections will be limited. Areas where we have the most confidence are that tropical SSTs will very likely warm, that the tropical upper troposphere is likely to warm more than the surface, that the tropical atmosphere will very likely contain more water vapor, and that the thermodynamic conditions will likely support tropical cyclones of greater intensity. The latter effect (which we model in the Knutson and Tuleya study and is supported by potential intensity theory), we expect to be modulated to some degree regionally by various dynamical influences as well as regional	Some text added to cover issues of TC uncertainty. See also new extremes table, and comment passed to Ch 10. Consistency ensured (CLAs, Isaac, Penny)

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				differences in the SST warming itself. [Thomas Knutson]	
11-24	A	0:0		Opening Comment: In the Chapters that I am reviewing, I choose to not provide an anonomous review. This choice allows the various Chapter authors to contact me directly on matters of errors, concepts, or questions of disagreement. I have already performed thorough reviews of chapters 1-5. Due to the looming November 4th deadline for reviews, I am choosing to review Chapters 6-11 in a drastically shortened way . Rather than going through all of them as I did before, I am choosing to review only the Executive Summaries of chapters 6-11. There are some clear advantages for this strategy, independent of the obvious one of speeding up the very tedious reading and reviewing process. In the previous chapters I have reviewed, I have seen some significant disconnects between two obviously differering reporting strategies. First, it seems obvious to me that the fundamental purpose of these IPCC FAR reviews is to establish the case, or lack therof, for many of the diverse aspects of the human-caused global warming problem. Second, it is noteworthy that this draft WG1 report is roughly twice as long as the WG1 IPCC TAR report. Third, it seems very obvious that the key IPCC assessment-relevant punchlines are hardly double those of IPCC TAR. It seems clear to me that the global-warming research-advancement doubling time scale is a lot closer to twenty years than it is to five years. The obvious conclusion for me is that we don't really need or desire to double the length of the WG1 chapter assessment every five years! For these nearly obvious reasons, and to help me and the other reviewers refocus on the fundamentally important conclusions that are centrally relevant to the IPCC's human-caused climate assessment's goals, I am thus choosing to reduce drastically my own submitted WG1 reviews. And, most importantly, this gives me a good shot at reviewing meaningfully all of remaining chapters 6-11 by the daunting November 4th reviewers' deadline. [Jerry Mahlman]	Noted.
11-25	A	0:0		GENERAL COMMENTS ON CHAPTER 11: REGIONAL CLIMATE PROJECTIONS This chapter could possibly profit from a direct communication with the "back half" of chapter 3: Observations: Surface and Atmospheric Change. They focus on the regional scale "trends", but the attention is paid almost exclusively on the so-called "modes of variability" of the climate system as a sort of substitute for the far more challenging detection of "regional trends due to anthropogenic forcing of the climate" problem. Can this chapter make genuine progress on this "real regional trends problem"? If so, this would provide a truly meaningful advance toward getting control of the regional trends "detection and attribution" problem. This chapter is 99 pages long, or 205 pages really long, depending upon how one counts references and figures. By any count, this is incontestably the longest chapter of the Working Group 1 Assessment Reports.	Editorial action taken (CLAs). Improved cross chapter references added.

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				Considering that the chapter's Executive Summary is the shortest of any of the previous ten chapters, it would seem that some serious rebalancing of chapter length and punchlines would be highly appropriate. I strongly recommend that you consider this, simply because the excessive length acts to discourage the interested reader from working through the many substantial and compelling points of this chapter. [Jerry Mahlman]	
11-26	A	0:0		FINAL COMMENT. It was a privilege to read and review this chapter 11. Clearly, the future of our responses to human-caused climate change, whether they are sharp reductions of emissions of greenhouse gases, or a stronger focus on coping/adaptation to regional climate changes, we must be very aware that the points of vulnerability to a "rapidly" changing earth's climate, the poorer countries that lack social resilience will suffer far greater local-regional challenges than will we "rich countries". [Jerry Mahlman]	Thanks
11-27	A	0:0		I would suggest that more be done to clarify throughout the chapter which statements are based on some emission scenario and which are scenario independent. [Martin Manning]	Noted; some discussion around this will be included
11-28	A	0:0		I estimate the chapter is about 48% over the target length at present. My suggestions for trying to crunch the chapter down would be: (1) maximize coverage of all the generic aspects of regional climate change in section 11.3.1 and simply refer to them in the rest of the section rather than repeating them. (2) consistent with that strengthen the focus on processes, particularly those that become dominant at the regional scale, e.g. orographic control of precipitation. (3) Shorten the introductory pieces of text everywhere and cut to the key points more quickly. (4) Drop discussion of individual projects and reduce discussion of how results were obtained in favour of using precious space to summarize the results themselves. (5) Consider making more use of tables to collect and compare information from different regions. [Martin Manning]	Yes. Reduced, and some structural changes made.
11-29	A	0:0		There seems to be some inconsistency in the use of acronyms to describe different regions. A simple table near the top of section 11.3 could summarize these. [Martin Manning]	Taken into considetration in the chapter changes.
11-30	A	0:0		TSU NOTE: Please see supplementary review material [Koki Maruyama]	Noted.
11-31	A	0:0		Although the Chapter 11 Figures are generally much better than those of Chapter 1, which were extremely poor, they should still be improved (particularly the axis labels). [Lourdes Maurice]	Figures were early versions only. Quality improved.
11-32	A	0:0		TSU NOTE: Please see supplementary review material [Aurel Moise]	Noted.

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11-33	A	0:0		Most of my comments on this chapter relate to its overall structure, and not to specific technical points. Chapter 11 is at least twice, and perhaps 3 times or more, the length of the corresponding chapter in the TAR. It is also very hard to read in many places. A lot more effort needs to go into actually synthesising the numerous research findings instead of just reporting them, as: study 1 found this, study 2 found that, etc, etc. For example, in the regional projections (11.3), the subsection on the various climate elements should start with a brief summary of the direction and amplitude of change. The Europe section (11.3.3) is one of the better examples of this. [A. Brett Mullan]	Structural changes have been made.
11-34	A	0:0		The chapter was obviously compiled from written comments by many different authors, and more effort needs to go into imposing more uniformity on the layout as well as style. A particularly clear case occurs with section 11.3.5 on North America. All the other regional subsections consider climate projections by climate element – ie, temperature, precipitation, circulation aspects, etc. Section 11.3.5 divides the analysis according to type of model – ie, CGCM, AGCM, statistical downscaling, etc. This must be made consistent. [A. Brett Mullan]	Sections have been homogenized.
11-35	A	0:0		Each regional subsection of chapter 11.3 starts with at least a page, and often more, on "key processes". This seems rather excessive, since most of the understanding is actually not new. I would recommend no more than a half-page with reference to relevant review articles for more information. [A. Brett Mullan]	Noted.
11-36	A	0:0		Advances since the TAR in understanding and predicting regional changes are rightly emphasised, but in some places the progress is presented with an unrealistically optimistic spin and the limits and caveats underlying some of the new material are not sufficiently well brought out. [James Murphy]	Noted: editorial action taken (All)
11-37	A	0:0		The quality of the presentation varies substantially within the chapter. The writing is clear and focused in some parts, but somewhat verbose and ambiguous in others. Many references are missing. Acronyms are not always defined prior to first use, and are not always used consistently (e.g. AOGCM, CGCM) [James Murphy]	Checked with TSU and editorially applied (CLAs)
11-38	A	0:0		Wind circulation changes are not addressed at this summary level, but they are important in areas where the circulation interacts significantly with topography, such as NZ, mid-latitude South America, the western USA, etc. Can circulation changes (especially mid-latitude westerlies) be brought out more clearly, in the overall summary, and in the later regional sections? Putting this in the context of likely changes in teleconnection patterns and associated regional circulation changes would be useful, and would tie in well with	Some discssion is present in text. Additional material included in the new extremes table.

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				Chapter 3. The first paragraph of 11.1.3 is a step in this direction. [James Renwick]	
11-39	A	0:0		Many references to the TAR are made. They should all be written "the TAR" rather than just "TAR". [James Renwick]	Noted.
11-40	A	0:0		Chapter 11 is long, and the style/quality varies between sections/regions. The Asian section is perhaps the most in need of trimming. Given the major uncertainties still inherent in model results, at least at the regional scale, a cautious tone should be adopted in most places. [James Renwick]	Chapter has been reduced and restructures. Statements have been re-evaluated.
11-41	A	0:0		I'm repeating here the comment for Chapter 10, since it is equally as applicable. Relating increased precipitation to wetter conditions in a warming climate is not justifiable, and there are many regional examples, including those at high latitudes, in which the soil moisture dries out due to increased evapotranspiration regardless of the precipitation increase. Why the soil moisture values from the models were not used to address this question directly, regardless of the uncertainties, is a mystery. [This occurs so often in the chapter that specific comments to it are not included here but I'll just give one example - p. 11-50, line 52.] [David Rind]	Source data / maps revisited and text adjusted. (Isaac and all)
11-42	A	0:0		I'll also repeat the comment that while Chapter 10 concudes that over most of the globe tropical storms decrease, the individual region discussions here, whenever they mention the topic, forecast increases (e.g., p. 50, lines 40-41). For such an important topic, the discussion in both chapters is handled in a pretty cavalier fashion. Better to say it's uncertain than to quote one particular study and make it appear as if that is definitive. [David Rind]	Noted; adjustments made in light of the new extremes table. (CLAs + ??)
11-43	A	0:0		There is a general theme running through this chapter than increasing (air) temperature is associated with increasing potential evaporation. This is too simplistic - potential evaporation is not very sensitive to an increase in the average temperature of air near the surface (see p. 187 of Monteith & Unsworth 1990 Principles of Environmental Physics). Also see Rosenberg et al. (1989 Agricultural and Forest Meteorology 47, 303-320.) The same criticism applies to many other parts of the chapter, and especially Chapter 3 - see comment 21. [Michael Roderick]	Do not understand what the "general theme is". Text adjustments made in the one place relevant.
11-44	A	0:0		As a courtesy to readers, further homogenisation of the structure and style across the regional subchapters is recommended. Especially the North American and Asian subchapter seems rather long. The discussion of Key Processes seems at times rather "text book" -style, or is not referred back to later in the respective sub-chapter. In both cases,	Length will be corrected, On the small islands issue, emphases changed.

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				one should consider deleting it. Alternatively, to some extent, Key Processes could be discussed under "Uncertainties" in respective regional subchapter or made into a general fact-box spanning all the regions. This would reduced duplication. The discussion on Climate Trends/Sea level rise/Tropical Cyclones (11.3.9.4-6) in Small Islands -section seems to belong elsewhere in AR4, where observations, detection, sea level and climate processes are discussed. [Markku Rummukainen]	
11-45	A	0:0		It would promote the aim of providing an Assessment if the discussion of each topic were started by discussing the issue in question, to be followed by references to individual models/papers/findings. Presently, in a number places the opposite is presently true and the discussion is structured in terms of specific papers (e.g. much if the Asian subchapter) possibly topped by a more general conclusion. Lengthy discussions on detailed results based on single models/simulations should be avoided. As an example, please consider page 11-35, lines 23-28. [Markku Rummukainen]	CLAs: note the paper by paper issue.
11-46	A	0:0		Some further homogenisations of frequently used concepts would be useful (e.g., avoid using all of GCM, OAGCM, CGCM; what is actually meant by "AR4 GCMs" (in the North American subchapter a set of 18 models is meant whereas elsewhere, e.g. Fig. 11.3.4.8, there are 20 models.) PCMDI and 20C3 seem used for the same thing. [Markku Rummukainen]	Will be corrected
11-47	A	0:0		Probably the figures will be further homogenised. Ideally, when showing AR4-GCM results per region, the same regional division as in Figures 11.2.2 and 11.3.1.1 would be used. These two figures, by the way, duplicate the regional division and could be merged. The colour scale used to indicate incr/decr Precipitation varies between figures (e.g. compare 11.3.2.4 with many of the other figures). [Markku Rummukainen]	Figures have been changed.
11-48	A	0:0		General comment: I have reviewed the ZOD draft of this chapter. The authors have done a great job and the chapter is much improved. Overall it is well on track and I do not see a single real serious issue. [Christoph Schar]	Thanks
11-49	A	0:0		Comment to EDITOR: Following my review of the ZOD, I find that I have been upgraded to an author (which I appreciate). However, the editor should be aware of that I do not count as an independent reviewer of this chapter. [Christoph Schar]	Noted.
11-50	A	0:0		This is the first Assessment Report wherein statements can be made about likely regional changes in climate with some confidence. The probabilistic framework being developed for AR4 permits this very important step in quatifying uncertainties and also provides	More explicit cross chapter references included to allay this problem. Introduction adapted to include a gloabl

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				insight into the mechanisms resulting in coherent regional shifts/changes. I read this chapter first, as regional scenarios are my primary interest (I expect many readers of the final product will do as I did). I found, however, that I did not have enough information - how do the regional scenarios fit within the global context? Are there large scale patterns of change within which a particular region is influenced? Going back and reading about the global scenarios - and then further back to see the model evaluations was critical to understanding the regional scenarios chapter. Perhaps Chapter 11 cannot be expected to stand on its own - but it is very likely that many will read it and go no further. If there is a way to incorporate the global context for each region it would be helpful. A possibility would be to include a (brief) summary of global projections and refer to chapter 10 figures (in particular 10.3.5 and 10.3.6) at the beginning of chapter 11. [Anji Seth]	framework of regional issues. (CLAs)
11-51	A	0:0		The discussions of uncertainty for the regions are not as quantitative as I would have expected, given the AR4 ensemble, and for Europe the PRUDENCE results. Why? [Anji Seth]	New material added ... see especially box 11.1
11-52	A	0:0		The emphasis on the A1B scenario in the regional projections is never clearly rationalized up front. It would help to have some mention (at the beginning of 11.3) of why in many cases analysis of A1B only, is shown. Or better, those figures which show AR4 ensemble means for A1B could include panels also for A2 and B1. E.g., Figure 11.3.3.2 for Europe is excellent as it includes DJF and JJA in addition to the annual mean. Additional panels for A2 and B1 would complete the picture, and this standard could be used for all the regions (N.B. the seasons shown would be regionally dependent). [Anji Seth]	Discussion on scenarios is clarified. New figures constructed.
11-53	A	0:0		The PDF files of the papers suggested for citation are available via ftp to "ccrp.tor.ec.gc.ca" (or 199.212.19.40): Login as "anonymous"; use your email address as the password; enter "passive" (if not passive by default); change to "pub/Papers/Leona" directory by entering "cd pub/Papers/Leona". [Xiaolan L. WANG]	Noted.
11-54	A	1:1	1:51	This chapter seems to be unnecessarily long. If every other paragraph were arbitrarily deleted, I doubt if the information content would decrease by 5%. [Andrew Lacis]	Chapter has been reduced and homogenized.
11-55	A	1:1	142:6	Chapter is too long and with confusing details - must be re-organizd to focus on key findings. [Murari Lal]	Chapter has been reduced and homogenized
11-56	A	1:1	142:6	Many scenarios have been used for future projections - IS92a is essentially an outdated emission scenario and could be dropped. The scenarios for all the six SRES scenarios should be summarzied in table and the text should highlight the projections for two	See 11:52

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				extreme scenarios only (the upper and lower limits). [Murari Lal]	
11-57	A	1:1	142:6	Why the models have been grouped into two sets namely PCMDI and IPCC models for projecting scenarios - it will create confusion among the readers. [Murari Lal]	See 11:52
11-58	A	1:4	5:	The regional chapter is a significantly improvement over IPCC 2001. This chapter, however, requires more work. One important need for undertaking more regional studies is that most of the aerosol forcing (the dominant source of uncertainty in climate model projectons of global warming) is on regional scales where the radiative forcing is about factors of 2 to 20 larger than the GHGs forcing (e.g, see Ramanathan et al, Science Vol 294, p2119, 2001) and these can impact large scale precipiiation and the monsoonal circulation with global implications. It is fundamentally important to unravel the regionla forcing and responses to reduce the uncertainty in global climate projections. I hop the authors will be more forceful in stressing the urgency of regional scale modeling and impact assessment. It is important that the regional models are developed to addres issues that are Germane for Agriculture impact studies. [Veerabhadran Ramanathan]	Chapter has been reworked.
11-59	A	1:13	1:13	Should be "Inger Hanssen-Bauer" [Inger Hanssen-Bauer]	Noted
11-60	A	2:1		What is the order of magnitude of the uncertainties in regional temperature changes? . This is a very important piece of information for the potential readers of the executive summary. If no order of magnitude can be given, this limitation should be also stated. [Eduardo Zorita]	Uncertainty text has been reworked
11-61	A	2:4	2:5	I suggest that is time to eliminate the acronym: AOGCMs. GCM is an ancient acronym for atmospheric models that now deserves to be given a respectful promotion to "Global Climate Model". That is what we now deal with, and that is what we now do. It also legitimizes the phrase "Regional Climate Model", to the benefit of this chapter. [Jerry Mahlman]	Taken under advisement, will be used as is consistent with the whole AR4
11-62	A	2:4	2:4	Change first line to "This chapter assesses regional climate projections from all available sources, including…" [James Renwick]	Noted
11-63	A	2:4		Replace "information" with "speculation" [Vincent Gray]	Disagree.
11-64	A	2:4		Aftter "sources" insert "except actual regional climate observations [Vincent Gray]	Redundant. The chapter is on projections, not observations.
11-65	A	2:6	2:7	No need for full naming of the TAR	Editorial action taken.

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				[James Renwick]	
11-66	A	2:7	2:7	In "become widely applied" widely seems a bit strong. [Daniel Caya]	Reworded.
11-67	A	2:7	2:7	In "In several cases" several should be replaced by some. [Daniel Caya]	Noted and changed.
11-68	A	2:7	2:8	The sentence "In several cases…" could be deleted. Assumedly, it refers to GCM-studies in a wider context than for regionalisation purposes and regional ensemble studies are not many. [Markku Rummukainen]	See above.
11-69	A	2:10	2:16	Just as important (arguably more important) for assessing uncertainty is gaining an understanding of the predicted regional changes, to judge whether model agreement/ disagreement is reasonable. I therefore think this should also be discussed in this paragraph. [Dave Rowell]	Agreed. Uncertainty text in chapter has been reworked and this issue covered there.
11-70	A	2:10	20:	Developments in the treatment of uncertainty are rightly welcomed, but this field is very much in its infancy, and the techniques used are characterised by large assumptions and substantial sensitivity to methodological choices and expert judgement. I think we should call them "experimental" rather than "defensible" (line 13), and we should back off from unqualified statements such as "we are now in a position to make clear assessments of regional change". Ultimately the results are only as good as the underlying multimodel ensembles on which they are based: the AR4 ensemble is not that much bigger or better than the TAR ensemble (systematic model errors have not gone away), so we need to be cautious about proclaiming too much of a sea change in the state of regional prediction. [James Murphy]	Noted. Text adjusted.
11-71	A	2:11	2:16	The use of multi-model ensembles for climate projections is weakened by the fact that the models are not truly independent of each other, with members of the ensemble sharing common approaches to characterization of climate drivers and outputs. Some discussion of the implications of this fact is needed at this point. [Lenny Bernstein]	Noted, see points 11:69 and 11:70
11-72	A	2:11	2:16	The multi-model ensemble approach is based, in part, on the assumption that the models are independent of each other. This is not the case, since many of the models in the ensemble are derived from each other or a common earlier model. The inter-model comparison programs described in Chapter 8 also drive models to common approahces. Because of this, one would expect that given the same inputs, the outputs of all models in the ensemble would be close. The authors need to discuss the degree to which climate models share common components and the implications of this sharing on the quality of multi-model ensemble outputs.	See above.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jeffrey Kueter]	
11-73	A	2:12		... developing probability density functions. [Jerry Mahlman]	Noted.
11-74	A	2:13	2:14	This is an impossibly strange sentence. Please reword this. [Jerry Mahlman]	Text has been reworked.
11-75	A	2:13	2:15	Sentence starting at the end of line 13 is very confused. Suggest "Use of multi-model ensembles allows us to better estimate the full PDF of future climates, and may also help constrain the likely range of future changes." [James Renwick]	See above.
11-76	A	2:14	2:14	to the span -> to span. Note "->" means "should be replaced by". This notation will be used for the whole review. [Daniel Caya]	Noted.
11-77	A	2:18	2:20	This paragraph sounds very bold, more definite than Chapter 10. Are you overstating our certainty about regional change? These words seem at odds with what is written later in many parts of section 11.3, such as in section 11.3.3.4 (page 42) and in section 11.3.5.5 (page 67). [James Renwick]	Agreed. See 11:70
11-78	A	2:18	2:20	This paragraph is inconsistent with the contents of the Chapter. In numerous places uncertainties related to the future ENSO or NAO are mentioned. Nowhere in the Chapter are uncertainties range given. Most of the projections are of qualitative nature (higher or lower, warmer or colder). These cannot certainly be considered a " strong assessment" [Eduardo Zorita]	Noted. See 11:69
11-79	A	2:18	:20	I don't find this statement particularly convincing, especially the claim that Chapter 11 can provide "clear assessments" of regional change. What the chapter shows is that recent work is starting to provide better estimates of the range in future regional climate change. To do an "assessment" requires looking at impacts, and ultimately doing a risk analysis, which is not addressed in this report. [A. Brett Mullan]	Noted, see above.
11-80	A	2:22	2:36	This paragraph is too long for the executive summary. It describes technical details of the model validation that can be referred to the main body of the text. [Eduardo Zorita]	Text reworked.
11-81	A	2:23	2:31	This is very nicely done. [Jerry Mahlman]	Thanks.
11-82	A	2:29	2:31	Would it be appropriate to attempt a statement of how empirical downscaling techniques might have developed since TAR? [Markku Rummukainen]	Yes. Will be considered within space limitations.

No.	Batch	Page:line From	Page:line To	Comment	Notes
11-83	A	2:30	2:30	Delete "consistently" from the sentence. [Daniel Caya]	Noted ... section has been reworked.
11-84	A	2:33	2:36	I don't understand the purpose of this paragraph. If the idea was to point to model reliability, I find the argument is not relevant. The bias of a multi-model mean is not a measure for model reliability. Is an ensemble more reliable, when large individual biases balance out than an ensemble with small individual biases that don't balance? [Christoph Frei]	Acknowledged ... exec summary has been reworked.
11-85	A	2:33	2:36	Add a brief explanation here as to why this ensemble approach increases confidence in its validity(Policymakers and Climate Sceptics could challenge this, lacking needed information.) [Jerry Mahlman]	See above
11-86	A	2:33	2:35	The ensemble mean bias may be small, but doesn't this just reflect a tendency for cancellation of errors in the ensemble of driving global models ? Unless we believe the ensemble-mean of the models gives a perfect prediction, we should also be concerned about variations in performance between models, which presumably depends on the range in performance of the driving models plus variations in the quality of the RCMs themselves. This should be commented on. [James Murphy]	See above
11-87	A	2:34		... can be very small. Generally, temperature biases remain ... [Jerry Mahlman]	See above
11-88	A	2:39		Replace "robust" with "speculative" [Vincent Gray]	Disagree with speculative ... but strength of statement re-evaluated.
11-89	A	2:40		Again, why not just use "GCM" in general(One can use Global Atmospheric Model as an appropriate substitute, if desired.) [Jerry Mahlman]	See comments earlier.
11-90	A	2:42	2:42	Box11.1 : it would be good to see this box. Who is producing it? [Aurel Moise]	It's coming.
11-91	A	2:42	43:	I presume the intention in Box 11.1, Figure 1 will be to base the assessment of robust regional changes on how consistent the multimodel changes are and how well the ensemble members simulate aspects of present climate. This emphasis on convergence is repeated in several places in the chapter, but it needs to be accompanied by the caveat that the multimodel ensemble is far too small to represent a comprehensive estimate of the space of "all possible models", and indeed was not even designed to sample this space evenly. The models share components, ideas and (inevitably) errors, and so could easily converge on a wrong answer. This is not to say that evidence of a more consistent response cannot be identified as progress, but convergence does not necessarily give rise to confidence to the extent implied.	This issue addressed in text.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[James Murphy]	
11-92	A	2:45	3:40	The subtitles "Climate means" and "Climate variability and extremes" do not fit very well as also the second paragraph much concerns mean values. Suggestion: "General features" and "Specific features" or "Regional features" [Inger Hanssen-Bauer]	Will be considered.
11-93	A	2:46	2:50	This is very insightful and credible. The very likely tag on regional temperature rise is highly credible. [Jerry Mahlman]	Noted.
11-94	A	2:48	2:48	Change "deficits" to "deficiencies" [James Renwick]	Noted.
11-95	A	2:50	2:50	Assumedly, words like "very likely", "likely" etc. will be used in some defined manner? This is not clear in the present Chapter draft. [Markku Rummukainen]	According to ARS definitions.
11-96	A	2:51	2:53	Again, there seems to be an assumed link between convergence and confidence which needs to be qualified. [James Murphy]	Noted. See earlier.
11-97	A	2:51	2:51	"These are comparable in magnitude to those of TAR". Ambiguous wording - what precisely does this statement refer to ? [James Murphy]	Wording changed.
11-98	A	2:51	3:2	I think it is conceptually useful to make it clear that temperature warming projections for continental regions are very likely to be credible. Precipitation projections are considerably more challenging because they depend upon changed atmospheric dynamics to provide a change in the flux convergence of water vapour from water sources(e.g., oceans and large lakes) into the continental interiors. The "For some regions" qualifier on Page 2, Lines 54-55 does illustrate that awareness. Remember, however, that regional precipitation projections do have a tendency to keep all of us humble.	Noted.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Jerry Mahlman]	
11-99	A	2:54	:55	I doubt that we should refer to the projected precipitation changes as "very likely". If the uncertainties on the GCM scale (e.g. on climate sensitivity) are factored in, the best that we can expect is "likely". [Christoph Schar]	Will be considered.
11-100	A	3:4	3:34	Much of the text does not, actually, concern climate variability and extremes as indicated on line 4. Rather general statements are made (especially for Asia, Central/South America, Polar, Small Islands). [Markku Rummukainen]	Section heading will be re-evaluated, see earlier.
11-101	A	3:4	:40	It is not very helpful to keep repeating "all of ... is very likely to warm" for each region. You could make one generic statement at the start, and then indicate how each region compares to the global-average temperature change. That would be a lot more informative. [A. Brett Mullan]	Noted ... addressed in rewrite.
11-102	A	3:5	3:6	Good! I think that this is the very first chapter cross-reference I have seen since I began reading all of the chapters. [Jerry Mahlman]	Thanks.
11-103	A	3:8	3:9	Historical changes are not covered by this chapter [Timothy Carter]	Yes.
11-104	A	3:9	3:11	Not clear whether the words after the semi-colon refer to historical changes (as did the previous statement), or to future changes. [James Murphy]	Noted ... text restructured.
11-105	A	3:10	3:10	replace "windstorms" with "storminess and associated phenomena (winds, storm surges, ocean waves…)" [Piero Lionello]	Yes.
11-106	A	3:11	3:12	A new heading is needed between these 2 lines - what follows is no longer just about "Climate variability and extremes" [Dave Rowell]	See 11:100
11-107	A	3:12	3:12	A sub-heading is missing here. The following doesn't only refer to variablity and extremes but to regional changes. [Timothy Carter]	See 11:100
11-108	A	3:12	3:34	The vast majority of these statements refer to changes regarded as "very likely". Couldn't this be stated up front and then the statements made without repeating the confidence each time? Perhaps an option here would be to have a set of statements that are "very likely" and another set of statements giving general information about the state of knowledge in each region (as for Small Islands).	Yes. See 11:101

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Timothy Carter]	
11-109	A	3:12	3:34	The regional summaries seem to be linked to the heading changes in climate variability and extremes, which they are not. [Roger Jones]	Yes, see 11:101
11-110	A	3:12	3:34	Although it is good to represent each area individually, I note that increases in temperature are projected as being very likely for all regions except the Antarctic (not mentioned) and small island states. Is it possible to summarise such changes in temperature in a couple of sentences to save space without saying virtually the same thing for each region? [Roger Jones]	Yes, see 11:101
11-111	A	3:12	3:34	All the regions are said as very likely to warm - why not club them together and save space. High light the differences between the regions as regards precipitation in a para. [Murari Lal]	Yes, see 11:101
11-112	A	3:12	3:14	I am not so sure that this deserves a "very likely" chance designation. The remarkably long time scales of decadal variability of precipitation over Africa have confused us before. I would keep my betting stash on "likely", but await more definitive information. [Jerry Mahlman]	Will be re-evaluated.
11-113	A	3:12		Replsce “is very likely” with “may” [Vincent Gray]	Disagree with “may”
11-114	A	3:13	3:13	To me "North Africa" means all African land north of 0degN, and so this statement contradicts the lack of certainty elsewhere about Sahel rainfall since the Sahel is part of North Africa. I suggest replacing this by "the far north of Africa". [Dave Rowell]	Wording reassessed.
11-115	A	3:13		Replace “will very likely” with “might” [Vincent Gray]	Disagree with “might”
11-116	A	3:15	3:15	Mediterraean and Europe does not match the WG II region, which is Europe, including only the European part of the Med. I don't think this is especially critical, but the distinction should probably be made somewhere. [Timothy Carter]	Will be taken into consideration in the rewrite.
11-117	A	3:15	3:17	It would good to include also a statement about increase in summertime temperatures in southern Europe. Higher than average increase is expected in the highest temperatures. [Erik Kjellström]	Done
11-118	A	3:15		Replace “is very likely to” with “might [Vincent Gray]	Disagree with “might”
11-119	A	3:15	:17	Please rephrase sentence: In northern Europe, winter minimum temperatures are very likely to incrrease more than mean temperatures.	Done

No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Christoph Schar]	
11-120	A	3:16		Replace "are very likely to" with "could" [Vincent Gray]	Disagree with "could"
11-121	A	3:18		Replace "will very likely" with "might" [Vincent Gray]	Disagree with "might"
11-122	A	3:19		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-123	A	3:21	3:21	The description for the Asia is clealy too short. Increase in heavy, as well as mean, precipitation in summertime appears to be a newly confirmed result by high-resolution models. [Masahide Kimoto]	Sections has been rewritten
11-124	A	3:21	3:21	The summary is too short ocompared with other regions. [Akira Noda]	See above
11-125	A	3:21		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-126	A	3:21		More description should be given to Asia. [Dr. Bundit Limmeechokchai]	Section has been rewritten
11-127	A	3:21		Precipitation distribution depends on regions, because Asia is very broad and various. Remarkable decrease in central Asia and notable increase in the arid region of the southern part of Arabia through Pakistan in the northern summer(MRI-CGCM2.3; Yukimoto et al., 2005) [Yasuo Sato]	Section has been rewritten
11-128	A	3:21		Executive Summary. More detailed information should be given to the climate variability and extremes for Asia, considering a geographic variety of the region. [Masato Shinoda]	Section has been rewritten
11-129	A	3:22	3:22	North America includes Mexico here, but not in WG II [Timothy Carter]	Sorry but WG1-Chap.11 has agreed upon this split earlier
11-130	A	3:22		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-131	A	3:23		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-132	A	3:24		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-133	A	3:25	3:25	Central and South America is defined as Latin America in WG II, and includes Mexico. [Timothy Carter]	The criterion was to name the regions geographically rather than culturally.

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11-134	A	3:25		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-135	A	3:26		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-136	A	3:26		Again, I would hold my precipitation betting chips in the "likely" bin for SE South America. [Jerry Mahlman]	Will be reconsidered
11-137	A	3:27		Replace "is very likely to" with "might" [Vincent Gray]	Disagree with "might"
11-138	A	3:28	3:28	Small but important point: rainfall will likely increase only in the west of the South Island of New Zealand, while rainfall will decrease and drought frequency will likely increase in the east (see Mullan et al 2001a, Mullan et al 2005). The west-east difference is due to significant topography combined with increases in windiness (discussed in more detail in section 11.3.7). [James Renwick]	Noted and accomodated
11-139	A	3:28		Replace "will very likely to" with "could" [Vincent Gray]	Disagree with "could"
11-140	A	3:28		Again, I am holding my blue chips for the "likely" bet, simply because the enormous heat capacity of the Southern Ocean and the highly baroclinically unstable westerlies south of New Zealand and Australia can roll some counter-intuitive "dice" to frustrate us "bettors". [Jerry Mahlman]	"Likely" will be re-assessed.
11-141	A	3:30	3:30	"Increased risk…" It would be good if this part could be a bit more specific, using key areas of interest like South-West W.A., South-East Australia (Victoria, NSW) and South Australia. [Aurel Moise]	Section re-written
11-142	A	3:30		Replace "is very likely" with "possible" [Vincent Gray]	Not useful, anything is possible.
11-143	A	3:31	3:33	Can nothing be said here about the Antarctic? [Timothy Carter]	Good point, accomodated.
11-144	A	3:31	3:32	Can conclusions for Antarctica be summarised? (This request came from the penguin lobby) [Roger Jones]	See above
11-145	A	3:31	3:33	How about the Antarctic? [Akira Noda]	See above
11-146	A	3:31		Replace "is very likely" with "possible" [Vincent Gray]	Not useful, anything is possible.

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11-147	A	3:32		Replace "is very likely" with "possible" [Vincent Gray]	Not useful, anything is possible.
11-148	A	3:33	3:33	The reduction of the extent of arctic sea ice is less obvious in Arctic winter than in summer [Bart Van den Hurk]	Noted
11-149	A	3:33		Replace "is very likely" with "possible" [Vincent Gray]	Not useful, anything is possible.
11-150	A	3:34		More impacts should be given to Small Islands. [Dr. Bundit Limmeechokchai]	Noted.
11-151	A	3:34		I would be less quick to pass on a comment on the potential fate of the small islands. Clearly, sea-level rise is a local/regional effect on the small, low-lying islands, even though the disrupting sea-level phenomenon is quite global, by definition. You might be able to add to this discussion meaningfully by looking somewhat carefully in the excellent Chapter 5: Observations: Oceanic Climate Change and Sea Level. Their carefully quantitative analysis/projections of sea-level rise could be of major climate impacts value for this Chapter 11. [Jerry Mahlman]	Noted ... will be reassesed
11-152	A	3:36	3:40	This paragraph is an excellent introduction to the social and economic challenges expected to the nations who are less resilient to the local/regional impacts of human-caused climate warming. [Jerry Mahlman]	Thanks.
11-153	A	3:37	3:38	The wording seems to imply that one needs to see convergence before coordinated analysis can be justified. If anything, I would suggest the opposite were true - we need to understand why changes are so uncertain in areas where convergence is absent ! [James Murphy]	The treatment of uncertainty has been changed.
11-154	A	3:37		...convergence of projected regional climate change over large ... [Jerry Mahlman]	Noted
11-155	A	3:38	3:39	I think further understanding is also essential in regions where there is convergence, to verify whether or not we believe the models are converging for valid regions. [Dave Rowell]	Noted, see 11:153
11-156	A	3:38		seems [Michel Déqué]	Noted
11-157	A	3:39	3:39	deficits -> behaviour. [Daniel Caya]	Noted
11-158	A	3:39	3:39	Suggest "deficiencies" rather than "deficits". Also on page 2 line 48. [James Murphy]	Noted

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11-159	A	3:39		....convergence of understanding … [Jerry Mahlman]	Noted
11-160	A	3:40		.... are still expected to be disadvantaged in their ability to respond meaningfully to the sophisitcation, ... [Jerry Mahlman]	Noted
11-161	A	4:1	4:43	Regional projections are made with little discussion of their reliability. There have been recent regional climate perturbations - the Sahel droughts in the 1970s and 1980s, the Arctic warming during the 1940s, the midwest dustbowl of the 1930s in the US - that have no real explanation as to what might have been responsible for these regional climate changes. (The Sahel droughts are thought to be related to Atlantic SST changes, but then, why and how did the SSTs change?). If the causes for these regional climate perturbations are not specifically understood, they can be categorized as "natural variability". It would seem that such uncertainty would loom over most of the regional projections. Some caveats would seem to be in order. [Andrew Lacis]	Noted. In addition to here, uncertainty discussion has been changed.
11-162	A	4:3		Section 11.1.1: Is this section necessary? Perhaps it could be significantly shortened, as much of the "justification" is not needed in this document. [James Renwick]	Section has been shortened.
11-163	A	4:5	4:5	I suggest that the authors use the word "anthropogenic ….forcing" instead of "anthropogenic … change" as they do further in the text. See also page 14, line 25 [Michel Boko]	Noted
11-164	A	4:5	4:43	The introduction is not convincing, especially the para spanning lines 24–32. This simple reason for regional integrated assessment where regional projections are used glosses over the evolution of scenario development. Coarse regional projections are almost never used in I and A assessment, except in top-down approaches where more complex representations of climate would be a hindrance (or cannot be generated with available resources). Many of the methods detailed in the chapter can be used both in constructing projections and scenarios, which are two different things (Ch 3, WG II TAR and Ch 13, WG I TAR). While projections of regional climate change are perhaps more useful for assessing mitigation needs, climate change information needed to inform adaptation at local scales requires the development of climate scenarios. Scenario needs for adaptation are increasingly being fed by bottom-up demand for representing climate hazards at the scale of impact, requiring applied methods of scenario development that uses climate model output in a variety of ways. Many of these methods have arisen because information is needed in greater detail than model resolution can provide, or because plausible changes spanning a range of results is required (e.g. for assessing risk). This need for detail	See 11:162

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				requires more spatially explicit output from single models and methods to span changes across a range of models. Advances in both areas are discussed in this chapter. Chapter 2, WG II can be referenced for data needs for scenarios. [Roger Jones]	
11-165	A	4:7		The statement "Opposed to mitigation, adaptation is.." represents a sweeping generalization that is not consistent with the findings of WG2 Chapter 18. As stated in Ch18, mitigation and adaptation clearly share both similarities and differences, including the ones noted here. I recommend a review of the exec summary of ch18 to re-state; for example these sentences could be re-phrased as: "International discussions on mitigation are primarily founded on our understanding of observed current and future projected global-scale change, and are aimed at identifying actions that can be taken by multiple nations or regions. In contrast, adaptation decisions and actions tend to be more of a local and regional scale issue, and is limited by the measure of confidence in the projected changes over smaller spatial scales." [Katharine Hayhoe]	Noted, also see 11:162
11-166	A	4:7		The text should probably mention the role of high-resolution scenarios to address impacts (which are also relevant to mitigation and not merely to adaptation. This could be done by extending the sentence that ends on line 7 by: … primarily founded on our present understanding of global-scale change, but consideration of regional impacts is needed in the context of the UN Framework Convention on Climate Change (UNFCCC), which postulates the stabilization of atmospheric greenhouse gas concentrations "at a level that would prevent dangerous anthropogenic interference with the climate system". Opposed to mitigation, adaptations is ... [Christoph Schar]	Noted, alspo see 11:162
11-167	A	4:10		This statement could be stronger: "…has already motivated some to …" and cite the following: (1) The Governor of the State of California. 2005. Executive Order S-3-05. Available online at: http://www.dot.ca.gov/hq/energy/ExecOrderS-3-05.htm (2) DEFRA. 2003. The Energy White Paper. Available online at: www.dti.gov.uk/energy/whitepaper/index.shtml (3) European Environment Agency. 2004. Impacts of Europe's Changing Climate. Available online at: http://reports.eea.eu.int/climate_report_2_2004/en [Katharine Hayhoe]	Peer reviewed literature is needed.
11-168	A	4:13	4:22	The first sentence in this paragraph appears to suggest that downscaling techniques are needed because global modellers don't pay enough attention to regional scales. I don't think this is correct, and in any case we can surely motivate the need for downscaling simply by pointing to the unresolved scales in global models, as is done in the second	Noted. Text adjusted.

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				sentence. [James Murphy]	
11-169	A	4:16	4:16	It's not obvious to me that paying special attention to phenomena like ENSO implies lack of attention for other regions in the world. It's not an obvious justification for alternative methods to address specific regions in the world [Bart Van den Hurk]	Noted and reworded
11-170	A	4:17		the World. At the same time, even though GCMs could provide regional climate information they are able to resolve in many regions of the World, we need finer-scale regional climate information. Therefore, ??? [Yasuo Sato]	Noted
11-171	A	4:21		Replace "validation" with "evaluation" [Vincent Gray]	It is more than evaluation. Sentence will be reconsidered
11-172	A	4:26		Delete "perhaps" [Vincent Gray]	Disagree
11-173	A	4:28	4:28	Suggest "derived from" rather than "derivatives of" [James Murphy]	Noted
11-174	A	4:30	4:31	I don't follow the statement that mapping GCM data as continuous fields fails to convey the low skill of the model in many regions. Why ? [James Murphy]	Noted. Section is rewritten
11-175	A	4:35	4:35	regional projections -> regional scenarios [Daniel Caya]	Disagree.
11-176	A	4:35	4:35	Delete "throughthe above mentioned methodologies" [Markku Rummukainen]	Noted. Section is rewritten
11-177	A	4:37	4:37	The reference to "empirical cross scale functions" is too cryptic. Please explain it more fully, or in less technical language. [James Murphy]	Noted.
11-178	A	4:39	4:39	There seems to be an implication that the impacts community will only find projections useful if they demonstrate convergence. Why ? Surely what the impacts community needs most is a realistic expression of uncertainty. This will still be useful even if (and perhaps especially if) the uncertainty is large. [James Murphy]	Noted. See earlier comments in this regard.
11-179	A	4:41	4:41	"otherwise the notion" is a vague phrase, I don't know what is meant here. Sentence should be cut in two. [Bart Van den Hurk]	Noted/ section is rewritten.
11-180	A	4:42	4:42	It is important to make the distinction that climate sensitivity is not more uncertain than previously believed but that the uncertainty is more thoroughly explored.	Agreed. Text has been changed.

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				[Daniel Caya]	
11-181	A	4:42	4:43	In fact, there is validity to this comment - with a factor of three uncertainy (or at best two), the regional projections may make no sense. I suppose one has to try but this basic uncertainty is downplayed throughout the chapter. And note it is not only a global sensitivity issue - there are also substantial differences in projections of the latitudinal temperature gradient change, which would affect storms tracks, jet streams, the NAO/AO, etc. Just mentioning this topic at the end of this paragraph does not do justice to the big problem it really does pose for this chapter. [David Rind]	Agreed, text has been changed.
11-182	A	4:45		Section 11.1.2: Much of this section seems uncessary also. There is much detail, covered either in the TAR, or in Chapter 10. This could be significantly shortened. [James Renwick]	Text has been shortened
11-183	A	4:54	5:40	This section can be condensed. There are many good examples of advances in the chapter and the older work only needs a general "word picture" to be drawn of the state of play at the time. [Roger Jones]	Text has been shortened
11-184	A	5:1	5:1	What is the meaning of "(5)"? [Daniel Caya]	Refers to SAR and FAR, changed.
11-185	A	5:2	5:3	punctuation not properly used. [Marina Baldi]	Noted
11-186	A	5:2	5:2	Suggest deleting "two seasons" [James Murphy]	Noted
11-187	A	5:5	5:5	I find the discussion in the text biased based on the authors listed as coordinating lead authors, much more discussion on Europe compared to other areas with considerable data [Thomas Karl]	This is true to a degree and will be addressed. However, it also reflects in part the literature (as opposed to data).
11-188	A	5:5	5:5	There is a far too heavy an emphasis on changes in the mean and far too little focus on changes in extremes or important weather/climate events like droughts heat waves, heavy precip, etc. [Thomas Karl]	Noted, and see previous response.
11-189	A	5:5	5:5	The dicussion in this chapter is very dense and the number of pages figures and tables out of proportion with other chapters in the book in my view. Much of the dicussion does not relate to important aspects of regional climate e.g., the full distribution of change as opposed to the means [Thomas Karl]	Text and figures have been adjusted in this regard, but again, there is the constraint of the literature.
11-190	A	5:6	5:6	AOGCMs -> CGCMs. multiple occurrences in the rest of the section. [Daniel Caya]	Noted, see 11:61

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11-191	A	5:21	5:21	Delete "with some confidence" that is implicit in very likely on the following line. [Daniel Caya]	Noted
11-192	A	5:21	5:22	"it was found with some confidence that it is very likely that with a few exceptions". Too wordy ! [James Murphy]	Agreed.
11-193	A	5:22	7:28	This is an important section of the chapter. The text here needs to be more focussed and concise. [Murari Lal]	Noted and reworked.
11-194	A	5:22		Replace "very likely" with "possible [Vincent Gray]	Disagree, and further "possible" does not convey anything of value
11-195	A	5:24		Replace " likely" with "possible [Vincent Gray]	Anything is possible. Disagree.
11-196	A	5:25		Replace "will" by "may" [Vincent Gray]	Disagree
11-197	A	5:26		Replace "will" by "could" [Vincent Gray]	Disagree
11-198	A	5:27		Replace "will" by "might" [Vincent Gray]	Disagree
11-199	A	5:33	5:33	Suggest deleting "at the regional level", as a reference to the regional scale is already on the previous line. [James Murphy]	Noted
11-200	A	5:34	5:35	"daily to interannual temperatures are likely to increase". I presume this is a reference to some measure of variability in temperature ? Needs to be clarified. [James Murphy]	Noted and clarified
11-201	A	5:34	5:36	Please re-examine this statement. Did the TAR really say that? [Eugene Takle]	Noted. Will examine.
11-202	A	5:34	5:34	Rephrase to "For example it was stated that the variability of daily to interannual temperatures…" [Bart Van den Hurk]	See above
11-203	A	5:35	5:35	"temperature are likely" -> "temperature variability are likely". [Daniel Caya]	See above
11-204	A	5:35	5:35	temperatures" should probably be "temperature variations [Inger Hanssen-Bauer]	See above
11-205	A	5:35		Replace "are likely to" by "might" [Vincent Gray]	Disagree

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11-206	A	5:36	5:36	Extremes cannot increase in frequency, since it's the frequency that defines the extreme. Rephrase to "Daily extreme temperature will increase." [Bart Van den Hurk]	Noted.
11-207	A	5:36		Replace "will likely " by "could" [Vincent Gray]	Disagree
11-208	A	5:37		Replace " will very likely " by "might" [Vincent Gray]	Disagree
11-209	A	5:44	5:44	What is meant by "external forcings"? [Daniel Caya]	Clarified
11-210	A	5:44	5:48	Maybe these sentences are OK, but ensure that the use of the term "forcings" ("external forcings", "local forcings") is consistent throughout the report. The term should also be defined somewhere, as it is used differently in the litterature. [Inger Hanssen-Bauer]	Noted
11-211	A	5:48	5:48	Suggest "...such as THOSE ASSOCIATED WITH ENSO and NAO..." [Markku Rummukainen]	Noted
11-212	A	5:50	5:50	Not clear what is meant by "troubled a quantitative assessment of". Do you mean that there was a wide spread of changes ? [James Murphy]	Clarified
11-213	A	6:1	6:5	The first sentence is rambling and badly constructed. Also, I think the reference in line 1 should be to Chapter 8, not 10. [James Murphy]	Noted
11-214	A	6:2	6:13	cross references with chapter 10 must be more explicit [ERIC MARTIN]	Noted, clarifications made
11-215	A	6:3	6:3	Start new sentence at "Many". [Bart Van den Hurk]	OK
11-216	A	6:3	6:3	has" -> "have [Bart Van den Hurk]	OK
11-217	A	6:11	6:13	This sentence needs to be more constructive, since it is noteworthy that for the first time in climate modelling history a study is being reported where numerical experiments of a global AGCM were made for present climate simulation and for future climate projection both with 20km resolution and for 10 years and, in particular, tropical cyclones have been simulated and/or projected in a global model with their basic structure elements such as eyes, eye walls and spiral bands resolved. A suggested modification would be as follows: Although coordinated multi-model experiments are thus needed to optimize the value of these studies for assessments, there are emerging new higher resolution time-slice studies with uncoupled atmospheric models, ranging up to the 20 km resolution by which basic	Will be considered in editorial reworking.

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				structure of tropical cyclones can be resolved in a global AGCM (e.g., Mizuta et al. 2005a and 2005b). [Hiroki Kondo]	
11-218	A	6:19	6:19	Plummer et al. 2006 should be added to the two references. [Daniel Caya]	Will be assessed if paper is in press.
11-219	A	6:20		Replace "validation" with "evaluation" [Vincent Gray]	Disagree; the process is intended as validation in addition to evaluation.
11-220	A	6:23	6:27	The interpretation of the Christensen et al (2001) study is too optimistic. C et al rightly emphasised that a credible quantification of uncertainty would require a much larger ensemble than currently feasible, so their study (two GCMs driving 3 RCMs) should be regarded more as an illustration of similarities and differences between alternative simulations. However this caveat is ignored in the way the study is reported here. [James Murphy]	Acknowledged; will reconsider text.
11-221	A	6:26	6:26	"quantify" -> "explore". I do not think that the overall uncertainty is quantified in regional projections yet. [Daniel Caya]	Acknowledged; will reconsider text.
11-222	A	6:29	6:34	The necessity of examining propagation of uncertainty from scenario to GCM to RCM should be noted more strongly. [Raymond Arritt]	Acknowledged; will reconsider text.
11-223	A	6:29	6:34	The chapter needs to be clearer about how uncertainty is partitioned between global and regional scales. Here there is a reference to "RCM uncertainty", whereas elsewhere there are references to "downscaling uncertainty" (section 11.2.2.5). The PRUDENCE project measures RCM uncertainty, but the effects of RCM uncertainty project onto relatively large regional scales which are resolved in global model ensembles, so RCM uncertainty overlaps with GCM uncertainty to some extent. In other words, if we were to create a fully populated matrix consisting of a large ensemble of alternative GCMs each driving a large ensemble of alternative RCMs, it would be a mistake to characterise "total uncertainty" as the sum of GCM spread plus RCM spread, as this would imply an element of double counting. On the other hand, if "downscaling" specifically means prediction on scales too fine to be resolved by GCMs, then downscaling uncertainty can reasonably be defined as a component of uncertainty which is distinct from, and can be added to, uncertainty arising from the spread of regional changes from GCM ensembles. This distinction is not brought out at all well in this chapter, which is a serious omission. [James Murphy]	Acknowledged; will reconsider text. Uncertainty, in general as an issue has been readdressed in the chapter as well.
11-224	A	6:32	6:33	Suggest change to "This enables some rough quantitative estimates to be made of the uncertainty in climate change projections due to the above factors." [James Renwick]	Acknowledged; will reconsider text.

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11-225	A	6:32		Replace "information" with "speculation" [Vincent Gray]	"information" is nopt in the text.
11-226	A	6:33	6:34	Cite also Rowell 2004 here [Erik Kjellström]	Will consider the addition.
11-227	A	6:33	6:33	Suggest "…uncertainty in REGIONAL climate…" [Markku Rummukainen]	Noted.
11-228	A	6:34		Write Fréi et al, (problem of punctuation) [Ibouraïma YABI]	OK.
11-229	A	6:34		Write Graham and Al, (problem of punctuation) [Ibouraïma YABI]	OK
11-230	A	6:38	6:38	Suggest replace "data backing" by "database available". Also pluralise event. [James Murphy]	Agreed
11-231	A	6:39	6:39	Clarify that by "three completely independent members" we mean three integrations of the same model distinguished by different initial conditions. [James Murphy]	OK.
11-232	A	6:39	6:41	Change to "Within the PRUDENCE project, two groups downscaled three independent members of a Hadley Centre ensemble global simulation (using the SRES A2 emissions scenario), thereby effectively enabling an analysis based on 90 years of simulation instead of 30 (references)." [James Renwick]	Noted, will reword.
11-233	A	6:39	6:39	Delete "completely independent". [Markku Rummukainen]	Agreed.
11-234	A	6:40	6:41	The later part of this sentence after "and" is not understangable for me and would not be necessarily clear for most readers, either. Some modification is needed for clarification. [Hiroki Kondo]	Noted, will reword.
11-235	A	6:40	6:40	Unclear which reference to Deque is meant. There are two Deque et al 2005's. [Bart Van den Hurk]	Will clarify
11-236	A	6:41	6:41	Delete "two times". [Daniel Caya]	Noted, will reword.
11-237	A	6:44	6:45	A similar adjustment to higher model resolution was conducted by Kleinn et al. (2004) showing an improvement of the representation of the topographic precipitation response in the Alps. Kleinn, J., C. Frei, J. Gurtz, D. Lüthi, P.L. Vidale and C. Schär, 2005: Hydrological simulations in the Rhine basin, driven by a regional climate model. J. Geophys. Res., 110, D04102, doi:10.1029/2004JD005143. [Christoph Frei]	Noted.
11-238	A	6:44	6:44	Change to "at the 20km or finer horizontal scale"	Noted

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				[James Renwick]	
11-239	A	6:44	:45	Christensen &christesen (check if there is no repetition) [Ibouraïma YABI]	OK
11-240	A	6:45	6:46	Change to "…one group had simulations at this resolution (reference), over a period long enough to give stable climate information." [James Renwick]	OK
11-241	A	6:45		Kurihara et al.,2005 should be referred as 20km resolution RCM. [Hidetaka Sasaki]	OK
11-242	A	6:45		Grell et al.,2000 is missing in the references. [Hidetaka Sasaki]	Noted
11-243	A	6:46	6:49	The comments on grid spacing (a better term than "inter grid distance") and rainfall simulation are correct, but this has been long known and studied in the mesoscale meteorology community. There is a large body of literature on this topic, while here it is presented almost as new knowledge. Simulations down to the 5km scale or less are known to be needed for useful prediction of precipitation in mountainous areas. Some reference to the relevant meteorological literature would be appropriate. [James Renwick]	Noted and considered in revised wording.
11-244	A	6:46	6:47	Consider omitting the sentence "It appears…" [Markku Rummukainen]	Noted.
11-245	A	6:49	:49	However, for below 10 km horizontal resolution, so-called " double counting problem" will emerge. That is, part of smaller-scale motion could be represented explicitly, whereas part of it continue to parameterize statistical average of sub-grid scale phenomena as a function of larger scale motion. Consistency between them is valid or not. That might be a crucial problem around 5 - 10 km horizontal resolution. [Yasuo Sato]	Noted and considered in revised wording.
11-246	A	6:53	7:2	This should be rephrased to say: "Coupled modelling is the norm in global climate modelling. The first coupled RCM was demonstrated for atmosphere-land-sea ice in the Arctic (Lynch et al. 1995) and for a full regional climate system model, including a coupled ocean, in Bailey et al. (1997). These developments were later implemented for the Antarctic (Bailey and Lynch 2000; Bailey et al. 2004). Steps towards coupled regional modelling by other groups have been taken since TAR (Döscher et al., 2002; Rummukainen et al., 2004; Schrum et al., 2003). In addition to providing a more realistic simulation of climate in regions where water bodies are characterised by sub-GCM detail, it is very useful for studies focusing on coastal regions, the marginal sea ice zone and regional oceans as such (e.g., Bailey et al. 2004; Döscher and Meier, 2004; Meier et al., 2004)." Relevant citations:	Noted and considered in revised wording.

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				Bailey, D.A., A.H. Lynch and K.S. Hedström, 1997: The impact of ocean circulation on regional polar simulations using the Arctic region climate system model. Annals of Glaciology, 25, 203-207. Bailey, D. A., and A. H. Lynch, 2000: Development of an Antarctic Regional Climate System Model: Part 1. Sea Ice and Large-Scale Circulation, J. Climate, 13, 1337-1350. Bailey, D.A., A.H. Lynch, and T.E. Arbetter, 2004: The relationship between synoptic forcing and polynya formation in the Cosmonaut Sea, II: Polynya simulation. J. Geophys. Res, 109, doi:10.1029/2003JC001838. Lynch, A.H., Chapman, W. L.,Walsh, J.E. and Weller, G., 1995: Development of a Regional Climate Model of the Western Arctic. J.Climate, 8, 1555-1570. [Amanda Lynch]	
11-247	A	6:55		Sasaki et al. 2005(submitted to JMSJ and IPCC TSU); Murazaki et al. 2005, published in SOLA and submitted to IPCC TSU. [Yasuo Sato]	Noted, subject to length reductions
11-248	A	7:0	20:	Section 11.2.1 seems to be mainly an essay on the various methodologies used to generate regional climate projections. Surely, most of this material can be found elsewhere. This sub-section should be reduced by 50%, focussing mainly on any new methodological approaches. On the other hand, section 11.2.2 on quantifying the uncertainties does deal with significant advances on the TAR. [A. Brett Mullan]	Major reductions in length have been made.
11-249	A	7:2		sea ice zone, regional oceans and ocean current distribution as such [Hidetaka Sasaki]	Noted.
11-250	A	7:2		Sato et al., 2005;"Response of North Pacific ocean circulation in a Kuroshio-resolving ocean model to an Arctic Oscillation(AO)-like change in Northern Hemisphere atmospheric circulation due to greenhouse-gas forcing", Sasaki et al.2005, Murazaki et al.2005. [Hidetaka Sasaki]	Noted, subject to length reductions.
11-251	A	7:4	7:4	Change "Few" to "A few" at the end of the line. [James Renwick]	OK
11-252	A	7:5	7:6	A range of dynamically downscaled transient simulations (1961–2100 forced by the IS92a scenario) at 125 km and 60 km resolution were undertaken for Australia using the DARLAM regional model (Whetton et al., 2001) nested in the CSIRO Mark2 model, providing detailed regional projections for Australia (Whetton et al., 2000) and the South Pacific (Jones et al., 2000). Whetton, P.H., K.J. Hennessy, X. Wu, X., J.L. McGregor, J.J. Katzfey and K.C. Nguyen (2000) Climate averages based on a doubled CO2 simulation. Victorian Dept. of Natural	Will be considered after length reduction of this section if these refs add to the message – these refs are TAR period.

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				Resources and Environment, Melbourne 43 p. (New reference) [Roger Jones]	
11-253	A	7:6	7:6	Add reference McGregor et al., 1999 [John McGregor]	Will be considered after length reduction of this section if these refs add to the message – these refs are TAR period.
11-254	A	7:6	7:6	Please change "Kjellström et al. 2005" to "Kjellström et al 2005b". (The reference list should be amended with "Kjellström, K., L. Bärring, S. Gollvik, U. Hansson, C. Jones, P. Samuelsson, M. Rummukainen, A. Ullerstig, U. Willén and K. Wyser, 2005b. A 140-year simulation of European climate with the new version of the Rossby Centre regional atmospheric climate model (RCA3). Reports Meteorology and Climatology 108. Swedish Meteorological and Hydrological Institute, SE-601 76 Norrköping, Sweden." (The existing entry on the reference list should, of course, be changed to Kjellström et al. 2005a as it is referred to elsewhere in Chapter 11.) [Markku Rummukainen]	Noted
11-255	A	7:11	7:11	The word empirical downscaling here and in the whole chapter is sometime misleading and might be avoided. It is mainly a statistical technique whichh is usually adopted to perform a downscaling [Marina Baldi]	Noted
11-256	A	7:16	7:18	Ad downscaling of "exotic variables": Might also refer to Blenckner T, Chen D (2003) Comparison of the impact of regional and North-Atlantic atmospheric circulation on an aquatic ecosystem. Clim Res 23:131-136 [Inger Hanssen-Bauer]	Noted. Reference will be included if spaces allows.
11-257	A	7:16	7:16	I don't see the connection to the need for "scenarios by the impacts community". What you are saying is that there are many statistical approaches, and little intercomparison has been done. This is an issue for the modelling community, not the impacts community, surely. [James Renwick]	Agreed and changed.
11-258	A	7:18	7:20	This is so unless the empiricism involves changing an aspect of climate variability within a baseline data set according to how a climate model may simulate a change in that aspect of variability (e.g. perturbing extreme daily rainfall according to frequency classes). [Roger Jones]	Noted. Text will be reformulated
11-259	A	7:19	7:20	What about projects such as MICE and STARDEX? [Markku Rummukainen]	Noted. Text will be reformulated
11-260	A	7:19		What about the efforts of the STARDEX FP5 project in Europe - something about this should be mentioned here! They did much work on the inter-comparison of statistical methods for downscaling.	Noted. Text will be reformulated

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				[Hayley Fowler]	
11-261	A	7:20		It is also worth mentioning the development of statistical-dynamical downscaling methods (these are a development since TAR). Using the outputs of RCMs but ith an intermediate statistical downscaling step for use in impact studies. This has now been used by many groups and has been found to improve on statistical downscaling from GCMs and also improves upon the direct use of dynamically downscaled outputs from RCMs, even when these have been bias-corrected. [Hayley Fowler]	Noted. Text will be reformulated if appropriate papers will be found; unfortunately, the reviewer does not suggest any paper.
11-262	A	7:22		Downscaling techniques have also been developed by the weather forecasting community and are now routinely used in this regard. These methods could also be used by the wider climate community. [Hayley Fowler]	Noted. Comment will be considered.
11-263	A	7:24	7:24	24 An additional reference on comparing statistical and dynamical downscaling: Wilby, R. L., L. E. Hay, W. J. Gutowki, Jr., R. W. Arritt, E.S. Takle, G. H. Leavesley, and M. Clark, 1999: Hydrological responses to dynamically and statistically downscaled general circulation model output. Geophys. Res. Lett. 27, 1199-1202. [Eugene Takle]	Noted. Reference will be considered
11-264	A	7:24	7:24	An additional reference on comparing statistical and dynamical downscaling: Wilby, R. L., L. E. Hay, W. J. Gutowki, Jr., R. W. Arritt, E.S. Takle, G. H. Leavesley, and M. Clark, 1999: Hydrological responses to dynamically and statistically downscaled general circulation model output. Geophys. Res. Lett. 27, 1199-1202. [Eugene Takle]	Noted. Reference will be considered
11-265	A	7:24	7:24	Add "Caires et al., 2005" (see Comment #58 below) after "Methrotra et al., 2004", because this study assesses the utility of different non-stationary extreme value models (GEV and GPD) for making projections of climate extremes. [Xiaolan L. WANG]	Noted. Reference will be added if space allows
11-266	A	7:26	7:26	Start new sentence at "This". [Bart Van den Hurk]	Noted
11-267	A	7:30	7:30	The title should be changed to "Assessment of Methods to Provide Regional Climate Information" because there is more than projections that are assessed. [Daniel Caya]	Suggestion noted
11-268	A	7:30	20:9	This section is too large and contains unnecessary details - must be drastically reduced. [Murari Lal]	Will be reduced in SOD, particularly on SDM
11-269	A	7:30		Section 11.1.3 should be merged in section 11.2. Info on GCMs, RSMs and Epirical downscaling developments since the TAR can be added in each related sub-sections of 11.2. This will prevent the impression of repetition while reading the text.	To be considered by CLAs

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				[Daniel Caya]	
11-270	A	7:34	7:39	The discrimination between projections and scenarios should be made: in general dynamical downscaling is associated to projections and empirical downscaling to scenarios (where observations arer used in conjonction with model output). [Daniel Caya]	Noted
11-271	A	7:34	7:39	I suggest to revise this paragraph, because is not clear. [CONSTANTIN MARES]	Will
11-272	A	7:37		Spelling: "dynamical" [Hayley Fowler]	Noted
11-273	A	7:42	7:43	Suggested revision to read: General circulation models that couple the global atmosphere and land-surface with ocean and sea-ice components (CGCMs) .... [Timothy Carter]	Noted
11-274	A	7:42		Global ---> Coupled [Yasuo Sato]	Noted
11-275	A	7:43	7:43	"cornerstone" is one word [James Renwick]	Noted
11-276	A	7:49	7:49	miroc_3.2_hires has T106 (refereed to as "roughly 100km" in Chap.10, p.29, l.56). [Masahide Kimoto]	But 2*delta_x at equator equals 200 km, not 100 km
11-277	A	7:50		FOOTNOTE dependency ---> dependence [Yasuo Sato]	Noted
11-278	A	8:0		11.2.1.3. Very recently there have been some attempts at two-way nesting between GCMs and RCMs. Two-way nesting could be an extraordinarily important development. [Raymond Arritt]	Noted (refers to 9:25, not 8:0); will reference Lorenz and Jacob, 2005
11-279	A	8:3	8:3	"robustness of climate change response". I'm not clear what is being said here. By a robust response, do we mean a statistically significant change, or do we mean a response similar to that of other models ? In either interpretation, it is far from obvious why a robust response should be seen as a criterion in the weighting of models. If our aim is to quanify uncertainty realistically, for example, I think it is dangerous to assume that an outlying ensemble member should be automatically weighted down. [James Murphy]	Point noted. No consensus of interpretation on this issue. An additional paragraph will be added in SOD to try to elaborate on what is meant.
11-280	A	8:4	8:4	(Giorgi and Mearns 2002,2003, Workshops of AIACC projects (http://www.aiaccproject.org) [MARIO BIDEGAIN]	"Grey literature is to be avoided when referred literature is available"
11-281	A	8:6	8:8	Experience from short-range NWP suggests that large spread is not necessarily a bad thing. An important goal of ensemble modeling is to assess the range of physically plausible outcomes from a given situation, not just to provide a skillful mean forecast. In	Should not confuse spread in deterministic solutions (as in NWP) that reflect natural, internal variability,

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				fact it is generally the case that ensembles are under-dispersive, that is, they do not give as broad a range of possible outcomes as exists in nature. [Raymond Arritt]	from spread in climate states that reflect different responses to imposed forcing.
11-282	A	8:6	8:7	A large spread may be associated to regions with important feedbacks. Therefore a large spread does not mean that this information cannot be used. [Daniel Caya]	Noted
11-283	A	8:7	8:7	The "quixotic" reference seems to repeat the theme that downscaling is pointless until a convergence of GCM responses can be demonstrated. I fail to see why. If downscaling makes a significant impact on either the mean or the spread of responses, it is surely important information for impact assessments, regardless of the underlying spread of the driving model ensemble. [James Murphy]	Downscaling is not intended to change the mean (although it may under some circumstances), but to add small-scale details ONTO large-scale solution from CGCMs
11-284	A	8:10	8:10	Not all impact studies need high resolution information; indeed it can be overwhelming in volume and quite unnecessary for some applications. [Timothy Carter]	Surprising comment, particularly from this reviewer! The point is that CGCMs may simply not see phenomena that impact studies require (e.g. stream flow in small-scale basin)
11-285	A	8:10	8:24	In my opinion this section could be removed. Relevant parts of it may go into 11.2.1.2 and 11.2.1.3 and 11.2.1.5 [Erik Kjellström]	The last 2/3 will be moved below in SD section.
11-286	A	8:10	8:24	This paragraph strays away from the stated topic of CGCMs. Perhaps include later? [James Renwick]	See reply to 11-285
11-287	A	8:12	8:12	scenarios -> projections [Daniel Caya]	Noted
11-288	A	8:14	8:15	While it is true that RCMs are based on a representation of dynamics that is physical based, the parameterisation schemes (perhaps misleadingly called 'model physics' as opposed to 'model statistics') still are empirical/statistical bulk formulae that are not necessarily physical nor tuned for each specific location and there are still problems dealing with the boundary layer representation or gravity wave drag (unphysical tuning). I think this sentence could be dropped because it's a bit misleading. [Rasmus E. Benestad]	See reply to 11-285 The degree of empiricism is not as large in dynamical model parameterisations, which are meant to react to the state of ambient variables. The fact that CGCMs can faithfully reproduce widely varying climates around the world lend confidence in their ability to handle climate changes.
11-289	A	8:14	8:18	The sentence should read: "The main advantage of dynamical downscaling approach (AGCM, RCM) is that it is physically based. Dynamical downscaling has the potential for providing added value particularly for capturing nonlinear effects under perturbed forcing conditions and to provide coherent information between multiple climate variables. Their	See reply to 11-285

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				main drawback is computational cost. [Daniel Caya]	
11-290	A	8:14	8:15	The statement is made that the main advantage of dynamical downscaling is that it provides added value, but surely the developers of statistical methods would make the same claim. [James Murphy]	See reply to 11-285
11-291	A	8:16		such as orography, sharp land-sea, ocean current distribution or land-use contrasts [Hidetaka Sasaki]	Noted
11-292	A	8:17	8:17	My suggestion is to replace " capturing nonlinear effects (such as mesoscale circulations)" with " capturing mesoscale nonlinear effects". [CONSTANTIN MARES]	Noted
11-293	A	8:18	8:24	Is this sentence on empirical methods needed in this paragraph ?. I suggest to create a table (or a specific paragraph) with the advantages and desadvantages of all the methods (two columns : advantages and desadvantages, and as many lines as colums) [ERIC MARTIN]	See reply to 11-285
11-294	A	8:19	8:20	Empirical downscaling should reflect a real physical association – otherwise it's useless. The difference between RCMs and empirical downscaling is not that one is more 'physical' than the other, but rather that the former is expressed in more analytical forms based on primitive equations (and then there is parameterisations). I suggest this point is also taken in the description – people often get the strange idea that a statistical association is 'unphysical'. Also, one should stress tha assumption that the statistical relationships used in parameterisations/tuning used in GCMs/RCMs also prevail under a perturbed climate. [Rasmus E. Benestad]	See reply to 11-285
11-295	A	8:22	8:24	While the statement is true, it is also true that climatic parameters at nearby locations often are not independent and that a geographical information system (GIS) approach actually can give information about the surrounding conditions (see Benestad, 2004, 2005 [GRL doi:10.1029/2005GL023401]) [Rasmus E. Benestad]	See reply to 11-285. One can wonder whether, when GIS interpolation is used instead of station data, whether there is really subgrid-scale information added by SDM.
11-296	A	8:26	8:44	More should be said here and elsewhere (at relevant locations) on the new findings obtained from very high resolution earth simulator experiments (particularly on the extreme events, cyclones etc.) [Murari Lal]	Results of Earth Simulator to be added
11-297	A	8:27	8:27	The models do not change their resolution - the modellers do that! [Timothy Carter]	Thanks
11-298	A	8:27	8:35	A rosy picture is painted of progress in the capability of global high resolution time slice	References and results of Earth

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				simulations, yet most of the references quoted are either old, missing or unreviewed grey literature. It is not terribly clear that the community is really taking up this approach with much enthusiasm. [James Murphy]	Simulator to be added
11-299	A	8:29	8:31	This statement is not true for some low frequency land-surface variable (frozen moisture content in northern regions). [Daniel Caya]	But land-surface variables are interactive in AGCMs (not prescribed) just as in CGCMs
11-300	A	8:34	8:35	This sentence may be replaced with one paragraph on very high-resolution AGCM such as: A time-slice global warming projection with 20 km resolution AGCM has been conducted with the aid of one of the largest existing computational resources (Mizuta et al. 2005b). This model better represents tropical cyclones than other existing models (Oouchi et al. 2005), and is more suitable for analysis of extreme temperature and precipitation indices and extreme events (Mizuta et al. 2005a; Hosaka et al. 2005). The model also presents suitable boundary conditions for much higher resolution regional climate model (RCM). Hosaka, M., D. Nohara, and A. Kitoh, 2005: Changes in snow cover and snow water equivalent due to global warming simulated by a 20km-mesh global atmospheric model. SOLA, 1, 93-96. Mizuta, R., T. Uchiyama, K. Kamiguchi, A. Kitoh and A. Noda, 2005a: Changes in extremes indices over Japan due to global warming projected by a global 20-km-mesh atmospheric model. SOLA, accepted. Mizuta, R., K. Oouchi, H. Yoshimura, A. Noda, K. Katayama, S. Yukimoto, M. Hosaka, S. Kusuunoki, H. Kawai and M. Nakagawa, 2005b: 20km-mesh global climate simulations using JMA-GSM model -mean climate states-. J. Meteor. Soc. Japan, submitted. Oouchi, K., J. Yoshimura, H. Yoshimura, R. Mizuta, S. Kusunoki and A. Noda, 2005: Tropical cyclone climatology in a global-warming climate as simulated in a 20km-mesh global atmospheric model. J. Meteor. Soc. Japan, submitted. [Akio Kitoh]	Noted
11-301	A	8:34		computational resources(Earth-simulator) [Yasuo Sato]	Noted
11-302	A	8:35	8:32	Please give an explanation for "(ref)" [CONSTANTIN MARES]	Will do
11-303	A	8:35	8:35	The "ref" is: Mizuta, R., K. Oouchi, H. Yoshimura, A. Noda, K. Katayama, S. Yukimoto, M. Hosaka, S. Kusuunoki, H. Kawai and M. Nakagawa, 2005: 20km-mesh global climate simulations using JMA-GSM model -- mean climate states --. J. Meteor. Soc. Japan, submitted. (to be accepted in Nov. 2005)	Noted

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				[Akira Noda]	
11-304	A	8:35	8:35	A reference to the Japanese Earth Simulator would fit in nicely here [Bart Van den Hurk]	Will do
11-305	A	8:35	11:35	Last word "ref" should be replaced by "Mizuta et al., 2005a" [Hiroki Kondo]	Will do
11-306	A	8:35		a 20 km resolution(Mizuta et al., 2005) [Yasuo Sato]	Noted
11-307	A	8:35		Not forget the references [Ibouraïma YABI]	Will do
11-308	A	8:37	8:37	Lehmann, A., Ph. Lorenz, D. Jacob (2004): Exceptional Baltic Sea inflow events in 2002 - 2003. Geophysical Research Letters, Vol.31, L21308, doi:10.1029/2004GL 020830 [Daniela Jacob]	Noted
11-309	A	8:37	8:37	I suggest to replace " dramatic" with "important". [CONSTANTIN MARES]	Noted
11-310	A	8:41	8:42	"nearly all quantities simulated by higher resolution models agree better with observations". This is in danger of giving the impression that higher resolution is the solution to all model biases, and thereforeneeds to be more carefully qualified. Actually, Duffy et al show that nearly all quantities improve in terms of global averages of local errors, but the effect on skill actually varies significantly with region - some regions improve and some get worse. Also, the physics needs to be retuned to avoid making some biases worse when resolution is increased. This needs to be emphasised too. [James Murphy]	Noted
11-311	A	8:44	8:44	Please insert "Bengtsson, 1995" in section " References". [CONSTANTIN MARES]	Noted
11-312	A	8:44	8:44	The "earth simulator ref" is: Oouchi, K., J. Yoshimura, H. Yoshimura, R. Mizuta, S. Kusunoki and A. Noda, 2005: Tropical cyclone climatology in a global-warming climate as simulated in a 20km-mesh global atmospheric model. Submitted. (to be accepted in Nov. 2005 or Jan. 2006) [Akira Noda]	Noted
11-313	A	8:44		Not forget the references [Ibouraïma YABI]	Will do
11-314	A	8:46	8:53	The potential problem of inconsistencies between the surface fluxes and the prescribed ocean boundary conditions in AGCM experiments is rightly mentioned, but evidence needs to be provided to support the statement at the end of the paragraph, which suggests without proof that we don't need to worry too much about this issue in practice. If no evidence is available, we just have to admit that no progress on this has been made since	Will provide a reference

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				the TAR. [James Murphy]	
11-315	A	8:48	8:49	The following two references should fit in for this sentence. Douville, H.,2005: Limitations of time-slice experiments for predicting regional climate change over South Asia, Clim. Dyn., 24, 373–391. Inatsu, M., and M. Kimoto, 2005: Difference of boreal summer climate between coupled and atmosphere-only GCMs. SOLA, 1, 105-108, doi: 10.2151/sola. 2005-028. [Seita Emori]	Thanks
11-316	A	8:55	9:13	Are there any coupled VRGCMs? I am not aware of any. Could be worth mentioning either way. [Raymond Arritt]	Not aware of
11-317	A	8:55	9:56	Krinner et al. 1997 (Studies of the Antarctic climate with a stretched grid GCM, J. Geophys. Res. 102, 13731-13745) was one of the earliest development and use of a VRGCM, with recent follow-on for paleoclimate (Krinner et al. 2004: Enhanced ice sheet growth in Eurasia owing to adjacent ice-dammed lakes, Nature 427, 429-432) and IPCC-related future (Krinner et al.: simulated Antarctic precipitations and surface mass balance at the end of the 20th and 21st centuries, Climate Dynamics, submitted) climates. A copy of the latter manuscriptr, only submitted at this time, can be obtained from the main author (krinneràlgge.obs.ujf-grenoble.fr) or myself. [Christophe Genthon]	Noted
11-318	A	8:55	9:1	Add reference - Lal et al. (2005). The citation is "Simulation of surface air temperature and rainfall and its temporal and spatial distribution over Fiji, Climate Dynamics – Communicated, October 2005 (Murari Lal, John L. McGregor and Kim C. Nguyen)" [Murari Lal]	Noted
11-319	A	8:55		The problem of the ocean should be addressed in the paragraph on VRGCM. [Daniel Caya]	Noted
11-320	A	9:3	9:3	change "including" to "usually including" [John McGregor]	Noted
11-321	A	9:13	9:13	add "The VRGCM approach is also being used with large stretching factors (McGregor, 2004) to achieve 14 km resolution, in conjunction with weak nudging from a prior CGCM simulation." [John McGregor]	Noted
11-322	A	9:15	9:24	In addition to land surface processes, it would be mentioning the impact of higher resolution on the hydrological cycle (sharper fronts, stronger vertical motion, more precip, less cloud) which have been documented in many studies, and imply a need to retune the moist physics when resolution is increased. [James Murphy]	Noted

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11-323	A	9:20	9:20	Include the statement "Snow melt processes have a large impact on the near surface temperature variability" at the end of the sentence. [Bart Van den Hurk]	Noted
11-324	A	9:22	9:23	Is there a reference for this statement? [Erik Kjellström]	Will find a reference
11-325	A	9:25		Section 11.2.1.3: While this is nicely written, it seems too long and detailed for AR4. Try to summarise the main points without getting into too much detail about boundary issues etc. [James Renwick]	Will try to shorten a bit. But LBC are an issue in RCMs since they are a free "arbitrary" aspect of the approach to nested models, that have been under attack for this reason (WGNE…)
11-326	A	9:27	9:27	Delete "the most" [Daniel Caya]	Noted
11-327	A	9:30	9:31	Delete "(winds, temperature and moisture data), supplied either by analyses or GCMs," [Daniel Caya]	Disagree; non experts do not know well which variables are meant (e.g. clouds, precipitation are not nested)
11-328	A	9:31	9:31	BC, (2) -> BC; (2). Same modification for 2 and 3. [Daniel Caya]	Noted
11-329	A	9:31	9:31	Insert before (2) "(2) lateral BC exert sufficient control on the RCM large-scale circulation to keep it consistent with the driving large-scale atmospheric circulation;" [Daniel Caya]	Noted
11-330	A	9:31	9:32	Re-number "(2)" and "(3)" to "(3)" and "(4)". [Daniel Caya]	Noted
11-331	A	9:33		orography, land-sea contrast, ocean current and sea ice distribution, land use [Yasuo Sato]	Noted
11-332	A	9:34	9:38	An example of two-way nesting is provided by "Lorenz, P. and D. Jacob, 2005. Influence of regional scale information on the global circulation: a two-way nesting climate simulations. Geophys. Res. Lett. 32, L18706, doi:10.1029/2005GL023351." [Markku Rummukainen]	Noted
11-333	A	9:34		Write… Was realised (Dicknson et al, (1989); Giorgi and Bates, (1989). (it is a proposal for a formulation) [Ibouraïma YABI]	Do not understand the point
11-334	A	9:36	9:38	There is a recent paper in GRL by Lorenz and Jacob describing a two-way nesting experiment. [Erik Kjellström]	Noted (comment 11-332)
11-335	A	9:38	9:38	Change "model state" by "information". [Daniel Caya]	Noted

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11-336	A	9:38	9:38	An exception is a study by Lorenz and Jacob [Bart Van den Hurk]	Noted (comment 11-332)
11-337	A	9:38	9:38	An exception is a study by Lorenz, P. and D. Jacob (2005): Influence of regional scale information on the global circulation: A two-way nesting climate simulation; GRL 32, L18796; doi 10.1029/2005GL023351 [Bart Van den Hurk]	Noted (comment 11-332)
11-338	A	9:40	10:3	The term "nudging" is unfortunate as it implies a form of "fudging" or "cheating". It would be valuable to emphasise strongly the scientific value of such a process in its explanation. [Roger Jones]	Nudging is the term that the designers chose. And it is still considered partly cheating by several scientists…
11-339	A	9:42	9:42	Davies 1976). -> Davies 1976; Laprise 2002). Laprise, R. 2002: Resolved scales and nonlinear interactions in Limited-Area Models. J. Atmos. Sci., 60(5), 768-779. [Daniel Caya]	Noted
11-340	A	9:45	9:45	boundary-value problem. -> boundary-value problem (Staniforth 1997). Staniforth, A. 1997: Regional modellin: a theritical discussion. Meteorol. Atmos. Phys., 63, 15-29. [Daniel Caya]	Noted
11-341	A	9:47	9:47	A reference is required after "weather regime". [Daniel Caya]	Will try
11-342	A	9:47		add reference to Castro, C.L., R.A. Pielke and G. Leoncini, 2005: Dynamical downscaling: Assessment of value retained and added using the Regional Atmospheric Modeling System (RAMS). Journal of Geophysical Research 110, D05108, doi:10.1029/2004JD004721. [Raymond Arritt]	Noted
11-343	A	9:48	9:48	season; -> season (Caya and Biner 2004); [Daniel Caya]	Noted
11-344	A	9:48	9:48	A reference is required after "through the domain". [Daniel Caya]	Will try
11-345	A	9:52	9:52	Insert "This divergence in phase space has limited impact on climate statistics (Caya and Biner 2004)" after "with global models." [Daniel Caya]	Noted
11-346	A	9:52	9:53	References after "RCMs" appear from "Kida et al." which is the first of this group's paper, but a note. Their full paper after that had better be added. Suggested modification starting from "RCMs" would be as follows: …RCMs (Kida et al. 1991; Sasaki et al., 1995; Waldron et al., … [Hiroki Kondo]	Noted
11-347	A	9:53	9:53	Replace "Biner et al. (2000)" by "Riette and Caya (2002)". Riette, S. and D. Caya, 2002:	Noted

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				Sensitivity of short simulations to the various parameters in the new CRCM spectral nudging. Research activities in Atmospheric and Oceanic Modelling, edited by H. Ritchie, WMO/TD - No 1105, Report No. 32: 7.39-7.40 [Daniel Caya]	
11-348	A	9:53		Mabuchi et al.(2000,2004) should be referred as spectral RCMs using "large-scale nudging". The former one is in WG1-TAR. [Hidetaka Sasaki]	Noted
11-349	A	9:55	10:3	What about disadvantages of applying large-scale nudging (or an explanation why it is hardly used in RCM-projections with BCs from GCMs)? [Markku Rummukainen]	Will do
11-350	A	10:0	11:	The statement that lateral boundary conditions (LBC) are very important in determining regional model results is true, but this fact was known well before the cited references, which again are all after 2000. In a 1983 review paper on regional models and their sources of errors in the Monthly Weather Review I wrote: "Lateral boundary conditions are undoubtedly a major source of errors in regional models." This was shown decisively in a 1985 paper in Advances in Geophysics, which showed the relative contribution of initial conditions and LBC in determining the solutions in regional models (Anthes et al., 1985). The conclusion to this paper says: "The most important practical result suggested by these experiments is that meso-?-scale models depend critically on accurate specification of the large-scale atmospheric variables at the lateral boundaries. For these simulations, minor differences in initial conditions on the mesoscale had no significant impact on the forecasts out to 72 hr." [Richard Anthes]	Noted
11-351	A	10:0		These experiments were done, by the way, with the Penn State-NCAR mesoscale model, which was the basis for the regional climate model developed by F. Giorgi and his colleagues. [Richard Anthes]	Known
11-352	A	10:0		Anthes, R. A., 1983: A review of regional models of the atmosphere in middle latitudes. Monthly Weather Review, 111(6), 1306-1335. Anthes, R. A., Y.-H. Kuo, D. P. Baumhefner, R. M. Errico, and T. W. Bettge, 1985: Predictability of mesoscale atmospheric motions. Contribution to "Issues in Atmospheric and Oceanic Modeling," Advances in Geophysics, Vol. 28B, 159-202. [Richard Anthes]	Noted
11-353	A	10:5		One paragraph on very high-resolution RCM may be added here: A cloud resolving non-hydrostatic regional model (NHM) with a horizontal grid size of 5 km is now successfully used for global warming simulation in East Asia (Kanada et al.	Will consider

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				2005; Yoshizaki et al. 2005). This model was run in June and July for ten years, applying a spectral boundary coupling method to the outputs of a global climate model with a grid size of 20 km. Although the changes of the Baiu front are similar to those of the AGCM, the NHM more realistically simulates precipitation amount and structures of clouds. It is found that the frequency of occurrence of heavy rainfalls greater than 30 mm/hr increases over the Japan Islands. Kanada, S., C. Muroi, Y. Wakazuki, K. Yasunaga, A.Hashimoto, T. Kato, K. Kurihara, M. Yoshizaki and A. Noda 2005: Structure of mesoscale convective systems during the late Baiu season in the global warming climate simulated by a Non-hydrostatic Regional Model. SOLA, 1, 117-120. Yoshizaki, M., C. Muroi, S. Kanada, Y. Wakazuki, K. Yasunaga, A. Hashimoto, T. Kato, K. kurihara, A. Noda and S. Kusunoki, 2005: Changes of Baiu (Mei-yu) frontal activity in the global warming climate simulated by a non-hydrostatic regional model. SOLA, 1, 25-28. [Akio Kitoh]	
11-354	A	10:7	10:7	It is worth emphasising that the Big Brother experiment is a perfect model test of the methodology of nested regional modelling. There is no validation of the fine scale signal against observations. [James Murphy]	Indeed
11-355	A	10:7		de Elia et al.,2002 is missing in the references. [Hidetaka Sasaki]	Thanks
11-356	A	10:9	10:9	Replace "with a rms means of error" by "on a day by day base" (or something more meaningful) [Bart Van den Hurk]	Will do
11-357	A	10:14	10:14	Worth mentioning that the Vidale et al study is based on observed lateral boundary forcing. [James Murphy]	Thanks
11-358	A	10:17	10:19	Delete the setence. [Daniel Caya]	Will move the sentence
11-359	A	10:20	10:22	Measured by which criteria? (I.e. define: satisfy.) [Markku Rummukainen]	Will specify
11-360	A	10:22	10:22	12 hours. -> 12 hours (Denis et al. 2003). [Daniel Caya]	Noted
11-361	A	10:24	10:35	This paragraph mentions a couple of specific examples of GCM errors, but readers should be pointed to Chapter 8 for a more comprehensive review. [James Murphy]	Noted

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11-362	A	10:25		de Elia et al.,2006 is missing in the references. [Hidetaka Sasaki]	Noted
11-363	A	10:30	10:30	RCMs" -> "RCM [Bart Van den Hurk]	Do not understand
11-364	A	10:30	:31	The fact that the regional model scenarios are mostly based on earlier global model projections (TAR), NOT AR4 coupled models, should be emphasized. There is a statement on page 10, but with no discussion of the implications. [Anji Seth]	Noted
11-365	A	10:37	11:3	The characterization of this method as off-line is incorrect. The Goyette and Laprise scheme is offline, but the Leung and Ghan scheme is actually fully coupled with the atmosphere and land surface models during the climate simulation, feeding the gridcell mean of the heating rates back to the gridcell dynamics. Please remove the text "off-line" from lines 37 and 39, change "PBOLD" to "PBD" throughout this section, insert "either during or" before "from a prior" on line 40, and replace "neglected" with "feedback" on line 56. [Steven Ghan]	Will correct
11-366	A	10:37		PBOLD in section 11.2.1.4 is a methodology than is marginally used and not very relevant for policy makers. I would removed the whole section. [Daniel Caya]	Not everyone would agree
11-367	A	10:42	10:42	Ghan et al., 2002 is not in " References" and " 2006" probable is a mistake. [CONSTANTIN MARES]	Noted
11-368	A	10:43	10:43	Leung and Ghan, 2005 must be specified in "References". [CONSTANTIN MARES]	Noted
11-369	A	11:1	11:1	Say what CAM2 is. [James Murphy]	Will define
11-370	A	11:1	11:1	"PDOLD" should be "PBOLD"? [James Renwick]	Noted
11-371	A	11:1	11:1	PDOLD" -> "PBOLD [Bart Van den Hurk]	Noted
11-372	A	11:1		What is CAM2? [Yasuo Sato]	Will define
11-373	A	11:7	11:7	and -> to [Daniel Caya]	Noted
11-374	A	11:13	11:32	The benefits of SD techniques listed include that they are inexpensive and can provide information on variables not provided by RCMs (some of the same information is presented in section 11.1.3.3). However, another very crucial benefit is that they may (at	Noted. Comment will be included if the space allows and appropriate examples will be found.

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				least potentially) be able to correct for some of the biases coming from the global models. This should be stated. In addition, if examples of SD bias correction exist they should be presented, at least as an example in section 11.2.1. [Michael Alexander Alexander]	
11-375	A	11:13		Write… inexpensive, edge… (space enters the two words) [Ibouraïma YABI]	Agreed
11-376	A	11:17	11:21	Delete "Important elements…climate-change signal". [Daniel Caya]	Agreed. Text deleted.
11-377	A	11:17	11:18	Missing Ref: Lorenz, Ph.; D. Jacob (2005): Influence of regional scale information on the global circula-tion: A two-way nesting climate simulation, Geophysical Research Letters, 2005GL023351 In addition, two way nesting techniques have been developed,allowing the the higjly resloved flow to feed bakc to the larg scale flow (Lorenz and Jacob, 2005) [Daniela Jacob]	Rejected. This reference is not appropriate for this section.It could be addressed in 11.2.1.3
11-378	A	11:18	11:18	Punctuation not properly used. [Marina Baldi]	Noted. Text deleted for brevity.
11-379	A	11:18	11:18	include: the predictors" instead "include; The predictors [Roxana Bojariu]	Noted. Text deleted for brevity
11-380	A	11:18	11:18	Replace the semi-colon in "include;" with a colon. [Xiaolan L. WANG]	Noted. Text deleted for brevity
11-381	A	11:20	11:20	How can non-stationarity be accommodated given that we can't know in advance what the impact of climate feedbacks will be on the statistical relationships ? [James Murphy]	Text deleted for brevity
11-382	A	11:21	11:22	Should give examples. [Rasmus E. Benestad]	Text deleted for brevity
11-383	A	11:25	11:25	Not only natural variability (variance) should be captured well, also higher order temporal correlation statistics must be derived from the training data, as these have a major impact on extreme events covering multiple days. [Bart Van den Hurk]	Noted. Text will be reformulated
11-384	A	11:26	11:26	Change to "Important developments have been made in SD research since the TAR, reflecting a maturing…" [James Renwick]	Noted.
11-385	A	11:27	11:28	Could also refer to the "clim.pact" package under "R" (Benestad RE (2004b) Empirical-statistical downscaling in climate modeling. EOS 85(42):417) [Inger Hanssen-Bauer]	Notet. Reference will be included if space allows.
11-386	A	11:30	11:30	After "Seem, 2004", add "; Leung et al., 2004". Making use of the global model grid	Notet. Reference will be included if

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				point data available from the IPCC Data Distribution Centre and the regression method, Leung et al. (2004) have made projections on the statistics of occurrence of days with extreme temperatures in Hong Kong. The full reference is given in comment # 23. It can be accessed online at http://www.weather.gov.hk/publica/reprint/r608.pdf. [Chiu-Ying LAM]	space allows
11-387	A	11:30	11:30	I think "Wang et al., 2003" here refers to "Wang and Zwiers and Swail, 2004" (to be listed as "Wang et al., 2004b"; see Comment #62 below), because there is no "Wang et al., 2003" listed in the reference section (page 122-123). Anyway, "Wang et al., 2004b" and "Wang and Swail, 2005a and 2005b" (see Comments #59-61 below) should be cited when talking about downscaling climate extremes. [Xiaolan L. WANG]	Noted.
11-388	A	11:31	11:31	"Caires et al., 2005" (see Comment #58 below) should be added after "e.g., STARDEX", because this study is also an inter-comparison study evaluating statistical methods (the use of non-stationary GEV and GPD models for making projections of climate extremes). [Xiaolan L. WANG]	Notet. Reference will be included if space allows
11-389	A	11:33	11:34	There are "Hewitson and Crane, 2005a" and "Hewitson and Crane, 2005b" listed in lines 43-46 on page 108, but no "Hewitson and Crane, 2005" in the reference section. So, all citations to "Hewitson and Crane, 2005" should be corrected accordingly (including many places on page 12). [Xiaolan L. WANG]	Noted . Text will be corrected.
11-390	A	11:33	11:33	"Wang and Swail, 2005b" (see Comment #61 below) should be added after "Hewitson and Crane, 2005", because this study is also about downscaling from multi-model and multi-ensemble simulations in order to characterize climate-model and forcing-scenario uncertainties. [Xiaolan L. WANG]	Notet. Reference will be included if space allows
11-391	A	11:34	11:34	Should add spatial interpolation based on GIS-approach utilising geographical dependencies. [Rasmus E. Benestad]	Comment added if space allows
11-392	A	11:36	11:37	Unclear sentence [Inger Hanssen-Bauer]	Text will be reformulated
11-393	A	11:40	11:41	I think the sentence "In most cases this will…" is correct when precipitation is the predictand, but not necessarily for other predictands. [Inger Hanssen-Bauer]	There is not such a sentence in this page and lines.
11-394	A	11:40		I would remove the title for this sub-section [Daniel Caya]	Noted
11-395	A	11:41	:53	In this paragraph, could add citation to Vrac et al 2005 (Climate Dynamics, submitted,	References included if space allows and

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				also attached) "Statistical downscaling of precipitation through a nonhomogeneous stochastic weather typing approach" where daily precipitation values for Illinois were downscaled using both upper-air circulation and suface-level precipitation patterns to produce values that represent a significant improvement over earlier upper air or surface-only methods. [Katharine Hayhoe]	the paper will be accepted for publication in time.
11-396	A	11:45	11:45	Replace "Lionello et al., 2003) and singular value" with "Lionello et al., 2003), redundancy analysis (Wang et al. 2004b, Wang and Swail 2005a), and singular value", because redundancy analysis has also been used to make climate projections. [Xiaolan L. WANG]	Noted
11-397	A	11:50	:52	Write (… Asin, 2005; Beckman and Buishand, 2002; Buishand et al., 2004; … storch, 2003; … Willby, 2005; … Al, 2004; … Swail, 2004; …) (Problem of punctuation). [Ibouraïma YABI]	Noted
11-398	A	11:52	11:52	Update "Wang et al., 2004" to "Wang et al. 2004b" (see Comment #62 below). [Xiaolan L. WANG]	Noted
11-399	A	11:52	11:52	Replace "Wang and Swail, 2005" with "Wang and Swail, 2005a and 2005b" (see Comments #60-61 below). [Xiaolan L. WANG]	Noted. Text reformulated. New references will be included only if space allows.
11-400	A	11:52	11:52	Suggest insert the following between "..., Pryor et al., 2005)." and "The main weaknesses…": In particular, regressions combined with non-stationary extreme value models (GEV and GPD) have been used to make projections of climate extremes (Wang et al., 2004b; Wang and Swail, 2005a and 2005b; Caires et al. 2005)." [Xiaolan L. WANG]	Noted. Text reformulated. New references will be included only if space allows.
11-401	A	12:3	12:5	The sentence is unclear. [Daniel Caya]	Noted. Text reformulated
11-402	A	12:3		Also weather generators developed by Fowler et al for statistical downscaling of GCM information for impact studies: see reference: Fowler, H.J., Kilsby, C.G., O'Connell, P.E., and Burton, A. 2005. A weather-type conditioned multi-site stochastic rainfall model for the generation of scenarios of climatic variability and change. Journal of Hydrology, 308(1-4), 50-66. [Hayley Fowler]	Noted. Reference will be added if space allows.
11-403	A	12:4	12:4	delete "WGs are" [Bart Van den Hurk]	Text deleted
11-404	A	12:11	12:24	This paragraph needs considerable edition. It is very hard to understand. [Daniel Caya]	Noted. Text will be reformulated
11-405	A	12:11	12:24	Please revise this paragraph, because is not clear. For instance analogue is an objective	Noted. Text will be reformulated

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				method and also analogue method is not "an extreme form of the weather typing". [CONSTANTIN MARES]	
11-406	A	12:16	12:16	Remark: as the definition of a climate change is a change in the pdf, then one perhaps needs to explain how a resampling of a pdf for the present can be used to describe an aletered pdf. [Rasmus E. Benestad]	Noted. Text will be reformulated
11-407	A	12:16	12:19	I have not seen Hewitson and Crane (2005), but I wonder if the fact that climate change is estimated using the frequency of circulation patterns is really an advantage. Circulation is only one of many influences on regional changes in GCMs (e.g. Rowell and Jones 2005), so could it be that the convergence which is seen is a consequence of ignoring some of the drivers of regional uncertainty ? [James Murphy]	Noted. Text will be reformulated.
11-408	A	12:21	12:21	Insert "." after "2005)" [Bart Van den Hurk]	Noted. Text will be reformulated
11-409	A	12:21		Write (… 2005). Year… (space and punctuation) [Ibouraïma YABI]	Noted.
11-410	A	12:22	12:22	It seems as "11.2.1.7" is meant rather than "11.2.1.6". [Markku Rummukainen]	Noted
11-411	A	12:29	12:29	Sds" -> "SDs [Bart Van den Hurk]	Noted
11-412	A	12:33	12:45	Delete the whole paragraph. [Daniel Caya]	Noted. Text deleted
11-413	A	12:35	12:35	Benestad, 2002' should be 'Benestad, 2002b' [Rasmus E. Benestad]	Noted. Paragraph deleted.
11-414	A	12:36	12:37	The sentence "These studies have highlighted … and tuning." does not read well. Maybe "… in the same manner as care is needed in RCM …"? [Xiaolan L. WANG]	Noted. Text deleted.
11-415	A	12:37	12:37	in" -> "is [Bart Van den Hurk]	Noted. Text deleted.
11-416	A	12:43	12:43	delete "for" at the end [Bart Van den Hurk]	Noted. Text deleted
11-417	A	12:43	12:43	Delete the last word "for" in this line? [Xiaolan L. WANG]	Noted. Text deleted
11-418	A	12:44	12:44	A recent study examining the use of GCM precipitation for daily precipitation statistics shows that the improvement in skill is comparable to that from RCMs (Schmidli et al. 2005). Schmidli, J., C. Frei and P.L. Vidale, 2005: Downscaling from GCM precipitation:	This paragraph was deleted.

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				A benchmark for dynamical and statistical downscaling methods. Int. J. Climatol., (in press). [Christoph Frei]	
11-419	A	12:50	12:50	Replace "Hansen Bauer et al." with "Hansen-Bauer et al.". [Xiaolan L. WANG]	Noted.
11-420	A	12:52	12:56	Remark: The question of stationarity in empirical relationship is probably not the bottle neck, but rather the question of whether the GCMs provide a representative description of the climate evolution on a regional/continental scale or whether the parameterisations used in GCMs are representative given a different climatic state. The question of stationarity can to some degree be examined through the relationship between large and small scales in GCM results themselves as well as in historical observations. [Rasmus E. Benestad]	There is not a clear proposal to improve the text;
11-421	A	12:54	12:54	Sds" -> "SDs [Bart Van den Hurk]	Noted
11-422	A	13:2	13:6	Rephrase the sentence since it is not clear the messagge [Marina Baldi]	The text will be reformulated
11-423	A	13:2	13:2	allow? [David Rind]	Noted. Text will be reformulated.
11-424	A	13:2		"that I both low and high" has no sense for me [Michel Déqué]	Noted. Text will be reformulated.
11-425	A	13:2		Surely some mention of the results of STARDEX should be made here? [Hayley Fowler]	Noted. Will be considered
11-426	A	13:3	13:6	Sentence starting with 'Most' doesn't make sense. [Rasmus E. Benestad]	Noted. Text will be reformulated.
11-427	A	13:3	13:4	The cited sentence is not clear. "Most appropriate are methods that I both low and high…" [Roxana Bojariu]	Noted. Text will be reformulated.
11-428	A	13:3	13:3	Should "I" read "combine" or "include" ? [Timothy Carter]	Noted. Text will be reformulated.
11-429	A	13:3	13:6	Unreadable sentence. [Daniel Caya]	Noted. Text will be reformulated.
11-430	A	13:3	13:3	Please revise phrase" Most appropriate…" that is not clear. [CONSTANTIN MARES]	Noted. Text will be reformulated.
11-431	A	13:3	13:3	Change to "Most appropriate are methods that capture both low and…" ? [James Renwick]	Noted. Text will be reformulated.
11-432	A	13:3	13:3	I" -> "include [Bart Van den Hurk]	Noted. Text will be reformulated.

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11-433	A	13:3	13:3	The sentence "Most appropriate are methods that I both low and high frequency..." does not read well. Something needs to be fixed here. [Xiaolan L. WANG]	Noted. Text will be reformulated.
11-434	A	13:5	13:5	Update "Wang et al., 2004" to "Wang et al. 2004a" or "Wang et al. 2004b"? [Xiaolan L. WANG]	Noted
11-435	A	13:8	13:11	Feedbacks are implicit in the SD modeling – at least within the observed range of values. However, the representation of feedback processes in GCMs/RCMs may involve systematic biases. [Rasmus E. Benestad]	Noted. Good comment that will be included if the space allows.
11-436	A	13:9		What about the Wood et al. (2004) study, and others in the special issue of Climate Research? [Hayley Fowler]	Reference will be include if the space allows.
11-437	A	13:10	13:10	Move comma to "example, under weak synoptic forcing, feedbacks from vegetation may play an important role." [James Renwick]	Noted
11-438	A	13:10	13:10	remove "," after "vegetation" [Bart Van den Hurk]	Noted
11-439	A	13:14	13:27	Grammar needs some attention in these two paragraphs [Timothy Carter]	Noted. The text will be corrected.
11-440	A	13:14	13:14	Change "obtaining" to "the development of" [James Renwick]	Noted. Text changed.
11-441	A	13:16	13:16	models" -> "model [Bart Van den Hurk]	Noted
11-442	A	13:20	13:21	This gives the impression that no more developments in pattern scaling took place before 2001, when in fact there were several papers during the 1990s on this topic. If the authors don't want to cite pre-TAR papers, then the sentence should be re-phrased accordingly and should reference the sections in TAR (chapter 13) that describes this technique. [Timothy Carter]	Noted. The text will be reformulated
11-443	A	13:21	13:21	More could be added describing the conclusions of Mitchell (2003), who conducted an in depth analysis of the pattern-scaling technique. [Timothy Carter]	Noted. Comment included if space allows
11-444	A	13:32	13:32	spatial/temporal" instead of "spatial/emporal [Roxana Bojariu]	Noted
11-445	A	13:32	13:32	Please replace "emporal" with "temporal" [CONSTANTIN MARES]	Noted
11-446	A	13:32	13:32	emporal" -> "temporal	Noted

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				[Bart Van den Hurk]	
11-447	A	13:32		Write… of spatial/temporal (not ‘’emporal ’’) [Ibouraïma YABI]	Noted
11-448	A	13:34	13:34	Replace "Hansen Bauer et al." with "Hansen-Bauer et al.". [Xiaolan L. WANG]	Noted
11-449	A	13:36	13:50	There is extensive discussion of this in the TAR, chapter 13. It really doesn't need to be repeated here. [Timothy Carter]	Noted. Text deleted and replaced with a short reference.
11-450	A	13:41	13:41	Reference suggested: Imbert & Benestad (2005). Theor. Appl. Climatol. 82, p. 245-255, DOI: 10.1007/s00704-005-0133-4 [Rasmus E. Benestad]	Paragraph deleted .Reference will be included in the first paragraph (13:30-34) if the space allows
11-451	A	13:41	13:41	measured values. -> measured values nor taking feedbacks into account. [Daniel Caya]	Text deleted
11-452	A	13:43	13:43	"Delta-change" might be more familiar to many than "change factor". [Markku Rummukainen]	Text deleted
11-453	A	13:47	13:47	Include a reference to "Buishand, A., G. Lenderink and W.A. van Deursen (in press): [Bart Van den Hurk]	Paragraph deleted .Reference will be included in the first paragraph (13:30-34) if the space allows
11-454	A	13:47	13:47	Include a reference to "Buishand, A., G. Lenderink and W.A. van Deursen (in press): Estimates of future discharges of the river Rhine using two climate scenario methodologies: direct versus delta approach; Hydr.Earth System Sci [Bart Van den Hurk]	Paragraph deleted .Reference will be included in the first paragraph (13:30-34) if the space allows
11-455	A	13:52	13:52	Paragraph 11.2.1.8 can not be understood. For example SD is defined before as " statistical downscaling" and which is the meaning of "SD downscaling methods" ? [CONSTANTIN MARES]	Noted. Text reformulated
11-456	A	13:53	14:16	My suggestion is that this paragraph to be revised, because is not clear. [CONSTANTIN MARES]	Noted. Text reformulated.
11-457	A	13:55	13:55	Replace "Hansen Bauer et al." with "Hansen-Bauer et al.". [Xiaolan L. WANG]	Noted.
11-458	A	14:3	14:3	methodology -> methodology. [Daniel Caya]	Noted.
11-459	A	14:3	14:3	Insert "." after "methodology" [Bart Van den Hurk]	Noted.
11-460	A	14:9	14:9	What are the "two approaches" [Daniel Caya]	Noted. The text edded (“SDs and RCMs”)

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11-461	A	14:9	14:16	Two aproaches (line 9) which ones ? This paragraph seems misplaced. I suggest to create a table or a specific paragraph devoted to intercomparison of methods. [ERIC MARTIN]	Noted. The text 11.2.1.8 was reformulated to "intercomparison of downscaling methods"
11-462	A	14:9	14:16	It is disappointing that so few comparisons of statistical vs dynamical downscaling have been carried out since the TAR. Actually the situation is even worse than implied in the text, as Wilby et al (2000) was actually reviewed in the TAR alongside earlier studies, and is not a new piece of work. Perhaps we can have a short comment encouraging more work in this area. [James Murphy]	Noted. The text will be reformulated and some new refrences will be included.
11-463	A	14:15	14:15	The conclusion that statistical and dynamical approaches have both got better, but are both comparable, is nicely diplomatic, but what objective criteria is it actually based on ? How do we measure comparability ? Do we mean that they reproduce present day observations with similar skill, or do we mean that they produce similar predictions of change ? [James Murphy]	Noted. Text will be reformulated
11-464	A	14:18		More deep discussion of uncertainties in precipitation evaluation would be valuable at this point as well as the assessment of possible causes of uncertainties. [Marina Baldi]	We will insert a sentence about different degrees of uncertainty applying to different climate variables, esp. Temp. vs. Precip.
11-465	A	14:20		See attached document for a suggested section 11.2.2.1 [Daniel Caya]	We will incorporate the aspect of the proposed version that are appropriate.
11-466	A	14:24	14:28	The list of uncertainties in this sentence needs to include uncertainty in the conversion of emissions to forcings [James Murphy]	OK, we will change this
11-467	A	14:24	14:27	Insert "(1)" after "include", "(2)" after "cover,", and "(3)" after "emissions, and" [Bart Van den Hurk]	We will do
11-468	A	14:35	14:52	The discussion of regionally-specific feedbacks should be put in context by mentioning the major global scale feedbacks (cloud, water vapour, surface albedo - see Chapter 8), which affect regional as well as global uncertainties. [James Murphy]	We will include this point, but since it is discussed in other chapters we will discuss it only briefly
11-469	A	14:35	:42	This paragraph should make explicit reference to the term "global climate sensitviity". There is a nice discussion and recent estimates in chapter 9, which should probably be referenced in chapter 11. The issue of climate sensitvity is central to the discussion on uncertainty. Indeed, as climate sensitvities of >8 K (<2 K) cannot be ruled out, future changes of our (global and regional) climate system could be much larger (smaller) than currently anticipated. It has also been argued that the current suite of GCMs does not appropriately represent this uncertainty, implying that current RCM estimates do not	We will do it, e.g. we can add a formulation like "Most of these factors are integrated in the property of the climate system (climate model) identified as climate sensitivity" and we will refer to the appropriate chapter in the report. However we do not feel that

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				cover the known uncertainties. Other studies that should be mentioned here are Black et al (2004), Schar et al (2005) and Schonwiese et al. (2004). Black, E., M. Blackburn, G. Harrison, B. J. Hoskins and J. Methven, 2004: Factors contributing to the summer 2003 European heatwave. Weather, 59 (8), 217-223 Schonwiese CD, Staeger T, Tromel S, 2004: The hot summer 2003 in Germany. Some preliminary results of a statistical time series analysis. METEOROLOGISCHE ZEITSCHRIFT 13 (4): 323-327 [Christoph Schar]	the suggested publications (all about the summer 2003 heat wave) are pertinent.
11-470	A	14:45	14:45	if the reference to Cox et al. (2000) is kept, one should probably refer also to Jones et al. 2003 (Geophys. Res. Lett. 30:9, 1479, doi:10.1029/2003GL016867.) [Markku Rummukainen]	OK
11-471	A	14:47	14:49	Again, the Lorenz and Jacob GRL paper show impact from different resolutions on the simulation of climate on regional and global scales [Erik Kjellström]	We will check that section 11.2.1 addresses the two-way nesting issue, and will use reference.
11-472	A	14:54		In Slovakia the comparisons of several statistical downscaling methods, including GCMs-Analogues projections for those variables not reliable in the GCMs outputs (snow cover, relative air humidity), was realized and published in: LAPIN, M. and MELO, M., 2004: Methods of climate change scenarios projection in Slovakia and selected results. Journal of Hydrology and Hydromechanics, 52, 2004, 4, 224-238. The obtained results are not in contrary with those in Chapter 11. [Milan Lapin]	OK
11-473	A	15:4	15:8	It should be noted also that models are of necessity developed and tested for regions where observed data are available. Thus, models may not accurately represent processes that are important in data-sparse regions. [Raymond Arritt]	Good point, we will mention it.
11-474	A	15:4	15:8	It's good to see observational uncertainty being given the prominence it deserves. [James Murphy]	Thanks!
11-475	A	15:4	15:17	This lacks an actual assessment flavour. Do present methods incorporate these aspects or do they not? What does this imply in terms of using the results? [Markku Rummukainen]	Some methods do, some methods do not. We will be clearer in the methods' description about the aspects that each method incorporates or not.
11-476	A	15:12	15:12	Section 10.5.4.3 could be quoted here, as it discusses the relative roles of modelling uncertainty and internal variability. [James Murphy]	OK
11-477	A	15:13	15:17	I am uneasy with the terminology of "natural" climate variability when it is background	This sort of comment – like many

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				or internal climate variability as simulated by a GCM. This term is also used to represent real world climate variability in the absence of greenhouse but there is no guarantee that a climate model adequately represents such variability adequately. This is one area that needs further investigation. The key point then becomes the purpose to which the resulting information is to be put. If it is to extract the climate change signal, then stripping out background variability within the model is important. This can be done through ensembles or by creating relationships between global warming (or forcing) and change in the target variable. Pattern scaling through regression of an entire run (not a time-slice of several decades) is one way to do this. The other way (mentioned in the paragraph) is to present a realistic projection of change that includes plausible representations of the enhanced greenhouse effect and natural variability. The distinction becomes especially important when attributing observed change and estimating whether that change is likely to be short or long-lived (e.g. in Southwest Western Australia). [Roger Jones]	others following, will be used to make the presentation/comparison of the methods more precise, tighter and explicit.
11-478	A	15:19		See attached document for a suggested section 11.2.2.2 [Daniel Caya]	See above
11-479	A	15:33	15:33	Delete the comma in "Pan et al. (2001),". [Xiaolan L. WANG]	OK
11-480	A	15:33		Pan et al.(2001) is missing in the references. [Hidetaka Sasaki]	We will add it
11-481	A	15:38	15:38	Insert "However" before start of paragraph [Bart Van den Hurk]	OK
11-482	A	15:39	15:39	I agree with the statement that large AOGCM ensembles are needed to provide probabilistic regional predictions, but this is not quite consistent with the statement on line 45/46, which says that probabilities can be generated from existing multi-model ensembles (which are certainly not large). Perhaps we need to distinguish between "robust" probabilistic estimates ideally based on large ensembles versus "interim" estimates based on smaller ensembles, or somesuch. [James Murphy]	Will do
11-483	A	15:43	19:12	Very interesting section but it needs to be tightened considerably. A better balance is needed between subsections in 11.2.2.2 [Roger Jones]	Will do, but an important factor in this will be to find out which methods are actually going to be featured, depending on their status in the publication pipeline.
11-484	A	15:43		Section 11.2.2.2.2: Very comprehensive, but perhaps too long? For instance, the interesting Figure 11.2.1 is partly explained in several places, but the differences in the derived PDFs are not discussed critically anywhere (that I could find).	We'll make sure to avoid repetitions. As far as "critical discussion" is concerned, we won't be judging good

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				[James Renwick]	or bad methods, but we will make sure to make the comparison of assumptions and results more compelling.
11-485	A	15:44	15:46	The use of multi-model ensembles for climate projections is weakened by the fact that the models are not truly independent of each other, with members of the ensemble sharing common approaches to characterization of climate drivers and outputs. Some discussion of the implications of this fact is needed at this point. [Lenny Bernstein]	True, but it is a starting point. How to measure and quantify "model dependence" is a research question. Also , the issue may be less dramatic, when dealing with regionally averaged quantities. We will definitely make the assumption clearer and discuss its consequences, which are likely going to be tighter PDFs , similarly to the consequences of dealing with a tight range of climate sensitivities in the AR4 ensemble of GCMs.
11-486	A	15:44	15:46	The multi-model ensemble approach is based, in part, on the assumption that the models are independent of each other. This is not the case, since many of the models in the ensemble are derived from each other or a common earlier model. The inter-model comparison programs described in Chapter 8 also drive models to common approahces. Because of this, one would expect that given the same inputs, the outputs of all models in the ensemble would be close. The authors need to discuss the degree to which climate models share common components and the implications of this sharing on the quality of multi-model ensemble outputs. [Jeffrey Kueter]	See above. Ch. 11 is probably not the place for a discussion of the commonalities and differences among models/their components. Is this discussed anywhere in WG1? If so we will cross reference.
11-487	A	15:50	15:50	"do not necessarily completely explore" sounds like an over-diplomatic piece of wordsmithing. There can be no question that a multi-model ensemble of 10-20 members is utterly inadequate to sample all possible combinations of options for the major parameterisations of a coupled model. We should be honest and say so ! [James Murphy]	We will be more straightforward
11-488	A	15:54	15:54	IPCC-AR4" replace by "this report [ERIC MARTIN]	OK
11-489	A	16:7	16:7	The poor reproduction of the right tail of pdf's is an important finding, and needs to be supported by a (number of) scientific reference(s) [Bart Van den Hurk]	The statement in the text was actually just a deduction. We may cite Harris et al. 2006 where the authors explore sensitivity of the regional PDFs to different factors and find the PDFs to be "sensitive" in the tails to climate

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					sensitivity.
11-490	A	16:9	16:13	It should be emphasised that Raisanen and Palmer (2001) is an idealised study of probabilistic vs deterministic climate prediction in which model simulations were treated as truth. [James Murphy]	Will do
11-491	A	16:9	17:36	Note that the text between these lines has been deleted in the suggested 11.2.2.2. This is because it takes a full page and do not add much to the understanding and is not very useful for policy makers. [Daniel Caya]	Do not agree, but in general we will work on shortening the section considerably.
11-492	A	16:12	16:13	One should probably explain WHICH advantages were demonstrated rather than just state that some were found. [Markku Rummukainen]	Ok, we can for example cite the use of forecasts by water resource management, or by re-insurance.
11-493	A	16:20	16:20	State how many models comprise the multi-model set in Figure 11.2.1. [James Murphy]	21
11-494	A	16:23		Comment Figure 11.2.1. For which period have the temperature changes been computed? [Eduardo Zorita]	2080-99 vs. 1980-99
11-495	A	16:23		Comment Figure 11.2.1. CAN should read CNA [Eduardo Zorita]	Done
11-496	A	16:35	17:9	Probably should somewhere mention the Bayesian approach for the SD approach too, where the best estimate is a weighted mean of trends based on a number of skill criterea (Benestad, 2005, doi:10.1029/2005GL023401). Furthermore, Benestad (2004) used SD in combination of a GIS approach to produce maps of probabilities. [Rasmus E. Benestad]	This may be more appropriate for Rob Wilby's section on statistical downscaling
11-497	A	16:37	18:31	The key assumptions and caveats underlying the probabilistic methods, particularly those depicted in Figure 11.2.1 and 11.2.2, need to be brought out. Table 11.2.1 is very useful and goes some way towards this, but the discussion in the text is somewhat uneven. For example, some limitations of the Greene et al method are rightly discussed, but the Tebaldi and Raisanen methods also need to be further discussed. For example, both methods assume that the members of the GCM ensemble provide independent predictions, which some would find highly questionable. Also, the Tebaldi method is highly sensititive to its "convergence criterion", as demonstrated by Lopez et al (2005). The setting for the parameter which controls the convergence criterion appears to be a subjective choice, hence the widths of the pdfs are significantly influenced by expert judgement. This should be pointed out much more explicitly. [James Murphy]	This is a good point and we will use it when working on making the presentation tighter, more precise.
11-498	A	16:51		Furrer etal.(2005) is missing in the references.	Noted

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				[Hidetaka Sasaki]	
11-499	A	16:53	16:53	Should 'high-dimensional' be 'two-dimensional'? [Rasmus E. Benestad]	High dimensional here refers to the fact that the global fields with their spatial features at different scales are modeled as a whole, accounting for the spatial correlation exhibited by temperature and precipitation "features". The number of dimensions in Furrer et al. is related to the number of grid points in the typical GCM grid (T42=8192)
11-500	A	17:8	17:8	It should be remarked that Baeysian approach makes the assumption that the skill for representing the past will also be representative for the future. This assumption is somewhat analogous to the assumption of stationarity in the SD-approach... [Rasmus E. Benestad]	This refers to Greene's method, and we can certainly point that out.
11-501	A	17:10	17:20	Not sure that Dessai et al.'s work is adequately represented here. Climate sensitivity is most significant in the lower tail of temperature distribution and forcing (emissions) in the upper tail. The most significant aspect of this approach is that it has the potential to cover the regional response to a range of emission scenarios, whereas each of the other studies illustrates the response to a single forcing scenario. [Roger Jones]	We will use this in the description.
11-502	A	17:10	17:20	The use of pattern scaling to estimate regional pdfs is subject to the caveat that the responses of different GCMs, or GCM variants, cannot be well predicted by scaling patterns from other GCMs for variables other than temperature (Murphy et al, 2004). Thus pdfs derived by scaling small ensembles will likely underestimate the range that would be obtained by running a larger ensemble of GCMs. [James Murphy]	We will add this caveat
11-503	A	17:10		Dessai paper is missing in the reference list [Marina Baldi]	Will add it in
11-504	A	17:23	17:23	"fail to provide formal pdfs" seems like an unfortunate choice of wording. Perhaps the authors should be congratulated on being realistic enough to admit that their method could not support the production of pdfs ! [James Murphy]	Will rephrase
11-505	A	17:23	17:23	Remove "Thus" [Bart Van den Hurk]	OK
11-506	A	17:23	17:23	Add "(e.g., Wang and Swail, 2005b)" after the words "statistical methods", because this study uses multi-model and multi-ensemble simulations to characterize climate-model and forcing-scenario uncertainties in climate change projections.	This study is about wave height.We do not think it is appropriate.

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				[Xiaolan L. WANG]	
11-507	A	17:36		Write adressed. (punctuation) [Ibouraïma YABI]	OK
11-508	A	17:37	17:37	I would like to add related to apply Maximum Entropy Method the following " Mares and Mares (2004) develop a procedure for the post-processing the information given by different methods/models. This method is also appropriate for the selection the optimal solution from ensemble multi-models solutions" [CONSTANTIN MARES]	The study is about weather forecasting. Does not seem to be directly relevant
11-509	A	17:38	17:45	The passage seems to duplicate the figure caption and could thus be reduced. [Markku Rummukainen]	Wll clean that up!
11-510	A	17:38		The Furrer et al. results should be included in Fig. 11.2.1 and 11.2.2. [Reto Knutti]	We will aggregate, compare and discuss the regional results briefly, but since the study is featured in Ch. 10 and was developed for gridpoint-level projections does not seem to be entirely appropriate. Perhaps in Supplementary material?
11-511	A	17:39	17:40	The histograms does not look as they have appropriate bin size or enough data points for comparison. Perhaps just show the pdfs in colour? [Rasmus E. Benestad]	Will try different bin sizes. Also we will specify the number of models involved in the caption and we will make the histogram less obtrusive graphically.
11-512	A	17:51		Write method's results (to check) [Ibouraïma YABI]	Incorrect suggestion.
11-513	A	17:52		Any difference between the two hemispheres? Include a discussion on differences, if any. [Marina Baldi]	We will try to discuss the difference in projections as they manifest large spatial patterns , e.g more agreement in high latitude than in the tropics.
11-514	A	17:53	17:55	Page 15, line 45 indicates that natural variability is taken from observations, and thus includes solar/volcanic forcing, i.e. this is natural unforced and forced variability which is added to the Raisanen and Tebaldi results. The AOGCMs only include unforced variability for the future (no solar/volcanic forcing). In that sense, the two are not entirely comparable (Page 17, line 53). This should at least be mentioned. [Reto Knutti]	Good point and we will add this to the discussion of the figure.
11-515	A	17:53	18:21	I get the impression from the figure that there is not enough data for making such comaprisons. Its probably better to say that the location of the distributions derived by	The entire point of the section is to compare methods, so the figure is

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				Greene et al (2005) have a different location to to their results. I think that Fig. 11.2.1 is a bit weak... (the statement may be right, but there doesn't yet seem to be enough data to support it – critics are likely to jump on issues like this). Perhaps it's worth while asking: what is the message here? And do these differences warrant all this discussion? [Rasmus E. Benestad]	important for that purpose, we would argue. That said , We will work on shortening the section considerably.
11-516	A	17:56	17:56	It is not at all clear what is meant with the phrase "(accomodating of)". Please explain or remove [Bart Van den Hurk]	Rephrased
11-517	A	18:1		Write model's agreement (to check) [Ibouraïma YABI]	Incorrect suggestion
11-518	A	18:3	18:4	The comment that the inter-model spread is similar to that of natural variability is sharply at odds with section 10.5.4.3, which quotes studies showing that the spread in temperature changes is explained mainly by variations in model formulation. [James Murphy]	This was actually a qualitative assessment, specific to the PDFs in the figure, not a rigorous analysis so we may eliminate it, or make clearer that it is not a general statement.
11-519	A	18:6	18:6	An important restriction to the pdf-construction is the assumption that GCMs truly span the natural variability adequately. I wonder whether this is true. For instance, spread of 500Z patterns in an ensemble NWP forecasts is often reported to be smaller than the spread of analyzed climatologies of 500Z, implying a difficulty of the (coarse resolution ?) models to span the true climatological variance. [Bart Van den Hurk]	In fact some methods use observations to derive an estimate of natural variability...of course you can argue that the length of the record is not enough, or the representativeness of the data in some regions is poor, so we will comment on this problem in the text.
11-520	A	18:8	18:21	In addition to the issues listed, the Greene et al study makes no allowance for uncertainties in historical focrings, unlike methods based on the uncertainties in fits derived from optimal fingerprinting (e.g. Stott and Kettleborough, 2002; Stott et al, 2005). The latter gets a much wider range for future changes, I suspect because it does a better job at accounting for sources of uncertainty in the fit to historical changes. [James Murphy]	This is another example of the comments we will use to make the description/discussion of the methods better.
11-521	A	18:35	18:56	The examples of interpreting the figure should be contained in the caption. The need for a lengthy explanation also casts some doubt on the usefulness of the figure. [Markku Rummukainen]	We will work on tightening the description.
11-522	A	19:2	19:2	Year missing in ref. [Reto Knutti]	Good and Lowe is still under review but we will monitor its progress through the authors
11-523	A	19:2	19:2	Proper reference still missing [Bart Van den Hurk]	Ditto

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11-524	A	19:2	19:2	The incomplete reference "Good and Lowe ()" is not listed in the reference section. [Xiaolan L. WANG]	Ditto
11-525	A	19:2		Not forget to specify the reference [Ibouraïma YABI]	Ditto
11-526	A	19:8	19:	present text: 'stable relation between sub-regional scale variability of the trends and inter-model variability, in a framework similar to pattern scaling' suggested replacement: 'stable relations linking inter-model variability at the sub-regional and regional scales, in a framework similar to pattern scaling. The relations are region-dependent.' [Jason Lowe]	Ok, we will replace if the study is citeable.
11-527	A	19:9	19:9	"the study claims" is less well in line of the aim of assessment. What is the assessment in AR4 - does the study serve or does it not serve the impact research community? [Markku Rummukainen]	I'm not sure we can find out, but we can reword this as something like " the study addresses the need of the impacts research community for finer scales' projections.
11-528	A	19:12	19:12	Is a discussion of the concept 'skillful scale' not relevant here? [Rasmus E. Benestad]	Ok
11-529	A	19:14	19:22	This is a bit redundant, because (a) it is a repetition from earlier chapters, and (b) is not a regional analysis. [Bart Van den Hurk]	We will go light on the description of what a PPE is, and just note the results that apply to regional scale projections.
11-530	A	19:14		Sect.11.2.2.2.3. Add results from Collins et al. (2005, Clim. Dyn., submitted, Figs.10&11) to this section. [Dave Rowell]	OK
11-531	A	19:17	19:20	Please change 26 to 29 on line 17. Also suggest delete "climate feedback parameters like" on lines 19/20 [James Murphy]	OK
11-532	A	19:24	19:24	Suggestion: 'Recent work by Harris et al. () was linked spatially complex projections with the equilibrium response...' [Rasmus E. Benestad]	OK
11-533	A	19:24	19:24	Year missing in ref. [Reto Knutt	Will add
11-534	A	19:24	19:24	Please introduce the year for "Harris et al." , in fact Harris is not given in the section References. [CONSTANTIN MARES]	Will add
11-535	A	19:24	19:31	The reporting of Harris et al is not quite accurate. Firstly, the bridge is between	Will use this description of Harris et al.,

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				equilibrium and transient patterns, secondly the net scaling error was not "assumed", it was calibrated in a cross-validation exercise, and thirdly the text gives is more negative about precipitation scaling than we were in the paper. I suggest the following text instead: "Recent work by Harris et al (2006) has developed a bridge between spatial patterns of the transient and equilibrium climate responses by way of a simple pattern scaling (Santer et al 1990). This allows large perturbed physics ensembles to be translated into pdfs of time-dependent regional changes. Uncertainties in surface temperature and precipitation changes are derived, which arise from poorly-constrained atmospheric model parameters, internal variability and pattern scaling errors. The latter are calibrated by matching the transient and equilibrium responses of 17 model versions with corresponding parameter settings. Scaling errors are largest when the transient response varies non-linearly with global temperature, as is the case for precipitation in certain regions." [James Murphy]	thanks!
11-536	A	19:24	19:24	Proper reference still missing [Bart Van den Hurk]	OK
11-537	A	19:24		Harris paper is missing in the reference list [Marina Baldi]	OK
11-538	A	19:24		Not forget to specify the reference [Ibouraïma YABI]	OK
11-539	A	19:33		Sect.11.2.2.2.4. Perhaps this is an appropriate place to include a paragraph discussing the role of understanding regional climate change in providing subjective assessments of regional climate change uncertainty. It seems to me that this is an essential complimentary approach to the other approaches described, as it can (in theory) account for unrealistic convergence (or perhaps divergence) of models. In particular, Rowell and Jones (2005) provide a good discussion and first use of this kind of approach to make explicit (subjective) statements about the uncertainty of regional climate change. [Dave Rowell]	Agreed. However, the entire chapter in the regional sections is applying just this approach.
11-540	A	19:38	19:39	Suggest replacing "for current climate simulations" by "from simulations of historical climate change" [James Murphy]	OK
11-541	A	19:47	20:9	Need to clarify what is meant by "downscaling uncertainty". The PRUDENCE project addresses uncertainty arising from RCM formulation, which projects onto domain-wide scales already resolved by the driving GCMs, in which case a matrix of GCM and RCM ensemble runs will lead to an element of double counting. See earlier comment relating to page 6 lines 29-34. [James Murphy]	We will revisit the text and clarify the meaning of downscaling uncertainty.
11-542	A	19:47		Section 11.2.2.2.5. Wang and Swail (2005b) have assessed the relative importance of the	Couldn't find the first reference.

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				uncertainty from climate-model (AOGCM) differences and from forcing-scenario differences; and Caires et al. (2005) have assessed the relative importance of the uncertainty from using different statistical downscaling approaches (namely the use of non-stationary GEV and GPD models for making projections of extremes). These studies should be cited in this section. [Xiaolan L. WANG]	Provide please. As for the second point, we will use the suggested reference/work.
11-543	A	19:48	19:49	From the emphasis given to various downscaling methods earlier in this chapter, this statement is not supported and could be bolstered through citation. My perception of the literature is that most researchers concentrate on ever more precise methods without adequate emphasis on uncertainty. The largest amount of text in the preceding sections describes methods that only cover part of the whole (very few attempt integrated uncertainty analysis). If this statement is indeed the case, it will need to be backed up be references. [Roger Jones]	We will better substantiate through references and by making the statement less general.
11-544	A	19:52	19:52	"There is abundant evidence": a difficult sentence. Maybe insert "on one hand" and "on the other" at appropriate places [Bart Van den Hurk]	OK
11-545	A	19:53	19:53	Reference to chapter still missing [Bart Van den Hurk]	Will fill in
11-546	A	20:1	20:6	This statement is correct only for monthly/seasonal averages. Very large differences between different RCMs in climate change simulations regarding daily maximum and minimum temperatures are described in Kjellström et al. (Subm. To Climatic Change, 2005). They compare 10 different RCMs all forced by the same global model and emission scenario. In a comparison to an earlier paper by Kjellström (Ambio, 33(4-5), 2004) they note that the differences between the 10 RCMs driven by the same GCM are as large as differences between simulations with one RCM driven by different GCMs and different emission scenarios. [Erik Kjellström]	Ok, we will elaborate on time scales.and include reference if in press.
11-547	A	20:2	20:2	What is the difference between GCM and AOGCM here? [Markku Rummukainen]	None.
11-548	A	20:5	20:5	Which Deque et al (2005)? There are 2 [Bart Van den Hurk]	Will specify
11-549	A	20:5		Which Deque paper ? A or B? [Marina Baldi]	Will specify
11-550	A	20:11		Section 11.3: The description is uneven in particular regarding Asia, the largest continent. This should be expanded in particular for central Asia. Also the summary on p. 23 is currently treating Southeast Asia only, it is vital this is extended to the other regions also.	The section has been homogenized with some restructuring – especially the Asia section.

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				Furthermore, in the summary in all regions there should be more emphasis on the conclusions regarding potential changes in weather and climate extremes. Finally, the treatment in 11.3 is too continential, changes in the oceanic areas are, except for the polar oceans, underrepresented. Best is perhaps if there would be a separate section assessing changes in the world oceans. [Gottfried Kirchengast]	
11-551	A	20:11		I thought the subsections in section 11.3 on model skill by region were very useful - but this may be an area where a well constructed table covering all regions could replace a lot of text. [Martin Manning]	Some tabular data has been added
11-552	A	20:11		Section 11.3: Much of the projections section seems to be a recitation of results from various studies, often with little synthesis. The section, while important, is very long, and is rather heavy going. It would benefit greatly from some actual assessment of what current results are saying overall, rather than just a list of different (and sometimes differing) findings. If regional changes could be put clearly in terms of larger-scale changes in the monsoon circulations and ENSO etc (where possible), it would would help the reader's understanding. [James Renwick]	The section has been homogenized with some restructuring
11-553	A	20:11		I suggest using tables similar to Table 11.3.3.1 in *all* sub-sections of Sect.11.3; in particular I notice it is missing for Africa. [Dave Rowell]	Some tabular data added and resturcturing has been done
11-554	A	20:11		Section 11.3 In this section, the comments about the (admittedly complicated) topic of THC reduction appear not self-consistent. E.g.' the small possibility of cooling over NW Europe' (page 11-22, lines 32-33) seems at odds with the statement that 'models do not support a reversal of the warming to cooling. (page 11-33, lines 26-27). I think it is important to make the following points clearer in this section: Firstly: there is a consensus of AR4 AOGCMs about a gradual weakening of the THC under increased greenhouse forcing. Within this consensus, there is no sign of cooling over NW Europe/N America. Secondly, given the modelling uncertainty there is a small (and currently undefined) possibility of a more rapid and substantial weakening of the THC (presumably the 'uncertainty' mentioned in line 32 on page 11-22 refers to this). A near-complete and rapid weakening of the THC would, in fact, cause cooling over the Northern Hemisphere. Whether or not such a cooling is strong enough to cause temperature to fall below pre-industrial levels depends (among others) on the timing of such an event: the earlier in the 21st century, the larger the area that would see colder than pre-industrial temperatures.Thirdly: in addition to temperature change, THC shutdown would also cause large changes in precipitation and sea-level. Precip. changes would be large at low	Noted – discussion will consider these issues. If the paper is available in time it will be considered.

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				latitudes. We (i.e. Vellinga and Wood) have recently written a paper that describes the impacts of THC shutdown in the 2050s, and place these in the context of climate change due to increased greenhouse gas concentrations. This paper is currently under review by Climatic Change, and we are on track for it to be possibly accepted by January 2006. If you wish, I can send you an electronic pre-print of this paper. [Michael Vellinga]	
11-555	A	20:15	94:9	It is important that there is reasonable balance in the length and detail of regional coverage, although the volume of literature in each region is obviously unbalanced. This is where the authors skills at synthesis are needed, so that key topics are covered in a similar manner for each region. The volume of literature can be condensed by using numbered references in summary Tables. [Timothy Carter]	The section has been homogenized with some restructuring, and tabular data added.
11-556	A	20:15	94:9	In relation to projected mean changes in seasonal temperature and precipitation, WG II authors have been asked to use pattern-scaled scatter diagrams from Ruosteenoja et al (2003) to frame the uncertainties in regional (sub-continental) projections based on AOGCM simulations that were reported in the TAR. Scenarios based on these model outputs (from the IPCC DDC) have been widely used in recent impact studies. The scatter plots are also cited in discussion for some regions in this chapter, but not all regions. Would it make sense to cite these for all regions? Since a systematic regional analysis of recent model projections (PCMDI) is being reported in Chapter 11, would it also make sense to draw comparison with the earlier TAR projections in this chapter as well? An evaluation of how the TAR results compare to the latest climate model projections reported in the AR4 is anyway required in Chapter 2 of WG II, but there is little space available to present this comparison. It would seem logical to fold this in (at little expenditure of space) to Chapter 11, and summarise the conclusions in Chapter 2, WG II. [Timothy Carter]	The section has been homogenized with some restructuring, and tabular data added. Uncertainty is addressed more systematically
11-557	A	20:33	20:33	Delete "judged to be" or is this not known? [Markku Rummukainen]	Noted
11-558	A	20:34	20:34	insert "are present" after "regimes" [Bart Van den Hurk]	OK
11-559	A	20:39	21:18	This discussion is generally very nice, and addresses some of my concerns about the need to discuss large-scale variability and teleconnections. [James Renwick]	Thanks.
11-560	A	20:42	20:50	I thought this was the most interesting statement in the whole chapter and a good example of the power of stating the generic way in which climate change can play out at the regional level. Even if this particular statement is challenged and needs modification I would still urge that this type of material be collected together before getting to the region	Noted – will keep in mind in preparing the SOD.

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				by region statements. [Martin Manning]	
11-561	A	20:44	20:46	While the increase in vapour would lead to the moisture convergence increasing, and the "wet regions" getting wetter, it does not follow that the regions of moisture divergence would automatically get drier, since there is assumed to be more moisture everywhere in a warmer world. GCMs may model such a drying, but that must be contingent on increased transport as well as increased water vapour. [James Renwick]	Noted; text will be re-evaluated.
11-562	A	20:46		the text "and regions of convergence will get wetter and regions of divergence drier" needs evaluation also with changes in air temperature and saturation deficit causing different conditions of evapotranspiration. Higher water vapor pressure can be accompanied also with higher saturation deficit and higher potential evapotranspiration. [Milan Lapin]	Noted; text will be re-evaluated.
11-563	A	20:55		ex., Sato et al.(2005, submitted to JMSJ and IPCC TSU) studied oceanic response to NAM-like atmospheric change due to global warming using Kuroshio-resolving ocean model. [Yasuo Sato]	Noted.
11-564	A	21:8	21:9	Seasonal frozen ground and permafrost have to be added to snow and ice when implications for regional climate are discussed.(e.g Oelke, C., T. Zhang, M. C. Serreze, and R. L. Armstrong, 2003: Regional-scale modeling of soil freeze/thaw over the Arctic drainage basin, J. Geophys. Res., 108(D10), 4314, doi:10.1029/2002JD002722.) [Roxana Bojariu]	Noted;
11-565	A	21:9		Write… cover. The (space enters the two words) [Ibouraïma YABI]	OK
11-566	A	21:16		Write… tropical climates. (punctuation [Ibouraïma YABI]	OK
11-567	A	21:32	22:12	For purely editorial reasons I would suggest NOT including these paragraphs inside the box. I think these would look better if they were laid out as text introducing the box. Then the box itself would start with the regional heading "Africa". [Martin Manning]	Noted: will consider in restructuring.
11-568	A	21:32	24:52	Perhaps the most important messages of the chapter are communicated in Box 11.1, and it is crucial that this Box receives appropriate attention from climate specialists from each region. I particular liked the way the supporting evidence for the statements is presented, utilising four sources. The statements here are likely to be cited heavily by researchers and policy makers alike in the years following this assessment. [Timothy Carter]	Noted.

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11-569	A	21:32	24:52	The authors will need to consider how much of this material can be taken into the Executive Summary, where there are currently far fewer statements. [Timothy Carter]	Noted.
11-570	A	21:34	21:34	Replace "Artic" by "Arctic". [Martin Stendel]	OK
11-571	A	21:38	21:38	Consider omitting "projection of". [Markku Rummukainen]	Noted
11-572	A	21:39	21:39	I look forward to seeing Figure 1. It is important for this to be ready in time for the Government Review. [Timothy Carter]	Noted.
11-573	A	21:43	21:43	Remove 2nd "by" [Bart Van den Hurk]	OK
11-574	A	21:43		remove ''by '' (repetition) [Ibouraïma YABI]	OK
11-575	A	21:48		The figure is missing (see page 203) [Marina Baldi]	Noted
11-576	A	21:50	22:7	Delete [Daniel Caya]	Do not understand the reason for this request.
11-577	A	21:50	22:7	The "key processes" discussion in box 11.1 duplicates the wording in the main body of the chapter (page 20-21). Can one be removed? [James Renwick]	Restructured
11-578	A	21:51	22:7	The three pragraphs with key processes are covering the same issues as those from pages 20 (start line 43) -21 (end line13). [Roxana Bojariu]	Restructured in new box
11-579	A	21:51	22:7	Duplicates the text on page 20, lines 42-56, and on page 21, lines 1-13. [Markku Rummukainen]	Restructured in new box
11-580	A	21:51	22:8	Text is a plain repetition and can be removed [Bart Van den Hurk]	Restructured in new box
11-581	A	22:9	24:50	Please make sure and use in a consistent manner terms like "very likely". Furthermore, what is meant by "a small possibility" should be made more clear, such as on line 32 on page 22 (and page 23, line 16-17). How small? At all quantifiable? Also, use the sources of information listed on page 22, lines 10-12 more rigorously. E.g., when referring to empirical evidence (e.g., page 22, line 44) or to process studies (e.g., page 22, line 49), consider using simply "Based on (4)". The same should be used in cases like on page 24, lines 5-8, i.e. use "Based on (1)". [Markku Rummukainen]	Checked.

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11-582	A	22:9	24:50	Instead of referring to the "four sources of information" (page 22, lines 9-12), please consider referring to the appropriate section of Chapter 11. [Markku Rummukainen]	We prefer to retain these.
11-583	A	22:12		Add at end "if you ignore actual climate observations" [Vincent Gray]	Disagree, this does not make sense.
11-584	A	22:14	24:50	Is this a box? - This and the subsequent section is too long - must be shortened such that the contents are more precise and clear. [Murari Lal]	Noted; box restructured.
11-585	A	22:14	24:50	It is noted that in all continents the temperature increase is stronger than the global mean. That implies that land warms harder than ocean. Is this a result of a slow transient response of oceans, or are there physical interactions (with clouds?) that play a different role above land than above sea? [Bart Van den Hurk]	Discussion on this point will be considered in revised draft.
11-586	A	22:14		The material in this box is almost all qualitative which seems to me to detract enormously from its utility. [Martin Manning]	Box has been heavily reworked.
11-587	A	22:14		As I am sure others will tell you - Asia is missing from the box. [Martin Manning]	See above
11-588	A	22:15		Replace "is very likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-589	A	22:15		Replace "is likely to"with "might [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-590	A	22:15		Statements like "All of Africa is very likely to warm this century" really do not need to be made here and certainly do not need to be repeated for each region. Frankly this detracts from more important information that you have to present. If comparative statements need to be made about regional warming then they would be more useful if they were referred to the average warming over LAND. But to my reading the more important statements are those that identify particular issues within regions (the Mediterranean warms more than the rest of Europe) rather than repeat things that are probably going to be true for most of the globe. [Martin Manning]	Reorked in new box.
11-591	A	22:19	22:26	Points 2 and 5 seem contradictory to me, since I interpret "North Africa" as all African land north of 0degN. It could be changed to "much of the far north of Africa and the northern Sahara" for example. [Dave Rowell]	Reworked in new box
11-592	A	22:19		Replace "is very likely to" by "might"	Disagree, this is our assessment of the

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				[Vincent Gray]	likelyhood.
11-593	A	22:21		Replace "very likely" by "possibly" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-594	A	22:23		Replace "will likely be" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-595	A	22:25		The value of the box giving region by region summaries is surely to emphasize what you have confidence in. I would suggest that statements like this where a direction of change can not be given should be left out of the box and covered only in the main text - in this case section 11.3.2. [Martin Manning]	Noted. Box has been extensively reworked.
11-596	A	22:29		Replace "is very likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-597	A	22:30	22:31	Replace "is likely to" by "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-598	A	22:30		Replace "is likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-599	A	22:32		Delete "small" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-600	A	22:34		Replace "are very likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-601	A	22:35		Replace "are likely to" by "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-602	A	22:38		Replace "is likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-603	A	22:39		Replace "is likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-604	A	22:41	22:42	"… changes in atmospheric circulation … contribute to the seasonal cycle …". Seasonal cycle of what? As it stands this implies to me the seasonal cycle of the basic state, which does not make sense. [Dave Rowell]	Noted, box reworked.
11-605	A	22:41	22:42	insert "change of the hydrological" between "contribute to the" and "seasonal cycle" [Bart Van den Hurk]	Noted, box reworked.
11-606	A	22:41		Change atmospheric circulation with large scale atmospheric circulation [Marina Baldi]	Noted, box reworked
11-607	A	22:43		What about the Western Mediterranean region? There are evidences of changes.	

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				[Marina Baldi]	
11-608	A	22:43		Replace "very likely" by "possibly" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-609	A	22:43		[And elsewhere]. The term "extremes of daily precipitation" is a phrase used in a number of places through this report. The phrase implies a two-sided distribution – ie, low extremes (such as number of days of no rain) as well as high extremes. However, it is only ever applied in the sense of the high extremes. The simpler "daily extreme precipitation" is less ambiguous. So it is more correct to talk about an increase in extreme precipitation, rather than an increase in precipitation extremes. [There are lots of other problems with English phraseology in this chapter which I have not commented on]. [A. Brett Mullan]	Noted; will check in reworked draft.
11-610	A	22:45		Replace "is very likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-611	A	22:47		Replace "is likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-612	A	22:49	22:49	I don't know what "evaporation efficiency" means: is it Water Use Efficiency? Evaporative Fraction? [Bart Van den Hurk]	Text clarified
11-613	A	22:50	22:51	This conclusion is not consistent with section 11.3.3.3.6 (p 40), where GCM results (from only one model) show an increased storm track activitiy in the Atlantic sector. Maybe better to make a distinction between the Atlantic and Mediterranean areas in this general conclusion [Bart Van den Hurk]	Noted, box reworked
11-614	A	22:52		Replace "are very likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-615	A	22:55	22:55	The description of East Asian change might include the increase in heavy precipitation as in comment #4 in the above. (cf. the 4th and 5th paragraphs of p.52 of Chap. 11) [Masahide Kimoto]	Noted, box reworked
11-616	A	23:1		Replace "is very likely to" by "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-617	A	23:1		Climatic change information in East Asia due to global warming should be added. Kurihar et al (2005, SOLA) projected climate change over Japan due to global warming using a high resolution Regional Climate Model of 20 km mesh size(RCM20) developed in MRI. Increased daily precipitation will be seen during the warm season from June to September around Japan. Except for this period, the precipitation amount will not change much or will slightly decrease. The increase during the warm season will be seen only in	Noted, box reworked

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				the western part of Japan. Surface air temperature is projected to increase more than 2 C around Japan in January. In summer, the temperature increase will be lower by about 1 C than in winter. [Hidetaka Sasaki]	
11-618	A	23:1		Related to the previous comment. The Asia summary in Box 11.1 should be organized by sub-region (when it is complete). [Anji Seth]	Noted, box reworked
11-619	A	23:2		Replace "is likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-620	A	23:5	23:7	If this result is supported only by GCMs, the suggestion is that there is a lot of uncertainty at the regional scale, more than suggested by your comment. Or is there a lack of RCM results? [James Renwick]	Noted, box reworked and includes all sources of info
11-621	A	23:6		Replace "is likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-622	A	23:8		Replace "is likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-623	A	23:10		Any info on snow covering of the Tibet Mountains? [Marina Baldi]	Will explore.
11-624	A	23:14		Replace "are likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-625	A	23:15		Replace "are likely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-626	A	23:16		Delete "small" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-627	A	23:18		Replace "are very likely to" with "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-628	A	23:20		Replace "are likely to" with "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-629	A	23:22		Replace "are likely to" with "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-630	A	23:23		Replace " likely to" with "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-631	A	23:25		Replace " is likely to" with "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.

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11-632	A	23:26	23:26	Add "except at the highest altitudes" at the end of this sentence. Simulations indicate that warming raises the snowline so that snow season length and snow cover decrease at most altitudes. For altitudes that are still above the snowline in the warmer climate, snow can actually increase because the warmer air can transport more water vapor to produce snow. See e.g., Kim, J., T.-K. Kim, R.W. Arritt and N.L. Miller, 2002: Impacts of increased atmospheric CO2 on the hydroclimate of the western United States. Journal of Climate, 15, 1926-1942. [Raymond Arritt]	Noted, box has been reworked
11-633	A	23:26		Replace " are very likely to" with "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-634	A	23:30		Replace " is very likely to" with "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-635	A	23:31		Replace " is likely to" with "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-636	A	23:31		Insert before "global" ; "surmised" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-637	A	23:32		Replace "will likely be" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-638	A	23:33		Replace "is lilkely to" by "may" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-639	A	23:35		Delete "it is likely that" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-640	A	23:35		Replace "will" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-641	A	23:36		Replace "Its is likely that": with "possibly" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-642	A	23:38	23:41	This conclusion sounds uncertain, and depends on circulation changes. Presumably there will be large topographically-related gradients in change. [James Renwick]	Noted, box reworked
11-643	A	23:38		Replace "is likely to" by "could" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-644	A	23:42		Replace "is very likely to" by "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-645	A	23:43		Insert "possible" at the beginning [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-646	A	23:51		Replace "are very likely to with "might"	Do not accept the need for this change

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				[Vincent Gray]	
11-647	A	23:52		Insert after "amplitude". "perhaps" [Vincent Gray]	Do not accept the need for this change
11-648	A	23:52		Insert after "but", "maybe" [Vincent Gray]	Do not accept the need for this change
11-649	A	23:53	23:53	The comment that warming is smaller in the south, especially in winter, is not quite the case for New Zealand. While the spatial pattern shows less warming in the south in all seasons, the winter warming is greater than the summer warming everywhere. To me, what is writen conveys the idea that winter warming is relatively small, which is incorrect in many regions. [James Renwick]	Change made
11-650	A	23:53		Insert "surmised" before "global" [Vincent Gray]	Do not accept the need for this change
11-651	A	23:53		Replace "The warming is" by "The putative" warming could be" [Vincent Gray]	Do not accept the need for this change
11-652	A	23:54		Insert "supposed" before "warming" [Vincent Gray]	Do not accept the need for this change
11-653	A	23:54		Replace "likely to remain" by "possibly remaining" [Vincent Gray]	Do not accept the need for this change
11-654	A	24:1		Replace "is likely to" by "might" [Vincent Gray]	Do not accept the need for this change
11-655	A	24:3	24:4	There is likely to be a significant east-west gradient in the rainfall change across the South Island, with increases in the west and decreases in the east, as discussed in Mullan et al (2001a, 2005). This based on (3) and (4), though (1) - GCMs - generally are too low-resolution to pick up the topographically-related gradient. Suggest changing wording to "likely an increase in rainfall in the west of the S.I. of NZ, and a decrease in the east". Could make this part of point 5 (lines 9-10), as they are strongly related. [James Renwick]	Change made
11-656	A	24:3		Replace "will very likely" with "could" [Vincent Gray]	Do not accept the need for this change
11-657	A	24:10		Replace "likely" by "possible" [Vincent Gray]	Do not accept the need for this change
11-658	A	24:12		Replace "very likely" by "conceivable" [Vincent Gray]	Do not accept the need for this change
11-659	A	24:13		Replace "will very likely" with "could possibly" [Vincent Gray]	Do not accept the need for this change

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11-660	A	24:16	24:16	Based on: 1 and 2. Not correct that the effect is related to increased temperature. More correct to say "The effect may be related to increases in low-level wind speeds." [John McGregor]	
11-661	A	24:16	24:17	Why should increasing temperature result in increasing potential evaporation? See comment 1 and 21. [Michael Roderick]	Change made
11-662	A	24:16		Replace "likely" with "possibly" [Vincent Gray]	Do not accept the need for this change
11-663	A	24:16		At end insert "putative" [Vincent Gray]	Do not accept the need for this change
11-664	A	24:18	24:18	Could expand this to say "southern areas of Australia and eastern areas of New Zealand" - see Mullan et al (2005). [James Renwick]	Chnage made
11-665	A	24:18		Replace "very likely" with "thought possible" [Vincent Gray]	Do not accept the need for this change
11-666	A	24:21		Replace "is very likely to" with "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-667	A	24:22		Replace "is very likely to" with "could possibly" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-668	A	24:22		Replace "is likely to" with "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-669	A	24:25	24:27	Are the Authors talking about rain or snow or both? There is any trend in the snow and in the rain separately? Is it expected more rain than snow? [Marina Baldi]	Noted, box reworked
11-670	A	24:25		Replace "is very likely to" with "could possibly" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-671	A	24:26		Insert at the beginning "Despite the current cooling phase" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-672	A	24:28		Replace "likely" by "possible" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-673	A	24:28		Replace "will" by "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-674	A	24:29	24:29	Remove "AOGCM" [Bart Van den Hurk]	Noted, box reworked
11-675	A	24:30	24:31	Warming (cooling) is inappropriate; warming (neutral) would be more accurate. See, e.g.	Noted, box reworked

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				http://www.nerc-bas.ac.uk/public/icd/gjma/trends2004.col.pdf [William Connolley]	
11-676	A	24:30		Insert "and" after "data" [Vincent Gray]	Box reworked
11-677	A	24:32		Insert at the begionning ":Despite observations to the contrary" [Vincent Gray]	Disagree.
11-678	A	24:32		Replace "is very likely to" by "might" [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-679	A	24:33	24:33	Rephrase to "Large scatter in…" [Bart Van den Hurk]	Noted, box reworked
11-680	A	24:44	24:44	Why is this the only location with an explicit magnitude listed? Perhaps remove, for consistency. [James Renwick]	Noted, box reworked
11-681	A	24:44	24:44	Rather than stating on the regional temperature increase, consider relating the statement to the global mean warming as for other regions considered. [Markku Rummukainen]	Noted, box reworked
11-682	A	24:44		Replace "are likely to" by "could [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-683	A	24:46		Replace "are likely to" by "could [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-684	A	24:47		Replace "are likely to" by "could [Vincent Gray]	Disagree, this is our assessment of the likelyhood.
11-685	A	25:0	52:	Africa At my view point, as presented, this part appears a little confused and long. It would have been necessary for 11.3.2.1 to pass directly to the under area analyses. Thus one would have: 11.3.2 Africa 11.3.2.1 Key process and generality 11.3.2.1.1 West-Africa 11.3.2.1.2 Southern Africa 11.3.2.1.3 East Africa Etc. This plan would facilitate better the comprehension (It is just a proposal) [Ibouraïma YABI]	Considered, but decision is that this would result in too fragmented a text
11-686	A	25:1	26:27	This is a nice discussion. [James Renwick]	Yeah!

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11-687	A	25:1	26:27	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move much of the regional "Key processes" into it. [Markku Rummukainen]	Overview of key proces has been shortened
11-688	A	25:3	25:3	Add: "and subtropical" after the word "tropical" [Mohamed El-Shahawy]	Considered, but considered to be unecessary
11-689	A	25:6	25:6	Replace :" depressions" with "frontal zones" [Mohamed El-Shahawy]	agreed
11-690	A	25:6	25:6	Add:" depressions and the induced" before the word "storm" [Mohamed El-Shahawy]	Considered to be uneccsary
11-691	A	25:24	25:24	I suggest adding references for the Atlantic (just as they are included for the Mediterranean and ENSO), especially the old work of Lamb, and the recent work of Cook and colleagues. [Dave Rowell]	agreed
11-692	A	25:27	25:27	Please add reference to Rowell (2001, QJRMS, p1683) which also provides a detailed examination of the links between ENSO and the Sahel (using observed and model data), and proposes a detailed mechanism for this link. [Dave Rowell]	Reference will be examined and considered for inclusion
11-693	A	25:40		Write (…; Hoerling and al, 2005 (punctuation) [Ibouraïma YABI]	noted
11-694	A	25:41	25:42	Consider citing Rowell et al. (1995, QJRMS, p669) which specifically shows that soil moisture feedbacks tend to amplify the underlying response to SST anomalies. [Dave Rowell]	Reference will be examined and considered for inclusion
11-695	A	25:45		If I remember correctly there are some papers by L. Li and by Sultan and/or Janicot assessing the role of SST on the WAM (see the comment on the bibliography) [Marina Baldi]	References will be examined
11-696	A	25:47	25:47	Add: "stratospheric" before the word "aerosol" [Mohamed El-Shahawy]	The discussiant here is in refernece to tropospheric, not stratospheric, aërosol; will be clarified
11-697	A	25:49		Held et al. (2005) is missing from references. [Anji Seth]	noted
11-698	A	25:49		Write… Held and al, (2005) (punctuation) [Ibouraïma YABI]	noted
11-699	A	26:0		Figure 11.3.2.1. The 2 centre panels could be removed, as these duplicate information shown most clearly in the lower panels.	Figure will be redone

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				[Dave Rowell]	
11-700	A	26:0		Table 11.3.2.1 has no caption [Dave Rowell]	Table to be eliminated in any case
11-701	A	26:1		New et al. (2004) is also missing from references, perhaps here should be New et al. (2003)? [Anji Seth]	noted
11-702	A	26:7	26:7	Replace: "and" with " in addition to the shift of cloud clusters (El-Shahawy et al, 1991)" [Mohamed El-Shahawy]	Reference considered but deemed to be unecessary
11-703	A	26:7	26:7	Chiang and Sobel (2002) does not appear in the reference list [Dave Rowell]	noted
11-704	A	26:7	26:7	Suggest citing Rowell (2001, QJRMS, p1683) alongside Chiang and Sobel, as the former also finds a stabalising effect of ENSO on the Sahelian atmosphere. Neelin et al. (2003, GRL) is also relevant here. [Dave Rowell]	Reference will be considered for inclusion
11-705	A	26:16		Write… Paeth, 2004; (punctuation) [Ibouraïma YABI]	noted
11-706	A	26:17	26:17	"Positive feedback" can be misunderstood. [Markku Rummukainen]	Wording will be clarified
11-707	A	26:19	26:22	Delete:from the word" But given" to " in time" [Mohamed El-Shahawy]	Text considered clear as is
11-708	A	26:19	26:22	Add:"Adding vegetation models with their characterized surface roughness parameters and variable albedo could lead to substantial variations." [Mohamed El-Shahawy]	Text considered clear as is
11-709	A	26:26	26:26	In the context of a section on recent climate change, "abrupt" implies to me a timescale of <10 years. Can this term be defined here, or a definition elsewhere referred to? [Dave Rowell]	Good point – text to be rewritten
11-710	A	26:31		It is worth to reason with precise decades in accordance with the standards of the Word Meteorology Organisation. Example: 1981-200 or 1971-2000 instead of 1979-1999. [Ibouraïma YABI]	Noted, but limited by data availability in PCMDI archive
11-711	A	26:31		It would be necessary to think of observing what occurs in March-April-May (MAM) which corresponds to the beginning of the agricultural season in West Africa. The changes of these months will certainly have effects on the agricultural calendar of the peasants. [Ibouraïma YABI]	Some information on MAM will be included either here or in supplementary material
11-712	A	26:42		Figure 11.3.2.1 caption typo: replace "id" with "is" [James Renwick]	noted

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11-713	A	26:53	26:54	How many models are in the subset (ie. 4 out 5 are realistic, or 4 out of 20, or …)? [Dave Rowell]	Will be clarified
11-714	A	27:3	27:3	Biases in "the frequency of rainfall events" should be introduced already earlier. [Markku Rummukainen]	Will be considered
11-715	A	27:4	27:5	This "although may indicate…" should be explained better. [Markku Rummukainen]	agreed
11-716	A	27:7	27:11	TAR-GCM based results should ideally be compared with AR4-GCM ones, rather than discussed in isolation. [Markku Rummukainen]	agreed
11-717	A	27:13		Write… of Hoerling and Al, (2005) (punctuation) [Ibouraïma YABI]	noted
11-718	A	27:16	27:16	Sutton et al. (2000) is not in the reference list [Dave Rowell]	noted
11-719	A	27:33		The section title implies to me that the section contains the same information as 11.3.2.2, ie. provides a regional validation of global models, as well as perhaps, regional models. I suggest changing it to "Regional model simulation skill" [Dave Rowell]	Will be considered
11-720	A	27:35	27:38	The "As climate change occurs…" feels both a bit strange and also too general to place in the discussion on a specific region. [Markku Rummukainen]	Agreed, will be amended
11-721	A	28:4		Not forget the reference [Ibouraïma YABI]	noted
11-722	A	28:25		Papers by D. Parker and C. Taylor on land-atmosphere interaction should be commented and added here. [Marina Baldi]	noted
11-723	A	29:7		Table 11.3.2.1: No caption. [James Renwick]	noted
11-724	A	29:20		Replace "predict rather well" with "simulate by adjustment of model parameters" [Vincent Gray]	To our knowledge, in climate model development no parameters are adjusted to fit trends in specific regions
11-725	A	29:22	29:24	Something should be wrong, concerning the time period. [Michel Boko]	noted
11-726	A	29:22	29:22	Hyphen missed: 2080-2100 (need to check for this in many other places too) [James Renwick]	noted
11-727	A	29:22		Write in the 2080-2100 [Ibouraïma YABI]	noted

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11-728	A	29:24		Write in the 2079-2099 [Ibouraïma YABI]	noted
11-729	A	29:24		Is it possible to use the period 2081-2100? [Ibouraïma YABI]	A shift of 1 yr would have no affect on the conclusions
11-730	A	29:27		Not forget to specify the year [Ibouraïma YABI]	noted
11-731	A	29:34		Section 11.3.2.4.2: I found this section very interesting to read, and it does illustrate some important points, but I suspect it could be shorter. For example, the discussion of changes in the Sahel region could be synthesised more succinctly, and the comparsion of empirical vs dynamical downscaling could be summarised further, without losing the important messages. [James Renwick]	This section will be shortened
11-732	A	29:34		Sect.11.3.2.4.2. I would imagine that a number of references to the material of Neelin et al. (2003, GRL) throughout this section would be helpful to explain some of the changes found. [Dave Rowell]	Possibly, but conclusions in this reference not thought to be sufficiently unambigous as yet
11-733	A	29:34		Sect.11.3.2.4.2. It would seem to me that extensive citation of Collins et al. (2005, Clim. Dyn., submitted, Figs.10&11) (which I presume has been made available to the authors) throughout this section is warranted. It too provides a multi-model comparison (using physics perturbations of HadCM3). Where their spread disagrees with that shown here would be particularly important. [Dave Rowell]	References to this significant paper will be included in revision
11-734	A	29:35		Delete "some of the robust aspects of" [Vincent Gray]	"robust" to be replaced by "consensus"
11-735	A	29:38	29:38	The ")" in mid-line should be ". " [James Renwick]	noted
11-736	A	29:41		Figure 11.3.2.3: The caption should explain the units for the top row of plots (fractional rainfall change, future rainfall divided by past rainfall?). The text states the values are percentages, but the colour bar labels do not support that. [James Renwick]	noted
11-737	A	29:43	29:43	Should be "A2 and B1" rather than "A1 and B2"? [Markku Rummukainen]	noted
11-738	A	29:44	29:45	Delete "the most robust featires", and remove the parentheses [Vincent Gray]	Change to be considered
11-739	A	29:47	29:47	The ")" should be removed. [James Renwick]	noted

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11-740	A	30:5	30:9	Lines 5-7: The main processes highlighted in Sect.11.3.3 are (a) a warming leading to reduced RH and (b) reduced soil moisture; since the reader has already been referred to Sect.11.3.3, I suggest this is sufficient, and the sentence on lines 5-7 of p30 is removed. The reference back to this sentence from line 9 then also becomes inappropriate. [Dave Rowell]	agreed
11-741	A	30:19		Replace "robust" by "much the same" [Vincent Gray]	Change to be considered
11-742	A	30:24	30:24	I would say this lack of consistency is easy to explain. The Sahel response will be dominated by SST changes in the Atlantic, Mediterranean, Pacific and Indian Oceans, and by the complex interaction between these changes and the changes to the tropical atmospheric basic state. Personally, I would be rather surprised if there were any consistency between the models given this complexity, and more so given the likely disparity in the SST responses in these regions between models (though I have not checked this). [Dave Rowell]	noted
11-743	A	30:25	30:25	should read "…is generated by a large response…" [James Renwick]	noted
11-744	A	30:26		write… model together). (Punctuation) [Ibouraïma YABI]	noted
11-745	A	30:29	30:33	The justification for focussing 2 sentences on just 2 of the models was not clear. In particular, the claim that they both have "realistic interannual variability" is subjective, unsubstantiated (the cited paper is not even recorded in the reference list), and in my experience of the skill of even the best models in this region, not very likely. Opportunities to make this very long chapter more concise should be taken, so I suggest removing these 2 sentences! [Dave Rowell]	Idea is to illustrate spread in model projection with two models that are considered to be of relatively high quality – changed in wording will be considered
11-746	A	30:30	30:30	Vizy and Cook (2005) is not in the reference list. [Dave Rowell]	noted
11-747	A	30:38	30:41	If the authors do not agree with the validity of the results of this paper (with good reason it would seem), could these 2 sentences simply be removed? [Dave Rowell]	agreed
11-748	A	30:39	30:39	The reference to Haarsma et al. (2005) does not appear in thereference list. The complete reference is given in remark 3 [Reindert Haarsma]	noted
11-749	A	30:39	30:39	Missing Ref: Paeth H., Born K., Podzun R., Jacob. D. (2005): Regional dynamical downscaling over West Africa: model evaluation and comparison of wet and dry days.	noted

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				Meteorologische Zeitschrift; 14, 349-367 ... wet and dry days (Paeth et al.)… [Daniela Jacob]	
11-750	A	30:48	11:50	This statement is dangerous in regards of Kyoto Protocol goals. Moreover, I think that it could be contradictory with historical consideration from Holocene climate change modelling. See page 32, lines 18-22. [Michel Boko]	We believe that the reviewer has misunderstood these sentences – will be rewritten for clarity
11-751	A	31:1	31:1	Should 2015 and 1996 be reversed? [Dave Rowell]	noted
11-752	A	31:11	31:17	I suggest referring to "HadAM3P" instead of "PRECIS", so that this model is clearly and appropriate identified with the Hadley Centre family of models. [Dave Rowell]	noted
11-753	A	31:11	31:11	Reference for Tadross et al. is missing. [Martin Stendel]	noted
11-754	A	31:11	31:11	Replace "scarcer" by "more scarce". [Martin Stendel]	noted
11-755	A	31:11		Not forget to specify the year [Ibouraïma YABI]	noted
11-756	A	31:12	31:12	Replace "GCM.." by "GCM.". [Martin Stendel]	noted
11-757	A	31:36	31:53	Can the methods part of this paragraph go in 11.2.1 and removed from the text here? [Dave Rowell]	Some of this material will be moved or omitted
11-758	A	31:36	32:22	It's not clear to me that the results of a single paper (led by one of the convening lead authors!) justifies such a disproportionally long contribution to this section, as well as the inclusion of a diagram. Also, I suppose the downscaling method of assuming that links between circulation and precipitation are unchanged in the future climate would have substantial concerns for many experts. I would suggest reducing this material to 2-3 sentences, and removing the diagram. [Dave Rowell]	This section will be shortened somewhat, but it remains a parrticularly relevant downscaling study in this region, and the lead author team as a whole considers it appropriate to include a figure
11-759	A	31:54	32:4	Might the training period for these empirical downscalings be biased on the recent dry decades in Sahel and would this affect these consensus results? [Markku Rummukainen]	This has not been addressed in the litterature. We believe emphasis on these findings are important in the present context.
11-760	A	32:0		Fig.11.3.2.4. The shading style of this diagram makes it very hard to interpret quickly. [Dave Rowell]	Figure will be revised
11-761	A	32:6		Figure 11.3.2.4: I find this figure hard to read. Hopefully the final version will be clearer.	Figure will be revised

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				[James Renwick]	
11-762	A	32:6		Comment Figure 11.3.2.4 ECHAM4.5 is not a coupled OAGCM. please, specify the ocean component [Eduardo Zorita]	noted
11-763	A	32:12	32:12	Date for Tadros et al reference [Andrew Lacis]	noted
11-764	A	32:12		Not forget to specify the year [Ibouraïma YABI]	noted
11-765	A	32:15	32:16	This statement is too bold - it is not true in some locations, for example, SE Africa. Better to say consistent with many of the native GCM fields at GCM resolutions. [David Rind]	Statement will be revised
11-766	A	32:22		Write literature. (Punctuation) [Ibouraïma YABI]	noted
11-767	A	32:24		Sect.11.3.2.5. I think another uncertainty needs to be added, that the changes in this region are highly dependent on the predicted SST changes, and SST gradient changes, which are highly uncertain at present (eg. Collins et al 2005, Clim. Dyn., p89, and probably a diagram elsewhere in AR4), as these depend on correctly modelling subtle and complex coupling of the ocean-atmosphere system. [Dave Rowell]	Good point, will be addressed
11-768	A	32:28		Write… reliability; (Punctuation) [Ibouraïma YABI]	noted
11-769	A	32:32	32:33	North America is influenced by more patterns than just ENSO and the AO, the most prominent of these being the PNA (which is at least partly independent from ENSO). [Michael Alexander Alexander]	(comment out of order) noted
11-770	A	32:32		Write… models (Punctuation) [Ibouraïma YABI]	noted
11-771	A	32:36		Write… SST); (Punctuation) [Ibouraïma YABI]	noted
11-772	A	32:41		Write… areas; (Punctuation) [Ibouraïma YABI]	noted
11-773	A	32:46	33:35	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move much of the regional "Key processes" into it. [Markku Rummukainen]	Agreed. Text shortened.
11-774	A	32:54	32:54	Scaife et al (2005) demonstrate using the first GCM simulation of the full NAO trend that	Agreed. Reference added.

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				changes in European climate in winter are dominated by the NAO and can be accurately reproduced if the NAO shift is included. Suggest adding Scaife et al (2005) to the references here. A.A.Scaife, J.R.Knight, C.K.Folland and G.K.Vallis 2005, A stratospheric Influence on the winter NAO and North Atlantic Surface Climate. Geophys. Res. Lett., 32, L18715. [Chris Folland]	
11-775	A	33:1	33:5	The phrase is slightly misleading: NAO has a high influence not only in northwestern European climate but also on Iberian climate (the classical precipitation dipole). The term "Mediterranean precipitation" is vague. More: even though there are regional differences in the magnitude of NAO-related precipitation in the Mediterranean regions, the cited studies identify there NAO/precipitation relationships on interannual scale, too. [Roxana Bojariu]	Agreed. Text reformulated
11-776	A	33:1	33:5	It would seem prudent to reformulate the following: "NAO has the highest influence upon", "is also responsible for", "and controls". Things do vary in concert with NAO, but whether NAO is a factor by itself or a reflection of something else is another matter. [Markku Rummukainen]	Agreed. Text reformulated
11-777	A	33:2		See also: reference: Fowler, H.J. and Kilsby, C.G. 2002. Precipitation and the North Atlantic Oscillation: A study of climatic variability in Northern England. Int. J. Climatol., 22, 843-866. [Hayley Fowler]	Noted. Most of the general discussion on NAO will be deleted.
11-778	A	33:5	33:5	"controls the snow cover" This sentence is too long. Precise also what is meant by "surface atmosphere temperature feedback". The paper by Beniston is more focussed on temperature. See also the paper by Scherrer and Appenzeller (Geophys. Research letters, Vol 31, L13215, doi:10.1029/2004GL020255, 2004) where it is said that the influence of NAO varies with the region of the Alps and that the recent decrease can mainly be attributed to the temperature increase [ERIC MARTIN]	Text deleted for brevity.
11-779	A	33:5		REPLACE: "the snow cover" by "the decadal trends in snow cover". COMMENT: The role of the NAO in explaining inter-annual varibailty in Alpine snow cover is small, but the NAO is important for decadal and longer trends as shown in detail by Scherrer et al. REFERENCE: Scherrer, S.C., C. Appenzeller, and M. Laternser, 2004: Trends in Swiss alpine snow days – the role of local and large scale climate variability. Geophys. Res. Letts., 31, doi:10.1029/2004GL020255 [Christof Appenzeller]	Text deleted for brevity.
11-780	A	33:6	33:6	Mediterranean cyclogenesis and blocking in cold seasons are also related to the NAO (e.g.	Text deleted for brevity.

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				Quadrelli et al. 2001). [Roxana Bojariu]	
11-781	A	33:13		Effects of WAM on Mediterranean climate variability are under studies and preliminary results shown at AMS conferences (Baldi et al, 2005) and discussed in the White Paper on Mediterranean Climate Variability and Predictability [Marina Baldi]	Rejected. Insufficient space.
11-782	A	33:24	33:24	Include a reference to "Klein Tank, A.M.G. and G.P. Konnen (1997): Simple temperature scenario for a gulf stream induced climate change; Climatic Change 37, 505-512." [Bart Van den Hurk]	Reference will be considered if space allows
11-783	A	33:36		The White Paper on Mediterranean Climate Variability and Predictability. Eds: P. Lionello. Elsevier. In press. 2005. Should be mentioned at this point [Marina Baldi]	Reference will be considered if space allows.
11-784	A	33:36		Summer climate variability in Med basin has been analized in Baldi M., F. Cesarone, G.A. Dalu, G. Maracchi, M. Pasqui, 2005: Heat-waves in the Mediterranean: a local feature or a larger scale effect? Revised for the Int Journal of Climatology. [Marina Baldi]	Reference will be considered if space allows.
11-785	A	33:37		121.3.2 should comment on how well the coupled models do in producing the observed SSTs, ENSOs, and NADW. [David Rind]	Rejected. These topics are discussed in Ch. 8.
11-786	A	33:42	33:42	SEU is not in Fig. 11.3.1.1. Also [David Rind]	Noted. Editorial action will be taken to ensure consistency.
11-787	A	33:47	33:47	Stray comma (after The). There are several similar small typos scattered through the text. [James Renwick]	Agreed.
11-788	A	33:47	33:47	remove "," [Bart Van den Hurk]	Agreed.
11-789	A	34:5	34:13	The point needs to be made somewhere that RCM-based simulations of present climate are still not normally adequate for direct application in impact modelling due to the biases described here (cf. Fronzek and Carter, 2005; Olesen et al., 2005). The "delta" change method, using observations to represent present-day conditions and adding the change between modelled present and modelled future climate (delta), is still preferred in most applications. [Timothy Carter]	Noted. Will be considered in Section 11.2.1.3 if space allows.
11-790	A	34:11		See also reference: Ekström, M., Jones, P.D., Fowler, H.J., Lenderink, G., Buishand, A. and Conway, D. Regional climate model data used within the SWURVE project. 1: Projected changes in seasonal patterns and estimation of PET. Hydrology and Earth System Sciences, in press; for a discussion of temperature/soil moisture/radiation	Rejected. Reference not essential to the point.

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				interactions in southern Europe and effects on the estimation of PET. [Hayley Fowler]	
11-791	A	34:12	34:12	Insert "Van den Hurk et al (2005) analysed the soil hydrological memory in the Rhine basin using large scale analyses of atmospheric water convergence and river discharge. They concluded that the depth of the hydrological soil reservoir in RCM models is indicative for the strength of the hydrological response of the whole river basin to a global temperature increase, and a proper specification of this depth is an important factor." after "both." (see next comment for full reference) [Bart Van den Hurk]	Agreed. Reference added. This paper may also be important for the 'hot and dry lock issue', with possible implications on (1) overestimate of present-day variability, (2) exaggerated greenhouse-gas induced warming in summer?
11-792	A	34:13	34:13	Too much sea ice in the Barents Sea? [Rasmus E. Benestad]	Noted. Unlikely to explain excessive variance in PRUDENCE simulations.
11-793	A	34:17	34:23	The study by Kjellstrom points at difficulties in modelling tails of temperature distributions, but this section does not give an indication of what is the suspected problem in the used models. It is therefore difficult to assess the value of claims of changed temperature variability under conditions of future global warming. [Bart Van den Hurk]	Noted. Some discussion added.
11-794	A	34:17		In the "regional simulation skill section" there is no reference made to Chapter 3, where many observational databases and analyses are used. [Bart Van den Hurk]	Noted. Comparison between simulated and observed changes is problematic due to the low S/N-ratio and is therefore mostly omitted here..
11-795	A	34:25	34:36	The large spread in return values a result of short time intervals in the time slices? [Rasmus E. Benestad]	Noted. Unlikely because of spatial averaging and common boundary conditions
11-796	A	34:33	34:33	Buonomo et al. (2005) is not in the reference list [Dave Rowell]	Agreed. Will be fixed. Subject to the paper been accepted!
11-797	A	34:49		Description of Prudence is good at this point. [Marina Baldi]	Thanks.
11-798	A	35:2	35:2	Radiative uncertainty is of course a larger issue than just the choice of SRES. [Markku Rummukainen]	Agreed. Reworded as emission uncertainty.
11-799	A	35:9		Replace "experiment" by "simulation" [Vincent Gray]	Disagree. A pair of two simulations compared with each other is commonly known as model experiment.
11-800	A	35:14	35:19	Should probably add the supscript (a) also on the second and last entry line on the table. [Markku Rummukainen]	Agreed. In fact, this should be (b) rather than A (for the B2 scenario)).
11-801	A	35:14		Replace "experiment" by "simulation" [Vincent Gray]	Rejected (see 11-799)

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11-802	A	35:16		Replace "experiments" by "simulation" [Vincent Gray]	Rejected (see 11-799)
11-803	A	35:23	35:28	Discussion based on single experiments should be avoided. Or is the bottom line that this is a major issue and that it is presently unknown for models in general perform in this respect? [Markku Rummukainen]	Agreed. Text deleted.
11-804	A	35:30		From this point and for the rest of the paragraph 11.3.3.3 it is not clear if Authors refer only to Prudence results or to a broader number of studies. [Marina Baldi]	Noted. Should not be unclear after finalizing PRUDENCE box layout.
11-805	A	35:33	35:33	"NEU" and "SEU" are undefined. [Daniel Caya]	Noted. Editorial action taken to ensure consistency.
11-806	A	35:33	35:34	I would prefer to see the extremes of seasonal means quoted, rather than annual means. The former are more relevant, and this would also be consistent with Sect.11.3.3.2. [Dave Rowell]	Disagreed. Annual means preferred. Seasonal means are given in a table.
11-807	A	35:33	35:34	Strange to express temperature change as a percentage. Use the number of degrees instead of %. [Bart Van den Hurk]	Disagreed. Ratio is more robust than absolute difference to the global mean.
11-808	A	35:36	35:38	Add references to Giorgi et al. (2004) and Rowell (2005b) which both discuss variations of the temperature response within Europe. Giorgi F, Bi X, Pal JS (2004) Mean, interannual variability and trends in a regional climate change experiment over Europe. II: climate change scenarios (2071-2100). Clim Dynam 23: 839-858. Rowell, D.P., 2005b: A scenario of European climate change for the late 21st century: seasonal means and interannual variability. Clim. Dyn., in press (available from the Clim. Dyn. website) [Dave Rowell]	Disagreed. Single-model studies bring nothing essentially new here.
11-809	A	35:40	35:41	The sentence needs to be clarified. [Daniel Caya]	Agreed. Will be clarified.
11-810	A	36:0		It was not clear for me if and how much the results presented on figures 11.3.3…. reflect also the possible cooling effect due to the decrease in the North Atlantic Thermohaline Circulation (THC) described in subchapter 11.3.3 page 36. [Jaak Jaagus]	Noted. Changes in ocean circulation are included. We will try to clarify this.
11-811	A	36:2	36:2	21000 -> 2100 [Daniel Caya]	Agreed.
11-812	A	36:5	36:5	It feels a bit off to call the bigger part of the warming "residual". [Markku Rummukainen]	Agreed. Wording will be modified.
11-813	A	36:6		Write… Van Ulden and al, (punctuation) [Ibouraïma YABI]	Agreed.

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11-814	A	36:8	36:10	The last sentence of this paragraph seems too tentative to me. Is it not self-evident that circulation variability will continue, on all time scales, into the foreseeable future? Hence, the sentence could end "…natural variations of the circulation will cause pronounced temperature variations on many time scales, into the future." [James Renwick]	Agreed. Wording will be modified.
11-815	A	36:23	36:23	An extensive discussion of temperture changes over Europe asociated with reduction of NADW and no increase in greenhouse gases is given in Rind, D., P. DeMenocal, G. Russell, S. Sheth, D. Collins, G. Schmidt and J. Teller, 2001: Effects of glacial meltwater in the GISS Coupled Atmosphere-Ocean Model: Part I: North Atlantic Deep Water response. J. Geophys. Res. 106, 27335-27354, and in Rind, D., G. Russell, G. Schmidt, S. Sheth, D. Collins, P. DeMenocal and J. Teller, 2001: Effects of glacial meltwater in the GISS Coupled Atmosphere-Ocean Model: Part II. A bipolar seesaw in Atlantic deep water production. J. Geophys. Res.106, 27355-27367. As shown in Figure 6 in each of those publications, with a complete NADW shutdown, cooling can exceed 3 C over extreme northwestern Europe (see Fig. 6 in part II for the clearest presentation), and if it goes on long enough, it can influence temperature globally (Fig. 6 part I after 100 years). [David Rind]	Agreed. Text modified to also reflect these studies.
11-816	A	36:23	36:23	Include a reference to "Klein Tank, A.M.G. and G.P. Konnen (1997): Simple temperature scenario for a gulf stream induced climate change; Climatic Change 37, 505-512." [Bart Van den Hurk]	Rejected. No quantitative information to this issue
11-817	A	36:26	36:28	Again, this is true for monthly/seasonal means but not necessarily for changes on shorter time scales [Erik Kjellström]	Noted. Our discsussion only refers to the mean climate; there is not enough information to extend it to daily scales.
11-818	A	36:28	36:28	Rowell (2005a) also shows that RCM uncertainty is less than GCM uncertainty [Dave Rowell]	Noted. We will consider the reference if space allows.
11-819	A	36:33	36:33	I have not read the entire draft and I'm not quite sure what constitutes the IPCC AR4 model ensembles: I had the impression it was the latest integrations posted at https://esg.llnl.gov:8443/index.jsp. In that case, it is to my knowledge only Benestad (2005) that is up-to-date, whereas the other references are for the TAR-integrations. [Rasmus E. Benestad]	Noted. Not critical for the general discussion.
11-820	A	36:38	36:40	Why the CC signal differences were largest in winter? [Daniel Caya]	Noted. No space to dicuss this in detail.
11-821	A	36:40	36:40	Unclear what is meant with the phrase "localities exposed to temperature inversions". Please rephrase and make clearer. [Bart Van den Hurk]	Wording clarified.
11-822	A	36:42	36:43	I do not understand the meaning of this sentence.	Agreed. Sentence deleted.

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				[Daniel Caya]	
11-823	A	36:45		Sect.11.3.3.3.2.Two key and very recent papers are both uncited in this section: Giorgi et al. (2004) and Rowell (2005b). Both study projected European temperature change on sub-continental scales, for all 4 seasons. Giorgi et al. use small box averages. Rowell shows grid-point maps, and discusses some of the mechanisms involved.Giorgi F, Bi X, Pal JS (2004) Mean, interannual variability and trends in a regional climate change experiment over Europe. II: climate change scenarios (2071-2100). Clim Dynam 23: 839-858. Rowell, D.P., 2005b: A scenario of European climate change for the late 21st century: seasonal means and interannual variability. Clim. Dyn., in press (available from the Clim. Dyn. website) [Dave Rowell]	Agreed. References added.
11-824	A	36:45		Sect.11.3.3.3.2. Would a couple of sentences at the end of this section on spring and autumn also be appropriate? Giorgi et al. (2004) and Rowell (2005b) provide the necessary material. [Dave Rowell]	Noted. Will be considered if space allows.
11-825	A	36:45		Pal, J., F. Giorgi and X. Bi, 2004: Consistency of recent European summer precipitation trends and extremes with future regional climate projections, GRL, 31, L13202, doi:10.1029/2004GL019836. Should be mentioned either in 11.3.3.3.2 or 11.3.3.3.4. [Anji Seth]	Agreed. Reference added to 11.3.3.4.
11-826	A	36:47		Proposed revision: "However, the magnitude, seasonal evolution and geographical location of the increase is model dependent." [Christoph Schar]	Original formulation preferred as simpler.
11-827	A	36:49	36:49	19611990 -> 1961-1990 [Daniel Caya]	Fixed.
11-828	A	36:49	36:49	1961-1990 [James Renwick]	Fixed.
11-829	A	36:49	36:49	insert "-" in "19611990" [Bart Van den Hurk]	Fixed.
11-830	A	36:51	36:53	Rowell (2005b) also discusses this mechanism for increased summer variance. [Dave Rowell]	Reference added.
11-831	A	36:55		Discussion of Giorgi and Bi (2005): This should probably be revised, as Giorgi and Bi do not provide data for Central Europe, which is the region where the previous studies show most effect. [Christoph Schar]	Agreed. Discussion of the magnitude of changes deleted.
11-832	A	36:56	37:2	Martin Beniston's work on the 2003 European heat wave also should be mentioned here, e.g., Beniston, M., 2004: The 2003 heat wave in Europe: a shape of things to come?	Noted. References will be considered if space allows.

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				Geophys. Res. Lett. 31, L02202, also Beniston, M. and H.F. Diaz, 2004: The 2003 heat wave as an example of summers in a greenhouse climate? Observations and climate model simulations for Basel, Switzerland. Global and Planetary Change 44, 73–81. [Raymond Arritt]	
11-833	A	36:56	37:2	What is the assessment angle here? One could consider omitting the sentence. [Markku Rummukainen]	Text revised.
11-834	A	36:56	37:2	Stott et al (2004) used a large averaging domain (which contains continental Europe as well as most of the Mediterranean sea). Averages on such large domains smooth out the variability effect (see also the corresponding discussion in chapter 9). The atmospheric model used by Stott et al actually has one of the strongest variability increases (cf. the high-resolution version of the HadAM3 considered in Vidale et al. 2005). As regards recent observations, a recent analysis of temperature variability in Europe (Scherrer et al 2005) shows a slight trend in observations, even when the summer 2003 is not included in the analysis. (continued in next comment) [Christoph Schar]	Partially agreed. Text shortened.
11-835	A	36:57	37:2	(continued from previous comment) It is thus suggested that the text, starting in the middle of l.56, p.36, is replaced as follows: "Schär et al (2004) raised the possibility that increased variability may have played a role in producing the European heatwave in summer 2003. A recent study of European temperature variability during the last decades shows a weak trend towards increasing summer temperature variability, but the trend is not statistically significant (Scherrer et al. 2005). Ref.: Scherrer, S.C., C. Appenzeller, M.A. Liniger and C. Schär, 2005: Central European temperature distribution changes in recent observations and climate change scenarios. Geophys. Res. Letters, 32, L19705 [Christoph Schar]	Noted. Text shortened.
11-836	A	37:0		11.3.3.3.3. The hot and dry "lock in" that occurs over some RCM simulations over southern Europe should be mentioned (spurious positive feedback loop with drying soil moisture). I think the first CLA is very familiar with this model phenomenon and can provide appropriate references. [Raymond Arritt]	Noted. The difficulty here is that it is not clear if this is spurious in the case of simulated climate changes.
11-837	A	37:8	37:8	There is a good explanation for the fact that central European temperature variability increases more than in Southern Europe: there a dry soil is encountered nearly every year, giving a robust and only marginally varying high summer time temperature signal. In central Europe, these dry soil conditions are met in some years, but not in all, giving a	We agree with the explanation but are forced to leave it out for brevity and because the dry summer bias in many models complicates the interpretation.

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				strong interannual variability. [Bart Van den Hurk]	
11-838	A	37:17	37:28	Also here, a reference to observed trends (Ch 3) would be valuable. Is a stronger warming of the cool tails consistent with observed trends? [Bart Van den Hurk]	Agreed. A reference has been added.
11-839	A	37:18	37:18	Also cite Giorgi et al. (2004) and Rowell (2005b) for interannual variability. [Dave Rowell]	Agreed
11-840	A	37:22	37:24	Rowell (2005b) also makes a similar point in the context of interannual variability. [Dave Rowell]	Agreed. Reference added.
11-841	A	37:25	37:25	"cold-air outbreaks" should be defined. [Daniel Caya]	Noted. Defined two rows below.
11-842	A	37:35	37:35	Rephrase to "The change was seasonally variable in northernmost Europe" [Bart Van den Hurk]	Agreed.
11-843	A	37:38		Section 11.3.3.3.3: Can the circulation changes be discussed in a broader hemispheric context? Are the differing results in Figure 11.3.3.4 symptomatic of different trends in the NAO? Does either model do a good job of NAO variability in the present climate? Since the precipitation change is so strongly linked to circulation change, at least in the higher middle latitudes, can we really say anything with confidence, when there are such differences in MSLP changes between models? [James Renwick]	Noted. The results in this figure will be put in the context of the AR4 simulations.
11-844	A	37:38		Sect.11.3.3.3.3. Again Rowell (2005b) and Giorgi et al. (2004) are published peer-reviewed studies that could be used to supplement the IPCC-DDC analysis.Giorgi F, Bi X, Pal JS (2004) Mean, interannual variability and trends in a regional climate change experiment over Europe. II: climate change scenarios (2071-2100). Clim Dynam 23: 839-858. Rowell, D.P., 2005b: A scenario of European climate change for the late 21st century: seasonal means and interannual variability. Clim. Dyn., in press (available from the Clim. Dyn. website) [Dave Rowell]	Disagreed. These single-model studies do not alter the multi-model message.
11-845	A	37:42	37:42	insert "and central" after "northern" and delete ", when models also tend to simulate increases in central Europe" [Bart Van den Hurk]	Agreed
11-846	A	37:48	37:48	physiography" -> "topography [Bart Van den Hurk]	Agreed.
11-847	A	38:0		Figure 11.3.3.3 is not annoced in the text [Ibouraïma YABI]	Fixed.
11-848	A	38:1	38:1	rather than referring to "top" and "bottom" panels, refer to "ECHAM4" and "HadAM3"	Noted. We try to minimize the use of

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				driven experiments [Bart Van den Hurk]	model names in the text.
11-849	A	38:6	38:7	It should be emphasized at the outset that this study is carried out at the monthly timescale, which will amplify circulation influences compared to seasonal timescales. [Dave Rowell]	Noted. The difference is not essential, particularly for the multi-model mean. Also, the figure has been eliminated.
11-850	A	38:15		Write.....1987). [Ibouraïma YABI]	Agreed.
11-851	A	38:32	38:35	This paragraph is in contradiction with lines 49-51 on page 11-37. [Daniel Caya]	Noted. Text will be reformulated and moved to Section 11.2.2.2
11-852	A	38:32	38:33	This is not always the case. See for instance the discussion concerning Figure 11.3.3.4 (p37,l49 to p38, l2).In addition, Kjellström and Ruosteenoja (Climatic Change, 2005, submitted) show a very large sensitivity of simulated precipitation to the choice of GCM (and in particular GCM SSTs) for the Baltic Sea in the PRUDENCE common A2 experiment. [Erik Kjellström]	Noted. See 11-851.
11-853	A	38:35	38:35	Rowell (2005a) also addresses RCM versus GCM uncertainty. [Dave Rowell]	Noted. Reference will be considered if space allows.
11-854	A	38:37		Although SD Methods have been developed note that many studies in the US have shown that using the outputs of RCMs directly but with bias correction provides a good estimate of local precipitation and temperature timeseries (i.e. simulated matches observed) for impact studies. Some work on thi has recently been done in Europe, in northwest England, where bias-corrected outputs from HadRM3H have been used to reproduce the statistics of observed river flows and then used to examine the impacts on the NW water supply system (see: Fowler, H.J. and Kilsby, C.G. Using regional climate model data to simulate historical and future river flows in the UK. Climatic Change, accepted subject to minor revisions, and Fowler, H.J., Kilsby, C.G. and Stunell, J. Modelling the impacts of projected future climate change on water resources in northwest England. Hydrology and Earth System Science, in press) Therefore, the use of SD methods is not always necessary! [Hayley Fowler]	Noted. Text reformulated.
11-855	A	38:43	38:43	Benestad 2002b' should be 'Benestad, 2005' (GRL,32 doi:10.1029/2005GL023401 No. 17, L17704) [Rasmus E. Benestad]	Agreed.
11-856	A	38:44	38:44	The precipitation change in Romania should be illustrated with a scientific reference [Bart Van den Hurk]	Noted. Reference added to reformulated text.
11-857	A	38:48	38:49	The precipitation increase in Iberia shown by Trigo and Palutikov is not consistent with	Noted. Discussion deleted since the

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				for instance 11.3.3.4. This is a clear demonstration where statistical and dynamical downscaling make different use of large scale information. [Bart Van den Hurk]	precipitation increase is associated more with the specific GCM (HadCM2) used than with differences between the downscaling and the GCM.
11-858	A	38:51		Sect.11.3.3.3.4. I found this section particularly long, and excessively focussed on the detailed results of a few papers, rather than drawing these (and perhaps other papers) together in an overview. [Dave Rowell]	Agreed. Section will be shortened and reorganized.
11-859	A	38:51		Sect.11.3.3.3.4. Should this section follow sect.11.3.3.3.2, and also cover interannual variability as well as daily extremes? The (nearly) published studies of Giorgi et al. (2004) and Rowell (2005b) provide useful material. [Dave Rowell]	Noted. Suggestion will be considered if space allows.
11-860	A	39:3	39:5	Avoid duplicating information in the figure caption in the running text (such as the coordinates). [Markku Rummukainen]	Agreed.
11-861	A	39:5	39:5	I wonder whether the precipitation changes between 2 and 11% reported by Frei et al are significant or not [Bart Van den Hurk]	Noted. Numbers have been eliminated.
11-862	A	39:20	39:48	I'm a bit unsure about the meaning in '2-20 year return period' and 'once in 8-18 years'. Unusual way of presentation 1/Pr(X < x). Should be checked/explanation given? [Rasmus E. Benestad]	Noted. Text will be clarified.
11-863	A	39:27		Note also that Ekstrom et al. (2005) found that projected changes in extreme rainfall were specific to return period - there was a larger increase at higher return periods, and for longer duration events (i.e. larger increases for 10 day rather than 1 day events) Note also that for the south of the UK, the more recent HadRM3H model projections suggest reductions in extreme rainfall at higher return periods! [Hayley Fowler]	Noted. We cannot go this deep into the details.
11-864	A	39:33	39:49	Increase in extreme one day or several day precipitation totals in warmer summer climate in Central Europe can be designed also by simple precipitation model. Considering 1.6 – 3.8 C warming and increase of water vapor content by 13 - 25% in summer (by 3 GCMs) it resulted in significant increase of extreme precipitation totals during strong thunderstorms and exceptional cyclonic weather (at 5-day totals it was modeled by 23 – 42% in the April to September season and the 2075 time frame). These results have been published in: LAPIN, M. and HLAVCOVA, K., 2003: Changes in Summer Type of Flash Floods in the Slovak Car-pat-hians due to Changing Climate. Proceedings of the International Conference on Alpine Meteoro-logy and MAP2003 Meeting, Brig, Swit-zerland, 19.-	Noted. Reference will be considered if space allows.

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				23.V.2003, Publ. Of MeteoSwiss, No. 66, 105-108. [Milan Lapin]	
11-865	A	39:36	39:39	The quoted increases in interannual variability of extreme precip by Raisanen and Giorgi and Bi are to my opninion fairly speculative, given the poor knowledge on the observed AND modelled precipitation extremes. I think such a disclaimer remark is justified here. [Bart Van den Hurk]	Noted. Text reformulated.
11-866	A	39:36	:39	Is this really "particularly in the Mediterranean" (see Fig.9 of Raisanen 2002)? The diagram is small and from low-resolution models, but to me it looks more like a signal in Central and Eastern Europe. [Christoph Schar]	Noted. From Fig. 6 in the mentioned paper, the original formulation is valid.
11-867	A	39:43		Also see results from Ekstrom et al (2005) - see above - examined events from 1 day to 10 day durations and found larger increases for longer duration events (both frequency and magnitude). [Hayley Fowler]	Noted. This goes deeper into the details than space allows.
11-868	A	40:6	40:10	Why should increasing temperature result in increasing potential evaporation? See comment 1 and 21. [Michael Roderick]	Noted. Wording modified.
11-869	A	40:18	:30	Can you return this part into 11.3.3.4? [Ibouraïma YABI]	Disagreed. This text belongs here.
11-870	A	40:44		Figure 11.3.3.7: Can you provide some synthesis text, or more explanation? This figure does not tell me much, at least not without a lot of study. [James Renwick]	Figure deleted.
11-871	A	40:46		Sect.11.3.3.3.6. A recent study (Castro et al, under review, I think) found a tropical cyclone in the Mediterranean in PRUDENCE data. Although uncertainty is large, the impacts would be huge, and so I think this study should be mentioned. [Dave Rowell]	Noted. Consideration conditional to acceptance of the paper.
11-872	A	40:52	40:53	It seems unnecessary detail to state on a single model, unless this says something about the models in general such as many lacking a certaing process. [Markku Rummukainen]	Agreed. Sentence deleted.
11-873	A	41:6	41:6	the words "… and intensity…" contradict the statements in the rest of the paragraph. Delete "and intensity" [Piero Lionello]	Agreed.
11-874	A	41:6	41:6	The areas of cyclogenesis were found not affected also by Lionello et al 2002. You migh consider adding this reference beside Somot, 2005 [Piero Lionello]	Noted. Space limitations force us to shorten this discussion.

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11-875	A	41:9	41:35	A number of key references are missed in this section. In particular the work of McInness and of Flather and of Lowe should be used to further illustrate the uncertainty due to different driving models. Woth's simulations are limited in the driving GCM. Work from the Stowasus and (possibly) WASA project should also be quoted. J Wolf has produced some work on climate change projections of wave characteristics. [Jason Lowe]	Noted. Lowe and Gregory is referred to in Box 11.4.
11-876	A	41:9	41:35	Suggest incorporating this in Box 11-4 (page 95). [Markku Rummukainen]	Agreed. At least parts of this text will be moved to Box 11.4.
11-877	A	41:15	41:15	It is a bit peculiar that on the sign of mean wave height there seems to be much less consensus than on the increase in storm track activity in the North Atlantic. What causes this discrepancy? [Bart Van den Hurk]	Noted. We shall see if this needs reformulation.
11-878	A	41:22	41:24	The text "The simulation…suggested a possibility of large changes"..is obscure and imprecise. It is not clear what is meant by "more than average sea level". The whole sentence should be rewritten. [Piero Lionello]	Noted. Will be reformulated.
11-879	A	41:23	41:24	I find it a bit doubtful that features of which there is lack of consensus only an example is mentioned that seems to confirm the fact that things may become worse than they are now, or that the results indicating only little change should be considered with care given the lack of correspondence with observations. This is done twice on this page [Bart Van den Hurk]	Agreed. A more balanced formulation will be used.
11-880	A	41:28		Replace "very likely" with "possibly" [Vincent Gray]	Origin of comment not identified.
11-881	A	41:30	41:31	I find it a bit doubtful that features of which there is lack of consensus only an example is mentioned that seems to confirm the fact that things may become worse than they are now, or that the results indicating only little change should be considered with care given the lack of correspondence with observations. This is done twice on this page [Bart Van den Hurk]	Agreed (see 879).
11-882	A	41:33	41:35	A possible synthesis could otherwise be. "The presently available studies are not sufficient to draw general conclusions, as few models and regions have been considered until now. The large interannual variability of extreme phenomena is often so large that the changes produced by scenario simulations are not statistically significant." [Piero Lionello]	Noted. Original shorter version preferred.
11-883	A	41:35	41:35	A reference to Van den Brink et al (2004 - see next comment) may be added to demonstrate how alternative methods (like archive of seasonal forecasts) may be used to improve statistical estimates [Bart Van den Hurk]	Disagreed. Reference not relevant in climate change context.

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11-884	A	41:37	41:53	Discussion on the cryosphere could be made into a fact-box or perhaps incorporated in Box 11.3 [Markku Rummukainen]	Disagreed. Present organisation seems more natural.
11-885	A	41:37		I am not sure that the title is appropriate here, since this part is only about snow and sea-ice and ignores the other components of the cryosphere. "Snow and sea-ice" would be more appropriate. [Christophe Genthon]	Agreed.
11-886	A	41:39	41:39	Rowell (2005b) (and perhaps other references) would be appropriate at the end of this sentence; this shows maps of the projected change in mean snow amount, and in snow days. Rowell, D.P., 2005b: A scenario of European climate change for the late 21st century: seasonal means and interannual variability. Clim. Dyn., in press (available from the Clim. Dyn. website) [Dave Rowell]	Agreed. Reference added later to the same paragraph.
11-887	A	41:40	41:41	Delete "but are unlikely to balance them" [Vincent Gray]	Disagreed. We do think this is unlikely.
11-888	A	41:40	41:40	Insert "reducing" between "the" and "effects". Insert "on snowfall" after "warming" [Bart Van den Hurk]	Disagreed. Original formulation covers both melting and change of phase.
11-889	A	41:40		Replace "will" by "might" [Vincent Gray]	Disagreed. Original formulation fully consistent with model-bsaed evidence.
11-890	A	41:41	41:43	Rowell (2005b) also shows changes in the number of snow days. [Dave Rowell]	Agreed. Reference added.
11-891	A	41:55	41:55	remove "a" [Bart Van den Hurk]	Agreed.
11-892	A	41:55	42:26	NOTE! LONG COMMENT! Discussion on regional oceans could be incorporated in Box 11.4 (Coastal zones) or by discussing both the Baltic Sea and the Mediterranean in a joint section. In case of Baltic Sea, new results on salinity and deepwater ventilation should be added: "According to four regional climate scenarios the total mean annual river flow to the Baltic changes between -2 and +15 % of present-day flow (Graham, 2004) and net precipitation will increase as well, resulting in 4-21% increased freshwater flow to the Baltic Sea. Based on these scenarios, Meier (2005a) calculated future salinity using a Baltic Sea ice-ocean model. Increased freshwater inflow and increased monthly mean wind speed (Räisänen et al., 2004), will lower the average salinity with 7-47% compared to the present climate (Meier, 2005a). The relationship between freshwater supply and average salinity of the final steady state is, however, nonlinear (Meier and Kauker, 2003). Even in the case of large freshwater forcing, a pronounced halocline still remains to separate the upper and lower layers in the Baltic Proper, limiting the impact of direct wind induced mixing to the surface layer. Although changes of freshwater inflow and wind	Noted. Proposed reorganisation will be considered. Given the space constraints, we need to exclude salinity and deep-water ventilation from our mandate.

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				speed may cause the Baltic Sea to drift into a new state with considerable lower salinity, the ventilation of the deep water will not significantly change because the vertical overturning circulation will partially recover (Meier, 2005a)." The used references are: "Graham, L. P. 2004. Climate Change Effects on River Flow to the Baltic Sea. Ambio 33:4-5, 235-241", "Meier, H.E.M., 2005a: Baltic Sea climate in the late 21st century - a dynamical downscaling approach using two global models and two forcing scenarios. Part 1: Model experiments and scenarios of the water balance. Clim. Dyn., submitted.", "Meier, H.E.M., 2005b: Baltic Sea climate in the late 21st century - a dynamical downscaling approach using two global models and two forcing scenarios. Part 2: Scenarios of the heat balance and extremes. Clim. Dyn., submitted.", "Meier, H.E.M., and F. Kauker, 2003: Sensitivity of the Baltic Sea salinity to the freshwater supply. Clim. Res., 24, 231-242." and "Räisänen, J., Hansson, U., Ullerstig, A., Döscher, R., Graham, L. P., Jones, C., Meier, H. E. M., Samuelsson, P. and Willén, U. 2004. European climate in the late twenty-first century: regional simulations with two driving global models and two forcing scenarios. Clim. Dyn. 22, 13-31." [Markku Rummukainen]	
11-893	A	42:0	55:	Asia is the largest continent and is home to easily half the human population. That 11.3.4 is broken into discussion of Central, South, East and Southeast Asia is crucial. I would go farther and suggest that each region be discussed separately, I.e., model skill, projections, extremes and uncertainties for South Asia. This would provide a more wholistic discussion for those readers interested in a particular part of Asia. [Anji Seth]	This will not be done as we want consistency across the regions and the regions were predefined by the IPCC plenary
11-894	A	42:0	55:	Asia It would be better to do a synthetic analysis where the parameters (temperature, precipitation passed, present and future) are studied for each under area to facilitate comprehension (see observations of pages 25-52). [Ibouraïma YABI]	See comment above
11-895	A	42:0	88:	These sections largely ignore tens of recent papers that have used models to show how absorbing aerosols reduce and modify regional rainfall in Africa, S. Asia and E. Asia. The paper by Lohmann and Rotstayn (J. Climate, 15, P 2103, 2002) suggest that the sahel drought was due to sulfate aerosols from N. America; Chung et al (J Climate, 15, 2462-2476, 2002) and Ramanathan et al (PNAS, Vol 102, p. 5326-5333,2005) show how absorbing aerosolsaffect the monsoon rainfall. In particular Ramanathan et al (2005) show the large reduction of solar radiation by soot and other aerosols have weakened the monsoon circulation and reduced summer rainfall over India since 1950. Menon et al (Science, 297, 2250-, 2002) suggested that absorbing aerosols may help explain the north-	Noted, a box on forcings will be presented in the SOD.

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				south shift of rainfall in China. There are scores of studies by Chinese and Japanese scientists on this topic. Likewise, the treatment of impavct of land-surface changes on regional climate need to be discussed too. [Veerabhadran Ramanathan]	
11-896	A	42:17	42:18	Move "by the end of the simulation" to the start of the sentence [Bart Van den Hurk]	Agreed.
11-897	A	42:22		I think that Li et al. (2005) is not in the list of references [Christophe Genthon]	Noted. Reference deleted.
11-898	A	42:26		Mediterranean sea oceanography has been described very briefly. More insights could be added and relationship betweeen Med sea circulation and Atlantic circulation and on the sea/atmosphere interactions in Med basin as foreseen in future scenarios by RCMs [Marina Baldi]	Noted. Lack of space precludes more.
11-899	A	42:41		Little references are reported in the discussion of uncertainties. Are those the only available? [Marina Baldi]	Noted. This is a summary of issues already discussed.
11-900	A	42:43		This paragraph will not be commented (11.3.4) [Marina Baldi]	
11-901	A	42:45	43:43	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move much of the regional "Key processes" into it. [Markku Rummukainen]	Disagree, but the text will be modified.
11-902	A	42:48		Baiyu --> Baiu (also in other places of this chapter) [Masahide Kimoto]	Done
11-903	A	42:48		Baiyu ---> Baiu [Hidetaka Sasaki]	Done
11-904	A	43:7	43:8	The reference (Knutson and Manabe, 1998) should be (Knutson and Manabe, 1995). Knutson, T.R., and S. Manabe, 1995: Time-mean response over the tropical Pacific to increased CO2 in a coupled ocean-atmosphere model. J. Climate, 8, 2181-2199. [Akira Noda]	Noted.
11-905	A	43:7	:8	This sentence does not make sense - south east asia is mentioned twice. [Hayley Fowler]	Comment not understood
11-906	A	43:13		write… (Douville and Al, (punctuation) [Ibouraïma YABI]	Will do
11-907	A	43:14	43:14	The association of ENSO with weak summer monsoon" should be replaced with "The association of warm phase of ENSO with weak summer monsoon.	Done

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				[Akio Kitoh]	
11-908	A	43:14		write… Giorgie and Al, 2001) (Punctuation) [Ibouraïma YABI]	Done
11-909	A	43:19	43:19	problem" should be "problems [James Renwick]	Done
11-910	A	43:27		Baiyu ---> Baiu [Hidetaka Sasaki]	Done
11-911	A	43:45		Isn't this sub-section (11.3.4.2) too long? [Akio Kitoh]	Agreed, will shorten.
11-912	A	43:47		Authors should give the reasons or limitations on the little analysis work in Central and SE Asia. [Dr. Bundit Limmeechokchai]	This is beyond our assessment task
11-913	A	44:46	44:47	Add reference - Lal et al.(1997). The citation is: Simulation of the Indian monsoon climatology in ECHAM3 climate model: Sensitivity to horizontal resolution, Intl. Jr. Climatol., 1997, 17, 847-858 (M. Lal, U. Cubasch, J. Perlwitz and J. Waszkewitz). [Murari Lal]	Irrelevant as the refernce is too old
11-914	A	44:55		Not to forget to specify the year [Ibouraïma YABI]	Will be done
11-915	A	45:0		write… (compared). (Punctuation) [Ibouraïma YABI]	Not clear
11-916	A	45:7	45:9	Add reference - Lal et al.(1998). The citation is: Indian summer monsoon variability as simulated by regional model nested in a global climate model, Chinese Jr. Atmos. Sci., 1998, 22(1), 93-102 (M. Lal, B. Bhaskaran, S. K. Singh). [Murari Lal]	Will update reference to more recent paper.
11-917	A	45:11	45:12	What is the relevance of sulphur cycle in the three member ensembles? - must be stated here. [Murari Lal]	Text modified and does not address this issue anymore.
11-918	A	45:14	45:15	Nothing is remarkable in this - higher resolution is ought to produce more realistic spatial patterns of rainfall - how about the rainfall amount? [Murari Lal]	Done.
11-919	A	45:31	45:36	Line 31 says the mid-latitude rain band is shifted northward in seasons other than summer; lines 35-36 say it is shifted northward in summer. [David Rind]	Accepted. The text will be modified
11-920	A	45:52	45:55	Traditionally GCMs have acceptable performances in simulating the East Asian monsoon circulation but a poor performance in simulating the monsoonal precipitations. An erroneously intensified precipitation center is usually found on the east periphery of the	Point about circulation is well taken, text modified and an appropriate reference will be added.

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				Tibetan Plateau, as the consequence of an overestimated sensible heating over and around the Tibetan Plateau. References: Zhou Tianjun, Zhaoxin Li, 2002, Simulation of the east Asian summer monsoon by using a variable resolution atmospheric GCM, Climate Dynamics,19:167-180; Yu Rucong?W Li ?X.H. Zhang?Y.Q. Yu?H.L. Liu?T.J. Zhou? 2000? Climatic features related to eastern China summer rainfalls in the NCAR CCM3. Advances in Atmospheric Sciences? 17? 503-518 [Rucong Yu]	
11-921	A	45:52	45:55	Traditionally GCMs have acceptable performances in simulating the East Asian monsoon circulation but a poor performance in simulating the monsoonal precipitations. An erroneously intensified precipitation center is usually found on the east periphery of the Tibetan Plateau, as the consequence of an overestimated sensible heating over and around the Tibetan Plateau. References: Zhou Tianjun, Zhaoxin Li, 2002, Simulation of the east Asian summer monsoon by using a variable resolution atmospheric GCM, Climate Dynamics,19:167-180; Yu Rucong?W. Li?X.H. Zhang?Y.Q. Yu ?H.L. Liu?T.J. Zhou? 2000? Climatic features related to eastern China summer rainfalls in the NCAR CCM3. Advances in Atmospheric Sciences? 17? 503-518 [Tianjun ZHOU]	See 11-921
11-922	A	46:1	46:12	Kimoto et al. (2005, SOLA) note that representation of precipitation is improved with the use of high resolution model. [Masahide Kimoto]	If relevant, will include
11-923	A	46:4		Not forget to specify the year [Ibouraïma YABI]	2006
11-924	A	46:10	46:12	Resolution may be more important than topography in this region, but the opposite is bound to be true elsewhere. As a rule, it is presumably important to resolve synoptic-scale (and smaller) motions well, as well as any underlying topographic forcing. [James Renwick]	Noted. Text modified
11-925	A	46:16	46:27	The grammar seems suddenly poor in this paragraph. [James Renwick]	Noted. Paragraph will be revised.
11-926	A	46:17		Kadokura and Kato, 2005 is missing in the references. [Hidetaka Sasaki]	Noted. Reference will be added
11-927	A	46:24	46:25	The ability to simulate typhoons must also relate to the size of the regional domain, something that is not discussed much in this chapter. A larger region, encompassing more of the tropical ocean, would presumably stand a better chance of developing typhoons. [James Renwick]	Noted. Text is removed from this regional section, but will be presented to section on technologies
11-928	A	46:30		If it is possible, to insert table 11.3.4.1 in the text to facilitate comprehension ? [Ibouraïma YABI]	Not possible. Table will be in supplementary material

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11-929	A	47:12	94:9	Too long and confusing. This needs re-writing in a more consistent manner focussing on summary of key findings of several recent studies. [Murari Lal]	Noted. The entire chapter is under revision.
11-930	A	47:13		Section 11.3.4.3.1: It would be good to describe the mean temperature changes in terms of percentages of the relevant model's global mean warming, as done for other regions. [James Renwick]	Noted. This will be in a table.
11-931	A	47:22		dependency ---> dependence [Yasuo Sato]	Accepted
11-932	A	47:43		If it is possible to use the period 2081-2100 compared with 1981-2000 ? [Ibouraïma YABI]	Noted. Yes, but we try to be consistent across AR4
11-933	A	47:46		general warming ---> global warming [Yasuo Sato]	Noted. However, we propose 'regional' in stead.
11-934	A	48:0		What is MME4? No explanation in the Text and Figure Caption. Figures 11.3.4.5 c) and d) should be removed. [Masato Sugi]	Noted. Text will be more focused and Figure likely to be removed.
11-935	A	48:9	48:10	What is the purpose of comparing the future warming for IS92a scenario with those of A2 and B2? Are these warming projections based on same model version? For what time slice - To me it does not appear to be correct - projections should be higher in A2 than in IS92a as RF would be more in formal case. None the less - this sentence is out of place and should be deleted. [Murari Lal]	Noted. This will be clarified. Kumar
11-936	A	48:41	48:41	Fig. 11.3.4.6 does not show B2 as is claimed here. [David Rind]	Noted. Point will be clarified, the section will be revised and possibly deleted. Won-Tea
11-937	A	48:45		high resolution (~20 km) MRI-GSM [Hidetaka Sasaki]	Done
11-938	A	48:46	48:46	Mizuta et al., 2005" should be changed to "Mizuta et al., 2005b [Hiroki Kondo]	Done
11-939	A	48:49		realistic topography and meso-scale atmospheric disturbances [Yasuo Sato]	Done. Is lifted to to methodology section
11-940	A	49:3		MRI-RCM20 [Hidetaka Sasaki]	Done
11-941	A	49:3		MRI-RCM20 [Yasuo Sato]	Done
11-942	A	49:21		Figure 11.3.4.7: You have a very detailed and wide-ranging colour bar (15 shades), but use only two colours in the plot. Decrease the contour interval, or do away with shading,	Noted. Figure will be deleted.

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				as it does not add to the illustration when one colour is so widespread. [James Renwick]	
11-943	A	50:1	50:24	Paragraphs 1 and 2 seem to be saying different things while apparently discussing the same region. [David Rind]	Noted. Material mentioned in para 2 will be shortened in order to make it more clear that this is information in addition to the AR4 results.
11-944	A	50:28	50:28	Do you mean Figure 11.3.4.4? [James Renwick]	Yes
11-945	A	50:28	50:28	Fig. 11.3.7.4 does not show rainfall. [David Rind]	See above
11-946	A	51:1	51:16	The first paragraph says that the increased precip is greater when the warming is greater. The second paragraph says there is a decrease in precip in winter, and an icnrease in the warm season. Is there more warming during the warm season here? [David Rind]	Noted. The both para will be revised for clarification
11-947	A	51:7		inter-model variability and also nature characteristics [Yasuo Sato]	We do not agree. But text will be rewritten aiming at clarification
11-948	A	51:14		Precipitation projection for 2081 to 2100 using MRI-RCM20 [Hidetaka Sasaki]	Done
11-949	A	51:15	51:15	Suggest replacing "will" with "may" [James Renwick]	Noted. Text revised
11-950	A	51:44	51:51	This paragraph seems to argue against the first sentence of the previous paragraph (lines 32-33), suggesting uncertainty in the simulation of present climate, and important topographic effects that may not be properly accounted for. Are we really that much more confident than Boer and Faqih (2004)? [James Renwick]	Noted. A few words for clarification have been added.
11-951	A	51:51		This model deficiency is due to both RCM and GCM. [Yasuo Sato]	Noted. 'Models' in thex refers to both type of models.
11-952	A	52:12	52:12	By "the country", do you mean "the Asian land mass"? [James Renwick]	Noted. Text will be modified for clarification
11-953	A	52:14	:17	Choose between mm.d -1 or mm/day to harmonize the writings [Ibouraïma YABI]	Noted. Consistency will be made throughout the chapter (report)
11-954	A	52:27	52:28	The double citation of Gao et al. (2002) is redundant - remove the second occurrence. [James Renwick]	Done.
11-955	A	52:33	52:33	What does this last half-sentence mean? What happens to the dominant path of tropical storms? [James Renwick]	Sentence deleted.

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11-956	A	52:42	52:42	Mizuta et al. (2005)" should be changed to "Mizuta et al. (2005b) [Hiroki Kondo]	Done
11-957	A	53:2	53:2	Mizuta et al. (2005)" should be changed to "Mizuta et al. (2005b) [Hiroki Kondo]	Done
11-958	A	53:38	54:14	Kimoto (2005) also discussed the change in East Asian summer monsoon in a warmer climate. He attributes the increased activity of East Asian monsoonal rain band to the strengthening of anticyclonic cells to its south and north. Kimoto, M., 2005: Simulated change of the east Asian circulation under global warming scenario. Geophys. Res. Lett, in press. [Seita Emori]	Noted. Paper will be assessed and cited if appropriate
11-959	A	53:40	53:41	Add the following references for 5-km mesh NHM: Yoshizaki, M., C. Muroi, S. Kanada, Y. Wakazuki, K. Yasunaga, A. Hashimoto, T. Kato, K. kurihara, A. Noda and S. Kusunoki, 2005: Changes of Baiu (Mei-yu) frontal activity in the global warming climate simulated by a non-hydrostatic regional model. SOLA, 1, 25-28, doi:10.2151/sola.2005-008. Kanada, S., C. Muroi, Y. Wakazuki, K. Yasunaga, A.Hashimoto, T. Kato, K. Kurihara, M. Yoshizaki and A. Noda 2005: Structure of Mesoscale Convective Systems during the Late Baiu Season in the Global Warming Climate Simulated by a Non-hydrostatic Regional Model. SOLA, 1, 117-120, doi:10.2151/sola.2005-031. Wakazuki, Y., M. Yoshizaki, K. Yasunaga, C. Muroi, S. Kanada, A. Hashimoto, T. Kato, K. Kurihara and A. Noda, 2005: Changes in the Characteristic Features of Disturbances appearing in the Baiu Frontal Zone over Western Japan Due to Global Warming. SOLA, 1, 129-132, doi:10.2151/sola.2005-034. [Akira Noda]	Noted. Papers will be assessed and cited if relevant. 1st may be relevant here, while the other two will be presented to the methodology section.
11-960	A	53:41		Yasunaga et al.,2005 is missing in the references. [Hidetaka Sasaki]	Will update.
11-961	A	54:20	54:25	Hori et al. (2005) (missing in the reference list) should be replaced with Hori and Ueda (2005). Hori, M.E. and H. Ueda, 2005: Impact of global warming on the East Asian winter monsoon as revealed by nine coupled atmosphere-ocean GCM. Geophys. Res. Lett. Submitted. [Akio Kitoh]	Done.
11-962	A	54:20		Hori et al.(2005) is missing in the references. [Hidetaka Sasaki]	See 11-961
11-963	A	54:39		It is better to review the results of discussion of TAR such as Bengtsson et al., Sugi et al., Yoshimura et al. etc. on typhoon occurence frequency	This is done throughout the chapter, but work on tropical cyclones is assessed in

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				[Yasuo Sato]	Chapter 10
11-964	A	54:52	55:3	Sakamoto et al. (2005) showed from a high-resolution AOGCM result that the current velocity of the Kuroshio and the Kuroshio extension would increase in a warmer climate, which implies that the sea level rise along the south coast of Japan would be relatively small comparing to that in the offshore. Sakamoto, T. T., H. Hasumi, M. Ishii, S. Emori, Ta. Suzuki, T. Nishimura, and A. Sumi, 2005: Responses of the Kuroshio and the Kuroshio Extension to global warming in a high-resolution climate model, Geophys. Res. Lett, 32, L14617, doi:10.1029/2005GL023384. [Seita Emori]	Noted . Papers will be assessed, but sub-sewction is moved to Box 11.4
11-965	A	54:52	55:3	Sakamoto et al. (2005) shows an increase in Kuroshio current speed with little change in separation latitude in a high-resolution AOGCM. Sea level increases notably in the Kuroshio recirculation region to the south of Japan. Sakamoto, T. T., H. Hasumi, M. Ishii, S. Emori, Ta. Suzuki, T. Nishimura, and A. Sumi, 2005: Responses of the Kuroshio and the Kuroshio Extension to global warming in a high-resolution climate model, Geophys. Res. Lett, 32, L14617, doi:10.1029/2005GL023384. [Masahide Kimoto]	Noted . Papers will be assessed, but sub-sewction is moved to Box 11.4
11-966	A	54:52	55:3	The discussion on regional sea level change should be moved to Box 11.4. [Markku Rummukainen]	Will be done
11-967	A	54:53	54:56	<After the end of the sentense, please add the following sentence for comparison with Choi et al.(2002).> "Kim et al. (2005a) analyzed the change of sea surface temperature and sea level rise due to thermal expansion in the Western North Pacific under A1B and B1 scenarios using CCSM3, and found that they were quite similar to projected global mean values even though the increase of the sea surface tempereture in the WNP was larger than the temporal change of global mean sea surface temperature. " . < Please add the following paper in the reference after line 7, page 111, Chapter 11>. Kim, D.-H., N. Nakashiki, D. Tsumune, Y. Yoshida, K. Maruyama and F. O. Bryan, 2005: Ocean Climate change in the Western North Pacific (WNP) under the Multi-Century Three-Menber Ensenble Prediction, Journal of the Korean Meteorological Society,41, 2-1, 2005, pp.239-247, accepted. [Koki Maruyama]	Noted. The paper will be assessed. But see also 11-966
11-968	A	54:53		Choi et al.(2002) is missing in the references. [Hidetaka Sasaki]	Will be corrected
11-969	A	54:56		Sato et al.(2005;submitted to JMSJ and IPCC TSU) projected 12 - 18 cm sea level rise along the coasts of Japan in a warmed climate(2061~ 2080) using a Kuroshio-resolving	Noted. The paper will be assessed. But see also 11-966

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				ocean model forced by an MRI_CGCM2.2 global warming experiment data. [Yasuo Sato]	
11-970	A	55:0	67:	(America North) At my view point, it sincerely missed with this part an effort of synthesis, which does not facilitate comprehension. If it is possible, it is better to synthesis by under-area (see observations of pages 25-52 and 42-55). [Ibouraïma YABI]	Assessment and synthesis aspects will be improved
11-971	A	55:9	55:12	These two major uncertainties suggest that we need to be circumspect about the detail of changes in Asia, especially in relation to second-order characteristics such as extremes. [James Renwick]	Agreed.
11-972	A	55:12		(multiple islands and very mountainous), land-sea contrast, ocean current distribution, [Yasuo Sato]	Agreed.
11-973	A	55:14		This section should have received an "in house" review before being sent to the official review process. [Daniel Caya]	Section will be improved
11-974	A	55:14		This section in conjunction with earlier discussions, seems to have a number of contradictions. First, the earlier sections said (1) the jet shifted northward; and (2) a more semi-permanent El Nino state would arise, strengthening the subtropical jet. While I suppose both jets could move in opposite directions - the polar front jet moving north, the subtropical jet intensifying to the south, both would mean less rainfall for Oregon. And yet, Fig. 11.3.5.5 shows big increases in precip over Oregon and the Pacific Northwest in winter. What does happen to the storm tracks over this region, and why does it get so wet if the jets are both diverting storms away from it, to the north and south? [David Rind]	Precipitation increases on the west coast from Oregon northwestward in part due to increased moisture in warmer atmosphere
11-975	A	55:14		The North America subchapter is structured very differently from the other subchapters. There are additional sections, which might be ok as such (alternatively additional fact-boxes could be introduced). That the regional projections are structured according to the source (GCM, RCM etc.) instead of by variable should, however, be changed to liken more the other subchapters. [Markku Rummukainen]	Will be corrected
11-976	A	55:16	55:51	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move much of the regional "Key processes" into it. [Markku Rummukainen]	Will develop further the key processes of climate change
11-977	A	55:18		Westerlies over North America typically extend to about 30N. [Raymond Arritt]	Noted

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11-978	A	55:21	55:21	Figure 11.3.5.1 is not really appropriate [Daniel Caya]	Figure deleted
11-979	A	55:32	55:37	North America also is influenced by the Pacific Decadal Oscillation (PDO). There is considerable evidence that ENSO effects over North America are very strongly modulated by the PDO; see e.g., Gutzler, D.S., D. Kann, and C. Thornbrugh, 2002: Modulation of ENSO-based long-lead outlooks of southwest U.S. winter precipitation by the Pacific Decadal Oscillation, Weather & Forecasting, 17, 1163-1172. The PDO reverses phase at intervals of 20-30 years and thus its examination would require RCM simulations much longer than any performed to date for North America. [Raymond Arritt]	noted
11-980	A	55:32	57:33	(section 11.3.5..1) Perhaps the long discussion of errors in CGCMs over North America could be replaced with a table, a figure or two, or a bulleted list, in order to reduce and focus the discussion. [Michael Alexander Alexander]	Will develop a table
11-981	A	55:52		Cite and discuss Castro, C.L., T.B. McKee, and R.A. Pielke Sr., 2001: The relationship of the North American monsoon to tropical and north Pacific sea surface temperatures as revealed by observational analyses. J. Climate, 14, 4449-4473. I know there is another paper by Castro et al on N America monsoon, but I do not find the reference.... [Marina Baldi]	Noted
11-982	A	55:53		All sub-sections 11.3.5.2.1 to 11.3.5.2.3 should be merged in a single sub-section 11.3.5.2 [Daniel Caya]	Will be entirely restructured
11-983	A	55:53		See attached document for a suggested section 11.3.5.2 [Daniel Caya]	Noted
11-984	A	56:9	57:14	This is rather too detailed and seems partially based on findings available at the time of TAR. Consider a major cut and integrating the very essential part with page 57, lines 16-33. [Markku Rummukainen]	Noted
11-985	A	56:25	56:25	Aren't seasonal cycles stronger in eastern North America? Temperature seasonality is at least stronger in the east, isn't it? [James Renwick]	Noted; text restructured and shortened substantially
11-986	A	56:45	56:50	An important reason for GCM deficiency in warm-season precipitation over North America is the prevalence of mesoscale convective systems that propagate over long distances, often 1000 km or more. These systems are much smaller than GCM node spacing and yet are fundamentally different from random subgrid convection so they are not represented in any present-day GCM parameterizations of deep convection. A good recent citation is Carbone, R., J. D. Tuttle, D. A. Ahijevych, and S. B. Trier, 2002: Inferences of predictability associated with warm season precipitation episodes. J.	Noted

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				Atmos. Sci., 59, 2033–2056. [Raymond Arritt]	
11-987	A	56:55	56:55	Data source to back up statement re. water and wetland cover in Canada could be used:Fernandes, R.A., Pavlic, G., Fraser, R., 2000, Waterbody Coverage of Canada, Digital Map Product, Natural Resources Canada, available via www.geogratis.ca. [Richard Fernandes]	Noted
11-989	A	57:9	57:11	Is there some reason the words La Nina are not being used here? [David Rind]	Noted
11-990	A	57:10	57:11	Citation Gerhunov and Douville (2004) is not in the reference list. [James Renwick]	Will add it
11-991	A	57:40		Regional reanalysis are now also available for N America and used by the RCMs modellers community. First results available. [Marina Baldi]	Noted; references were not available at the time of FOD
11-992	A	57:48	57:51	Reported NARCCAP results are very preliminary and not published [Daniel Caya]	Noted
11-993	A	57:53		Figire 11.3.5.3: Explain the different points for each model. Are these with different LBCs, or different physics, or different regions, or something else? Are the LBCs all from reanalyses? [James Renwick]	Figure deleted
11-994	A	58:38		We have recently found that very substantial improvements in amount and diurnal timing of warm season precipitation can be obtained by decreasing the RCM grid spacing (from 51 to 17 km in our study) and by improving the way that the convective parameterization interacts with grid-scale moist physics (in our case, the Kain-Fritsch convective scheme). I hope it is not too self-serving to cite my own work here. The relevant paper is Anderson, C.J., R.W. Arritt and J.S. Kain, 2005: Test of a hybrid convective parameterization in a regional climate model. Submitted to Journal of Hydrometeorology. (The paper has been accepted subject to some modest revisions.) [Raymond Arritt]	Noted
11-995	A	58:44	57:47	The reported sensitivity on LBC (precipitation and temperature) is hard to see in Plummer et al [Daniel Caya]	We only refer to temperature in winter in FOD. The exact text in referenced paper is: "The CGCM-driven simulations using the standard set of physical parameterizations displayed a significant warm bias in surface air temperate for the DJF season over much of the model domain and an overprediction of summertime (JJA

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					season) precipitation. The use of boundary conditions from NCEP reanalysis resulted in much better agreement for DJF surface air temperatures, though had little effect on summertime precipitation.
11-996	A	58:53	59:4	This seems rather too general to place in a regional sub-chapter. [Markku Rummukainen]	Noted
11-997	A	58:56	58:56	The use of the word model is confusing, statistical downscaling model should be used. [Daniel Caya]	Noted
11-998	A	59:8	59:11	Delete lines. All CGCMs runs are now using transient CO2 evolution therefore talking about 2xCO2 is confusing. [Daniel Caya]	"to CO2 doubling" has been removed
11-999	A	59:8	59:11	This seems rather too general to place in a regional sub-chapter. [Markku Rummukainen]	Noted
11-1000	A	59:13	59:41	Consider reducing the discussion and focusing on AR4-generation GCMs. One also notes that the IMSD is really used only in the North American subchapter. Any chance of extending this to other regions or reducing the emphasis on IMSD for this region? [Markku Rummukainen]	Noted
11-1001	A	59:25	59:25	change to "…the null hypothesis of no change." [James Renwick]	Done
11-1002	A	59:36	59:36	remove the last word in the line ("that") [James Renwick]	Done
11-1003	A	59:53	59:53	It is not altogether clear why only 18 of the AR4 GCMs are sampled. For other region, results of all of the models are used. [Markku Rummukainen]	Corrected
11-1004	A	60:1	:16	Discussion related to AR4 A1B temp and precip changes for North America. This relates to comment #11 above as well. If additional panels could be added to show the robustness of this signal within the multi-model ensemble, it would be helpful. [Anji Seth]	New figures will be included in SOD
11-1005	A	60:5		Comment Figure 11.3.5.4 Please specify which scenario and which period have been used to compute temperature differences [Eduardo Zorita]	This info was given in pages 11-59 lines 43-44.
11-1006	A	60:17		Comment Figure 11.3.5.5 Please specify which scenario and which period have been used to compute temperature differences [Eduardo Zorita]	This info was given in pages 11-59 lines 43-44.

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11-1007	A	60:19		Distribution of snow depends heavily on local geographical features and altitude, thus reproduction of these needs high horizontal resolution. A time-slice experiment by a 20 km mesh MRI/JMA AGCM shows an increase of snow water equivalent in the northern parts of North America (Hosaka et al. 2005). [Akio Kitoh]	Noted; the point on snow will be developed further in SOD
11-1008	A	60:31	60:32	: 20-25% increase in SH winter and summer, and 26% increase in NH summer including over the East Coast of North America. [Masato Sugi]	Corrected
11-1009	A	60:34	61:43	Some suggestions to clarify this subsection (11.3.5.5): a) Line 49-50 Does the sentence which begins "For example, it is found that more spatial structure of precipitation was found in the RCM simulations that employed the higher resolution. RCMs simulations…", mean that RCMs have more spatial structure than GCMs or that higher the RCM resolution the greater the spatial structure. In either case isn't this true by definition? b) I suggest showing Precipitation – Evaporation (P-E) in addition to precipitation in Figure 11.3.5.7 (page 11-175) to show the drying of the continent. c) Lines 27-30 page 11-61. The sentence which begins "For example, Chen et al. (2003) found that the two RCMS differed from each other" seems to contradict the rest of the sentence which implies that the RCMs differed from the GCM that forced them. d) lines 32-43 (last paragraph of section) this paragraph provides a (catch as catch can) list of studies of extreme events over North America, either the paragraph should be more focused or it can be dropped altogether. (Note the reference to Bell et al. 2004 is missing from the reference list). [Michael Alexander Alexander]	a) Yes b) Good point; we are considering this option, at least for discussion. However P is better understood by most users c) Will clarify d) Noted
11-1010	A	60:36	60:36	driving GCMs should be used instead of nesting GCMs [Daniel Caya]	Suggestion noted
11-1011	A	61:8	61:8	recent decades; this -> recent decades. This [Daniel Caya]	Suggestion noted
11-1012	A	61:17	61:17	corrected a significant -> significantly reduced a [Daniel Caya]	No. Sentence is correct as is.
11-1013	A	61:19	61:19	uncertainty -> spread [Daniel Caya]	There is more to uncertainty than spread; ensemble spread is a necessary, however incomplete, piece of information towards estimating uncertainty
11-1014	A	61:19	61:19	change "exploring" to "exploration of" [James Renwick]	Suggestion noted

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11-1015	A	61:26	61:30	The examples given compare RCMs to GCMs, while the sentence introducing them was discussing RCM responses being differ from one another. [David Rind]	Will clarify
11-1016	A	61:45		All section 11.3.5.3.4 is related to impact and adaptation works and should not appear in IPCC WG1 report. [Daniel Caya]	A decision will have to be made
11-1017	A	62:14	62:16	This seems rather too general to place in a regional sub-chapter. [Markku Rummukainen]	The 2 lines serve to introduce the following sentence
11-1018	A	62:16	62:17	Was the neural network approach successful? [James Renwick]	No one can tell if a climate-change projection is successful
11-1019	A	62:16	62:36	This might place better in WGII. [Markku Rummukainen]	A decision will have to be made
11-1020	A	62:36		should read "degraded wine grape quality" (can't really make statements on the wine itself) [Katharine Hayhoe]	Noted
11-1021	A	62:38	64:3	It's unclear whether this subsection on land use changes is more appropriate here or in one of the other working group reports. [Michael Alexander Alexander]	Will regroup this material in a box in SOD
11-1022	A	62:38		Section 11.3.5.3.5 seems incompatible with chapter 11. [Daniel Caya]	Will regroup this material in a box in SOD
11-1023	A	62:38		The section on land use change experiments appears to be out of place and not analytic enough to provide a useful evaluation. Why is Amazon deforestation treated as part of North America? [Robert E. Dickinson]	a) a new box on lcc for the entire regional chapter is being produced, so there will nolonger be a specific section in NA section b) Will regroup this material in a box Because it has an impact at a distance through the Hadley circulation, on NA climate,we will make the discussion more analytical
11-1024	A	62:39	63:48	This is to some degree rather too general to place in a specific regional sub-chapter and probably overlaps other WGI-chapters. A fact-box on land-use effects on regional climates might be an option to omitting this section entirely. [Markku Rummukainen]	Will regroup this material in a box in SOD
11-1025	A	63:15	63:33	Effect of landuse and landcover changes on climate change in N America have been	We will include more references for the

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				discussed in detail, here. However, since a large amount of litereature is available on this topic, few more references could be added to the list. [Marina Baldi]	box
11-1026	A	63:38		Not forget to specify the year [Ibouraïma YABI]	Will include date
11-1027	A	64:5	66:42	Consider integrating this text with the earlier discussion on temperature/precipitation/extremes changes if this is chosen as the format even in the subchapter or with the discussion presently structured after the source of information (GCM, RCM etc.). Much of the numbers on pages 65-66 could be better provided as a table. [Markku Rummukainen]	Will be restructured in SOD
11-1028	A	64:5		Section 11.3.5.4 should be placed immediately after section 11.3.5.3.1 [Daniel Caya]	No. The sections 11.3.5.3.n described the various methods to develop CC information, and 11.3.5.4 does the assessment based on all these methods, not just the CGCMs results described in 11.3.5.3.1.
11-1029	A	64:11	64:11	(after eliminating some clear outliers) should be clarified. How the outliers were identified, how many GCMs left after the outliers have been removed. [Daniel Caya]	Will clarify
11-1030	A	64:12	64:12	How many GCMs in Tebaldi et al. [Daniel Caya]	Will clarify
11-1031	A	64:14	64:14	What the "first one" is should be stated explicitly. [Michael Alexander Alexander]	Will clarify
11-1032	A	64:14	64:14	"…than the first one." - being the range taken directly from the GCMs. This could be worded more clearly, it took me several attempts to understand. [James Renwick]	Will clarify
11-1033	A	64:17	64:46	Much of what is stated in these two paragraphs already appears earlier in the chapter so the paragraphs could be dropped. [Michael Alexander Alexander]	The NA section will be reworked considerably for SOD
11-1034	A	64:33	64:35	Delete lines. This sentence describes scenario construction and does not fit in the paragraph. [Daniel Caya]	Noted
11-1035	A	64:38	64:38	change to "southwest-most" [James Renwick]	Noted
11-1036	A	64:42	64:42	Mote and Mantua (2002) is not in the reference list	Noted

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				[James Renwick]	
11-1037	A	64:50	64:50	change "2980" to "2080" [James Renwick]	Corrected
11-1038	A	64:52	64:52	ALA, ENA and GRL are regions not defined yet. [Daniel Caya]	A figure will be provided at the beginning of the chapter
11-1039	A	64:54	64:55	SRES A2 is larger by a factor 1.15 and 1.29. Is larger with respect to what ? [Daniel Caya]	Will copy "than that under SRES A1B"
11-1040	A	65:7	65:7	Deque (2006) is not in the reference list - do you mean Deque et al (2005b)? [James Renwick]	Paper submitted. Will make the reference coherent.
11-1041	A	65:9	66:42	A figure showing the different regions would of great value and could be inserted here. [Marina Baldi]	A figure will be provided at the beginning of the chapter
11-1042	A	65:9		A Figure showing the regions and summarising results is required. [Daniel Caya]	A figure will be provided at the beginning of the chapter
11-1043	A	65:12	66:42	The regional information here could be presented in a table (or perhaps as a bar chart) and then briefly described. [Michael Alexander Alexander]	The NA section will be reworked considerably for SOD
11-1044	A	65:19	65:23	What is the impact in this area on snowcover? [David Rind]	Discussion of snow changes will be expanded in SOD
11-1045	A	65:25	65:25	CGI is not on the map for 11.3.1. [David Rind]	A figure will be provided at the beginning of the chapter to describe the regions
11-1046	A	66:6	66:6	Is Central North America "CAN" on the map? [David Rind]	A figure will be provided at the beginning of the chapter to describe the regions
11-1047	A	66:7	66:7	Illinois missing from this region, despite the fact that all of its surrounding states are included. [Eugene Takle]	A figure will be provided at the beginning of the chapter to describe the regions
11-1048	A	66:35	66:35	First word should "Averaged" [James Renwick]	Will be corrected
11-1049	A	66:39	66:39	Change is to are [Thomas Karl]	Will be corrected
11-1050	A	66:45	66:47	I would suggest to replace the three lines with: The uncertainties in regional climate change over North America are strongly linked to the ability of CGCMs in reproducing the dynamical features affecting the region. [Daniel Caya]	Suggestion noted

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11-1051	A	66:45	66:46	This comment could usefully be applied in the summary for all regions. [James Renwick]	Noted
11-1052	A	66:49		Write… largely unknown; (Punctuation [Ibouraïma YABI]	Changed "is" for "are"
11-1053	A	66:52		Write… area; (Punctuation) [Ibouraïma YABI]	Noted
11-1054	A	66:56		Write… quite probable; (Punctuation) [Ibouraïma YABI]	Noted
11-1055	A	67:0	73:	Central and South America have been lumped together, but it would be much easier on the reader if this section was organized such that Central America discussion was separated from South America discussion, rather than having to jump back and forth between the regions. [Anji Seth]	Suggestion noted. To be considered by CLAs.
11-1056	A	67:1	67:3	Seems to dismiss work by Emanuel w/r to theoretical aspect related to changes in intensity and the several simulations studies completed by NOAA GFDL [Thomas Karl]	Coherence will be ensured with Chap. 10 on topic of tropical cyclones
11-1057	A	67:3		Write… mainly unknown; [Ibouraïma YABI]	Noted
11-1058	A	67:6		Write… America North; (Punctuation) [Ibouraïma YABI]	Noted
11-1059	A	67:14	67:15	I do not understand the meaning of this sentence. [Daniel Caya]	Will clarify
11-1060	A	67:25		Write… CGCMs; (Punctuation) [Ibouraïma YABI]	Noted
11-1061	A	67:28		Write… in summertime); (Punctuation) [Ibouraïma YABI]	Noted
11-1062	A	67:30		Write… (100hpa); [Ibouraïma YABI]	Noted
11-1063	A	67:33		Write… RCMs; [Ibouraïma YABI]	Noted
11-1064	A	67:37	67:37	Change "to reduce" to "the reduction of" [James Renwick]	Suggestion noted
11-1065	A	67:42		This paragraph will not be commented (11.3.6) [Marina Baldi]	Noted.
11-1066	A	67:44	68:29	Much of this description might place better in WGII and/or could be shortened and	Suggestion noted. A uniform strategy

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				integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move [Markku Rummukainen]	accross regions was designed.
11-1067	A	68:2	68:2	Has IAS been defined? [David Rind]	Noted
11-1068	A	68:13	68:13	"storm" misspelled (at the end of a nice descriptive paragraph) [James Renwick]	Noted, thanks.
11-1069	A	68:15	68:29	Amazonia has had increasing rainfall over the last 40 years, despite deforestation, due to global-scale water vapor convergence. This confounds attribution of regional climate change. Ref: Chen, T.-C., J.-H. Yoon, K. J. St. Croix, and E. S. Takle, 2001: Suppressing impacts of the Amazonian deforestation by the global circulation change. Bull. Amer. Meteor. Soc. 82, 2209-2216. [Eugene Takle]	Noted.
11-1070	A	68:23	68:23	displacements of the SACZ, as a consequence of poleward expansion of the South Atlantic subtropical high,would have important regional impacts. [MARIO BIDEGAIN]	Will consider suggestion.
11-1071	A	68:24	68:24	Unbalanced parentheses [James Renwick]	Corrected.
11-1072	A	68:45		Figure 11.3.6.1: Axis labels too small. If different symbols are to be used to denote different models, provide a key. Otherwise, use the same symbol for all. [James Renwick]	Figures reworked
11-1073	A	68:47	68:47	Is Central America not more standard than Centralamerica? [Andrew Lacis]	Noted, will assess
11-1074	A	68:47	68:55	This is more like regional detection and could be moved away from Chapter 11. [Markku Rummukainen]	Noted, text reworked.
11-1075	A	68:55		Figure 11.3.6.2: Are observations on this plot? Perhaps the Y-axis limits could be reduced to make past changes clearer. A key to the different model runs would be useful. "ensemble" is misspelled in the caption. [James Renwick]	Figure reworked
11-1076	A	69:12		Figure 11.3.6.3: The aspect ratio is very distorted. Could you drop the top panel and make the remaining two taller? [James Renwick]	Improved figures were included.
11-1077	A	69:17	69:17	Typo in "bias". [James Renwick]	Corrected.
11-1078	A	69:22	69:23	Simulated subtropical climate is also adversely affected by a dry bias over most of south-eastern South America and in the SACZ region, especially during rainy season.	Will consider suggestion.

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				[MARIO BIDEGAIN]	
11-1079	A	69:26		Figure 11.3.6.4: The aspect ratio is very distorted. Could you drop the top panel and make the remaining two taller? [James Renwick]	Improved figures were included.
11-1080	A	69:28		Write… Marengo and Al, 2003; Zhou and..) (Punctuation) [Ibouraïma YABI]	Corrected.
11-1081	A	69:33	69:33	displaces toward the central Andes, with distortions resulting both west and east of the Andes. [MARIO BIDEGAIN]	Corrected.
11-1082	A	69:34	69:35	What do you mean by "smoother than observed?" Is the wavelength longer, or the amplitude lower? (should say "…wave pattern is smoother…") [James Renwick]	Models generally underestimate the amplitude. Text corrected.
11-1083	A	70:8	70:8	It is actually Figure 11.3.6.5. [David Rind]	Noted
11-1084	A	70:12		Figure 11.3.6.5: Define the "20th century climatology" used - is it for 1970-99? Also, "ensemble" is misspelled in the caption. [James Renwick]	Will do
11-1085	A	70:19		Figure 11.3.6.6.: Define the dashed lines. [James Renwick]	Will do
11-1086	A	71:6	71:7	Perhaps reword to read "The composite pattern of precipitation change indicates an equatorward displacement of the eastern tropical Pacific ITCZ activity (Figure 11.3.6.10)." [James Renwick]	Noted
11-1087	A	71:9	71:10	There isn't much indication of increased precipitation along the Caribbean coast in Figure 11.3.6.10, despite its similarity to an ENSO pattern otherwise. [James Renwick]	Noted – figures reconsidered
11-1088	A	71:27	71:27	Change to "…SRES A1B scenario, climate change simulations…" [James Renwick]	Noted
11-1089	A	71:27		My name is misspelled here; should be Arritt (two "t"s). [Raymond Arritt]	Fixed
11-1090	A	72:36		Write… projections. (Punctuation) [Ibouraïma YABI]	OK
11-1091	A	72:40	72:47	Comment should be made here on general GCM deficiencies with regard to ENSO simulation and simulation of the tropical seasonal cycle, which must add to the uncertainty of the results for much of central and south America. [James Renwick]	Will do.

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11-1092	A	73:3	73:4	Lack of knowledge/information on the changes in extremes, frequency, intensity and path of mid-latitude cyclones. [MARIO BIDEGAIN]	Will consider suggestion.
11-1093	A	73:6		This paragraph will not be commented (11.3.7) [Marina Baldi]	
11-1094	A	73:8	73:53	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move [Markku Rummukainen]	This has been shortened significantly
11-1095	A	73:9	73:10	change to "…the south-western Pacific and the eastern Indian Ocean." [James Renwick]	Change made
11-1096	A	73:48		Write… 1996). (Punctuation) [Ibouraïma YABI]	Change made
11-1097	A	74:1		Section 11.3.7.2 While it may be true that there are not very many studies using the AR4 model output, one such study has been submitted to the Journal of Climate in July 2005: "Australian Climate and its Potential Changes Simulated by some IPCC AR4 Models", by A. MOISE. This paper also investigates the 20c3m runs from most models available at the time. [Aurel Moise]	This paper will be considered for citation
11-1098	A	74:5		Write Australia/New Zeland [Ibouraïma YABI]	Change made
11-1099	A	74:13	74:22	Figure 1 in above mentioned submitted paper shows spatial plots of where most of the models show positive and negative bias across Australia for DJF and JJA. Particular for rainfall, this gives a bitter overview of model performance. [Aurel Moise]	This paper will be considered for citation
11-1100	A	74:16		Table 11.3.7.1 : does the table need a reference? [Aurel Moise]	Table is now consolidated into one table for all regions. The calculations are undertaken by the authors for the chapter and does not need a reference
11-1101	A	74:27		"Cai et al., 2004" : is NOT in the reference list. [Aurel Moise]	Changed to Cai et al 2003a which is in the references
11-1102	A	74:34	74:36	Start of sentence: 'Our..' Who is doing the analysis here - papers or an IPCC person? [Aurel Moise]	This refers to the results presented in Table 11.3.7.1. Sentence has been deleted.
11-1103	A	74:38	74:49	The nino3.4 variability of AR4 models - expressed as year-to-year (DJF) STD in (degC) - compared to observations is shown in Figure 9 of above mentioned paper. It also	These reuslts have now been cited in the text.

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				investigates correlation to northern Australia rainfall. [Aurel Moise]	
11-1104	A	75:2	75:3	'Timball' should be 'Timbal' (there are a few other occurences) [Ian Simmonds]	These have been corrected
11-1105	A	75:8	75:18	Another recent paper on regional projections using CMIP2 models is: "Coupled Model Simulations of Current Australian Surface Climate and its Changes under Greenhouse Warming: an Analysis of 18 CMIP2 Models" by A.MOISE, R. COLMAN, H. ZHANG, accepted and to be published in AMM. The paper was submitted in March2005 and a copy was sent to one of the Lead Authors. [Aurel Moise]	This paper will be cited
11-1106	A	75:13		The reference 'Whetton et al. (2005)' is not in reference list. [Aurel Moise]	It was in the references as Whetton et al (in prep). It is now published, and the reference list is corrected.
11-1107	A	75:16	75:16	Change reference 'IOCI 2005' to : 'IOCI 2005: Editors: B. Ryan and P. Hope' [Aurel Moise]	Change made
11-1108	A	75:22	75:22	change "12 km' to "14 km" [John McGregor]	Change made
11-1109	A	75:33	75:48	Temperature changes simulated by AR4 models are shown in Figure 5, 6 and 8 of above mentioned paper submitted to JCli. Those figures show how many models simulate changes over the Australian continent; the time evolution of Australian temperature changes for all 3 sres scenarios; and the range of temperature changes over special areas such as South West W.A. (in JJA) and Murray-Darling Basin (in JJA) and tropical Australia (in DJF). [Aurel Moise]	This paper will be considered for citation
11-1110	A	75:34		Table 11.3.7.2 : again, is there a reference missing? [Aurel Moise]	See above for table 11.3.7.1
11-1111	A	75:36		Figure 11.3.7.1: Why show only sresA1B changes? I think that it would be interesting to see all 3 sres scenarios. [Aurel Moise]	Revised (and consolidated) table now includes information for other scenarios
11-1112	A	75:55		Sudden end of sentence. [Aurel Moise]	Change made.
11-1113	A	75:55		"By 2030, the warming…" : Is this dicussing AR4 simulations? It is not clear here. If yes, no reference for these results is given. [Aurel Moise]	Reference is given as CSIRO (2001). This reference was introduced at the beginning of the section and the models used described (not AR4).
11-1114	A	75:55		(… for 2030 and). Incomplete sentence (to check)	Change made.

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				[Ibouraïma YABI]	
11-1115	A	76:1	76:31	It should be made clear, which model simulation where used here. [Aurel Moise]	Models are those in CSIRO (2001)
11-1116	A	76:21	76:23	"Given the…" : I don't think this sentence should be included. There should be a more scientific description about why results from AR4 simulations are believed to be similar to the results using older simulations. [Aurel Moise]	Sentence revised
11-1117	A	76:26		missing word ('and') [Aurel Moise]	Change made
11-1118	A	76:35		Figure 11.3.7.2 : confusing colour choice (blue=drier;red=more wet). Should be switched around. [Aurel Moise]	Figure has been revised
11-1119	A	76:37	76:52	Precipitation changes simulated by AR4 models are shown in Figure 4, 7 and 8 of above mentioned paper submitted to JCli. Those figures show how many models simulate changes over the Australian continent; the time evolution of Australian rainfall changes for all 3 sres scenarios; and the range of precipitation changes over special areas such as South West W.A. (in JJA) and Murray-Darling Basin (in JJA) and tropical Australia (in DJF). [Aurel Moise]	This paper will be considered for citation
11-1120	A	76:38	76:39	"out of 20" : since I have analysed some of the AR4 data myself, I am interested to know since when there were 20 model simulations available. With some models, the 20c3m runs were erroneous (see errata page). Who produced this plot? No reference is given. [Aurel Moise]	There are 23 simulations available. Selection of models is described in the chapter. Analysis described was done by the authors for the chapter.
11-1121	A	76:40	76:40	Table 11.3.7.1'- It would be good if it is mentioned what data they are comparing the results against. (Perhaps this is mentioned earlier in the chapter?) [Aurel Moise]	This is now included in the consolidated table
11-1122	A	76:40	76:40	Actualy Table 11.3.7.2. [David Rind]	Change made
11-1123	A	76:47	76:48	Replace sentence starting with 'Hope (2005a)...' with: "Hope (2005) has shown a southward or longitudinal shift in storms away from SWWA in the AR4 future simulations." [Aurel Moise]	Change made
11-1124	A	76:50	76:52	We need to be cautious about rainfall changes over the South Island due to the significant topography and the existing large gradients. I suspect that RCM (or SD methods) would show more of an increase in the west of the South Island, and little change (or even drying) in the east, if the TAR-based results are a guide, since the westerlies are expected	Change to text made

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				to increase across the South Island. This is noted in the third paragraph of page 77. [James Renwick]	
11-1125	A	76:54		Figure 11.3.7.3 is old figure. Could use figure 8 from above mentioned paper. [Aurel Moise]	Figure has been updated for AR4 simulations
11-1126	A	77:1	77:5	Is this paragraph based on AR4 simulations? Which scenario? [Aurel Moise]	This is a continuation of the discussion of figure 11.3.7.2 which are the AR4 simulations.
11-1127	A	77:7	77:11	Needs updating. [Aurel Moise]	The corresponding figure is now updated.
11-1128	A	77:15		"BTE,2004" : is not in reference list. [Aurel Moise]	References have been updated.
11-1129	A	77:24		Captalize "south" [Aurel Moise]	Change made
11-1130	A	77:29	77:32	I am aware of a paper submitted to GRL recently which argues that beyond the large-scale atmospheric forcing, local land clearance might have enhanced the rainfall decline ("Land cover change as an additional forcing to explain the rainfall decline in South West of Australia" by, B. Timbal and J. Arblaster, submitted to Geo. Res. Letters) [Robert Colman]	This paper will be considered for citation
11-1131	A	77:30	77:31	Replace text 'particularly a poleward displacement of the westerlies' with: "particularly a decrease in the frequency of rain-bearing systems over the region," [Aurel Moise]	Change made
11-1132	A	77:31	77:31	delete "there is evidence that" as it makes the statement too strong [John McGregor]	Change made
11-1133	A	77:34	77:36	The argument in Timbal et al (2005) makes the point that the greenhouse forcing is contributing to the rainfall decline, rather than natural fluctuations. BTW, "Timbal" is mispelled troughout chapter 11. [Robert Colman]	Timbal citation removed. Spelling corrected
11-1134	A	77:36	77:36	Timbal et al (2005) reference missing (correct reference is in chap 9 references) [Robert Colman]	This reference now not cited.
11-1135	A	77:41	77:41	Do you mean "equatorward" here, or "poleward"? The spin-up of the polar vortex is essentially a poleward contraction of the storm track, and a poleward expansion of the subtropical high pressure belt, isn't it? [James Renwick]	Text has been changed to refer to strengthened westerlies – which better describes the relevant model results for this location.
11-1136	A	77:49	77:51	Recently (Abs, 2004) …." : sentence does not make sense. Maybe should be: "Recently, Abs (2004) … [Aurel Moise]	Change made

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11-1137	A	77:49	77:49	Change to "Recently, Abbs (2004) dynamically downscaled…" [James Renwick]	Change made
11-1138	A	78:0	18:20	Insert "that simulates little change in precipitation" after "global model". [Steven Ghan]	Change made
11-1139	A	78:12	78:20	This could be put in a common fact-box covering mountain regions and cryosphere. [Markku Rummukainen]	It was decided to keep such disucssions in their relevant regional section
11-1140	A	78:12	79:17	Suggest combining the discussion on these other aspects than temperature and precipitation under one heading (though called smth else than the "Extremes" used in many other regional subchapters). Tropical cyclones are likely discussed elsewhere in AR4 as well. [Markku Rummukainen]	It was decided to keep such disucssions in their relevant regional section
11-1141	A	78:19		Not forget to specify the year [Ibouraïma YABI]	Will be corrected
11-1142	A	78:23	78:24	Using the method described in Walsh et al. (1999) changes to potential evaporation have …. Walsh, K., R. Allan, R. Jones, B. Pittock, R. Suppiah, and P. Whetton (1999) Climate change in Queensland under enhanced greenhouse conditions: first annual report, 1997-1998. CSIRO Atmospheric Research, Melbourne, 84 p. [Roger Jones]	Change made
11-1143	A	78:23	78:34	Several of the cited studies are before 2004, yet the text implies that they used a method published in 2004? How was potential evaporation calculated in the 2002 and 2003 studies? [Michael Roderick]	The above correction removes this issue
11-1144	A	78:26	78:26	change 2004 to 2004b [John McGregor]	Change made
11-1145	A	78:27	78:27	insert sentence "Simulations with the CSIRO CGCM indicate the increases over central Australia are correlated with small increases in 10 m wind speeds; dynamically downscaled simulations with CCAM also support this relationship." [John McGregor]	Change made
11-1146	A	78:30	78:34	The observed decrease in pan evaporation would only be in conflict if the model predicted increasing pan evaporation over the same time period. What is the relation between the model calculations of potential evaporation for 1970-2002 and the observations (Roderick & Farquhar 2004)? This needs to be described in more detail before describing projections into the future. [Michael Roderick]	The results requested are not available in the literature.
11-1147	A	78:49		Figure 11.3.7.4: This is discouraging for Wellingtonians! The patterns of change seems to	No further comment added

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				correspond to a westward retrogression of the climatological mean trough that presently lies east of New Zealand in winter - worthy of comment? [James Renwick]	
11-1148	A	78:51	79:8	Again, are the results described here from the same class of simulations (AR4, TAR)? [Aurel Moise]	Change made
11-1149	A	79:19	79:36	Integrate with Box 11.4. [Markku Rummukainen]	This change is being considered
11-1150	A	79:25	79:27	The increase in the 100 year storm tide event at both locations was around 0.45 and 0.5 m respectively with the changes dominated by the sea level rise, and the frequency changes being almost insignificant". This is unclear. Change to "…with the frequency changes having little effect. [Kevin Walsh]	Change made
11-1151	A	79:31	79:32	"Whetton (2005)" : is not in reference list. [Aurel Moise]	References have been changed
11-1152	A	79:38		Section 11.3.7.4: Quite uncertain on many fronts, again going against the comments on increased certainty at the start of the chapter. [James Renwick]	In revised version robust statements (likiely, very likely) will also appear here. This will make the summary material more balanced
11-1153	A	79:42	79:47	Regarding a more detailed description of model performane over Australia: please check BOTH of the above mentioned papers for description of seasonal cycles, Taylor plots of temperature and rainfall over Australia and simulation of Australian monsoon. [Aurel Moise]	This has now been factored into the chapter.
11-1154	A	80:0		Arctic (paragraph 11.3.8.1): OK [Marina Baldi]	Noted
11-1155	A	80:1	85:35	Most of the material presented on the Arctic (section 11.3.8.1) comes from CGMS and thus could be reduced in scope and combined with the material in section 10.3.3. In addition, several sentences are grammatically incorrect or could be clearer. For example: a) line 15 page 11-80. It's unclear what in the "Arctic large means". b) Lines 43-46. The authors refer to figures 11.3.8.1-11.3.8.3 to indicate biases in the current climate simulations. However, these figures show how the climate changes due to global warming and thus do not provide information on model error. In addition, these figures are very small and very difficult to read, especially the text within the figures. c) line 12-13 page 11-82 how does the consistency between observations and near future model projections "support the concept of Arctic amplification". [Michael Alexander Alexander]	Noted. Both chapters are balanced: Ch.10.3.3 focusses on cryosphere, while Ch.11.3.8.1 does on temperature and precipitation. Accepted. Cross-reference to Ch. 8 is made. Noted. Text is modified to clarify.

No.	Batch	Page:line From	Page:line To	Comment	Notes
11-1156	A	80:1		This section ignores land surface processes beyond permafrost and in particular the relationship between observed summer warming and increased shrubbiness in the Arctic. [Amanda Lynch]	Beyond the scope of this chapter. Cross- reference to WGII is made.
11-1157	A	80:2	80:30	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move [Markku Rummukainen]	Noted. Introduction is modified to link with the projection part.
11-1158	A	80:17	80:17	Part of an explanation for this is the fact that the polar region represents much fewer degrees of freedom than lower latitudes. [Rasmus E. Benestad]	Noted.
11-1159	A	80:42	80:51	Please consider adding a note on: "There are also some special challenges in the Arctic as regards to the availability and interpretation of measurements such as the presence of optically thin clouds (e.g. Wyser and Jones 2005)." The reference is: Wyser, K. and Jones, C. 2005. Modeled and observed clouds during Surface Heat Budget of the Arctic Ocean (SHEBA). J. Geophys. Res. 110, D09207, doi:10.1029/2004JD004751. This could also place in section 11.3.8.1.4 (uncertainties). [Markku Rummukainen]	Accepted; the reference is taken into account.
11-1160	A	81:27	81:29	This is a bit cryptic: an agreement exists but so does an improvement. [Markku Rummukainen]	Accepted; text is modified to clarify
11-1161	A	81:43	81:44	Neither Fig 11.3.8.1 nor 11.3.8.3 address sea ice, do they? Do you have missing figures? [James Renwick]	Accepted; cross-reference to Ch. 8 is made.
11-1162	A	81:44	81:44	It would have been nice to see sea ice thickness - instead the figure shows surface temperature. [David Rind]	Rejected. Ch. 10.3.3 shows sea ice plots, while we focus on temperature and precipitation
11-1163	A	81:44	81:44	It seems as if some other figure should be referred to. [Markku Rummukainen]	Accepted; cross-reference to Ch. 8 is made
11-1164	A	82:12		Write 2010-2029 Is it possible to use the period 2011-2030? [Ibouraïma YABI]	Noted. The given specific time slice is cited from the reference.
11-1165	A	82:20	82:20	Rather than stating "comparable" (from the discussion one gathers that these ranges would be 3.4-5.6 degrees and 2.8-7.8 degrees), please consider "both considerable". [Markku Rummukainen]	Accepted; text is modified to clarify
11-1166	A	82:30	82:30	Suggest "THE ABILITY TO CAPTURE the present-day sea ice state". [Markku Rummukainen]	Accepted; text is modified.
11-1167	A	82:55		Perhaps to go in the temperature projections section on page 82 where it says: "But, this projected cooling is in disagreement with the recent strong warming trend in Alaska	

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				(ACIA, 2005; Hinzman et al., 2005) indicating a decreased confidence in the summer projections (associated with the models inability to accurately simulate the present-day summer synoptic patterns)." Add: "Observational studies demonstrate that terrestrial summer warming in arctic Alaska correlates with a lengthening of the snow-free season that has increased atmospheric heating locally by about 3 W m-2 decade-1. Continuation of current trends in shrub and tree expansion (not represented in any GCM or RCM scenario considered here but demonstrated in earlier RCM sensitivity studies) could further amplify this atmospheric heating 2-7 times (Lynch et al. 1999, Chapin et al. 2000; 2005)." Relevant cites Chapin III, F.S., W. Eugster, J.P. McFadden, A.H. Lynch, and D.A. Walker, 2000: Regional climate forcing: Summer differences among arctic ecosystems. J. Climate, 13, 2002-2010. Chapin III, F.S., M. Sturm, M. C. Serreze, J. P. McFadden, J. R. Key, A. H. Lloyd, A. D. McGuire, T. S. Rupp, A. H. Lynch, J. P. Schimel, J. Beringer, W. L. Chapman, H. E. Epstein, E. S. Euskirchen, L. D. Hinzman, G. Jia, C. L. Ping, K. D. Tape, C. D. C. Thompson, D. A. Walker, and J. M. Welker, 2005: Role of Land-Surface Changes in Arctic Summer Warming. Science (published online 22 September 2005), DOI: 10.1126/science.1117368. Lynch, A.H., G.B. Bonan, F.S. Chapin III and W. Wu, 1999: The Impact of Tundra Ecosystems on the Surface Energy Budget and Climate of Alaska. J.Geophys.Res., 104, 6647-6660. [Amanda Lynch]	This is beyond the scope of the chapter. The chapter deals solely with projections. However, agreement or disagreement with observed trends are considered in the assessment statements
11-1168	A	83:1	83:2	Is the statement in the parentheses an established fact? [Markku Rummukainen]	Yes; this is shown in the AR4 model analysis of Cassano et al. (2005)
11-1169	A	84:5	84:6	Lack the placeholder/reference to the Table. [Markku Rummukainen]	Noted; this was in the figure part.
11-1170	A	84:5		It's worth giving to give the references of the table [Ibouraïma YABI]	Noted, this was in the figure part.
11-1171	A	84:9	84:9	Change "Taken" to "Taking" [James Renwick]	Accepted; text is modified
11-1172	A	84:13	84:13	Change "arisen by" to "resulting from" [James Renwick]	Accepted; text is modified
11-1173	A	84:15	84:16	Change to "…but a lower frequency (10-19%) for Alaska…" [James Renwick]	Accepted; text is modified
11-1174	A	84:15	84:15	Word missing at the end of the line - increase? [David Rind]	Accepted; but sentence is deleted.

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11-1175	A	84:38	84:45	<After the end of the sentense, please add the following sentence related with a new finding.> " Kitabata et at. (2005) analysed the ensemble projection data under A1B scenario by CCSM3 and found that the upper permafrost in circum-arctic regions would melt significantly in the 21st century due to global warming. They suggest that in Central Alaska and Eastern Siberia, the increase of soil temperature is remarkable in summer seasons due to meltiong of the permafrost. Howerver in colder regions, for example, Canadian Arctic, the increase of soil temperature is dominant in April because April is the coldest month when the upper permafrost at 3m depth under the ground still exists." < Please add the following paper in the reference before line 8, page 111, Chapter 11.> Kitabata, H., K. Nishizawa, Y. Yoshida and K. Maruyama, 2005: Permafrost Thawing in Circum-Arctic and Highland under Climatic Change Scenarios projected by CCSM3, SOLA, Meteorological Society of Japan, submitted (see :http://210.189.77.208/Result/Kitabata.pdf) [Koki Maruyama]	Disagreed. There is no reason to speculate on general statements based on results from individual models. Furthermore, the finding is in general line with other results (ACIA, 2005). Permafrost issues are discussed in detail in WGII.
11-1176	A	84:38	84:45	One simulation has been conducted (Stendel, M., V.E. Romanovsky, J.H. Christensen and T. Sazonova, 2005b: Global warming and permafrost: Closing the gap between climate model simulations and local permafrost dynamics, submitted to Glob. Plan. Change) where an off-line permafrost model is forced by high resolution (50 km) RCM data which have been obtained from a time-slice experiment. For East Siberia, an increase in active layer depth on the order of 0.5 to 2 m is simulated with largest values in mountainous regions and upland areas with coarse glacial sediment and on the east bank of the Lena river. [Martin Stendel]	Noted, however, the finding is in general line with other results (ACIA, 2005). The text has to compress and permafrost changes are discussed in WGII in detail.
11-1177	A	84:38		The section on permafrost ignores the effects on the carbon cycle. For example, the following could be added: One-third of the global soil carbon pool is stored in northern latitudes. Oechel et al. (2000) found that long-term sequestration of carbon in Alaskan tundra ecosystems was reversed in the early 1980s, resulting in substantial losses of terrestrial carbon (Oechel et al. 1993), but that this trend reversed in the 1990's. However, a new conceptual model of response to warming, where warmer soils and increased decomposition of plant litter increase nutrient availability, which, in turn, stimulates plant production and increases ecosystem carbon storage has been developed (Sturm et al. 2001). Mack et al. (2004) tested this conceptual model experimentally using manipulation experiments and found that it is an important potential feedback mechanism. Relevant cites: Mack MC, Schuur EAG, Bret-Harte MS, Shaver GR, Chapin FS, 2004: Ecosystem carbon storage in arctic tundra reduced by long-term nutrient fertilization. NATURE 431 (7007):	This is beyond the scope of the chapter.

No.	Batch	Page:line From	Page:line To	Comment	Notes
				440-443 OECHEL WC, HASTINGS SJ, VOURLITIS G, et al., 1003: RECENT CHANGE OF ARCTIC TUNDRA ECOSYSTEMS FROM A NET CARBON-DIOXIDE SINK TO A SOURCE. NATURE 361 (6412): 520-523 OECHEL WC, VOURLITIS GL, HASTINGS SJ, et al., 2000: Acclimation of ecosystem CO2 exchange in the Alaskan Arctic in response to decadal climate warming. NATURE 406 (6799): 978-981 Sturm M, Racine C, Tape K, 2001: Climate change - Increasing shrub abundance in the Arctic. NATURE 411 (6837): 546-547 [Amanda Lynch]	
11-1178	A	85:0		Antarctic (paragraph 11.3.8.2): OK, only few more papers authored by Bromwich and his collaborators should be discussed and added to the papers list. [Marina Baldi]	Noted.
11-1179	A	85:2	85:6	Compare with page 82, lines 35-41. This might require some more explanation. [Markku Rummukainen]	Rejected; authors believe that text is clear.
11-1180	A	85:8	:35	Can one return this part to the level of 11.3.8.2.4? [Ibouraïma YABI]	Rejected. We keep both uncertainty sections separately.
11-1181	A	85:9	85:14	Why have 2 degrees and 20% been chosen as thresholds? [Markku Rummukainen]	Noted; text is modified and table is removed
11-1182	A	85:16		Figure 11.3.8.4: Really interesting, but all labels are tiny. Please use much bigger fonts. [James Renwick]	Noted; but figure is removed.
11-1183	A	85:20	85:20	Change to "…represented in either GCMs or RCMs are clouds, planetary…" [James Renwick]	Accepted; text is modified
11-1184	A	85:25	85:25	Rather than "it remains difficult to project significant", one could opt for "a given magnitude of change is less significant in the Arctic than on the global scale and for many other regions". [Markku Rummukainen]	Noted; text is modified
11-1185	A	85:35	85:35	Might "near-future" be meant instead? [Markku Rummukainen]	Accepted; text is modifed
11-1186	A	85:45	85:45	Insert "directly" between "not" and "alter" [John King]	Accepted; text is modified
11-1187	A	85:47	85:48	The Antarctic circumpolar wave (ACW) has to be added, in my opinion, to SAM and ENSO, as SH-relevant phenomenon (e.g. White and Annis, 2004, also cited in the section 3.6). [Roxana Bojariu]	Rejected; we restrict to the SAM and ENSO as they are the most relevant patterns of variability. But, cross-reference to Ch. 3.6 is made
11-1188	A	85:49	85:52	I don't think it is correct to state that SAM affects the east while ENSO affects the west.	Accepted; text is modified .

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				Both SAM and ENSO affect the west (e.g. Genthon et al. 2003: . Interannual Antarctic tropospheric circulation and preciprecipitation variability, Climate Dynamics 21, 298-307) much more than the east. [Christophe Genthon]	
11-1189	A	85:55	86:1	Confusing description of ENSO impacts. "Pacific sector" is not well defined. By "ENSO index" do you mean SOI (negative during warm ENSO events) or a tropical SST index (positive during warm ENSO events)? [John King]	Accepted; text is modified.
11-1190	A	85:55	86:1	Mention also the suppression (enhancement) in sea ice associated with increased (decreased) SST related to ENSO variability? e.g. Renwick (2002, J. Clim, 15, 3058-3068), Yuan (2004, Ant. Sci., 16, 415-425). [James Renwick]	Accepted, text is modified
11-1191	A	86:1	86:3	Signatures of ENSO in net snow accumulation are confirmed in firn/ice core data (Kaspari et al. 2004: climate variability in west Antarctica derived from annual accumulation-rate records from ITASE firn/ice cores, Annals of Glaciology, 39, 585). However, the stability of this signature has been questioned both from model studies (Genthon and Cosme 2003, already in the chapter list of references) and in the observartions (Genthon et al. 2005: Interannual variability of the surface mass balance of West Antarctica from ITASE cores and ERA40 reanalyses, Climate Dyn. 24, 759-770) [Christophe Genthon]	Noted; text is modified
11-1192	A	86:10	86:13	It is true that model precipitation is not evaluated in models because there are no reliable observation. As a consequence, model hydrology is evaluated from glaciological observation of the surface accumulation (or surface mass balance), which also involves sublimation, melt and wind erosion, all processes which are poorly or not taken into account in models. [Christophe Genthon]	Noted; a discussion of surface mass balance estimates is beyond the scope of this section. This is discussed in Ch. 10
11-1193	A	86:13	86:13	Is this a general issue or of particular importance for the Antarctica? [Markku Rummukainen]	Accepted; this difficulty is a general problem; sentence is deleted
11-1194	A	86:17	86:21	VRGCMs have also been used for high-resolution climate and climate change studies over Antarctica. Most recently at 60 km resolution, comparable to RCMs, see Krinner et al., submitted, already mentioned above. [Christophe Genthon]	Accepted; it is added and the text is modified. Krinner et al cannot be used as it is not in print, but earlier references have been included
11-1195	A	86:24	86:26	The statement that the model bias in the southern oceans is in the range 2-6 oC is odd. See http://www.antarctica.ac.uk/met/wmc/hadcm3-era-sfct-ann.png which just shows hadcm3 – ERA. Over large section of the southern oceans the model bias is less than 1 oC. [William Connolley]	Noted, and text is modified. Given range is from Carill et al. using the AR4 model ensemble mean.
11-1196	A	86:29	86:29	Saying that model biases are within -6 – 6 oC over the continent glosses over the fact that	Noted, and text is modified. We note

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				sfc T isn't all that well known over the continent; different (re)analyses disagree (W. M. Connolley and S. A. Harangozo, A comparison of five numerical weather prediction analysis climatologies in southern high latitudes, J. Climate, v14, 1 Jan 2001, pp 30-44). [William Connolley]	about the difficulty of temperature evaluation in the first paragraph in 11.3.8.2.2
11-1197	A	86:31	86:31	What is meant be lateral nudging of a GCM does not seem readily understandable. [Markku Rummukainen]	See Genthon et al, 2002
11-1198	A	86:35	86:37	The statements that the AR4 runs capture the peninsula warming seems dubious. See http://www.antarctica.ac.uk/met/wmc/20c3m_jul_ts_tr.png. Whatever the mean may do, it is plain that few (if any) of the 20 runs captures the observed warming pattern. The individual aogcm runs are plainly in great disagreement. [William Connolley]	Noted. Our statement is that a *subset* of the models *qualitatively* capture the warming trend; and this is an improvement compared to the TAR models.We agree that the individual models do show very different trends. We modified the text to clarify.
11-1199	A	86:35	86:38	The two papers referenced here do NOT support the assertion that the AR4 models capture the observed 20th century warming in the Antarctic Peninsula. Both papers only examine projected future (21st century) trends. A paper which would appear to support the assertion is "A synthesis of Antarctic temperatures", by W.L. Chapman and J.E. Walsh (J. Climate, in press, I believe). However, close examination of the 20th century temperature trends in the vicinity of the Antarctic Peninsula across all of the AR4 models shows a very variable response in this region. Enhanced 20th century warming in the Antarctic Peninsula does not, therefore, appear to be a robust feature of the AR4 model simulations. [John King]	Accepted. The Chapman & Walsh paper is included. Although the model ensemble shows a warming trend, we know that the individual models do show very different trends. Therefore, we modified the text to clarify.
11-1200	A	86:35	86:35	Why a subset (i.e. was only a subset used in the study or did the rest of the AR4 GCMs fail in this respect?). This is a bit unclear, especially as the same reference (Carrill et al 2005)is also used on line 24-25 in conjunction with "The AR4 ensemble…" [Markku Rummukainen]	Noted; the rest of the models fails this respect; text is clarified
11-1201	A	87:1	87:3	Again, a VRGCM has been recently used for fine-scale (60 km) evaluations of climate change in Antarctica (Krinner et al, submitted, see above). [Christophe Genthon]	Krinner et al. cannot be used as it is not an accepted paper at this moment.
11-1202	A	87:7	87:7	First note in parentheses should read "(with a range of 1.4-4.9C)" [James Renwick]	Accepted; text is modifed
11-1203	A	87:7	:8	(figures 11.3.8 and 11.3.8_f) be more explicit to facilitate the reading [Ibouraïma YABI]	Accepted; text is modified
11-1204	A	87:11	87:13	"the latter can primarily be attributed to the sea ice" - the former too, since the large changes are usually due to seaice retreat. [William Connolley]	Accepted; text is modified

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11-1205	A	87:11	87:13	<After the end of the sentense, please add the following sentence related with a new finding.> "Kim et al (2005b) analyzed the data under A1B scenario projected by CCSM3 and they founnd that the regional cooling in the South Pacific sector of the Southern Ocean around the Antarctica depended on the topographic effect of the ridge off the Ross Sea, which seems quite different from the regional cooling near the Greenland." < Please add the following paper in the reference after line 7, page 111, Chapter 11.> Kim, D.-H., N. Nakashiki, Y. Yoshida, K. Maruyama and F. O. Bryan, 2005: Regional cooling in the South Pacific sector of the Southern Ocean due to global warming, Geophys. Res. Lett., Vol. 32, No. 19, L19607,accepted. [Koki Maruyama]	Rejected; our chapter is focussed on land.
11-1206	A	87:19	87:27	Would "Fyfe, J. C. 2003. Extratropical Southern Hemisphere cyclones: Harbingers of climate change? J. Climate, 16, 2802-2805", "Kushner et al. 2001. Southern Hemisphere atmospheric circulation response to global warming. J. Climate 14, 2238-2249." and "Brandefelt, J. and Källén, E. 2004. The response of the Southern Hemisphere atmospheric circulation to an enhanced greenhouse gas forcing. J. Climate, 17, 4425-4442." place here as additional references? [Markku Rummukainen]	Noted. However, text is compressed, and cross-reference to Ch. 10.3.5 is made.
11-1207	A	87:42	87:42	By "Bellingshausen to Ross Sea cyclones" I presume you mean cyclones within this sector of the Antarctic? Since cyclones propagate eastward, it might be better to write "cyclones in the Ross Sea to Bellingshausen Sea sector". [John King]	Accepted; text is modified
11-1208	A	88:25		Some discussion on the skills of GCMs and RCMs at those high latitudes is necessary. [Marina Baldi]	Accepted; text is modified
11-1209	A	88:29	88:56	These two paragraphs do not read very well. Suggest a critical re-reading and shortening. [James Renwick]	Accepted.
11-1210	A	88:29	88:56	Much of this description might place better in WGII and/or could be shortened. Moreover, the discussion on recent trends probably places better elsewhere in AR4. [Markku Rummukainen]	Trend section removed; some description retained and shortened; used only as evidence of change.
11-1211	A	89:2	90:16	Much of this description might place better in WGII and/or could be shortened and integrated with a regional view on "Uncertainties". Another alternative could be to prepate a new fact-box on large-scale (circulation, SST etc.) forcing on regions and move [Markku Rummukainen]	Shortened to include mainly section relevant to climate change.
11-1212	A	89:20	89:26	It is widely known that sea breezes and sea-breeze convection are strongly influenced by the large-scale flow, yet I don't know of any studies that have looked specifically at this question in the context of climate change for tropical or subtropical islands. Perhaps this could be mentioned as a topic that should be studied in future RCM experiments.	OK, but not the function of Chapter 11 to recommend

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				[Raymond Arritt]	
11-1213	A	89:25	89:26	Key processes: The dry belt in the Caribbean is found in the coastal zones of Venezuela and Colombia and on the islands along the their coasts in the Caribbean Sea west of Trinidad, including the Venezuelan islands, the Netherlands Antilles and Aruba. Besides being much drier (about 500 mm annually), the wet season is in the winter (Oct-Jan), whereas elsewhere in the Caribbean it falls in the summer (May-Oct).This is mainly due to the dominant subsiding air over the islands caused by the sea-continent breeze, speed and directional divergence in the lower atmosphere along the coast of Venezuela, suppressing rainfall activity especially during the summer. In addition the easterly trade winds generate an upwelling zone with cooler SST along the Caribbean coast of Venezuela and Columbia, which diminish the moisture content in the lower atmosphere. Martis AAE, Predicting rainfall in the Dutch Caribbean – More than el Nino? International Journal of Climatology 22: 1219-1234 (2002) [Albert MARTIS]	Too much text to include. Section on key processes has been shortened but reference will be included.
11-1214	A	89:25	89:26	Inter-annual variability in the Caribbean dry belt is also mainly governed by ENSO events. This is in agreement with the results of Ropelewski and Halpert (1987) for the northern South America. Moreover, in this zone one can find the highest correlations of station rainfall data with NINO3 index in the world. Only areas directly affected by the shift of the convective zone have significant higher values. During a La Nina event the rainfall season tends to by very wet (500 mm) and during El Nino the rainfall season tends to be very drier (150 mm). Martis AAE, Predicting rainfall in the Dutch Caribbean – More than el Nino? International Journal of Climatology 22: 1219-1234 (2002) [Albert MARTIS]	Reference will be included.
11-1215	A	89:30		consider deleting diminishes [Albert MARTIS]	Accepted.
11-1216	A	89:32		Write… 2002). (No space) [Ibouraïma YABI]	Accepted.
11-1217	A	89:35	89:47	Paragraph could be shortened. The phrase "this trough of low pressure" is used too much. [James Renwick]	Accepted.
11-1218	A	90:0		11.3.9.2.1. Recently Martinez-Castro et al. (2005) found that RegCM3 was capable of simulating the main climate features over the Caribbean region, including precipitation over the larger islands, specifically Cuba. Sensitivity studies showed that model performance was quite sensitive to domain size and physics parameterizations (both for boundary-layer and deep convection). Martinez-Castro, D., R.P. da Rocha, A. Bezanilla-Morlot, L. Alvarez-Escudero, J.P. Reyes-Fernandez, Y. Silva-Vidal and R.W. Arritt,	Reference to be included.

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				2005: Sensitivity studies of the RegCM-3 simulation of summer precipitation, temperature and local wind field in the Caribbean Region. Theoretical and Applied Climatology (in press). [Raymond Arritt]	
11-1219	A	90:24		Write 60W; (Punctuation) [Ibouraïma YABI]	Accepted.
11-1220	A	90:25		Write 100E; (Punctuation) [Ibouraïma YABI]	Accepted.
11-1221	A	90:26		Write 120W; (Punctuation) [Ibouraïma YABI]	Accepted.
11-1222	A	90:27	90:27	A southern limit of 55S seems generous for this region - it encompasses most of the Pacific Basin south of the Equator. Should the limit not be closer to (say) 35S? [James Renwick]	We are using the Giorgi defined regions.
11-1223	A	90:27		Write 80W; (Punctuation) [Ibouraïma YABI]	Perhaps full stop better.
11-1224	A	90:32	90:32	What are the percentages in parentheses in relation to? +1.5C is 5% of 30C, presumably close to the climatological annual mean temperature of this region. But 30C is 303K, making the bias more like 0.5% - perhaps percentages should be removed. [James Renwick]	Accepted.
11-1225	A	90:36	90:36	Omit "temporal evolution". [Markku Rummukainen]	Accepted.
11-1226	A	90:37	90:37	Has MSD been defined? [David Rind]	Yes (See 89-16)
11-1227	A	90:40	90:40	Date for Santer reference [Andrew Lacis]	Removed by TSU.
11-1228	A	90:40		Not to forget to specify the year [Ibouraïma YABI]	Temoved by TSU.
11-1229	A	91:11	91:11	change "10 km" to "8 km" [John McGregor]	Accepted.
11-1230	A	91:28		Write… (2000). (Punctuation) [Ibouraïma YABI]	Accepted.
11-1231	A	91:45	91:45	Change "will" to "is projected to" [James Renwick]	Accepted.
11-1232	A	91:48		Write… Asia). (Punctuation) [Ibouraïma YABI]	Accepted.

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11-1233	A	91:54		Write Lal and al., (2002); (Punctuation) [Ibouraïma YABI]	Accepted.
11-1234	A	92:21	92:23	The figure seems to detail annual data rather than seasonal ones. [Markku Rummukainen]	Accepted.
11-1235	A	92:31	93:42	This page of text deals with purely observed changes, not projections, and should not be in this chapter. [A. Brett Mullan]	Trends section removed; some description retained and shortened; used only as evidence of change.
11-1236	A	92:31	94:9	Suggest removing the discussion on climate trends as it stands out compared to the other regional subchapters and probably places better elsewhere in AR4 (under Detection or in WGII). [Markku Rummukainen]	Trends section removed; some description retained and shortened; used only as evidence of change.
11-1237	A	93:5	93:5	colds" should be "cold [James Renwick]	Accepted.
11-1238	A	93:17	93:17	After "Griffiths et al.", insert year in "( )". [Chiu-Ying LAM]	Removed by TSU
11-1239	A	93:17		Not forget to specify the year [Ibouraïma YABI]	Removed by TSU
11-1240	A	93:38		the Pacific Ocean near 90 E should be false. [Hidetaka Sasaki]	Sentence removed.
11-1241	A	93:44	94:9	This should be checked against text in chapters 3, 9 and may be other chapters. [Christoph Schar]	Accepted.
11-1242	A	93:44		A time-slice experiment by a 20 km mesh MRI/JMA AGCM shows that, in a future climate simulation, the number of tropical cyclones generally decreases in most of regions except for North Atlantic Ocean. The number of tropical cyclones generation is conspicuously diminished in the globe, but the geographical distribution of generation and tracks is not greatly changed. Oouchi, K., J. Yoshimura, H. Yoshimura, R. Mizuta, S. Kusunoki and A. Noda, 2005: Tropical cyclone climatology in a global-warming climate as simulated in a 20km-mesh global atmospheric model. J. Meteor. Soc. Japan, submitted. [Akio Kitoh]	To be referrd to and discussed.
11-1243	A	93:44		Section 11.3.9.6: This is not too well written and could do with editing and tidying up. [James Renwick]	Accepted.
11-1244	A	93:44		It is better to review the results of discussion of TAR such as Bengtsson et al., Sugi et al., Yoshimura et al. etc. on frequency change of tropical cyclone occurence. [Yasuo Sato]	This is done in Chap. 10. Reference will be made to Chap.10
11-1245	A	93:46	93:46	Change "southeast" to "southwest"	To check and correct of necessary.

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				[James Renwick]	
11-1246	A	93:54	93:54	Should be a reference to chapter 10, not 11 (?) [James Renwick]	Accepted.
11-1247	A	93:54	93:54	Seems funny to say refer to Chap 11 when this is Chap 11. [David Rind]	Should be Chap. 10.
11-1248	A	94:13		Box on mountain regions should be avoided and text introduced as a full paragraph [Marina Baldi]	Diasgree. The purpose of the box is to avoid overlap in text between differnt regional sections
11-1249	A	94:20		dependency ---> dependence [Yasuo Sato]	Noted.
11-1250	A	94:28		the Japanese Islands in Asia [Hidetaka Sasaki]	Noted. The text will include this
11-1251	A	94:31	94:32	Frei et al. 1998 missing in references. Frei, C., C. Schär, D. Lüthi and H.C. Davies, 1998: Heavy precipitation processes in a warmer climate. Geophys. Res. Lett., 25, 1431-1434. [Christoph Frei]	This reference will be added. Thank you
11-1252	A	94:31	94:31	At least Bergström et al. (2001) do NOT use such high-resolution RCM input. [Markku Rummukainen]	Agreed. The text will be modified to reflect this.
11-1253	A	94:31	94:33	These references seem to be absent from the reference list (this seems true even for some of the other references entered in Box 11.3). [Markku Rummukainen]	Noted. References will be added
11-1254	A	94:31		Bergstrom et al.,2001 is missing in the references. [Hidetaka Sasaki]	Noted. References will be added
11-1255	A	94:31		Reference to Frei et al (1998): This paper uses an RCM at 50 km resolution, and is thus probably not the proper reference. Alternative paper that could be referenced are listed below. These use convection-resolving models at km-scale resolution: Walser, A., D. Lüthi and C. Schär, 2004: Predictability of Precipitation in a Cloud-Resolving Model. Mon. Wea. Rev., 132 (2), 560-577 Weisman, M. L., W. S. Skamarock, and J. B. Klemp, 1997: The resolution dependence of explicitly modeled convective systems. Mon. Wea. Rev., 125, 527–548 [Christoph Schar]	Noted. Will revise accordingly
11-1256	A	94:32	94:32	Insert ")" after "2003" [Bart Van den Hurk]	Done
11-1257	A	94:32		Kanada et al., 2005 and Yasunaga et al., 2005 should be referred to after the text 'extreme events such as precipitation' [Yasuo Sato]	Will consider this after assessment of paper.

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11-1258	A	94:35	94:36	"Projections of changes in precipitation patterns in mountains are tenuous in most climate models because the controls of topography on precipitation are not adequately represented." This is true for GCMs. Replace climate models by GCMs. But this statement alone is very pessimistic considering the potential of todays downscaling techniques. Since TAR there were several studies indicate that the higher resolution of RCMs can represent observed mesoscale patterns of the precipitation climate that are not resolved in GCMs (Frei et al. 2005; Schmidli et al. 2005). Schmidli, J., C. Frei and P.L. Vidale, 2005: Downscaling from GCM precipitation: A benchmark for dynamical and statistical downscaling methods. Int. J. Climatol., (in press). [Christoph Frei]	Agreed. The text will be modified to take this into consideration.
11-1259	A	94:35	94:36	The statement alone for GCMs is very pessimistic considering the potential of todays downscaling techniques. I suggest adding something like: Recent applications of downscaling techniques however demonstrate that the higher resolution of RCMs can reproduce observed mesoscale patterns of the precipitation climate that are not represented in GCMs (Frei et al. 2005; Schmidli et al. 2005). Schmidli, J., C. Frei and P.L. Vidale, 2005: Downscaling from GCM precipitation: A benchmark for dynamical and statistical downscaling methods. Int. J. Climatol., (in press). [Christoph Frei]	See 11-1258
11-1260	A	94:47	94:48	"For every … 150 m." Please check this assessment. This is only the application of the vertical gradient 0.6 C/100m. Although the concept of snowline is difficult to determine on the field, we know that for the lower elevations the snowline will rise more than by 150 m / C. The explanation is done in the sentence above, line 44-46 of the same page. See also : Martin E., Brun E., Durand Y. (1994) : "Sensitivity of the French Alps snow cover to the variation of climatic variables", Annales Geophysicae, 12, 469-477. Sorry for this quite old self-citation, but it si still valid for this question of gradient ! See also Chap 4, paragraph 4.2.4.2.1 and the corresponding figure 4.2.3 [ERIC MARTIN]	The comment is acknowledged. The text will be revised to reflect on this
11-1261	A	94:48		Recent estimations of the sensitivity of the snow line to temperature in the Alps range from 60-70 m/ C (Vincent 2002: Influence of climate change over the 20th century on 4 glacier mas balance, J. Geophys. Research 107) to 160m/ C (Gerbaux et al., 2005: Surface mass balance of glaciers in the french Alps, distributed modeling and sensitivity to climate change, Journal of Glaciology, in press)., the latter obtained from an extensive multi-variable analysis using a physically-based snow-ice model. [Christophe Genthon]	Acknowledged. See 11-1260
11-1262	A	94:52	95:52	replace "in" by "induced by" [Bart Van den Hurk]	Done
11-1263	A	95:12		Box on coastal zone should be avoided and text introduced as a full paragraph	Do not agree. Material of importance

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				[Marina Baldi]	and not specific to regions.
11-1264	A	95:19	95:19	Strange sentence "independently, or by substantially enhancing, the…". Please make clearer [Bart Van den Hurk]	OK
11-1265	A	95:31	95:31	The reference is absent on the reference list. [Markku Rummukainen]	Will clarify.
11-1266	A	95:39		You might consider adding that "The statistical and dynamical approach can be combined, using a statistical model to produce the high resolution wind fields forcing the wave and storm surge dynamical models (Lionello et al 2003)". The text could be added at line 10, page 96 [Piero Lionello]	Agreed.
11-1267	A	95:44	95:44	insert "," before "though" [Bart Van den Hurk]	OK
11-1268	A	95:47	95:47	Add reference to Van den Brink et al (2004), see comment 71 [Bart Van den Hurk]	Will assess reference for relevance.
11-1269	A	96:23	96:35	The paragraph is not necessary - all issues presented here were discussed in the subsection dealing with the methodology for regional climate change assessment. [Roxana Bojariu]	Will ensure overlaps are rmoved.
11-1270	A	96:23	96:35	This seems rather too general. [Markku Rummukainen]	Will provide relevant focus and clarification.
11-1271	A	96:31	96:31	Insert after "storms.": "For instance, Van den Brink et al (2004b - see next comment) found from a large ensemble of simulations with an intermediate complexity climate model that the scaling behaviour of extreme storms (with return times of more than 1000 yrs) changed under changing greenhouse gas conditions. Statistical extrpolation of extreme storm events in present day climate conditions was unable to capture the wind speeds associated with unprecedented "super storms", which seemed to originate from different physical mechanisms than encountered in the control climate simulations." [Bart Van den Hurk]	Will assess reference for relevance.
11-1272	A	96:56	96:56	In the extreme sea level section, mention should also be made of work by Hardy et al. (2004), referenced at the end of this chapter. For instance, add to line 56: "Similar increases for Cairns and other coastal locations were found by Hardy et al. (2004)." [Kevin Walsh]	OK
11-1273	A	97:3	97:24	Much - if not all - of the contents of this box is already mentioned in the main text: may be removed. [Bart Van den Hurk]	Agreed, will be removed from main text and retained here.
11-1274	A	97:17		Box 11.4, Figure 1: Units? Metres?	Will clarify.

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				[James Renwick]	
11-1275	A	97:19	97:24	Is this paragraph worth including? [James Renwick]	Will revise and include in the context of new material on waves/surges.
11-1276	A	97:22	97:22	replace "significant" with "statistically significant" [Piero Lionello]	OK.
11-1277	A	97:27	97:41	This doesn't say very much, and could be shortened considerably. [James Renwick]	Agreed, will be shortened and included in the context of new material.
11-1278	A	97:35		Not forget to specify the year [Ibouraïma YABI]	OK.
11-1279	A	97:51		Question 11.1: This answer is very short, and rather general. We also note that the question has been changed to "How Useful are Regional Scale Projections", from the orginally suggested question which was "Are there factors which are expected to cause regional variations in climate change". We wonder whether the chapter authors found that it was difficult to satisfactorily frame and answer a regional climate change "question". If this is the case some options include: (a) Drop Question 11.1 entirely (there is no reason why every chapter needs to have a question), or (b) Revert to the originally posed question and draw on some of the material from Chapter 10 - On how various regional circulation features are likely to change under greenhouse warming - and discuss how this may lead to regional variations in future climate changes, or (c) Retain this material in the chapter as a short box (ie a "chapter box" rather than a "climate change question"). [David & David Wratt & Fahey]	Question has been changed
11-1280	A	97:53	98:8	I do not understand the meaning of such a question posed here. It should be introduced in the main text or avoided. [Marina Baldi]	Question has been changed
11-1281	A	97:53		Useful to what? The response to question 11.1 depends on who the target is for these projections. Are they useful from a scientific perspective in understanding the smaller-scale processes that affect climate at the regional level? To those in other disciplines who are attempting to understand the potential impacts of climate change on ecosystems, agriculture, etc? To decisionmakers who need to know what is likely to happen to their region or country in order to put in place policies to encourage mitigation and/or adaptation? [Katharine Hayhoe]	Question has been changed
11-1282	A	97:53		Question 11.1: The answer seems very equivocal, as is perhaps appropriate, but I feel there is much value in regional projections that at least incorporate important topographic	Question has been changed

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				effects, where mean circulation forcing is important. I think the answer could be more positive than at present. [James Renwick]	
11-1283	A	97:53		Actualy, this question is very dangerous for this chapter, and one that has not been at all properly explored. The comment on p.4 about regional changes depending on the large-scale response raised the question over whether regional response depiction has any meaning given the large uncertainty in both climate and latitudinal sensitivity. That was never answered and should be addressed here. In addition, the assumption that what the majority of models get is in some sense more likely has never been proven fora climate change situation. The tests that have been done to verify the multi-model ensemble approach - weather forecasts, and patterns driven by input SSTs - both have as their dominating influence dynamical processes (providing advection of temperature, and precipitation changes). But for the future climate projections, it is the physical changes that are important - cloud cover, water vapor feedback, sea ice response - these are what lead to the overall warming, and whatever dynamical changes occur as an additional influence. There is no proof that the multi-model ensemble can give the right physical changes (it certainly did not give the right chemical changes when forecasts for CFC impacts on ozone were being made and the ozone hole phenomenon was missed entirely). This really should have been addressed at the beginning of the chapter when the multi-model concept was first brought up - and it definitely should be addressed here. This chapter could prove to be extremely misleading, and caveats to that effect need to be strengthened with this type of discussion. [David Rind]	Question has been changed
11-1284	A	97:55	97:55	Consider rephrasing "a direct function". E.g.: "Regional climate change incorporates the global radiative forcing change, but is compounded by possible change in regional circulation, land use change and local-to-regional feedback. Compared to global scale change, regionally-varying forcing agents such as aerosols and tropospheric ozone also add additional detail." [Markku Rummukainen]	Question has been changed
11-1285	A	97:55		The answer to Question 11.1 is almost incomprehensible. I'd hate to see the long answer. [A. Brett Mullan]	Question has been changed
11-1286	A	97:56	97:56	After "componded with changes" I suggest adding something like "in SST patterns, changes". This is a key driver of regional climate change in the tropics, as has been detailed in this chapter. [Dave Rowell]	
11-1287	A	98:4	98:4	Omit "empirical and dynamical". [Markku Rummukainen]	Question has been changed

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11-1288	A	98:6	98:6	Consider omitting "Although dependent on region and variable". [Markku Rummukainen]	Question has been changed
11-1289	A	98:8		Write details. (Punctuation) [Ibouraïma YABI]	Question has been changed
11-1290	A	99:0		Papers by the French Authors (Janicot, Sultan, Fontain, Moron, Li, etc) dealing with the extension/position of ITCZ, rainfall associated with WAM and its relationship with SST in tropical Areas (New Guinea Gulf and Al Nino) should be mentioned in the text and listed in the bibliography. [Marina Baldi]	Considered as space allows
11-1291	A	99:0		To add to the bibliography and discuss in the text: V Moron, N Philippon, B Fontaine, 2004: Simulation of West African monsoon circulation in four atmospheric general circulation models forced by prescribed sea surface temperature. J. of Geophys Research. Rowell, D. P. (2001), Teleconnections between the tropical Pacific and the Sahel, Q. J. R. Meteorol. Soc., 127, 1683–1706. Papers by Janicot and Sultan on WAM and SST [Marina Baldi]	Considered as space allows
11-1292	A	99:0		Diongue, Lafore, et al., 2002: Numerical study of a Sahelian synoptic weather system: initialization and mature stages of convection and its interactions with large scale dynamics. QJRMS, 128, 1899-1928 [Marina Baldi]	Considered as space allows
11-1293	A	99:0		To add for landuse related issues in West Africa: Taylor, Parker, Lloyd, Thorncroft, 2005: Observations of synoptic scale land surface variability and its coupling with the atmosphere. QJRMS. In press [Marina Baldi]	Considered as space allows
11-1294	A	99:1	125:2	Highlight all those references that are not yet published. [Chiu-Ying LAM]	References consolidated in SOD
11-1295	A	99:1		"References" : I only looked through the references concerning the initial part of Chapter 11 and the section about Australia and New Zealand and I was surprised to see that there are quite a few references to non-peer-reviewed publications such as reports, departmental assessments and so on. Given that the group of people who submitted projects analysing AR4 simulations (which includes myself) were instructed that their work could only be included if it was submitted to a peer-review journal, does this mean that there were no suitable peer-reviewed papers that could be referenced? [Aurel Moise]	Where relevant work only appears in reports etc, but these are published and available they have been used.
11-1296	A	102:13	102:14	Add the following reference between lines 13 and 14: "Caires, S., V. R. Swail, and X. L. Wang, 2005: Projection and analysis of extreme wave climate. J. Climate, accepted subject to revision." (see Comment #37, #40, #46 above). See file "CairesSwailWang_GEV_GPD.pdf" on the anonymous ftp site given in Comment 36	Considered as space allows

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				above. [Xiaolan L. WANG]	
11-1297	A	104:25		[in press] [Michel Déqué]	Noted
11-1298	A	105:22	105:22	Add: "El-Shahawy, M.A. and El-Rafy A.M.: Energetic interaction leading to African droughts during El-Nino periods Proc. LRWPR- Trieste-ITALY, 1991." at the start of line 22. [Mohamed El-Shahawy]	Noted
11-1299	A	105:26	105:27	Reference is included twice [Bart Van den Hurk]	Noted
11-1300	A	105:54	105:58	"Frei, C., R. Schöll, S. Fukutome, J. Schmidli, and P.L. Vidale, 2005b: Future change of precipitation extremes in Europe: An intercomparison of scenarios from regional climate models. Journal of Geophysical Research, submitted." The status of this publication is "in press". [Christoph Frei]	Noted
11-1301	A	109:11	109:12	Hope 2005a' will change to 'Hope 2005' because of correction above. [Aurel Moise]	Change made
11-1302	A	109:13	109:14	Hope 2005b' author list is now: P.K. Hope, W. Drosdowsky and N. Nicholls [Aurel Moise]	Change made
11-1303	A	110:33	110:33	<Please Insert the paper, which is already cited on page 46, line 17; maybe misprint !> Kadokura, S. and H. Kato, 2005: Seasonal/Regional Variation of Variability Characteristic of Daily Maximum/Minimum Temperatures in Japan Observed and Reproduced by RegCM Nested in NCAR-CSM. Journal of the Meteorological Society of Japan, 83, 69-87. [Koki Maruyama]	Noted
11-1304	A	112:33	112:33	In association with comment # 20, insert a new reference "Leung, Y.K., E.W.L. Ginn, M.C. Wu, K.H. Yeung and W.L. Chang, 2004: Temperature projections for Hong Kong in the 21st century. HK MetS Bulletin, 14(1/2), 21-48." [Chiu-Ying LAM]	Considered as space allows
11-1305	A	113:21	113:21	If you agree suggestion from "13" please insert after line 21: "Mares C., Ileana Mares, 2004: Optimal Ensemble Analysis for the Improvement of long-range Forecasting. Workshop on Ensemble Methods, from weather forecasting to climate change. 18-21 October, 2004, Met Office, Exeter, UK, http://cccma.seos.uvic.ca/cgi-bin/ensemble/." [CONSTANTIN MARES]	Noted
11-1306	A	113:27		Martis AAE, Predicting rainfall in the Dutch Caribbean – More than el Nino?International Journal of Climatology 22: 1219-1234 (2002)	Noted

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				[Albert MARTIS]	
11-1307	A	114:15	114:15	insert after L15 McGregor, J. L., J. J. Katzfey and K. C. Nguyen, 1999: Recent regional climate modelling experiments at CSIRO. Research Activities in Atmospheric and Oceanic Modelling Report No. 28 (ed. H. Ritchie). WMO/TD-No. 942, 7. 37-7. 38. [John McGregor]	Noted
11-1308	A	115:13	115:14	Insert new lines after the 13th line as follows: Mizuta, R., K. Oouchi, H. Yoshimura, A. Noda, K. Katayama, S. Yukimoto, M. Hosaka, S. Kusunoki, H. Kawai and M. Nakagawa, 2005a: 20km-mesh global climate simulations using JMA-GSM model. J. Meteor. Soc. Japan, submitted. [Hiroki Kondo]	Considered as space allows
11-1309	A	115:14	115:14	2005 should be changed to "2005b" [Hiroki Kondo]	Noted
11-1310	A	118:9	118:10	The correct citation for this paper is now: Rowell, D.P., 2005a: A demonstration of the uncertainty in projections of UK climate change resulting from regional model formulation. Climatic Change, submitted (It is now accepted subject to minor revisions.) [Dave Rowell]	Noted
11-1311	A	119:4	119:5	Insert new lines after the 4th line as follows: Sasaki, H., H. Kida, T. Koide and M. Chiba, 1995: The performance of long-term integration of a limited area model with the spectral boundary coupling method. J. Meteor. Soc. Japan, 73, 165-181. [Hiroki Kondo]	Considered as space allows
11-1312	A	120:11	120:11	Reference Stendel and Roeckner (1998), referred to in chapter 11, page 8, line 40, is missing. The reference is: Stendel, M. and E. Roeckner, 1998: Impacts of horizontal resolution on simulated climate statistics in ECHAM4. Max Planck Institute for Meteorology Report No. 253, 57 pp. [Martin Stendel]	Noted
11-1313	A	121:4	121:4	Timbal and Jones (2005) title is "Future projections of winter rainfall in south east Australia using a statistical downscaling technique." [Robert Colman]	Paper deleted as it has not been accepted for publication
11-1314	A	121:38	121:38	Brink, H.W. van den, G.P. Können, J.D. Opsteegh, G.J. van Oldenborgh and G. Burgers, 2004. Improving 104-year surge level estimates using data of the ECMWF seasonal prediction system. Geophys. Res. Lett. 31, L17210, doi:10.1029/2004GL020610 [Bart Van den Hurk]	Noted
11-1315	A	121:38	121:38	Brink, H.W. van den, G.P. Können and J.D. Opsteegh, 2004b. Statistics of extreme synoptic-scale wind speeds in ensemble simulations of current and future climate. J. Climate 17, 4564--4574 [Bart Van den Hurk]	Noted

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11-1316	A	121:40	121:40	Van den Hurk, B., M. Hirschi, C. Schär, G. Lenderink, E. van Meijgaard, A. van Ulden, B. Rockel, S. Hagemann, L. Graham, E. Kjellstrom and R. Jones (2005): Soil Control on Runoff Response to Climate Change in Regional Climate Model Simulations; Journal of Climate 18, 3536–3551. [Bart Van den Hurk]	Noted
11-1317	A	123:3	123:3	Update "2004" to "2004a", because there are three "Wang et al. 2004" cited in this chapter. [Xiaolan L. WANG]	Noted
11-1318	A	123:9	123:9	Update "2004" to "2005a". [Xiaolan L. WANG]	Noted
11-1319	A	123:12	123:13	Add the following reference between lines 13 and 14: "Wang, X. L., and V. R. Swail, 2005b: Climate change signal and uncertainty in projections of ocean wave heights. Climate Dynamics, in press." (see Comment #39, #42, #45-46, #55, and #57 above). See file "WangSwail2005_ClimDyn.pdf" on the anonymous ftp site given in Comment 36 above. [Xiaolan L. WANG]	Considered as space allows
11-1320	A	123:13	123:13	Update "2004" to "2004b", because there are three "Wang et al. 2004" cited in this chapter. [Xiaolan L. WANG]	Noted
11-1321	A	126:0		Table 11.2.1.: To facilitate the reading of the table, I suggest that each heading should be separated well by features of column [Ibouraïma YABI]	Noted
11-1322	A	128:0		Table 11.3.2.1: The title of the table is not specified I suggest that the column of the temperatures should be separated from the column of precipitations [Ibouraïma YABI]	Table reworked
11-1323	A	128:1		Table title and/or caption is missing [Marina Baldi]	Noted
11-1324	A	128:2		Legend missing for table [David Rind]	Noted
11-1325	A	129:0	137:	I suggest that the column of the temperatures should be separated from the column of precipitations for all the tables [Ibouraïma YABI]	Noted
11-1326	A	129:1	129:2	Please include in the caption that "max" and "min" refer to the highest and lowest model	Noted

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				in the ensemble, not the climatological min/max. [Bart Van den Hurk]	
11-1327	A	138:0		Table 11.3.82: informations of the 1ist line are confused. You'd better present [Ibouraïma YABI]	Tabl;e has been reworked
11-1328	A	144:0	205:	Most of figures are in general too small. Can the Authors provide a better quality images? [Marina Baldi]	Figures reworked
11-1329	A	144:0		Fig. 11.1.1Add label and units to color bar. [Melinda Marquis]	Noted
11-1330	A	144:0		Figure 11.1.1 is a powerful illustration of the value of regional modelling. [James Murphy]	Thanks
11-1331	A	144:6	144:6	Reference Schwarb et al. (2001) is missing in References: Schwarb, M., C. Daly, C. Frei and C. Schär, 2001: Mean annual precipitation in the European Alps 1971-1990. Hydrological Atlas of Switzerland, Landeshydrologie und Geologie, Bern, Plate 2.6. (Available from Institute of Geography, University of Berne, Hallerstr. 12, CH-3012 Bern, Switzerland.) [Christoph Frei]	Noted
11-1332	A	145:0		Histogram (AOGVCM count) appears to be based on too few data points. [Rasmus E. Benestad]	Corrected
11-1333	A	145:0		The figure label of middle panels display "C N A" instead of "C A N" [Bart Van den Hurk]	Fixed
11-1334	A	146:0		Fig. 11.2.2 Add labels and units to X and Y axes. [Melinda Marquis]	Fixed
11-1335	A	147:0		Fig. 11.3.1.1 Add labels and units to X and Y axes. [Melinda Marquis]	Fixed
11-1336	A	147:0		Figure 11.3.1.1: The figures of the squaring are not quite readable with the impression [Ibouraïma YABI]	Fixed
11-1337	A	148:0	151:	Improve quality? (Font) [Rasmus E. Benestad]	Fixed
11-1338	A	148:0		Fig. 11.3.2.1 Fix typo in caption: "Middle panel id" change "id" to "is." Add labels and units to X and Y axes. [Melinda Marquis]	Fixed
11-1339	A	149:0		Fig. 11.3.2.2 Add labels and units to X and Y axes and to color bar. [Melinda Marquis]	Fixed
11-1340	A	150:0		Fig. 11.3.2.3 Add labels and units to X and Y axes. [Melinda Marquis]	Fixed

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11-1341	A	151:0		Fig. 11.3.2.4 Add labels and units to X and Y axes. Consider labeling top 3 graphs as (a): 2070-2099 and (b): 2080-2099 - 30 yr. Order graphs to match order of reference in caption. [Melinda Marquis]	Fixed
11-1342	A	151:0		Figure 11.3.2.4: The figures of the squaring are not quite readable with the impression [Ibouraïma YABI]	Fixed
11-1343	A	152:0		Fig. 11.3.3.1 Add label and units to color bars. Precipitation change is relative to what? [Melinda Marquis]	Fixed
11-1344	A	152:5		The analysis area is not defined [David Rind]	Fixed
11-1345	A	154:0		Almost illegible – improve quality! [Rasmus E. Benestad]	Agreed.
11-1346	A	154:0		Fig. 11.3.3.3 Add labels and units to X and Y axes. [Melinda Marquis]	Agreed.
11-1347	A	154:0		Figure 11.3.3.3. : The figures of the x-axes are not quite readable with the impression [Ibouraïma YABI]	Agreed. Will be improved
11-1348	A	155:0		Fig. 11.3.3.4 Add label and units to color bar. [Melinda Marquis]	Agreed. Units will be added.
11-1349	A	156:0		Fig. 11.3.3.5 Add labels a, b and c to graphs. [Melinda Marquis]	Noted. Figure deleted.
11-1350	A	157:0		Fig. 11.3.3. 6 Consider adding label to graphs (a) winter (on left) and (b) summer (on right) just to help reader. [Melinda Marquis]	Noted. Will be considered.
11-1351	A	157:1	157:14	This Figure could be provided in color. Results for individual models in different colors. For easier reading. [Christoph Frei]	Thanks for informing.
11-1352	A	157:11	157:12	The figure indicates an unproportional increase in extreme precipitation in winter but not in summer." I don't know if this statement is understood correctly. Suggestioin: "The figure indicates that changes in extremes are comparable to changes in average events in winter but not in summer. [Christoph Frei]	Fixed.
11-1353	A	158:0		Fig. 11.3.3.7Consider spelling out labels: Changes (or leave as delta sumbol) in annual mean wind speed (Y axis); Changes in average yearly maximum wind speed. [Melinda Marquis]	Figure deleted.
11-1354	A	159:0		Fig. 11.3.4.1 Add labels and units to X and Y axes. [Melinda Marquis]	Figure deleted

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11-1355	A	159:0		Figure 11.3.4.1: What do the arrows indicate? Be more precise. [Ibouraïma YABI]	Figure deleted
11-1356	A	160:0		Fig. 11.3.4.2 Pretty difficult to pick out the "observed" data line in opt (a) graph. Can this be done w/o using color? [Melinda Marquis]	Figure will be revised
11-1357	A	160:0		Figure 11.3.4.2.: It is desirable to remind the period of projection in the title. [Ibouraïma YABI]	Figure will be revised
11-1358	A	161:0		Fig. 11.3.4.3 "Referred" has three "r"s. Fix in both instances. Could caption be edited to clarify what graph is showing? [Melinda Marquis]	Figure and caption will be revised
11-1359	A	161:0		Figure 11.3.4.3: Is it possible to have also GT45 for a better appreciation? [Ibouraïma YABI]	Figure and caption will be revised
11-1360	A	162:0		Improve quality? (Font) [Rasmus E. Benestad]	Figure and caption will be revised
11-1361	A	162:0		Fig. 11.3.4.4 Departure from what? Anomaly from what? Also, can "departure" be changed to "anomaly"? [Melinda Marquis]	Figure and caption will be revised
11-1362	A	162:0		Figure 11.3.4.4: The letters and the figures are unreadable after printing [Ibouraïma YABI]	Figure and caption will be revised
11-1363	A	163:0		Fig. 11.3.4.5 Consider labeling left graphs as Temp changes (compared to what?) and right graphs as PPt changes (compared to what?). Refer to a, b, c and d in caption. Add more complete labels and units to X and Y axes. [Melinda Marquis]	Figure and caption will be revised. MME4 part will be deleted.
11-1364	A	163:0		Figure 11.3.4.5: to specify if it is about minimal temperature, maximum or average [Ibouraïma YABI]	Figure and caption will be revised. MME4 part will be deleted.
11-1365	A	164:0		Fig. 11.3.4.6 Consider labeling graphs a, b, c, d, e, and f. Consider labeling elft graphs as "Near surface temp change" and right graphs as "Precipitation change" [Melinda Marquis]	Figure will be deleted
11-1366	A	165:0		Necessary? [Rasmus E. Benestad]	Figure will be deleted
11-1367	A	165:0		Fig. 11.3.4.7 Add label and units to color bar. [Melinda Marquis]	Figure will be deleted
11-1368	A	166:0		Specify 'all AR4 simulations'. [Rasmus E. Benestad]	Figure will be updated. Caption will be consistent with text elsewhere in Chapter

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11-1369	A	166:0		Fig. 11.3.4.8 Add labels and units to X and Y axes and to color bars. Label graphs with "DJF" and "JJA" or "winter" and "summer," just to help reader. [Melinda Marquis]	Figure will be updated
11-1370	A	166:0		Figure 11.3.4.8.: The titles of graphs 3 and 4 are unreadable after printing [Ibouraïma YABI]	Figure will be updated
11-1371	A	167:0		Not legible. Improve quality. [Rasmus E. Benestad]	Figure will improved or deleted or replaced.
11-1372	A	167:0		Fig. 11.3.4.9 Label color bar, e.g., precipitation or change in precipitation compared to mean of period from X to Y. [Melinda Marquis]	Figure will improved or deleted or replaced.
11-1373	A	167:0		Figure 11.3.4.9: The figures of the legend are not at all readable after printing [Ibouraïma YABI]	Figure will improved or deleted or replaced.
11-1374	A	168:0		Which GCMs? [Rasmus E. Benestad]	Figure will be updated
11-1375	A	168:0		Fig. 11.3.4.10 Add labels and units to X and Y axes and to color bars. Add reference in caption to outline of Asian in graphs. [Melinda Marquis]	Figure will be updated
11-1376	A	169:0		Figure 11.3.5.1. The figure caption reads only "Key regional processes for North America". However what are the fields shown: Precipitation, SLP, some measure of the storm tracks? How is the latter computed? In addition, these are not processes but rather aspects of the climate. [Michael Alexander Alexander]	Text clarified
11-1377	A	169:0		Caption very brief. Legends not very clear in right hand panel. [Rasmus E. Benestad]	Fixed
11-1378	A	169:0		Fig. 3.5.1 Add labels and units to color bar. [Melinda Marquis]	Fixed
11-1379	A	169:0		Fig. 11.3.5.1. It is not clear why this figure has been included. The left panel shows pressure and temperature contours, but for what season, and why are these "key processes", rather than outcomes of processes? Panel b shows cyclone track density, so presumably this must link with panel a as a way of explaining the important influences in the N. American climate. I suggest deleting these pictures and replacing them with a schematic diagram that is more closely linked with the text. [Kevin Walsh]	Dropped / changed
11-1380	A	170:0		What ensemble? GCMs? [Rasmus E. Benestad]	Fized
11-1381	A	170:0		Fig. 11.3.5.2 Add labels and units to X and Y axes and to color bars. Consider adding	Dropped / changed

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				labels a, b, and c to three graphs, and refer to each in caption. [Melinda Marquis]	
11-1382	A	170:0		Fig. 11.3.5.2. Caption should actually tell us what the figure shows, namely "Comparison between observed and simulation precipitation (mm/day) from an ensemble mean of AR4 CGCMS; (a) observed precipitation from CMAP (Xie and Arkin 1996); (b) Ensemble mean precipitation; (c) difference, simulated minus observed." The colors of the panel (c) should be reversed so that blue indicates higher precipitation and red lower. This change should be made for other, similar pictures in this chapter also, unless authors were instructed to use this color scheme. [Kevin Walsh]	Dropped / changed
11-1383	A	170:0		Figure 11.3.5.2: The period of projection is not specified in the title [Ibouraïma YABI]	Dropped / changed
11-1384	A	171:0		Could benefit ofr further specification. Southern plains in the US? [Rasmus E. Benestad]	Dropped / changed
11-1385	A	171:0		Fig. 11.3.5.3 Expound on axes labels and caption to clarify figure. [Melinda Marquis]	Dropped / changed
11-1386	A	171:0		Fig. 11.3.5.3. Define the "southern plains" region by giving latitude and longitude bounds. In any event, why just produce this plot for a region rather than the whole continent, if in the text the whole continent is being discussed? There must be closer linkage between the text and the figures. [Kevin Walsh]	Dropped / changed
11-1387	A	172:0	173:	Which GCMs? Improve quality. [Rasmus E. Benestad]	Figures changed
11-1388	A	172:0		Fig. 11.3.5.4 Add labels and units to X and Y axes and to color bars. Label graphs with "DJF" and "JJA" or "winter" and "summer," just to help reader. Specify changes compared to what. [Melinda Marquis]	Figures changed
11-1389	A	172:0		Fig. 11.3.5.4. Again, the caption should describe the figure. Add "… surface temperature changes, 20-year period 2079-2098 minus 1979-1998, …" [Kevin Walsh]	Figures changed
11-1390	A	172:0		Figure 11.3.5.4: The period of projection is not specified in the title [Ibouraïma YABI]	Figures changed
11-1391	A	173:0		Fig. 11.3.5.5 Add labels and units to X and Y axes and to color bars. Label graphs with "DJF" and "JJA" or "winter" and "summer," just to help reader. Specify changes compared to what. [Melinda Marquis]	Dropped / changed

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11-1392	A	173:0		Fig. 11.3.5.5. Same comment as for 11.3.5.4. Additionally, the colors should be reversed so that red is drier. [Kevin Walsh]	Dropped / changed
11-1393	A	174:0		Fig. 11.3.5.6 Add labels and units to color bars. [Melinda Marquis]	Dropped / changed
11-1394	A	175:0		Fig. 11.3.5.7 Add labels and units to color bars. Add period to last sentence of caption, inside close-parenthesis. [Melinda Marquis]	Dropped / changed
11-1395	A	176:0		Fig. 11.3.6 1 Add key to graphs, i.e., define colored shapes in graphs. [Melinda Marquis]	Dropped / changed
11-1396	A	176:0		Figure 11.3.6.1: The significance of illustrated is not indicated [Ibouraïma YABI]	Dropped / changed
11-1397	A	178:0	179:	Which GCMs? Improve quality. [Rasmus E. Benestad]	Dropped / changed
11-1398	A	178:0		Fig. 11.3.6.3 Add labels and units to X and Y axes and to color bars. Correct typo in caption: observation. [Melinda Marquis]	Dropped / changed
11-1399	A	178:0		Figure 11.3.6.3: The period of projection is not specified in the title [Ibouraïma YABI]	Dropped / changed
11-1400	A	179:0		Fig. 11.3.6.4 Add labels and units to X and Y axes and to color bars. [Melinda Marquis]	Dropped / changed
11-1401	A	179:0		Figure 11.3.6.4 b: The period of projection is not specified in the title [Ibouraïma YABI]	Dropped / changed
11-1402	A	180:0		Necessary? [Rasmus E. Benestad]	Dropped / changed
11-1403	A	180:0		Fig. 11.3.6.5 Add labels and units to X and Y axes. [Melinda Marquis]	Dropped / changed
11-1404	A	181:0		Which GCMs? [Rasmus E. Benestad]	Dropped / changed
11-1405	A	181:0		Fig. 11.3.6.6 Add key to graphs, i.e., define colored shapes in graphs. Consider labeling X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	Dropped / changed
11-1406	A	181:0		Figure 11.3.6.6: To specify the meaning of illustrated I suggest that you should write J F M. A M. J J A S O N D instead of 1 2 3 4 5 6 7 8 9 10 11 12	Dropped / changed

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				[Ibouraïma YABI]	
11-1407	A	181:5		Replace "experiments" by "simulations" [Vincent Gray]	Dropped / changed
11-1408	A	182:0		What ensemble? GCMs? [Rasmus E. Benestad]	Dropped / changed
11-1409	A	182:0		Fig. 11.3.6.7 Add labels and units to X and Y axes and to color bars. Add labels a, b, c and d to graphs. Add to caption change from what (time period). [Melinda Marquis]	Dropped / changed
11-1410	A	183:0		Fig. 11.3.6.8 Add key to graphs, i.e., define colored shapes in graphs. [Melinda Marquis]	Fixed
11-1411	A	183:0		Figure 11.3.6.8: To specify the period of projection To specify meaning of illustrated [Ibouraïma YABI]	Fixed
11-1412	A	184:0		Fig. 11.3.6.9 Add key to graphs, i.e., define colored shapes in graphs. Consider labeling X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	Dropped / changed
11-1413	A	184:0		Figure 11.3.6.9: To specify significance of figurative [Ibouraïma YABI]	Dropped / changed
11-1414	A	185:0		Fig. 11.3.6.10 Add labels and units to X and Y axes. [Melinda Marquis]	Figure altered
11-1415	A	185:0		Figure 11.3.6.10: To specify the period of projection [Ibouraïma YABI]	Figure altered
11-1416	A	186:0		Fig. 11.3.6.11 Add labels and units to X and Y axes and to color bars. In last sentence of caption, after "(left)" and "(right") to respectively descriptions. [Melinda Marquis]	Figure altered
11-1417	A	186:0		Figure 11.3.6.11: I suggest that the periods should be 2081-2100 and 1981-2000 [Ibouraïma YABI]	Figure altered
11-1418	A	186:5		Replace "robustness" by "differences" [Vincent Gray]	Disagree
11-1419	A	187:0		Improve quality? (Necessary?) [Rasmus E. Benestad]	Figure altered
11-1420	A	187:0		Fig. 11.3.7.1 Add labels and units to X and Y axes and to color bars. [Melinda Marquis]	Figure now deleted

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11-1421	A	187:0		Figure 11.3.7.1: I suggest that the periods should be 1981-2000 and 2081-2100 [Ibouraïma YABI]	
11-1422	A	188:0	189:	Which GCMs? [Rasmus E. Benestad]	As stated, all AR4 runs
11-1423	A	188:0		Fig. 11.3.7.2 Add labels and units to X and Y axes and to color bars. Label graphs with "DJF," "JJA" and "Ann" (or "winter," "summer" and "annual" just to help reader. [Melinda Marquis]	Figures have been redrafted
11-1424	A	188:0		Figure 11.3.7.2: I suggest that the periods should be 1981-2000 and 2081-2100 The figures of the ordinate and x-axes are not quite readable [Ibouraïma YABI]	Figure has been redrafted
11-1425	A	190:0		Improve quality? (Font) [Rasmus E. Benestad]	Figure has been redrafted
11-1426	A	190:0		Fig. 11.3.7.4 Add labels and units to X and Y axes and to color bars. Does color bar need to extend from +10 to -10? If not, consider giving clear bar, with higher resolution of the color range (and corresponding values) actually in the corresponding graph. [Melinda Marquis]	Figure has been redrafted
11-1427	A	190:0		Fig. 11.3.7.4. It is not clear from the text or the captions which time periods this projection refers to. [Kevin Walsh]	Chnage made
11-1428	A	190:0		Figure 11.3.7.4: To specify the period of projection [Ibouraïma YABI]	Change made
11-1429	A	191:0		Fig. 11.3.7.5 Consider clarifying text in caption to help reader understand graph. [Melinda Marquis]	Change made
11-1430	A	191:0		Fig. 11.3.7.5. It is not clear from the text or the captions which time periods this projection refers to. [Kevin Walsh]	Change made
11-1431	A	191:0		Figure 11.3.7.5.: To specify the period of projection [Ibouraïma YABI]	Change made
11-1432	A	192:0		Which GCMs? [Rasmus E. Benestad]	Fized
11-1433	A	192:0		Fig. 11.3.8.1 Add key to graphs, i.e., define colored shapes in graphs. Consider labeling X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	Fixed
11-1434	A	192:0		Figure 11.3.8.1: I suggest that you should write J F M A M J J A S O N D instead of 1 2	Figure redrafted

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				3 4 5 6 7 8 9 10 11 12 I suggest that the periods are 1981-2000 and 2081-2100 [Ibouraïma YABI]	
11-1435	A	193:0	194:	Enlarge! (which GCMs?) [Rasmus E. Benestad]	Fixed
11-1436	A	193:0		Fig. 11.3.8.2 Add labels and units to color bars. [Melinda Marquis]	Dropped / changed
11-1437	A	194:0		Fig. 11.3.8.3 Add labels and units to color bars. [Melinda Marquis]	Dropped / changed
11-1438	A	195:0		Not legible. Improve quality. Which GCMs? [Rasmus E. Benestad]	Dropped / changed
11-1439	A	195:0		Figure 11.3.8.4: The legends are very unreadable after printing [Ibouraïma YABI]	Dropped / changed
11-1440	A	196:0		Enlarge! (which GCMs?) [Rasmus E. Benestad]	Fixed
11-1441	A	196:0		Fig. 11.3.8.5 Add labels and units to color bars. [Melinda Marquis]	Fixed
11-1442	A	197:0	201:	Which GCMs? (legends not clear, and should probably be rounded off to two digits) [Rasmus E. Benestad]	Dropped / changed
11-1443	A	197:0		Fig. 11.3.8.6 Add key to graphs, i.e., define colored shapes in graphs. Consider labeling X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	Dropped / changed
11-1444	A	197:0		Figure 11.3.8.6: I suggest that the periods should be 1981-2000 and 2081-2100 The legends are unreadable after printing [Ibouraïma YABI]	Dropped / changed
11-1445	A	198:0		Fig. 11.3.9.1 Add key to graphs, i.e., define colored shapes in graphs. [Melinda Marquis]	Dropped / changed
11-1446	A	198:0		Figure 11.3.9.1: I suggest that the periods should be 1981-2000 and 2081-2100 The legends are unreadable after printing [Ibouraïma YABI]	Dropped / changed
11-1447	A	199:0		Fig. 11.3.9.2 Add key to graphs, i.e., define colored shapes in graphs. Consider labeling	Dropped / changed

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				X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	
11-1448	A	199:0		Figure 11.3.9.2: The period of projection is not indicated The meaning figurate is not indicated in the legend [Ibouraïma YABI]	Dropped / changed
11-1449	A	200:0		Fig. 11.3.9.3 Add key to graphs, i.e., define colored shapes in graphs. Consider labeling X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	Dropped / changed
11-1450	A	200:0		Figure 11.3.9.3.: The period of projection is not indicated the meaning figurate is not indicated in the legend [Ibouraïma YABI]	Dropped / changed
11-1451	A	201:0		Fig. 11.3.9.4Add key to graphs, i.e., define colored shapes in graphs. Consider labeling X axis with names of months (e.g., perhaps even or odd-numbered months only), rather than using numbers. [Melinda Marquis]	Fixed
11-1452	A	201:0		Figure 11.3.9.4.: The period of projection is not indicated The meaning figurate is not indicated in the legend [Ibouraïma YABI]	Fixed
11-1453	A	202:0		Fig. 11.3.9.5 Add labels and units to X and Y axes and to color bars. [Melinda Marquis]	Fixed
11-1454	A	202:0		Figure 11.3.9.5.: The period of projection is not indicated [Ibouraïma YABI]	Fixed
11-1455	A	203:0		Box 11.1, Fig. 1 missing [Melinda Marquis]	Done
11-1456	A	203:1		Figure 1 is missing [Marina Baldi]	Done
11-1457	A	204:0		Box 11.2, Fig. 1 Consider clarfiy graph in caption,e.g., do different shades have different meanings? Need color to understand? [Melinda Marquis]	Reworked
11-1458	A	205:0		Box 11.4, Fig. 1 Add labels and units to X and Y axes and to color bars. In caption, insert "in" after "Changes."	Fixed

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				[Melinda Marquis]	