IPCC Working Group I Fourth Assessment Report

Expert Review Comments on First-Order Draft

Chapter 5

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-1	A	0:0		While the material presented on the rise in sea level is clear, focused, and relevant, the section devoted to trends in heat and salt content (5.2) is somewhat incomplete and much of the material presented on section 5.3 seems only tangentially related to global warming. [Michael Alexander Alexander]	Rejected: It is unclear what the reviewer means by "the section devoted to heat and salt content (5.2) is somewhat incomplete". Noted: c.f. 5.3: global "warming" is not the only form of anthropogenic climate change
5-2	A	0:0		Excellent chapter-no comments [Richard Anthes]	Thank you.
5-3	A	0:0		The favored teminology for describing global ocean circulation has changed from thermohaline circulation to meridonal ocean circulation. An explanation of the difference between these two terms and why the change has been made would help non-expert readers. Also, consistent terminology should be used through the chapter. At times (e.g. Box 5.1) the term thermohaline circulation is reintroduced. [Lenny Bernstein]	Accept
5-4	A	0:0		Chapter 5 is clear and supports the other two observation chapters. [Roxana Bojariu]	Noted
5-5	A	0:0		Chapter 5 describes in some detail the long-term changes in ocean heat content, salinity and sea level. Heat content and sea level are certainly rising and this is the first IPCC report where the trends are reliably estimated. Salinity is decreasing but the trend is less convincing due to uncertainties in historical salinity data. Also considered are changes in regional ocean circulation, carbon, oxygen and pH. The Chapter is much improved over the earlier version I read last Spring, especially the referencing seemed complete and accurate. [Harry Bryden]	Noted
5-6	A	0:0		A paper was recently published (one day before this reviewer's comments were supposed to be sent to the TSU) in Nature (Church et al. 2005 Nature 438, 3 November 2005, doi:10.1038/nature04237). This paper shows a significant decadal-scale impact of volcanic eruptions (e.g., Pinatubo 1991) on sea level and ocean heat content. Thus, observed increases in sea level may be partly caused by recovering processes after cooling induced by eruptions. This new findings must be commented in this chapter. [Paolo Cherubini]	Noted, paper is referenced
5-7	A	0:0		This chapter makes clear that there have been some very important changes in the world's oceans over the last several decades. Congratulations to the authors! However, I also have some significant concerns about the comprensiveness of the chapter, which are revealed in an examination of the outline. In particular, I was surprised to find no	noted: Patterns of variability are mentioned, now cross-referenced to chapter 3. Surface waves are covered in Section

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				sections on major ocean patterns on climate variability (eg ENSO and its decadal variability/change, monsoons and their decadal variability/change, NAO, SAM etc) and nothing on surface waves. While the climate variability issues are briefly touched on in Section 5.3 and may be partially covered elsewhere in the AR4, there should be an oceanic perspective on these issues. This should address not just the changes in the ocean but the interaction of the ocean as part of the climate system. By way of example, a key issue for Australia is "has and will climate change affect ENSO?" Also, some of the individual sections are not comprehensive and do not adeqautely "assess" the literature, as distinct from compiling the literature. [John Church]	3.5.6 of Chapter 3.
5-8	A	0:0		I am glad to see that there is mention of a consensus that GSLR is nearer 2 mm per year rather than 1, although Plag, Tamisiea, Mitrovica, and others might disagree that such a consensus exists, or is correct. Surely they deserve some space. Unfortunately, the chapter also does not mention issues related to J2-dot. I also felt that the Wadhams and Munk results deserved more respect than they received. Finally, I believe that the Church et all paper may not be definitive, even though I like the results. For example, the chapter says that the altimeter record is not long enough yet to analyze decadal variability (a true statement), but oddly implicitly endorses mixing altimeter and tide gauge data (as done by Church et al) with purely statistical methods with the claim that the modes of variability are adequately captured by the altimeter data. Westwardpropagating Rossby waves are one example of the disconnect that exists between mid ocean and coastal sea level. In fact, mid-ocean decadal variability does not at all resemble what tide gauges see except in special cases or perhaps very long (100 years) time scales (see figure below). A data assimilation that had physics in it would seem to be the only reasonable way to combine a decade or two of altimeter and tide gauge data. [Bruce Douglas]	Rejected: Wadhams & Munk work is based on the salinity anomaly fields of Boyer et al. (1998). Antonov et al (2002) made estimates of the addition of freshwater to the world ocean based on these salinity anomaly fields but noted that: a) there is far less salinity data than temperature data and that the estimates of global haline expansion and the mass change component of sea level change based on these estimates are uncertain. While publishing these results in the literature will stimulate work on the importance of these contributions to sea level change, the present results are not authoritative enough for this Assessment. The point that the Reviewer is trying to make with the sentence "Westward propagating Rossby waves."is not clear. In fact, Hong & Sturges have shown such a connection in a series of papers. The Reviewer asserts that a data assimilation model that "had physics in it" would be a reasonable way to combine 1-2 decades of altimeter data

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					is interesting. However, this depends on how well the model and its physics simulate observations. The fact that there are still practical issues of how models and model assimilations conserve mass at present makes interpretation of results difficult.
5-9	A	0:0		A larger problem deals with the overall emphasis on "Ocean Climate Change." The authors apparently think that there is enough global hydro data to make accurate statements about the global ocean on a 50-year time scale without resort to a data assimilation process that has physics in it. Based on my knowledge of the distribution of hydrographic profiles of data, especially CTD and bottle data, I would be reluctant to say anything definitive about the southern Hemisphere and some large portions of the northern hemisphere as well. I am also not impressed that Levitus and Ishii and Willis get the same results. They are after all using the same database. BTW, why the emphasis on the last 50 years? If we are going to say 2 mm per since 1950, isn't there also enough evidence to say that 2 mm per year applies to the entire 20 th century? [Bruce Douglas]	Rejected: The Reviewer states that he does not think there is enough ocean profile data to make estimates about the ocean without resorting to an assimilation model. The collection of historical and modern ocean profiles continues to increase. Our estimates of various ocean climate variables is a best estimate at this time based solely on data and certainly it has biases due to sampling. It is not obvious at all that an assimilation can do better. In any event we believe it is important to have estimates of ocean variability that are completely independent of any model with its associated biases. Comparison of both sets of results provides a guide to improving both methods. The Reviewer has contradicted himself. In Comment 5.8 he wants to see Wadhams & Munk receive "more respect" for their work, however, in Comment 5.9 he notes a lack of ocean profile data supporting the observational results discussed in this chapter. The work of Wadhams & Munk is based on the work of Boyer et al. (2005) which is based on the hydrographic data used by Boyer et al.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					(2005.) We agree as noted above that estimates of salinity variability and freshwater variability estimated from the salinity changes are very uncertain. The emphasis on the past 50 years is due to the lack of data for the first 50 years of the 20th century. This is true for both tide gauge data and ocean profile data. Holgate & Woodworth sea level work provided estimates of sea level variability for the past 50 years based on 177 records averaged over 13 regions. Douglas (2001) estimated sea level from tide gauges for the past 70 years using 27 records averaged over 10 regions. The fact that both estimates agree (1.7 & 1.8 mm/yr respectively) provides more certainty for this Assessment.
5-10	A	0:0		I also think that the downgrading of global sea level rise to a lower status is a disservice. SLR is probably the first manifestation of climate change that will affect people, and thus deserves its own section. The authors have chosen instead to write a Phys O text which policy makers won't understand (they have to go to another section to see why sea level rise is important) and students will find misleading. [Bruce Douglas]	Editorial decision
5-11	A	0:0		One last thing; reading the chapter you would think that regional decadal variability of sea level was discovered recently! I have been pounding on this problem (see figure below - TSU Note: Figures are located in a separate supplemental pdf) since my 1991 JGR paper "Global Sea Level Rise." It is nice to see variability and the need for long records finally accepted, but a reference at least to my book would have been nice. Other well known authors are also left out or at least underrepresented. Dick Peltier's contributions are very inadequately acknowledged, for example. Failure to acknowledge contributions by all scientists who have made significant contributions does not contribute to much-needed collegiality. [Bruce Douglas]	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-12	A	0:0		TSU NOTE: Please see supplementary review material [Bruce Douglas]	Noted
5-13	A	0:0		- [Savitri GARIVAIT]	
5-14	A	0:0		It is surely obligatory to quote accuracies in terms of a 95% chance of inclusion. This means that you must use TWICE the standard error as a measure of accuracy. This procedure has been adopted in Chapter 3 (see page 3_7 lines 14 to 24).and it should be a standard procedure of the IPCC. You must adjust all you figures accordingly. You should provide a suitable introductory statement to explain what you have done: as follows . "Following standard procedure, all uncertainty figures in this Chapter are twice the standard error, giving 95% confidence" [Vincent Gray]	Accepted: We will provide the requested +/- 2 S.E. values. We will be recomputing some of our estimates of the confidence intervals with a different computational procedure.
5-15	A	0:0		The chapter gives a great review on changes in heat content, fresh water and sea level. I think the authors did a good job in general. I am glad that regional differences are emphasized. However, many regional patterns are clearly associated with either the PDO or the NAO and this could be mentioned more prominantly with cross- references to other chapters (especially the heat content and sea level changes show the signatures). The chapter is a bit inconsistent on the description of the basins. In particular, the Indian Ocean is described from the viewpoint of variability and coupled processes. That is less the case for other parts which are more on the observations only. There is sometimes an overinterpretation using model results (in particular with respect to NAO forcing). There is hardly mentioning of long time series of transport, while some are of relevance (Florida Current, 11 S in the Atlantic, Faroer bank overflow, the slowdown of the subpolar gyre). Also, the synthesis between the biogeochemistry and physics is not very strong. The impact of physical changes on the solubility pump can be better assessed. I would start the chapter with Figure 5.6.1 instead of finishing with it. Finally, the South Atlantic is not treated at all and the tropical Atlantic is treated marginally. [Wilco Hazeleger]	noted: Fixed Atlantic with more on the tropics. Fixed Indian – material balanced to match other basins. Taken into account: tighter link between change in heat and carbon
5-16	A	0:0		New information (brief update) on SST trends would be useful because of its importance for impacts such as coral and pelagic flora and fauna that live in the upper waters, e.g. Lough on SST trends in the Great Barrier Reef [Roger Jones]	noted
5-17	A	0:0		Thank you for providing this rich overview on oceanic observations. [Fortunat Joos]	Noted
5-18	A	0:0		This chapter misses the opportunity to discuss ventilation time scales of the thermocline and the deep ocean as evidenced from CFC-11 and CFC-12, bomb- and natural-produced radiocarbon or Argon39 data. Such a discussion would be very important to demonstrate	Accepted, CFC-uptake discussed in synthesis section.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				that the rate by which the ocean is taking up heat and carbon and other tracers is well constrained by these tracer observations. The ventilation time scale is important to constrain the transient climate sensitivity. The ventilation time scale tracers are heavily used in the biogeochemical modeling community to evaluate or tune carbon cycle models (Oeschger et al., Tellus, 1975; OCMIP). There is also an increasing amount of GCMs, used in chapter 10 , that is evaluated with respect to these tracers. The GLODAP data-base at CDIAC provides global compilations (Key et al, GBC, 2004). I strongly suggest that a quantitative discussion is added here to clearly demonstrates that it takes indeed decades to many centuries to transport heat and carbon into the thermocline and the deep ocean. Figures (lat-depth) of CFCs and C14 should be included in this chapter. The ventilation time scales are important for the broad issue of inertia in the climate -socio economic system and needs in my opinion simply be addressed from a data perspective. [Fortunat Joos]	
5-19	A	0:0		TSU NOTE: Please see supplementary review material [Terry Joyce]	Noted, text rewritten/shortened
5-20	A	0:0		This chapter draws heavily on estimates of sea level drawn from ocean heat content records as if they were almost exact. Yet other chapters dismiss, for example, the variability of this record since it is difficult to simulate with models. There appear to be outstanding questions about the uncertainty of estimates of total ocean heat content that are only briefly mentioned in an appendix to the chapter. For example, the Gregory et al. (2004) analysis seems to question primarily the net 50-year heat uptake, not the variability as stated in the appendix. And Levitus (2005) confirms the magnitude of variability. Suggest careful assessment should be carried out on the record of ocean heat content in this chapter, and this assessment used uniformly throughout the report. [Haroon Kheshgi]	Climate-system models do simulate the 50-year trend in ocean heat content (contrary to what the reviewer states) but indeed do not simulate the observed interdecadal variability of ocean heat content. The decadal variability as derived from observations is now discussed in more detail in the text. In addition these models can not simulate the altimeter record of sea level which shows large changes in sea level during the 1997-98 El Nino (+/- 15 mm) which most likely has a large component due to thermal expansion as noted by Levitus et al. (2005, the EOF paper).
5-21	A	0:0		I can not reach the paper Millot et al, 2005 mentioned down, it seems that is has been only submitted, not accepted jet. I think it should be finally admit only papers accepted for publication. Just submitted papers can not be checked as the one cited here or at least, as most of the cited in the document, they should mention the publication where it has been submitted.	Noted. Only accepted papers will be included.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Millot, C., J.-L. Fuda, J. Candela, and Y. Tber, 2005: Warming and salting of the Mediterranean outflow due to shifts in dense water formation zones. submitted. [ALICIA LAVIN]	
5-22	A	0:0		It is understandable and useful to base the report on a particularly deep article (Levitus et al, 2005); unfortunately, it only covers the post 1955 period. This leaves critical information out to be able to more clearly discuss the very low frequency of variation (the "regime" variation) that has a period of ~60 years. It does not include the 1940s regime shift, for instance. I feel it would have been very informative to include some other source (there are several articles) to complement the full picture. [Daniel Lluch-Belda]	Rejected: We do not have enough ocean temperature profile data to make ocean heat content estimates for the pre-1955 period. It would have been helpful if the reviewer had listed the papers he is referring to.
5-23	A	0:0		I found this chapter to be well written, with a sound quantitative focus on secular trends in the ocean on questions of great relevance, especially concerning the magnitudes of long and near-term trends in global ocean temperature, of anthropogenic carbon dioxide uptake by the oceans, and of global sea level. It is gratifying to see the impressive quality and magnitude of the effort required to obtain quantitative evaluation of the long-term trends(and acceleration of the trends) of these quantities. These trends show impressively high-quality diagnostic work, especially when the quantities are globally averaged. There may be concern that some of the quantities measured exhibit surprisingly counter-intuitive results. My personal concern is that the exhaustive computations of temperature measurements in the upper 700 m of the world ocean may be subject to significant aliasing errors due to unfortunate horizontal measurement gaps. Unfortunately, substituting ther straightforward Levitus, et al., 2005 approach to data averaging with different data sampling approaches offers little change in the oddly counter-intuitive 0-700m ocean heat content in the time series "bump" from roughly 1970-1985(Fig. 5.2.4). I was gratified to see the the Levitus, et al, 2005 "global averaging" technique stand up to subsequentindependent analyses of his data. I can only speculate that the (comparatively) more data rich area of the North Pacific Ocean somehow contributed to aliasing the global-averaging process. If there is a solid, physically plausibe explanation for this 1970-1985 "bump" in the 0-700 M global-mean temperature, it should be highlighted in the Chapter 5 Executive Summary, Item No. 3, as a major challenge that has not yet been fully solved. If that cannot be achieved, then Item No. 3 in the Executive Summary needs a far more convincing explanaton than is offered there, or in the body of the text. (I am betting that major PDO events in the N. Pacific over that period led to this very counter-intuitive change in the "global 0-700m" average"). Clearly, the authors involved with this section need to address this from a physical/dynamical vertical transport perspective to create more plausible hypotheses than have been stated in this Chapter 5 first-order	Noted: We have identified the "counterintuitive" heat content variability with the PDO in two papers (Stephens et al. (2001) and Levitus et al. (2005). We argue for the reality of this result as follows: 1) Our yearly estimates of ocean heat content are independent of each other except that all data regardless of year have been used to compute the climatologies of temperature used to compute the anomalies. Plotting the "odd" and "even" years as two separate curves shows similar interdecadal variability (I will send this figure before the next LA meeting or bring it with me) . We plan to compute two separate climatologies, one based on data for all "odd-numbered" years and one based on data from all even-numbered years. This will give use 2 completely independent estimates of ocean heat content. 2) note that the altimeter record also

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				draft, or in previous publications. [Jerry Mahlman]	shows similar "counterintuitive" variability during the 1997-98 El Nino when sea level rises by ~15mm during 1997 and then decreases by a similar amount in 1998. These changes can only be due to thermosteric, and/or mass change and halosteric factors. These results (ocean heat content and altimeter sea level) strongly indicate that internal variability of earth's climate system can result in temporary imbalances of earth's radiation balance at the top-of-the-atmosphere. The reviewer seems to be unaware of these results, perhaps by not reading the original papers. We need to emphasize this more.
5-24	A	0:0		There still seem to me to be some boundary problems in the coverage of the carbon cycle across chapters 2, 5 and 7 and the cross-referencing between chapters is not right in some places. Clearly some people have worked very hard on the carbon cycle material and a lot of progress has been made since the ZOD but I feel that the coverage could be made more complete and easier to follow if some structural adjustments were considered by the authors of all 3 chapters. At present chapter 7 seems to be left to do too much and this defeats the aim of emphasizing the broad importance of the carbon cycle by having it covered in all three chapters from different perspectives. The original plan, in the "Notes to LAs" distributed at LA1, was that Ch02 would cover the relationship between (observed) changes in atmospheric composition and emissions. At the moment both Ch02 and Ch07 seem to be doing this and we have two rather different presentations of fossil fuel emissions and CO2 increases in both text and figures, and both sets of figures could do with improvement. The best way of doing that would probably be to pool our resources and make cleaner decisions on what goes where. Similarly the "Notes to LAs" planned for air-sea fluxes and their changes over time inferred from observations to be covered in Ch05. The boundary issues in this case may be more arbitrary and perhaps require a pragmatic rather than theoretical approach. But I miss seeing a map of delta-pCO2 or inferred air-sea fluxes in Ch05 that would illuminate say the TRANSCOM3 results on the latitudinal distribution of ocean fluxes in Ch07. The information in Ch05 on	Noted, the distribution of carbon cycle material on different chapters is editorial decision.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				change over time in the ocean uptake fraction is left dangling at present and begs the obvious question: Is this consistent (at least to an order of magnitude) with model results? Can that question be picked up in Ch07? For pragmatic reasons of length and structure I would argue that the material on carbonate chemistry and pH (acidification) would fit better in Ch05 - if necessary with changes to the contributing author list. That still leaves the bulk of material on the carbon cycle in Ch07 but it could then become more tightly focused around: the big picture of different reservoirs and time scales; the terrestrial carbon cycle which is so heavily influenced by climate variability and change; and all the inverse and coupled modelling work. [Martin Manning]	
5-25	A	0:0		I have read Chapter 5: Observations: Oceanic Climate Change and Sea Level and find it a much better report than the comparable chapters in the TAR, but still seriously lacking in several important respects. [Laury Miller]	noted
5-26	A	0:0		The introductory remarks point to several messy problems posed by the TAR, but nowhere is there a clear summary of how and to what extent these have been resolved by the FAR. For example, the TAR accepted a wide range for the 20th century rate of sea level rise (SLR), 1 to 2 mm/yr, but now the consensus seems to be much closer to 2 mm/yr than 1 mm/yr. How did this happen? The recent papers by Holgate & Woodworth (2004) and Church et al (2004) are helpful but neither of these provides the kind of break-through incite that would move everyone towards a consensus value. There needs to be a much clearer explanation why the TAR range was so wide and how it is that we've arrived at ~2 mm/yr for the 20th century rate. [Laury Miller]	Papers by Holgate & Woodworth (2004) and Church et al. (2004) are mostly studies of sea level observations and concluded that the 20th century (actually 50+ years) sea level rise is on the order of 2 mm/yr. The 'better' consensus towards the 2 mm/yr (over the last 50 years or last century) is due primarily to the fact of improved confidence in studies for a number of geophysical causes of sea level rise, notably thermosteric (and to a lesser extent halosteric) effect, recent glacier and Greenland ice sheet margin melts (Ch. 4), etc (see also 5.5.6 on sea level budget)
5-27	A	0:0		I'm glad that the contribution of satellite altimetry has been given a lot of attention. The fact that altimeter observations from 1993 to the present show global sea level rising almost steadily at a rate of 2.8 +/-0.4 mm/yr, roughly 50% greater that the "consensus" 20th century rate, has really pushed a number of issues to the forefront. In particular, it's now clear that the Southern Ocean can experience very large rates of sea level rise on decadal time scales, large enough to dominate estimates of the global mean. This poses a big problem for studies claiming to measure global steric sea level trends from	Noted: The papers referred to in Chapter 5 acknowledge the lack of data in the S. Hemisphere. Until more historical and modern data are available it is difficult to assess how much of the signal we might be missing due to possible inadequate sampling. The fact

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				hydrographic observations (Table 5.5.1). Prior to Argo (2001) there is simply not enough in-situ data in the Southern Ocean to compute reliable trends. This applies to all of the studies cited in Table 5.5.1. There needs to be a clear statement indicating the limitations of these studies in comparison to the altimetric analyses. [Laury Miller]	that the altimetric sea level rate (actually 3 mm/yr after correcting of geoid change due to GIA) being 50% larger than the global rate (2 mm/yr) could also due to the fact that the data span of satellite altimetry (1 decade) is too short to separate sea level trend and decadal (or longer) variations. This comment is stated in the Executive Summary and elsewhere in Ch. 5
5-28	A	0:0		Finally, I'd like express my disappointment that global sea level rise has been combined with other ocean climate issues in one chapter. While the all of these processes are interconnected, burying GSLR inside a large chapter makes it difficult for a non-scientist to see the essence of the debate. The New Yorker magazine recently carried a cover depicting the tall buildings of New York City submerged underwater with fish swimming around. As far as the public is concerned, global sea level rise is THE ocean climate issue. The FAR should reflect the public's concern. [Laury Miller]	Editorial decision
5-29	A	0:0		This chapter is full of terrific information and will be very helpful. Much of it is easy to understand and to follow. However, I think the chapter is too long for the material it contains, and this makes reading it quite a task, especially since the style is (as discussed in some examples above) rather ponderous. Too many "blow-by-blow" descriptions instead of assessments. The chapter can be reduced substantially and the flow of the material can be improved a lot, just by editing. And try to reduce overlaps and duplications with Chapter 3, Chapter 4 and Chapter 9. [Neville Nicholls]	Noted.
5-30	A	0:0		I found this a good and useful chapter, especiallt with reference to my coastal interests [Robert Nicholls]	noted
5-31	A	0:0		This is a good and instructive Chapter. My specific comments are mainly directed to minor details. Consistency with Chapter 4 and in expression of error bars need to be assured. [David Parker]	noted
5-32	A	0:0		The chapter is primarily a catalogue of evidence that the oceans have changed, on a range of time-scales. Given the short and gappy record of ocean observations, this is ultimately	Noted: The reviewer makes some good points but we need to document

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				not very satisfying, as it is generally not possible to distinguish climate variability from climate change. I think readers will have trouble wading through this section and coming away with a clear view of what is climate change and what is climate variability, of which changes are most significant, and of the candidate physical explanations for the observed changes. I also believe the chapter should devote more space to a state-of-the-art assessment of the role of the ocean in the climate system (ie rather than just summarise evidence of ocean changes, most of which are difficult to interpret given the short records). I think the chapter as it stands is too much of a scientific review of the literature, rather than a synthesis of policy-relevant information (noting the instructions to reviewers that the chapters "must present a concise assessment of current knowledge, not a scientific review of how that knowledge was derived. The report focuses on policy-relevant aspects of climate change". The (very brief) section on ocean climate processes in the TAR did a good job of summarising the important issues and uncertainties; in particular, the use of brief summary paragraphs at the end of each section was very effective and might be a model to be adopted here. [Stephen R Rintoul]	observed variability based on available data and do the best we can. There is excellent agreement between the 50-year record of ocean heat content and several AOGCMs and we strongly believe this is a major step forward in advancing the issue of "detection and attribution of climate change".
5-33	A	0:0		The chapter is very northern hemisphere focused. The discussion of the thermohaline circulation equates this with the sinking of North Atlantic Deep Water. This NADW-centric view of the global overturning circulation is a decade out of date. Sinking of dense water in the southern hemisphere is of comparable magnitude to that in the northern hemisphere (eg Orsi et al. 2002); the southern hemisphere overturning is also sensitive to climate change, according to a variety of climate models; paleoclimate evidence suggests the sinking of dense water in the northern and southern hemispheres is linked, in complex ways (eg Knutti et al., Nature, 2004); and perhaps most importantly, there is growing evidence that the Southern Ocean closes the NADW overturning circulation by recycling upwelled deep water into mode and intermediate water (as discussed briefly on p. 5-18). Other specific examples of an apparent northern hemisphere emphasis are noted on the relevant pages. [Stephen R Rintoul]	Noted. Discussion of Southern Hemisphere issues is strengthened in text.
5-34	A	0:0		General comments: good balance between 1) ocean heat and salt content 2) ocean biogeochemical changes 3) sea level changes. Conclusions for 1) and 2) are clear and show the 'maturity' of underlying scientific achievements. However, I found the conclusions of point 3) rather unclear. Maybe the IPCC panel wants to send the message that this research area deserves much more effort in the future? [Michel Rixen]	Noted: We do not understand the reviewer's point "I found the conclusions of point 3 rather unclear. Maybe the IPCC panel wants to send the message that this research area deserves much more effort in the future?" It seems to me we were making a strong statement. Of course

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					this area (ocean heat content) should be further developed.
5-35	A	0:0		The chapter does not flow well with each paragraph and topic seeming to jump around in no particular order. For example 5.2.3 is a short paragraph on Ocean Salinity within a large discussion of heat content issues. [Sheila Stark]	Noted: We are revising the order of these sections.
5-36	A	0:0		I was very impressed by the summary in this chapter in sections 5.2, 5.3, and 5.4 The authors did an incredible job in summarizing what we know about oceanic climate change. I hope that these authors will give chapter 3 a good read so that errors in that chapter will not appear in the final document. [LuAnne Thompson]	Noted
5-37	A	0:0		General comments: There appear to be important inconsistencies in this chapter with Chapter 4 on effects of ice melt on sea level, and chapter 3 on atmospheric and SST changes. There are also major issues not dealt with in ocean observations on spatial and temporal sampling and biases that should be discussed especially as part of the error analysis. Some material is brought forward from publications without adequate assessment. There are multiple internal inconsistencies within the chapter on salinity and temperature and their effects on sea level. On salinity, meteorological literature relevant to rainfall and salinity is missing and there needs to be consistency with chapter 3 and the material in 5.2.3 should be integrated with that in 5.5.3. The thermo-steric changes are dealt with much better and more comprehensively in section 5.5.3 than in 5.2.2 and Figures 5.5.5 and 5.5.6 are much better than 5.2.4 because they deal with all available data which 5.2.2 fails to do. [Kevin Trenberth]	Some inconsistencies, in particular with chapter 4, have been eliminated, and all of the mentioned text parts have been rewritten, in particular the parts about salinity changes. However, some of the reviewer's comments are not supported by similar comments by other reviewers. The air-sea fluxes are part of chapter 3. --- REVIEWER: There are also major issues not dealt with in ocean observations on spatial and temporal sampling and biases that should be discussed especially as part of the error analysis. RESPONSE: What are the inconsistencies? --- REVIEWER: Some material is brought forward from publications without adequate assessment.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					RESPONSE: What materials and what do you mean by "adequate assessment"? --- REVIEWER: There are multiple internal inconsistencies within the chapter on salinity and temperature and their effects on sea level. RESPONSE: What are the inconsistencies? --- REVIEWER: On salinity, meteorological literature relevant to rainfall and salinity is missing and there needs to be consistency with chapter 3 and the material in 5.2.3 should be integrated with that in 5.5.3. RESPONSE: Rainfall estimates as well as E-P estimates, air-sea heat flux estimates are notorious for large uncertainties. A comparison of all these will not be very informative for this assessment. This same reviewer in Comment No. 5-271 states that "There is no basis to believe the surface fluxes in the NCEP/NCAR dataset and there are many publications demonstrating that the are in fact not credible at al,." Boyer et al. (2002- "Harmonic analysis of climatological sea surface salinity" compares the climatological annual cycles of E-P and SSS. Clearly advection etc. is important in some regions. Not that relevant to this

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					assessment. --- REVIEWER: The thermo-steric changes are dealt with much better and more comprehensively in section 5.5.3 than in 5.2.2 and Figures 5.5.5 and 5.5.6 are much better than 5.2.4 because they deal with all available data which 5.2.2 fails to do. RESPONSE: Section 5.2.2 deals with ocean heat content. Although the thermosteric component of sea level change is closely related to ocean heat content they are not the same quantity.
5-38	A	0:0		Much of 5.2.2 should be eliminated and merged with the 5.5.3 material or the material in 5.5 on salinity and temperature should be dealt with much more completely in 5.2. The material related to the thermohaline circulation (especially box 5.1) seems inconsistent and incomplete and it is missing from Figure 5.6.1, which is the summary figure. Large differences in style exist across the chapter, and the sections on the Pacific and Indian oceans have a different approach than other sections by dealing less with the ocean and more with the surface and atmosphere, in ways that are often inconsistent with chapter 3, and they venture into attribution without adequate justification. There is considerable confusion over two items: one is the error bars which should be 95% and not rms, and the other is the GIA effect of -0.3 mm/yr but seems not to be accounted for in tables or figures. Major revisions are in order. [Kevin Trenberth]	Noted: We are revising these sections.Accept: Indian section rewritten. Noted: Pacific section not changed as much.
5-39	A	0:0		Thank you for doing all this hard work… Good luck with the revision. :) Mikis Tsimplis [Michael Tsimplis]	Noted
5-40	A	0:0		Excellent contribution to the overall assessment. [Philippe Tulkens]	Noted
5-41	A	0:0		It must be made clear ince in the chapter, and then stuck to, whether "sea level rise contributions", can include negative contributions. [David Vaughan]	Accepted, yes they can. Text changed to “SL change”
5-42	A	0:0		The topic of this chapter is one in which huge developments have taken place since the TAR, and it is good that this is reflected in a stand-alone ocean observations chapter for the first time. Overall I found the draft informative, relevant and easy to read. There were	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				a few places where the text had more of the feel of an oceanographic tutorial than an assessment of policy-relevant research. Given that this is the first time this chapter has appeared in an IPCC report, I think a certain amount of such background material is appropriate, but there are places where I thought such material could advantageously be shortened (I have indicated some in specific comments below). There were also a few places where some key papers were not discussed (raised in specific comments). [Richard Wood]	
5-43	A	0:0		In general, I like the idea of writing a chapter on changes in the ocean overall including sea level. However, I think the present draft needs some work to make it more readable. For example, it has rather a lot of hydrography and the amount of referencing is very uneven. I also liked including the oxygen and bio stuff, in principle, although I learned almost nothing from them about ocean change. [Philip Woodworth]	noted
5-44	A	0:0		Another general remark is that I was editor of special issue of Phil Trans containing a number of sea level papers, several of which were copied to Anny Cazenave in draft form. These are now all 'in press'. At the end of this email I'll list the contents of the volume in case others are of interest. (See Supplementary materials: CONTENTS OF IN PRESS PHIL TRANS SEA LEVEL VOLUME.) [Philip Woodworth]	noted
5-45	A	0:0		Figures - I think there are far too many figures, especially all the hydrography. Also 5.5.4 tells you nothing except that there is reasonable agreement. I like 5.27 which I haven't seen before. fig caption 5.5.10 line 2 - decades [Philip Woodworth]	Noted. Some figures have been removed, however 5.5.4 has been kept because of specific interest in small islands sea level change.
5-46	A	0:0		TSU NOTE: Please see supplementary review material [Philipe Woodworth]	noted
5-47	A	0:0		TSU NOTE: Please see supplementary review material [Walter Zenk]	Noted, reference included
5-48	A	1:8	1:13	The list of authors should be given with the full name of the people, as in Chapter 1 [Philippe Tulkens]	noted
5-49	A	1:35	1:35	Content for Box 5.1 is not necessary to be included in Table of Content [kyung-ryul Kim]	editorial decision
5-50	A	2:0	3:	Executive summary: there is nothing in the exec summary on salinity: a major oversight, especially given substantial observed trends and the summary figure. [Kevin Trenberth]	Accept: will be fixed
5-51	A	2:0		Item 1: Even so there are major gaps in space and time in the observations.	Noted, item removed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Kevin Trenberth]	
5-52	A	2:0		Item 3: There are "apparent" strong decadal variations in the heat content but these are not verified by sea level. It is not established that these are robust to sampling variations. Reproducibility is not an answer to this because the data are the same. [Kevin Trenberth]	Noted. The sea level estimates are also problematic before altimetry became available.
5-53	A	2:0		Item 7 should account also for observed changes in salinity and say why those do not constitute a short-term trend as suggested in Fig 5.6.1. [Kevin Trenberth]	Noted, salinity changes included in exec summ.
5-54	A	2:0		Item 8: surely it is very clear that the 1993 to 2003 sea level rise is much greater than the 20th century. What is not clear is whether it will be sustained: the language needs improvement. [Kevin Trenberth]	noted
5-55	A	2:1	3:18	EXECUTIVE SUMMARY In addition, to the mega question raised above, some other concerns are offered here as part of the Executive Summary: A. The writing tends to lack clarity for an Executive Summary. B. There is too much quoting of the data and too little examination of what the datasets are, or are not, telling us that is centrally relevant for the AR4 C. I found that unexplained jargon adds confusion to the stated conclusions, often which are not pinpointed to what regions/depths, or even the atmosphere are even being discussed. D. For the WG1 Chapters 1-5, this Chapter is the most intellectually straightforward. But, the writeup and the explanations are sometimes quite confusing, and often do not speak directly to the quality of the data and their analyses. Thus, the IPCC Assessment-relevant punchline messages are lost in the cryptic wording, especially in the attempts to "tease" the punchlines out of the less-than-definitve data sets. The problems with the data are usually not the fault of the analysts, but their interpretive insufficiencies, or confusing implications, can lead informed, but not expert, readers into serious confusion. I thus recommend that the Executive Summary be re-evaluated with the goal of speaking more clearly and credibly to the intended goals of this Chapter 5: Observations chapter. [Jerry Mahlman]	Noted, will be rewritten
5-56	A	2:1		The length and level of detail of the Executive Summary of this chapter is fine, in contrast to some other chapters. [Gerrit Burgers]	Noted
5-57	A	2:3	2:16	A paper on thermohaline circulation (THC) variations, the AMO and climate variations already in the reference list should probably be referenced here. The fully published ref is:	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Knight, J., Allan R.J., Folland, C.K., Vellinga, M. and M.E. Mann, 2005: Natural Variations in the thermohaline circulation and future surface temperature. Geophys. Res. Lett., 32, L20708, doi: 1029/2005GL024233. This section should also cross refer to Chapter 3 where the THC also gets discussed. [Chris Folland]	
5-58	A	2:3		Executive Summary needs more synthesis statements, conveying the main messages. Most of the summary describes details of changes, with little interpretation or comment on the significance of the result. A non-expert reader will not know what to make of much of the following text. [Stephen R Rintoul]	Noted
5-59	A	2:5	2:5	Sea level change should be explained clearly here as in the full text [Michel Boko]	Not accepted, is explained in section 5.5
5-60	A	2:7	2:12	The rate of heating for the oceans is given here as 0.28 W/sq. M, but as 0.2 W/sq. M on Pg. 5, line 41. One of these numbers is in error. Also, this text gives an estimate of global ocean content, but Pg 5, line 39 indicates that the estimate is for the 0 – 3000 m layer. [Lenny Bernstein]	accepted, number changed
5-61	A	2:7		Is this "since AT LEAST 1955"? [Neville Nicholls]	Noted
5-62	A	2:8		give error estimates [John Church]	Noted
5-63	A	2:9	2:9	Clarify "0.037 C per decade" [Robert Marsh]	Noted, text changed
5-64	A	2:10	2:10	"warming rate" implies a temperature change. I think "heating rate" is more accurate here. [Chris Folland]	Noted
5-65	A	2:10	2:12	saying "W m^-2 (per unit area of ocean surface)" is tautologous and, I think, incorrect - perhaps saying W m^-2 (i.e. per unit area are of ocean surface) would be better - if this really has to be repeated! If you are trying to say x.yy W m^-2 over the ocean surface it could be expressed better, or perhaps left out, as it is implied by the earlier part of the sentence. [Neil White]	Noted, text changed
5-66	A	2:10		area instead of are [Douwe Dillingh]	Noted, text changed
5-67	A	2:10		The comment "…range from 0.70 +/- 0.11 W m-2 to 0.86 +/- 0.12 W m-2" is not very useful as an executive reader will immediately recognise that 0.86 is outside the bounds 0.70 +/- 0.11, and vice versa. This needs more careful explanation to look plausible. I know this comes from different estimation methods, but we have to make that clear.	noted, text changed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Howard Freeland]	
5-68	A	2:10		Area, not are. [Peter Gent]	Noted
5-69	A	2:10		I didn't understand the '(per unit area of ocean surface)'. The W m-2 is already per unit area. Perhaps reword. [Philip Woodworth]	Noted, text changed
5-70	A	2:11	2:11	Why are there two ranges (0.7+- and 0.86+-) here? [Trevor McDougall]	Noted, text changed
5-71	A	2:11	2:11	You have omitted Ishii et al's 0.58 +/- 0.09 W m-2. [David Parker]	Noted
5-72	A	2:11		Double the error figures to 95% confidence : 0.70±0.22; 0.86±0.24 [Vincent Gray]	Noted, done
5-73	A	2:14	2:16	I read the statement on page 5-4, lines 14-16 with the knowledge that Banks and Wood's analysis had 'proved' with HadCM3 coupled model simulations that anthropogenic forcing causes freshening of SAMW, though the observations showed that the freshening had recently changed to salting. I like the sentiment in the statement but should it be used here if the specific application was unsuccessful? [Harry Bryden]	Noted, the idea with the citation was not a model-data comparison but rather to point to the value of ocean parametrs for climate change detection.
5-74	A	2:15		There is also evidence for a forced response as distinct from natural variability. [John Church]	Noted
5-75	A	2:16		I remain unconvinced that the results are robust to sampling variations. [John Church]	Noted
5-76	A	2:18	2:18	Change to… between 1750 and 1994…. [Richard Feely]	Noted
5-77	A	2:18	2:18	Knight et al (2005) also show a component of THC/AMO related warming of Europe, likely dominated by the North Atlantic surface temperature increase, though by no means all recent N. Atlantic warming is likely to be THC related. [Chris Folland]	Noted
5-78	A	2:18	2:20	From the figures quoted is it not clear that there has been a decrease in the fraction of anthropogenic CO2 taken up by the ocean. More importantly, the figures quoted here are not the same as those given in Chapter 7 [Richard Wood]	Taken into account: uncertainty revised by chapter 7
5-79	A	2:18		Double the error figures to 95% confidence : 118±0.38 [Vincent Gray]	Rejected. The given uncertainty is not a statistical calculation but a propagation of errors estimated by the authors.
5-80	A	2:18		Help me out here guys. What is 118 Pg ? 10^17g = 10^14 kg = 10^11 tonnes ~ 0.3 mm	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				layer of carbon over the entire sea surface? Sounds tiny, but still does mean much to me. [David Vaughan]	
5-81	A	2:19	2:19	Since the ranges overlap, is it correct to describe the temporal change as having "decreased"? [Trevor McDougall]	Noted (yes).
5-82	A	2:19		Double the error figures to 95% confidence; 42±19% [Vincent Gray]	Rejected. The given uncertainty is not a stastical calculation.
5-83	A	2:20		Double the error figures to 95% confidence : 36±18% [Vincent Gray]	Rejected. The given uncertainty is not a statistical calculation.
5-84	A	2:20		comment on WHY ocean uptake has decreased, post-1994 [Stephen R Rintoul]	Rejected. Comment is included in the main text.
5-85	A	2:21		everywhere roughly'. what does that mean. perhaps 'The surface .. by nearly the same amount throughout the ocean'. [Philip Woodworth]	Accepted
5-86	A	2:23		depth has decreased (rather than depth has risen) [Stephen R Rintoul]	Accepted
5-87	A	2:25	2:31	This bullets on O2 needs rewritting. The first sentence on surface O2 changes is not relevant. The associated O2 chages at the surface are very small, much smaller than in the deep and it is unclear how to interpret this surface O2 changes. I suggest to delete this sentence from the abstract. Second the bullet should state that the majority (all except perhaps one) of the many studies linked the observed O2 changes to changes in ventilation (see Keeling and Garcia and Plattner et al, GBC, 2002 for an overview on experimental studies.. The next sentence dealing with changes in chlorophyll and nutrient should be include d in a new bullet. The last sentence of the bullets on NADW freshening appears not to belong here.. [Fortunat Joos]	Accepted except for changes in biology, which remain in the same bullet because there is not enough information for an independent bullet.
5-88	A	2:25	2:31	I was left asking 'so what?". The apparent changes are reported without inference on the cause or effect. [Philip Woodworth]	Noted, text changed
5-89	A	2:26		How large is the decrease? [John Church]	Accepted, values given in section 5.4
5-90	A	2:30	2:31	The statement on page 5-2, lines 30-31 is in conflict with that on 5-11, lines 45-47. [Harry Bryden]	Noted, discussion consolidated
5-91	A	2:30	2:31	This statement is not consistent with the discussion of the Hansen et al paper later on (but see also comment on that statement!). It also seems a bit out of place in a bullet point on changes in biogeochemical tracers.	accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard Wood]	
5-92	A	2:30		the "strength" has not "freshened" [Stephen R Rintoul]	accepted
5-93	A	2:30		comment on deep overflows seems out of place at end of paragraph on oxygen and nutrient changes [Stephen R Rintoul]	Accepted
5-94	A	2:33	2:37	This paragraph would be clearer if the time period of the observed warming was indicated. [Eric Leuliette]	Noted
5-95	A	2:33	2:33	Perhaps this is over-pedantic, but here and throughout the chapter I suggest use of the past tense, "have warmed", rather than "are warming", to make a clear distinction between observations and interpretation. [Richard Wood]	noted
5-96	A	2:33	2:37	there is inevitably perhaps a lot of jargon. Will a normal reader know what mode water is, for example? [Philip Woodworth]	Noted, however to some extent the use of jargon is forced by page limits.
5-97	A	2:35		How strong is the evidence for reduction in subtropical ventilation? [John Church]	Noted
5-98	A	2:39	2:41	This paragraph would be clearer if the time period of the observed cooling was indicated. [Eric Leuliette]	Noted
5-99	A	2:39		"Cooling …creating colder water masses" is not very informative [Stephen R Rintoul]	accepted
5-100	A	2:40	2:41	The direct evidence of the new Bryden paper should also be considered. See comment on p 9 ll 6-7 and p 12 l 23. [Richard Wood]	accepted
5-101	A	2:40		State what the indirect evidence of changes in overturning is [Stephen R Rintoul]	see box 5.1
5-102	A	2:41	2:41	Change ".. . But no confirmed trend." to "… and, at 25 degrees N, may have weakened by about 30% since 1957." [Robert Marsh]	Noted
5-103	A	2:43	2:47	If there is an increase in sea level rise, as measured by T/P, it should also be seen in the tide gauges records. In the Dutch tide gauge records this is not the case. I should like to have more attention in this chapter on the diffence between T/P and tide gauge records. [Douwe Dillingh]	Noted. Global sea level change need not to coincide with regional tide gauge records.
5-104	A	2:43	2:47	there is an IMPORTANT issue here and throughout the chapter in that in the use of +/- which to most people implies a standard error. The TAR stated that	The use of +- implies 95% error throughout.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				sea level rose between 1 and 2 mm/year. That is not the same as 1.5 +/- 0.5! [Philip Woodworth]	
5-105	A	2:43		Double the error figures to 95% confidence : 1.8±0.6 [Vincent Gray]	see 5-104
5-106	A	2:44		Double the error figures to 95% confidence; 1.5±1.0 [Vincent Gray]	Noted, see 5-104
5-107	A	2:45	2:45	"(TP)" should appear after "Topex/Poseidon" as the abbreviation is used later on the page [Melissa Bowen]	Noted
5-108	A	2:45	2:46	It would be more correct to either drop "Topex/Poseidon" from this sentence or add "Jason", as data from both missions were used to compute the trend. [Eric Leuliette]	Noted
5-109	A	2:45	5:45	After "Topex/Poseidon", add "(TP)" as the accronym is used late (p. 2 L. 53) without being defined. [Philippe Tulkens]	Noted
5-110	A	2:45		Topex/Poseidon add: abbreviation (TP) [Hartmut Grassl]	Noted
5-111	A	2:46	:47	I believe the statement on acceleration should be reworded and can be updated - see comments on the main text. [John Church]	Noted
5-112	A	2:46		Double the error figures to 95% confidence ; 3.1 ±0.8 [Vincent Gray]	Noted
5-113	A	2:49		Double the error figures to 95% confidence : 0.4±0.2 [Vincent Gray]	Noted
5-114	A	2:50	2:50	line 50 estimates range [Philip Woodworth]	Noted
5-115	A	2:52	2:54	I think this statement is incorrect as it stands. It needs to be qualified by a time period over which the trends are up to 5 times the global mean rise - the shorter the period the greater the magnitude of the trends (e.g. for 1993-1998 the mean trend was ~3mm/year, and the extreme magnitude was ~30mm/year i.e. 10 times the global mean), but as the time period gets longer the magnitudes of the extreme trends decrease as we average over the decadal variability. For example, Church et al. (2004) show a range of trends of ~0 - 4 mm/year over 1950-2000 - the difference between the extreme and the mean is more-or-less equal to the mean global trend (~1.8mm/year). The multiple of 5 quoted here is about right for a 10-12 year period (see e.g. the current maps on http://sealevel.colorado.edu). [Neil White]	rejected, the statement holds for both periods (1960-2003 and 1993-2003) which are considered
5-116	A	2:52	2:54	are the 'rates up to 5 times' for the last 50 years or 1990s (the T/P period)?	Noted, see 5-115

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Philip Woodworth]	
5-117	A	2:52		replace "highly non-uniform spatially" with "not spatially uniform" [Stephen R Rintoul]	Noted
5-118	A	2:53	2:53	"PT" is unexplained at this location [Walter Zenk]	Noted
5-119	A	2:53	2:54	It would be more correct to either replace "TP" with "altimetry" in this sentence or add "Jason", as data from both missions were used to compute the spatial patterns. [Eric Leuliette]	Noted
5-120	A	2:53		"TP" not defined [Stephen R Rintoul]	Noted
5-121	A	2:56		If the anthropongenic land water storage term is now more uncertain than the Antarctic ice sheet and glacier contribution, this will be seen as major news compared to the TAR, are you sure that this is the conclusion of the panel. If it is then it should be spelt out what the word "contribution " implies here (the period, steric, eustatic, etc). [David Vaughan]	noted
5-122	A	3:0		Item 12: This item is at odds with chapter 4 values. [Kevin Trenberth]	Noted, has been modified
5-123	A	3:1	3:2	I didn't understand this sentence. Does it mean that the assessment that the land water storage is the most uncertain term hasn't changed since the TAR, or does it mean something more quantitative? [Richard Wood]	Noted, text changed
5-124	A	3:1		These numbers are not actually given in the text. They should be added. [John Church]	Noted, numbers will be given.
5-125	A	3:1		Over what period is the trend in land storage calculated? Should be given here. [Wilco Hazeleger]	Noted, will be given.
5-126	A	3:2		What is the implication of the observation that "decadal fluctuations in sea level due to land water storage are negatively correlated with change in ocan heat content" [Stephen R Rintoul]	Noted, will be rewritten
5-127	A	3:2		Give a value for this uncertainty [David Vaughan]	Noted
5-128	A	3:6	3:9	Should this be part of the executive summary? It seems to be a conclusion from the previous chapter, though it is important to the interpretation in this chapter. Also, the numbers given here do not agree with those in Table 5.5.2. [Mark Tamisiea]	Noted, item deleted from exec summ.
5-129	A	3:7		This is not the value from chapter 4 or in the text. [John Church]	corrected

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-130	A	3:7		Double the error figures to 95% confidence : 0.76±0.28 [Vincent Gray]	see 5-129
5-131	A	3:8	3:9	Is this sentence consistent with chapter 4 - especially their Exec Summary page 4 lines 9 - 18? [Martin Manning]	see 5-129
5-132	A	3:9	3:9	Chapter 4 has a central estimate of +0.1mm/year contribution from Greenland and Antarctica. The two Chapters need to be consistent. [David Parker]	see 5-129
5-133	A	3:9	3:9	It is noted that in chapter 4 page 4 line 16 the value for the sea level rise for Greenland and Antarctica combined is 0.1 mm/year compared to the value in chapter 5 page 3 - 0.0 mm/year. [Klaus Radunsky]	see 5-129
5-134	A	3:9	3:9	The contribution from the Greenland and Antarctica ice sheets estimated to 0.0 +/- 0.2 mmm/year should be given consistently with the figure given in Chapter 4 (p. 39 L 24 to 27). As given here they nearly match. Identical figure can be given. [Philippe Tulkens]	see 5-129
5-135	A	3:9		This is not the value from chapter 4 or in the text. [John Church]	see 5-129
5-136	A	3:9		the numbers differ compared to chapter 4 [Hartmut Grassl]	see 5-129
5-137	A	3:9		Double the error figures to 95% confidence : 0.0±0.4 [Vincent Gray]	error bars have been changed to 95% throughout the chapter
5-138	A	3:11	3:13	the paragraph to be separated in the second line as like " worldwide, and variations….." [kyung-ryul Kim]	Noted
5-139	A	3:11	3:13	I suggest to replace "related to changes in regional climate", with "related to regional changes in sea level" [Piero Lionello]	Noted
5-140	A	3:13		What is the implication of the fact that "extremes in high water are closely related to regional climate"? [Stephen R Rintoul]	Noted
5-141	A	3:15		"the sum of climate contributions to sea level rise" is not clear to me. Does it include contributions of Antarctica to sea level fall (storage of more ice), and contributions from Antarctica that may not be related to contemporary climate change (changes in West Antarctica). [David Vaughan]	Noted
5-142	A	3:17		Double the error figures to 95% confidence : 2.6±0.4	see 5-137

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Vincent Gray]	
5-143	A	3:17		It seems to me that 3.1+/-0.4 and 2.6+/-0.2 are not significantly different and so, within error, the budgets are probably balanced! [David Vaughan]	accpeted
5-144	A	3:18	3:18	reword - ... balanced, estimates of climate-... are now in closer agreement with the observations. [Philip Woodworth]	Noted
5-145	A	3:18		what does "relatively closer to the observations" mean? [Stephen R Rintoul]	Noted
5-146	A	4:3	4:16	Sometimes I am confused on whether regional or global changes are being discussed. [Jerry Mahlman]	Noted
5-147	A	4:3	4:16	Sometimes I cannot interpret whether the introductory discussion is globally or regionally focussed. [Jerry Mahlman]	Noted, focus is global
5-148	A	4:4		This sentence and the following one read as if the increased ocean storage of heat since 1955 plays a dominant role for climate variations … such as El Nino. I don't think this is what is meant; the text needs clarification. [Stephen R Rintoul]	Noted, and text changed regarding role of variations.
5-149	A	4:9	4:9	change "almost certainly" to "may" [LuAnne Thompson]	Noted
5-150	A	4:9		suggest remove "almost certainly" [Stephen R Rintoul]	Noted
5-151	A	4:13	10:57	References cited in review are listed separately in supplemental review doc [Joyce Terry]	Noted
5-152	A	4:13		Sea level changes due to melting of glaciers dare not directly tied to changes in ocean circulation. Suggest re-wording… [Joyce Terry]	Noted
5-153	A	4:13		Sea level changes are NOT always linked to oceanic circulation. In fact many people would argue that long term global sea level rise is not at all linked with oceanic circulation. Maybe you need to say regional or local sea level variability or say something like:" Changes in the oceanic circulation, global or regional may affect the sea level variability at the coasts" [Michael Tsimplis]	Not always, but very often.
5-154	A	4:14		Is sea level an oceanic parameter? If yes it is of primary importance in the detection of climate change, provided we understand its forcing. [Michael Tsimplis]	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-155	A	4:14		This statement is problematic. Indeed the deep ocean is less noisy but is the signal as strong and what is the delay to get the signal there? [Michael Tsimplis]	Noted, and discussed in Section 5.3 and Appendices
5-156	A	4:16		I guess the Barnett et al 2005 reference is appropriate here. [Wilco Hazeleger]	Noted
5-157	A	4:16		The statement that signal-to-noise ratio is higher in "deeper layers", referenced to Banks and Wood 2002, should be checked. My impression is that these authors have backed off on this statement, after realising that the model variability was smaller than observations (thus giving an overly optimistic assessment of signal-to-noise). [Stephen R Rintoul]	Noted, they have, but the point remains true in the sense that decadal variations are smaller in the regions they identified, relative to other regions of the worlds oceans.
5-158	A	4:18	4:24	Global decadal variability is a rather odd concept when we are discussing the world ocean. Clearly, one must be cautious when making such statements, especially so when most of the ocean's heat capacity lurks at greater depths than the measurements penetrate. [Jerry Mahlman]	Noted
5-159	A	4:18		Paragraph is worded as if the Levitus et al 2005 work was discussed in the TAR. [Stephen R Rintoul]	Noted
5-160	A	4:20	4:20	Would "The warming underlies strong global decadal variability…" be better than :The warming is superimposed on…" [Neil White]	Noted
5-161	A	4:21		The 300 m is erroneous. According to the orginal paper and the following pages it should be 3000 m [Hendrik M. van Aken]	number quoted from the TAR
5-162	A	4:27		Insert after"PgC" "(using only one standard error)" [Vincent Gray]	Noted
5-163	A	4:30	4:30	see comment page 2, line 43-47 [Philip Woodworth]	Noted
5-164	A	4:37		Insert after" mm yr-1" " (that is 1.5±1.0 for two standard errors) [Vincent Gray]	Noted
5-165	A	4:38	4:38	see comment page 2, line 43-47 [Philip Woodworth]	Noted
5-166	A	4:40		After "0.5" insert "for one standard error" [Vincent Gray]	Noted
5-167	A	4:42		The chapter would be more useful if it went beyond simply assessing observations of changes in oceanic parameters. I believe the chapter should summarise what has been learned about the ocean's role in climate since the TAR. [Stephen R Rintoul]	Noted, however chapter is restricted to discussion of observations

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-168	A	4:42		Rather than "parameters" it should be "variables". The term parameters is usually kept for things like means, standard deviations, etc. [Kevin Trenberth]	Noted
5-169	A	4:48	4:48	Is the chapter supposed to be using models to infer reasons for changes? I think that the whole chapter would benefit if the observations were interpreted in the context of model results but this needs to be consistent. [Helene Banks]	Noted, see 5-167
5-170	A	4:53		The crucial question in relation to Climate chnage is whether the atmospheric and oceanic warming contribute directly or indirectly to the observed sea level rise. Within this context it is important to resolve the regional and global forcing mechanisms. [Michael Tsimplis]	noted
5-171	A	5:0	9:	The section on hydrographic changes is really nice. The EOF analysis is a bit obscure (it isn't clear what it adds to the analysis without an attributionin terms of wind stress changes) but is very provocative. If this can be added by the final draft it would be worthwhile. [Anand Gnanadesikan]	Noted: The EOF material is being replaced by other material.
5-172	A	5:0		Figure 5.2.1 The panel showing the fourth EOF should be removed as it is not very relevant to the discussion. It could be replaced by a panel showing the heat content and EOF-reconstructed heat content (a summary of the first three panels of Figure 5.2.2). [Melissa Bowen]	Accepted: The EOF material is being replaced by other material.
5-173	A	5:0		Section 2.2.2: This section clearly conveys the observed increase in ocean heat content that is a critical component of climate change. However, there are major omissions from this section, including the work with the SODA analysis, the Stammer and ECCO analysis and the many more regional analyses. All of these studies are an important component of ocean heat content variation studies and should not be just thought of as contributing to water mass property changes. The opportunity should be taken to build a strong and comprehensive case. Also, the critical issue of sampling errors was relegated to the appendix - this material is important and should be included in the text. The long discussion on EOF patterns should be shortened considerably. [John Church]	Accepted: The EOF material is being replaced by other material.
5-174	A	5:0		Sec 5.2.1. There is nothing here or elsewhere on the problems with spatial or temporal sampling, or on problems with XBT drop rate corrections and related biases. These must be discussed even if only briefly. They should carry over into the error assessment. [Kevin Trenberth]	Accepted: XBT drop rate error is corrected for. If appropriate metadata are not available as to whether the correction needs to be applied the data are not used.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-175	A	5:1	9:32	While this section (5.2) presents reasonably good evidence that the oceans are warming, some key issues need to be addressed: a) The section conveys the impression that there is agreement across the community on the nature of both global and basin-wide warming. However, the estimate of the warming have varied substantially even from papers published by authors in this section (Levitus, Antonov, Boyer) over the last 5 years (ironically, these differences are alluded to in section 5.5.3). In addition, other papers have also found differences in the rate of warming. For example, Polyakov, I. V., U. S. Bhatt, H. L. Simmons, D. Walsh, J. E. Walsh, X. Zhang, 2005: Multidecadal variability of North Atlantic temperature and salinity during the twentieth century, J. Climate, 18(21), 4562–4581. found that warming is confined to the upper portion of the Atlantic while the deeper ocean (1000 -3000 m) is cooling and the overall rate of warming in the Atlantic is roughly ½ of that reported in Levitus et al. (2000, Science). These discrepancies and their potential causes should be discussed. [Michael Alexander Alexander]	Noted: Of course as we add more data the results have changed, but not qualitatively- The ocean is warming. The Polyakov paper results disagree with not just the Levitus results but with results from many other studies as Polyakov state in their paper.
5-176	A	5:1	9:31	Section 5.2 on Global Trends is dominated by recent works by Levitus and collaborators. Works by Ishii et al and Willis et al are mentioned at the beginning of 5.2 (page 5.5, line 12) and then at the end of 5.2.2 (page 5-7. line 36) where their values are taken to confirm the Levitus analyses. For this reader, the range of these 3 estimates was what convinced him that heat content was indeed increasing and within a quite narrow range. So I suggest that the results of Ishii and Willis be intermingled with those of Levitus throughout Section 5.2. [Harry Bryden]	Accepted, and done, with comparison of the three analyses through section 5.2.
5-177	A	5:1		Section 5.2 and 5.3. I think it is difficult to consider heat content changes independently of temperature and salinity changes. These two sections are very difficult to follow and I think could be reorganised for clearer reading. I would suggest reorganising the sections perhaps: 5.2 Global trends in heat and freshwater content 5.2.1 Background 5.2.2 Changes in ocean temperature properties 5.2.3 Ocean heat content 5.2.4 Ocean surface heat fluxes and meridional transports 5.2.5 Changes in ocean salinity properties 5.2.6 Ocean surface freshwater fluxes and meridional transports 5.3 Changes in ocean circulation 5.3.1 Introduction	Noted: We are reorganizing relevant material.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				5.3.2 Changes in each ocean basin 5.3.3 Summary [Helene Banks]	
5-178	A	5:5	5:8	This paragraph should be moved to the introduction. [Helene Banks]	Accepted
5-179	A	5:5		Three major challenges for the climate-system science community are ... [Jerry Mahlman]	Accepted
5-180	A	5:10		Detail on data sources is not needed here - it is contained in the referenced papers. [Stephen R Rintoul]	Noted, text modified
5-181	A	5:12	5:12	Levitus et al., 2005 will be 2005a, 2005b, or 2005c (see reference of page 44) [kyung-ryul Kim]	Noted
5-182	A	5:13		The fact that the estimates of ocenic heat content come from various instruments there is a need for a comment on whether the accuracy of these measurements is comparable and how this affected the folloing eestimates. [Michael Tsimplis]	Noted: The precision of various instruments is different but the accuracy of our estimates is improved as the number of observations increases ,e.g., the standard error of the mean decreases with the increasing number of observations. See also new appendix on data.
5-183	A	5:14		Why is no mention made of the Japanese work (Ishii et al) here? Section 5.5.2 is vastly superior in this regard. [Kevin Trenberth]	Rejected: Ishii and Willis results are cited 2 lines above this line. However we are reorganizing this section.
5-184	A	5:27	7:49	It would be less confusing if the section on heat transport began with all the global estimates of heat transport (including those on page 7, lines 34-47) before discussing regional variations. [Melissa Bowen]	Noted. This section is about ocean heat content not heat transport. We are revising this section.
5-185	A	5:27	7:47	In terms of changing ocean temperature in section 5.2.2, I would like to know how the average increase in ocean temperature over the upper 700 m from 1955 to 1998 compares with the increase in sea surface temperature so carefully estimated for the period 1979 to 2004 to be 0.14 C per decade in Chapter 3. Is the entire upper 700 m of the ocean warming at the same rate as sea surface temperature over decadal time scales? Maybe it is only the upper 200 m that is warming. Could we see the distribution of global ocean	Noted: Fig. 5.2.2 shows the linear trends of zonally integrated ocean heat content as a function of depth which the Reviewer apparently did not notice. We are replacing this figure with the linear trends of zonally averaged temperature

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				warming as a function of depth? Is the warming at depth related to earlier warming, say at 500 m depth the rate of warming is that from 50 years ago? [Harry Bryden]	which should better meet the reviewer's request.
5-186	A	5:27	7:47	This section should include mention of the Earth's energy imbalance results from Hansen et al., (2005) based on climate modeling simulations of radiative forcings over the past century. The results show that given the accumulated radiative forcing of increasing GHGs, and because of the large heat capacity of the oceans, the Earth is currently absorbing 0.85 W/m2 more solar energy than is being emitted back to space as thermal radiation. The modeled TOA energy imbalance and resulting heat storage in the oceans for the decade of 1993 - 2003 is in close agreement with the observed change in ocean heat content (Willis et al., 2004). This is a kind of climate modeling closure experiment that establishes credibility for global climate modeling whereby the expected 0.85 W/m2 TOA energy imbalance (which is too small to be verifiable by direct satellite measurement) can be convincingly verified by the measured heat content change in the oceans. [Andrew Lacis]	This is addressed in chapter 9
5-187	A	5:29	5:30	This very important work still contains significant elements of uncertainty, most likely due to insufficience of global data coverage. In my view, that data insufficiency contributes substantially to some of the major challenges in this Chapter. In my view, the Chapter participants need to brainstorm to help all of us understand the mechanism, or lack therof, that can allegedly change the entire sluggish upper ocean's temperature substantially on the scale of a decade. Clearly, we need to take this intellectual challenge very seriously, with new ideas and new data that might correct some of the data insufficiencies. I think that we have now moved well beyond the times when we blamed these confusing behaviors on the data analysts, and not on the dubious quality of the data. [Jerry Mahlman]	Noted: We disagree that the ocean response is sluggish". It is radiative heating and cooling that results in such fast changes in the heat content and sea level of the world ocean. We are adding text to provide our view on this matter.
5-188	A	5:29	7:47	This is a synthesis report. I would suggest that the whole section is restructured and start by observing that all three studies are in broad agreemen i.e. the last paragraph. (Which is not surpising as they use the same data). If it is then considered necessary to go into the spatial characteristics of warming then the report can concentrate on one of the published studies. [Michael Tsimplis]	Accepted. We are reorganizing this section
5-189	A	5:29		In reference to figure 5.2.1. The time series shows heat content ANOMALY relative to some mean. Should indicate the period for which that mean is computed (presumably the full period). [Frank Bryan]	Accepted: By definition the anomaly is with respect to climatology. We will state this more clearly.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-190	A	5:29		This is an important figure and should be defined. For example these must be interannual anomalies of heat content since negative heat is not possible (in reality). What period was the base for the reference used in the calculation? Explanatory text is needed in the figure caption and in the body of the document. [Joyce Terry]	Noted: See response to comment 5-189.
5-191	A	5:30		But these millions of profiles are not uniformly ditributed either in space or time. A plot with the number of measurements with time at the upper and lower waters would remedy that especially if done in oceanic basins. [Michael Tsimplis]	Noted: One plot will not do justice to the data distributions involved which we agree are variable in space and time. As noted on page 5-5, lines 23-25 of the FOD, a comprehensive set of data distribution plots is available online. See also Appendix 5.A.1 of second order draft.
5-192	A	5:37		The sentence on this line contains the punch-line of the paragraph - suggest move it to start rather than bury it in a long paragraph. [Stephen R Rintoul]	Noted
5-193	A	5:41	5:41	Clarify "0.037 C per decade" [Robert Marsh]	Noted: add "volume" before "mean" in the preceding line.
5-194	A	5:43	5:46	The relation between ocean cooling and volcanic eruptions is not so easy to argue. Some links seems to be also between El-Nino occurrence and eruptions. For the Pinatubo event Stenchikov et al JGR 2003, argued an influence on the AO (Arctic Oscillation). [Renzo MOSETTI]	Noted: Our language was not strong, e.g., we stated that some of the cooling "may" be related to volcanic aerosols. We indicated the record is convolved. We do not believe there is serious evidence linking El Nino & volcanic eruptions.
5-195	A	5:43		due in part to the volcanic" delete: "in part [Hartmut Grassl]	Accepted
5-196	A	5:43		I have added text below suggesting that the AMO may also be influencing the dip in heat content around 1985. The AMO has an SST signal in the Pacific ocean but is largest in the N. Atlantic. If this seems as reasonable as a volcano eruption, this might be mentioned here. [Joyce Terry]	Rejected: The Atlantic Multidecadal Oscillation (AMO) is not involved. The papers on which this text is based clearly show that most of the decrease in ocean heat content (between 1980 and 1985) occurs in the Pacific Ocean. Cautionary not: We have performed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					EOF analyses and found relations between ocean heat content and SLP variability, e.g., specifically the NAO. However, when we add more years or data to the beginning and end of the records the correlation disappeared. This is a common occurrence as documented in the scientific literature and suggests some caution in interpretation of relationships.
5-197	A	5:44	5:45	Enfield et al. (2001, GRL, 10, 2077-2080) show cooling after 1960 in the North Atlantic related to the Atlantic Multi-decadal Oscillation which also includes cooling over large parts of the Pacific. [Robert Molinari]	See response to 5-196
5-198	A	5:46	5:48	The difference in spatial coverage during different periods could also contribute to the variation in heat content. [Robert Molinari]	Noted: This could be true in part but we have examined data plots for pentads before and after the "Regime shift' and do not believe data distribution changes play a role.
5-199	A	5:47		add- The internal variability of earth's climate system includes internal ocean circulation variability on multi-decadal time scales (Schneider et al. 2002). Full citation, Schneider, N., A. J. Miller and D. W. Pierce, 2002: Anatomy of North Pacific decadal variability. J. Climate, 15 586-605. [Franklin Schwing]	Noted: This is one of many such references that could be added.
5-200	A	5:49		"deconvolving the heat content integral" is unnecessary jargon [Stephen R Rintoul]	Noted: We disagree that this is jargon. It is a succinct way to communicate the problem. Our understanding is that the individual chapters are for scientists (who should understand this term) and that the Executive Summaries are for the public.
5-201	A	6:0	7:	In my opinion, the EOF description is too long and/or too technical for the general audience of the IPCC report [Michel Rixen]	Accepted: The EOF material is being replaced with other material.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-202	A	6:1	6:13	This material should be assessed as to how good it is and how it fits with other studies. [Kevin Trenberth]	Noted: This material has been in the literature for several years and is well-known to the scientific community.
5-203	A	6:2	6:5	This comment appears to be very speculative [Helene Banks]	Noted:
5-204	A	6:2	6:5	a good citation here is Cummins and Lagerloef (2004). Full citation, Cummins, P.F. and G.S.E. Lagerloef. 2004. Wind-driven interannual variability over the Northeast Pacific. Deep-Sea Research. 151, 2105–2121. [Franklin Schwing]	Noted: The IPCC Assessment is not a literature review so we can not be accepted to cite all relevant papers.
5-205	A	6:6	6:13	Here the ocean heat content is mixed up with discussion on changes to water mass properties [Helene Banks]	Noted: The reviewer states "Here the ocean heat content is mixed up with discussion on changes to water mass properties. She means water mass temperature and of course they are linked.
5-206	A	6:7	7:20	I did not find the EOF analysis helpful. Does it add anything since it is expressly stated that it depends on the period and the regions involved? [Michael Tsimplis]	Accepted: The EOF material is being replaced with other material.
5-207	A	6:12	6:13	Delete final sentence on page 5--6, lines 12-13. [Harry Bryden]	Why delete this? Readers should know these computation were done and where to find them.
5-208	A	6:12		Why not include variances explained by linear trends in caption or on figure? [Stephen R Rintoul]	Noted: The percent variances are a function of latitude and depth and can not be summarized by one number for each figure.
5-209	A	6:17	6:17	EOF needs to be defined here and also on Figure 5.2.3. It is not defined until Pg. 24,line 25. Some explanation of the technique would also be useful. [Lenny Bernstein]	Noted: The EOF material is being replaced with other material.
5-210	A	6:17	6:28	The sign of the EOF time series and pattern in Figure 5.2.3 can be reversed such that it shows more clearly the warming (now it is negative pattern and pcs, resulting in positive warming, but that can be confusing, in particular when comparing with 5.2.4). Also, better to normalize the pc with its standard deviation and show the pattern in degrees C. Now the contour levels are not shown. [Wilco Hazeleger]	Accepted: The EOF material is being replaced with other material.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-211	A	6:17	6:57	I think that if one wants to invoke EOF diagnostics as a canonically superior way to present data, then a solid physical interpretation should accompany the data presentation. See also, Page 7, Lines 1-28. If this were an ocean model that we were analyzing to provide insight into this problem, one would explicityly calculate the vertical heat fluxes explicitly to get some sense of how setting the floor to vertical heat flux at 300m or 700 m in the observations, rather than explicitly calculating the areal integral of vertical heat flux at the lower surface of the control volume. Clearly, that has to somehow come into play when one is establishing the experimental "control volume". [Jerry Mahlman]	Accepted: The EOF material is being replaced with other material.
5-212	A	6:17	6:57	This is all the result of one study based on data that is incomplete. It needs to be assessed by someone other than the author. [Kevin Trenberth]	Noted: All datasets are incomplete or lacking in some way. The material in the second draft is quite different.
5-213	A	6:17	7:28	It is unclear how much the EOF analyses of ocean heat content contributes to our understanding of the ocean warming, especially EOFs 3 and 4. While the authors make a reasonable case for the physical interpretation of the first two EOFs, the latter two look like noise. In addition, EOFs 3 and 4 explain a very small fraction of the variance and are likely not to be separable from the higher order patterns. Furthermore, most of the warming signal can be recovered from just the first two PCs (shown in Fig. 5.2.4). Thus, I recommend dropping EOFs 3&4 from the discussion in section 5.2 and Figures 5.2.3 (page 5-58) and 5.2.4 (page 5-59). The authors should also report the correlation between the observed and PC generated time series for the top panels in Figure 5.2.4 [Michael Alexander Alexander]	Noted: The EOF material is being replaced with other material.
5-214	A	6:17	7:28	The long discussion on EOF analysis of heat content adds very little to the interpretation of the observations. The authors should consider removing this. [Helene Banks]	Accepted: The EOF material is being replaced with other material.
5-215	A	6:17	7:28	Section should be considerably shortened. It contains detail appropriate for a scientific paper rather than a summary. The main points (page 7/lines 22-28 ?) should lead the discussion and only major features in the EOFs described. [Melissa Bowen]	Accepted: The EOF material is being replaced with other material.
5-216	A	6:17	7:28	This discussion could be shortened considerably if one refers to Levitus et al 2005c for details. A shorter discussion would have the advantage that the conclusions stand out much more clearly: (1) much of the variability in heat content stems from large-scale phenomena such ENSO and PDO (2) estimated ocean heat content variability is not due to sampling mesoscale features such as eddies. [Gerrit Burgers]	Accepted: The EOF material is being replaced with other material.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-217	A	6:17	7:20	This material should be shortened considerably in favour of being more comprehensive. [John Church]	Accepted: The EOF material is being replaced with other material.
5-218	A	6:17	7:28	This long section on the EOF analysis of the heat content records is factually correct but it is also difficult to get a clear message from it. Lines 22 to 28 give the appropriate caveats that EOF modes are not necessarily dynamical modes but then what is the non-specialist reader to make of the lengthy analysis of the spatial and temporal structures of each of the 4 leading modes. [R Allyn Clarke]	Accepted: The EOF material is being replaced with other material.
5-219	A	6:17	7:6	: the discussion on EOF could be shortened (it is not necessary to describe detailed patterns) and selected important results could be put forward : the weight of the pacific ocean, the PDO transition in the late 70's, the drift… and the confidence we can have in this kind of approach [Pascale Delecluse]	Accepted: The EOF material is being replaced with other material.
5-220	A	6:17	7:28	Together with the 8 panels of Fig 5.2.3 and 5.2.4, this seems to be a lot of space for one paper using one data set, whose analyses could have been done in a number of other ways. [Chris Folland]	Accepted: The EOF material is being replaced with other material.
5-221	A	6:17	7:28	The EOF plots (Fig 5.2.3 and 5.2.4) and long description do not seem to me to add very much information to that contained in Fig 5.2.1 and 5.2.2. The EOFs do not describe very much of the variance in heat content. At the least, if the features are to be described in detail, there needs to be more discussion of what conclusions can be drawn from them. [Stephen R Rintoul]	Accepted: The EOF material is being replaced with other material.
5-222	A	6:17	7:33	This material should be deleted. It doesn't add much, displays 4 EOFs which explain only 29% of teh variance, and in some plpaces is arguably incorrect. For example, on line 43, the PDO does not look very much like EOF 1 in Fig. 5.2.3. The strong zonal SST change in the equatorial Pacific is quite different from the PDO which has little amplitude in the western tropics. Perhaps if some rotated EOFs were presented, showing some clear relation to climate modes this would be worth showing. However, this would need to be published first before appearing in a assessment like IPCC. [Joyce Terry]	Accepted: The EOF material is being replaced with other material. However, note that the reviewer is confusing the SST changes observed as part of the PDO with the ocean heat content changes observed as part of the PDO. The change in the depth of the pycnocline is responsible for the ocean heat content change and thus these "different looking" responses but both are correct.
5-223	A	6:17	7:34	The EOF analysis and the accompanying text could easily be deleted, as it adds little to	Accepted: The EOF material is being

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				the chapter, since the conclusions from the analysis are vague in terms of clearly delineating the important modes of variability in the climate system. [Kevin Walsh]	replaced with other material.
5-224	A	6:17		Here the EOF analyses is mentioned for the first time in this chapter. It seems worthwhile to me to dedicate a box on this type of analyses with a short description and a simple example. [Douwe Dillingh]	Accepted: The EOF material is being replaced with other material.
5-225	A	6:18	6:19	In general, EOF analysis is a way of characterizing spatial covariability (nothing to do with time and space scales). [Roxana Bojariu]	Accepted: The EOF material is being replaced with other material.
5-226	A	6:24	6:25	It seems to me more appropriate to write that EOF analysis statistically identifies linear modes of heat content variability (not necessary stationary ones - this depends on how one uses the EOF analysis, for instance, on temporal sub-intervals). [Roxana Bojariu]	Accepted: The EOF material is being replaced with other material.
5-227	A	6:30	7:6	This paragraph could be shortened without loss of relevant policy information. Much of the description of the EOF patterns could be dropped. The paragraph on page 5-7, lines 22-28 does a great job summarizing the relevant information learned from the analysis. [Eric Leuliette]	Accepted: The EOF material is being replaced with other material.
5-228	A	6:30	7:28	Much of this detail of the EOF analysis could be reduced or omitted for space reasons. Synthesize into "regional differences exist", most of the mode is positive, etc. Summarize to describe the trends in the time series +/- decadal variability. Not sure EOFs are useful in this big picture synthesis. [Franklin Schwing]	Accepted: The EOF material is being replaced with other material.
5-229	A	6:41	6:41	It is not directly clear what is meant by a time series of EOF 1, as EOF 1 is a plane. [Douwe Dillingh]	Accepted: The EOF material is being replaced with other material.
5-230	A	6:42		should read: "The spatial patterns and time series…" [Frank Bryan]	Accepted: The EOF material is being replaced with other material.
5-231	A	6:43	9:39	The acronym PDO should be defined when used for the first time. It is explained on page 9, line 39. There are other acronyms not fully defined in the text (VOS, etc.) [Michel Boko]	Accepted: The EOF material is being replaced with other material.
5-232	A	6:43		Here PDO is first mentioned without explaining what the abbreviation means. Later in this chapter it is explained (page 9). I suggest to give a list of abbreviations and their meaning somewhere in this chapter.	Accepted: The EOF material is being replaced with other material.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Douwe Dillingh]	
5-233	A	7:0	8:	Section 5.2.3: How come salinity is discussed here yet does not refer to any of the changes documented in Figs 5.3.1, 5.3.2 and 5.3.3? This is woefully inadequate and needs to be integrated with the material on salinity in 5.2.5.1 and 5.3, and also 5.5.3. [Kevin Trenberth]	The salinity variability documented in section 5.2.3 is the result of a global analysis of salinity data from one global database. Despite the inadequacies of this database it is a well-documented, consistently analyzed set of data and represents a first step towards systematic study of the ocean component of earth's freshwater balance. We will integrate these results.
5-234	A	7:0		Section 5.2.3: As for comments on section 5.2.2, this section ignores many previous studies, including the SODA analysis, the Stammer and ECCO analyses and the many more regional analyses. All of these studies are an important component of ocean salinity variation studies and should not be just thought of as contributing to water mass property changes. The opportunity should be taken to build a strong and comprehensive case. [John Church]	Noted: These studies are now referred to in Section 5.2, or 5.5.
5-235	A	7:0		I found all this hydrography rather long [Philip Woodworth]	Noted
5-236	A	7:1	7:28	I think that if one wants to invoke EOF diagnostics as a canonically superior way to present data, then a solid physical interpretation should accompany the data presentation. See also, Page 7, Lines 1-28. If this were an ocean model that we were analyzing to provide insight into this problem, one would explicityly calculate the vertical heat fluxes explicitly to get some sense of how setting the floor to vertical heat flux at 300m or 700 m in the observations, rather than explicitly calculating the areal integral of vertical heat flux at the lower surface of the control volume. Clearly, that has to somehow come into play when one is establishing the experimental "control volume". (From comment on p. 6, lines 17-57.) [Jerry Mahlman]	Noted: The EOF material is being replaced with other material.
5-237	A	7:8	7:20	In the whole paragraph I would rather use "filtered data" instead of "reconstructions". In fact, the data are filtered by projecting them onto the first EOFs. Reconstructed data are usually past projections from present data. [Roxana Bojariu]	Noted: The EOF material is being replaced with other material.
5-238	A	7:8	7:20	Comments about the PDO should be shortened to one sentence, especially as there is no	Noted: The EOF material is being

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				clear signal. [Melissa Bowen]	replaced with other material. .
5-239	A	7:8		Is the contribution of EOF #3 and perhaps # 4 to the explained trend in the 1990's is consistent with the statement on page 29, line 42 that there is a shift in the patterns of variability in sea level rise/heat content in hthe 1990's? [Frank Bryan]	Noted: The EOF material is being replaced with other material. .
5-240	A	7:12	7:14	Delete this sentence, it is not justified. It is based on "if". [Kevin Trenberth]	Noted: The EOF material is being replaced with other material.
5-241	A	7:19	7:19	29.0% for the first 4 EOFs is not very much. Seems hardly significant to me. [Douwe Dillingh]	Noted: The EOF material is being replaced with other material.
5-242	A	7:19	7:19	The technical details (i.e. covariance matrix) are probably not needed here [Michel Rixen]	Noted: The EOF material is being replaced with other material.
5-243	A	7:19		correct: "account" to "accounts"; "variance covariance" is lacking a hyphen [Hartmut Grassl]	Noted: The EOF material is being replaced with other material.
5-244	A	7:22	7:25	While this explanation is correct, it may be misleading, since it implies (at least to this reader) that eddies are unimportant in the transfer of heat in the oceans. This is obviously not the case. See, for example, Hughes, C.W. (2002): An extra dimension to mixing. Nature, 416, 136-139, for a discussion of the role of eddies in redistribtuion of heat in the oceans. [Lenny Bernstein]	Noted: The EOF material is being replaced with other material.
5-245	A	7:22	7:25	I don't know how to interpret the statement "is not due to sampling mesoscale features". I assume that the effect of the mesoscale features are reflected in part in the standard deviations shown in figure 5.2.1 and contribute to some of the interannual variability that is not captured by the EOF analysis. [R Allyn Clarke]	Noted: The EOF material is being replaced with other material.
5-246	A	7:22	7:25	This explanation implies that eddies are unimportant in the transfer of heat in the oceans. If this is what the author mean, it is incorrect. See, for example, Hughes, C.W. (2002): An extra dimension to mixing. Nature, 416, 136-139, which discusses of the role of eddies in redistribution of heat in the oceans. If the problem is in the wording, it should be changed to clearly state that eddies are important for heat distribution. [Jeffrey Kueter]	Noted: The EOF material is being replaced with other material.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-247	A	7:22	7:28	The dependence of the trends on the start and end years should be stressed. Starting in the mid-1940's would probably result in no or negative trends in SST if the AMO index of Enfield et al. (2001) were used. [Robert Molinari]	Noted: The EOF material is being replaced with other material. .
5-248	A	7:26	7:26	I thought that the important point concerning ENSO was that the EOF analysis failed to capture a clear ENSO signal in a single mode over an entire ocean basin. Instead, the signal appeared to impact different regions through different EOF modes. [R Allyn Clarke]	Noted: The EOF material is being replaced with other material.
5-249	A	7:27		Should "increase" on line 27 of page 5-7 be "decrease"? If not, what "increase" should the reader be looking at? [Harry Bryden]	Noted: The EOF material is being replaced with other material.
5-250	A	7:29	7:50	Needs a much more critical assessment. [Kevin Trenberth]	Noted
5-251	A	7:34	7:48	Section 5.2.2 On p 5-7 L 34-48 we finally learn of two other data sets. The datasets should all be assessed and discussed together rather than just the results of Levitus studies, and the assessment should not be by the author. See also section 5.5.2 which is much preferred. Major revisions are recommended. [Kevin Trenberth]	Accepted
5-252	A	7:38	7:38	Using an RMS error of 1.6 W/m2 would seem to me to overstate the actual error, as it is dependent on the time interval used. I think it would be fair to take a time step of five years instead of one [Anand Gnanadesikan]	Noted
5-253	A	7:39		A linear trend is not a good basis as it is clearly not linear. Instead the smoothed beginning and end points should be used to establish the change over a period. [Kevin Trenberth]	Rejected- There are several procedures to calculate trends. We use a least squares computation which is a standard procedure.
5-254	A	7:41	7:41	linear'. you mean a linear fit produces that trend, and not infer there is no evidence for non-linearity? [Philip Woodworth]	Noted: Determining the linear trend using a least-square fit to the data is a standard technique. We are not inferring that there "is no evidence for non-linearity." We clearly note that large interdecadal variability in our time series.
5-255	A	7:44	5:47	The similarities between the three heat content estimates could also be due to the fact that thye all use the same data with instrumental biases and essentially the same coverage and thus could all give erroneous estimates.	Noted: The work by Willis also uses altimeter data. QC of the 3 analyses of the data used to compute heat content is

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Robert Molinari]	different. We are not aware of any instrumental biases that have not been accounted for that affect would affect our results. Having 3 separate analyses strengthens our conclusions in the same way that 3 independent analyses of global surface temperature strengthen the results that earth's surface is warming. See also response to Comment 5-188.
5-256	A	7:44	7:44	I don't understand where instrumental bias comes into this discussion. I had thought that all three analysis begin with more or less the same instrumental record. Are different recalibrations being applied in the different analysis? [R Allyn Clarke]	Noted: The only data that need to be systematically corrected for instrumental data are XBT data. This correction has been done as part of the computation of the XBT temperature data before vertical interpolation to standard depth levels. Observed depth level data (the original data) are left unchanged.
5-257	A	7:45		rather than just saying "we are confident the heat content estimates can be used in climate change studies", this section should conclude with a clear statement of what the section has shown. [Stephen R Rintoul]	Noted: Will add statements that indicate what this section has shown per the reviewers comment.
5-258	A	7:45		There was indeed cooling in the surface temperatures, see chapter 3. This includes SSTs globally (relative to the otherwise upward trend). [Kevin Trenberth]	Rejected: This Comment makes no sense and my guess is that it is mis-numbered
5-259	A	7:47	7:48	Sampling is also a major potential source of decadal variability. [Kevin Trenberth]	Noted: Quantitatively these estimates have changed a data have been added but qualitatively the results have not changed.
5-260	A	7:47		The oceans are heating but shouldn't we try to show that heating comes from the atmosphere by excluding the heat from deep vents? I guess one line with a reference will do. [Michael Tsimplis]	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-261	A	7:49	7:49	Caption heading for 5.2.5 refers to three global data sets. While this is technically correct if it refers to the heat content estimates, it could suggest to a reader that these are based on three independent ocean data sets. Should refer to three differennt global analysis. [R Allyn Clarke]	Noted: The reviewer is partly correct in that all 3 heat content estimates use much of the same *in situ* data. However Willis also uses altimeter data (indirectly) and Ishii uses synoptic SST data multiplied by climatological MLD.
5-262	A	7:50	7:50	It would be good to complete this section by linking near surface heat content changes to SST changes discussed in Chapter 3. Are they consistent - do we expect them to be consistent? [Chris Folland]	Noted: This is new work. It is "doable" but because of the large heat capacity of the ocean there does not have to a correlation. As pointed out by Levitus et al. (2000) most of the change in heat content of the N. Atlantic occurs beneath 300 m depth.
5-263	A	7:53	7:53	Run off changes can also change salinity [Wilco Hazeleger]	Accepted: We agree with the reviewer we should add river run-off and also melting sea-ice to the text.
5-264	A	7:53	8:10	This discussion more naturally belongs with the more detailed discussion of changes in water mass properties [Helene Banks]	Noted: The authors states that the discussion of salinity variability "more naturally belongs" in the discussion of water mass properties. We will try to achieve better integration. As we noted in our response to Comment 5-233, "The salinity variability documented in section 5.2.3 is the result of a global analysis of salinity data from one global database. Despite the inadequacies of this database it is a well-documented, consistently analyzed set of data and represents a first step towards systematic study of the ocean component of earth's freshwater balance." The water mass material is

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					now in section 5.3 of SOD.
5-265	A	7:53	8:10	It would be useful to the non-oceanographer to express these salinity changes in terms of changes in fresh water content. [R Allyn Clarke]	Noted: This could be done but we do not think it is necessary. It is important for readers to see what the changes are in terms of the original measurements, not a derived quantity.
5-266	A	7:53		The description of figure 5.2.6 lacks quantitative estimates of the absolute and/or relative observed changes in ocean salinity. Some estimates taken from the figure could be given in order to provide with some information on the magnitude those changes. [Philippe Tulkens]	Noted: The absolute changes in salinity can be obtained by multiplying the values in the figure by the length of the record. Our interest is in presenting the linear trend while acknowledging the non linear interdecadal variability.
5-267	A	7:54	7:54	Add units to caption of Fig 5.2.6 [Chris Folland]	Accepted: Units will be added to the caption for Fig. 5.2.6
5-268	A	7:54		What are the error bars on the linear trends in Fig 5.2.2? [Kevin Trenberth]	Noted: We referred to the % variance accounted for by the linear trends in the original papers that these figures appeared in.
5-269	A	7:56	8:1	It seems as if these three sentences are trying to give the reader an impression of how much to trust the results of Boyer et al. They could be replaced by "Most of the variance in the North Atlantic and southern hemisphere bottom waters can be accounted for by the linear trend." Another option would be to remove the three sentences altogether. [Melissa Bowen]	Noted: This Comment is confusing. We are simply presenting facts. We did not discuss bottom waters at all. No other reviewers commented on this. We feel no change is required.
5-270	A	7:57		clarify wording: "one third of the size of the data set used for the ocean heat content estimates" [Stephen R Rintoul]	Noted: We can clarify. 2.3 million profiles is about 1/3 of 7.1 million profiles.
5-271	A	8:0	8:	Section 5.2.5.1: There is no basis to believe the surface fluxes in the NCEP/NCAR dataset and there are many publications demonstrating that they are in fact not credible at all. They bear little relation to reality. The salinity changes need to be integrated with 5.3. [Kevin Trenberth]	Noted: We will try to achieve better integration with Section 5.3 as we stated in Comment 264.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					The reviewer states that the NCEP/NCAR fluxes are not believable. It would have been helpful if he suggested other possible datasets to use, if in fact any such flux estimates are reliable to the degree required.
5-272	A	8:0	9:	Sections 5.2.5.2 and 5.2.5.3: There are several other studies documenting means and variability of P-E or latent heat fluxes based on atmospheric moisture budgets: [Kevin Trenberth]	Noted: It would have been helpful if he suggested other possible datasets to use, if in fact any such flux estimates are reliable to the degree we need.
5-273	A	8:1	8:1	given in original paper'. either drop this or say what it shows. [Philip Woodworth]	Noted: The reviewer wants us to drop our statement "The percent variance accounted for by the linear trend in each basin and the world ocean is given in the original paper." He gives no reason and we see no reason to drop this statement.
5-274	A	8:1		add % variance explained numbers to caption of Fig 5.2.6 [Stephen R Rintoul]	Rejected: This suggestion makes no sense.
5-275	A	8:1		Why mention things that are left unstated but can be found in the original paper? Either delete this phrase or re-state what was in the original paper here. While on the subject, a lot more attention is given to heat content than freshwater or salt content. There is only 1 paragraph about salinity before the text returns to heat content again. Is there, for example, an estimate of the global mean salinity content change? Is this showing a decrease consistent with melting of glaciers and/or permanent sea ice? [Joyce Terry]	Noted: a) Similarly to Comment 5-273 the reviewer wants us to drop the statement. We disagree. b) More attention is given to heat content than freshwater or salt content because there is much more temperature data to work with. The salinity estimates are much more uncertain. c) We are reorganizing the text so that all heat content work is sequential.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-276	A	8:4	8:4	Here, reference should be made to the study Influence of the Atlantic Subpolar Gyre on the Thermohaline Circulation by H. Hatun, A. B. Sando, H. Drange, B. Hansen, and H. Valdimarsson Science, 16 September 2005 (vol 309, pages 1841-1844) that shows that there has been a significant increase in salinity in the northeastern Atlantic off of Norway and Iceland driven by circulation changes, so that freshening has not occurred everywhere. [LuAnne Thompson]	Noted: Fig. 5.2.6 shows that freshening has not occurred everywhere in the North Atlantic, and we refer to the this paper. The zonal average at these latitudes does show freshening.
5-277	A	8:8	8:10	The mid-latitudes seem to extend on the Northern hemisphere from the equator to 30 N, and on the souther hemisphere to 50 S, according to fig. 5.2.6d. Please refer more specific to latitudes. [Hendrik M. van Aken]	Noted: We have eliminated this sentence.
5-278	A	8:9	8:10	Change "mid-latitudes" to "sub-tropical" and "polar" to "sub-polar" for consistency with Figure 5.2.6d. [David Parker]	Noted: We have eliminated this sentence.
5-279	A	8:9		correct: "mid-latitudes" to "latitude" [Hartmut Grassl]	Noted: We have eliminated this sentence.
5-280	A	8:16	8:25	This adds discussion, but not necessarily clarity. [Jerry Mahlman]	Noted: The reviewer states " This adds discussion, but not necessarily clarity." We disagree
5-281	A	8:17	8:17	heat of fusion' ok? [Philip Woodworth]	Noted: Seems O.K. What is the reviewer's point?
5-282	A	8:17		"different components of …various components of" is awkward wording [Stephen R Rintoul]	Accepted: We agree the wording is awkward and we will revise.
5-283	A	8:20		why "possible" increase in heat content? [Stephen R Rintoul]	Noted: "Possible" is used because of the uncertainty as to whether continental glaciers are melting or adding mass".
5-284	A	8:23	8:23	critical metric' ok? why not just 'is a critical factor'? [Philip Woodworth]	Noted: We have changed "metric" to "variable".

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-285	A	8:29	8:57	page 8, lower half: I found this hard going [Philip Woodworth]	Noted, text rewritten/shortened
5-286	A	8:32	8:32	"VOS" should be defined [Melissa Bowen]	accepted
5-287	A	8:32	8:33	Using the word biases implies to me that these errors or uncertainties are systematic rather than random errors. Is this the correct interpretation of this literature arising out of the work of the WG ON Air-Sea fluxes? [R Allyn Clarke]	Noted, text modified
5-288	A	8:32	8:49	Here or somewhere else the trend in the NAO should be discussed, as many of the trends in other properties in the Atlantic have been linked to this. [Stephen R Rintoul]	Noted, NAO is discussed mainly in chapter 3
5-289	A	8:32	8:49	It might be useful to define the sign of heat fluxes, e.g. Positive fluxes mean ocean cooling/heating. This would be particularly helful for IPCC public audience [Michel Rixen]	Accepted – flux sign will be defined and co-ordinated with the Chapter 3.
5-290	A	8:32	8:32	both reanalyses'. will a reader know what reanalyses are? [Philip Woodworth]	Accepted – there will be short explanation what reanalyses are.
5-291	A	8:35		Insert after Josey, 2003): System bias is a major problem for all heat flux products (Yu et al 2005). The system bias has the largest effect on the realistic representation of the low frequency (decadal and longer timescales) variability of air-sea exchanges. Satellite derived turbulent heat fluxes (Chou et al. 2001; Kubota et al. 2002) and radiation budgets derived from the International Satellite Cloud Climatology Project (ISCCP) (Zhang et al. 2004) improve considerably the variability of heat fluxes, but there is still the problem of mean bias (e.g. Curry et al. 2004). This is due to the fact that satellite data require validation with ground truth, while in situ flux buoys are limited in both space and time. One promising approach to overcome the problem is the objective analysis that synthesizes flux related variables fields from satellite observations, in situ measurements, and atmospheric model reanalyses (Yu et al. 2004a;b). End of Paragraph, begin with a new paragraph following: "Reanalysis of surface fluxes ..." [Joyce Terry]	Noted. Satellite observations are still represented by quite short time series. Some mentioning of SSM/I latent flux possible with serious caveats concerning the accuracy of the product. 10-year WNOI fluxes of Yu et al. (2004) are short and have not yet been analysed in a view of climate change (at least I do not know such an analysis)
5-292	A	8:40	8:42	It is likely that the NCEP surface fluxes will have large errors over the northwest Atlantic, thus the errors may not be solely due to sampling biases in the VOS fluxes. (Note the acronym VOS should be defined when it is first used – line 32 on this page?). [Michael Alexander Alexander]	Accepted
5-293	A	8:42	8:45	The errors in the time dependent P-E fluxes are likely to be very great. In addition,	Noted – the tenor concerning

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				processes such as advection (e.g. the movement of the great salinity anomaly) and ice melt are likely to complicate the fresh water balance in the subpolar North Atlantic. Thus how confident are we that P-E is the key driver of the salinity budget in the Atlantic? Note: The impact of the atmospheric hydrologic cycle on the salinity budget is stated as "confident" in the summary (lines 55-56 page 5-35) but the evidence isn't presented here to warrant that confidence. [Michael Alexander Alexander]	confidence about accuracy of the estimates of the will be changed.
5-294	A	8:42	8:49	This assessment implies a consistent trend over the past 40-50 years, but at least one longer-term study raises the question of whether this trend is not part of a longer term oscillation (See: Bryden, H.L., E.L. McDonagh and B.A. King, (2003): Changes in ocean water mass properties: Oscillation or Trends? Science, 300, 2086-2088.) A discussion of this and similar findings needs to be included in this section. [Lenny Bernstein]	Accepted – the work will be mentioned in the context of long-term variations which might be seen as trends and is dicussed in Section 5.3.4 of SOD.
5-295	A	8:42	8:49	Bryden, H.L., E.L. McDonagh and B.A. King, (2003): Changes in ocean water mass properties: Oscillation or Trends? Science, 300, 2086-2088, provides one of the few long term studies of ocean water properties and they find oscillating behavior in the Southern Indian Ocean. As the title of their paper implies, they raise the question as to whether the warming of the oceans seen since the 1950s is a trend or part of long-term cyclic behavior. This question need to be addressed in this section. [Jeffrey Kueter]	See 5-294
5-296	A	8:45		nsert " of the North Atlantic" after "gyre". At the end of this sentence insert the following before "in the North Pacific..." Mignot and Frankignoul (2003) showed the importance of advection in the time time dependent salinity anomaly balance over surface forcing for much of the basin. SInce there is no negative feedback on salinity from the atmosphere, surface salinity anomalies can last longer than temperature anomalies (Hall and Manabe, 1997) and can therefore play a significant role in decadal and inderdecadal climate variability. [Joyce Terry]	Importance of Ekman advection will be likely mentioned.
5-297	A	8:47	8:47	Aleutian low is to be corrected as Aleutian Low [kyung-ryul Kim]	Noted, changed
5-298	A	8:51	9:31	Section 5.2.5: This section was very repetitive (eg, Curry et al., 2003) is referenced in both the surface fluxes section and the meridional fluxes of freshwater section. This could do with some reorganistion to make the text more coherent. The authors could consider showing the observed heat and freshwater meridional transports. This would allow the authors to summarise the observations and the error bars for both heat and freshwater transport.	Noted, text significantly shortened

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Helene Banks]	
5-299	A	9:0		Refs include: Trenberth, K. E., and C. J. Guillemot, 1998: Evaluation of the atmospheric moisture and hydrological cycle in the NCEP/NCAR reanalyses. Climate Dyn., 14, 213–231. Trenberth, K. E., J. M. Caron and D. P. Stepaniak, 2001: The atmospheric energy budget and implications for surface fluxes and ocean heat transports. Clim. Dyn., 17, 259–276. Trenberth, K. E., D. P. Stepaniak and J. M. Caron, 2002: Interannual variations in the atmospheric heat budget. J. Geophys. Res., 107(D8), 4066, 10.1029/2000JD000297 Trenberth, K. E., J. M. Caron, D. P. Stepaniak and S. Worley, 2002: Evolution of El Niño Southern Oscillation and global atmospheric surface temperatures. J. Geophys. Res., 107(D8), 4065,10.1029/2000JD000298. Trenberth, K. E., and D. P. Stepaniak, 2003: Co-variability of components of poleward atmospheric energy transports on seasonal and interannual timescales. J. Climate, 16, 3690-3704. [Kevin Trenberth]	Noted. Some references of this list will be used.
5-300	A	9:1	9:2	"Qualitatively agree" should be quatified (I.e., any they similar enough to reduce the heat flux biases of 30 W/m**2? [Robert Molinari]	Noted, text changed
5-301	A	9:6	9:7	The recent results of Bryden et al (2005, Nature, now accepted I believe) suggest that the MOC and heat transport at 24N have decreased substantially in 1998 and 2004, compared to the earlier sections. This result will need to be discussed and interpreted here. [Richard Wood]	accepted
5-302	A	9:6		There are further reoccupations of the 24N section after 1992 and published accounts of continuing changes in the Atlantic. See Vargas-Yanez et al, GRL, doi:10.1029/2003GL019308, 2004. [Frank Bryan]	Noted, section revised
5-303	A	9:7	9:7	symbol of degree is omitted for 43-48N [kyung-ryul Kim]	noted
5-304	A	9:7		Bryden et al. (Nature, in review) states that a change has occured beginning in 1998 and extending until 2003. I don't know the satus of this mamuscript, but if it is accepted, this statement needs modification. [Joyce Terry]	accepted
5-305	A	9:9	9:9	symbol of degree is omitted for 48N [kyung-ryul Kim]	noted
5-306	A	9:10		Double the error figures to 95% confidence : 0.2±0.24 [Vincent Gray]	Noted, see 5-137

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-307	A	9:11	9:14	These lines are an example of model interpretation of observations. Model interpretation should either be used throughout the chapter (including interpretation of water mass changes) or not at all [Helene Banks]	Will be accepted as was recommended during the discussions in NZ – general spirit – to explicitly say when we are dealing with model interpretations of observations
5-308	A	9:19	2:19	Change "a little" to "little". [Trevor McDougall]	Noted
5-309	A	9:19		Take out the a. [Peter Gent]	Noted
5-310	A	9:22	9:23	First sentence seems a bit obvious. Removing it and starting with the second sentence "The salinities in the North Atlantic…" would get to the point more quickly. [Melissa Bowen]	Noted
5-311	A	9:22	9:31	I agree with this presentation. Other lines of atmospheric evidence are consistent with this reasoning. [Jerry Mahlman]	Noted
5-312	A	9:22	9:23	I cannot folow how changes in the freshwater fluxes imply an imbalance in these fluxes. Should it be "Changes in the freshwater content .... imply an imbalance in the fluxes"? [Hendrik M. van Aken]	Noted, will be reformulated
5-313	A	9:22		"Changes in the freshwater flux... imply an imbalance in the fluxes" is not very informative [Stephen R Rintoul]	See 5-312
5-314	A	9:25		There is no basis here for referring to "increased hydrological cycle" or how the hydrological cycle may or may not respond to global warming. What can be done is to relate the changes in salinity to changes in rainfall and moisture transports. Any discussion of the hydrological cycle should deal with all aspects. [Kevin Trenberth]	More neutral formulation will be provided
5-315	A	9:27	:31	I would like to see a discussion of where the additional freshwater comes from. [John Church]	We will attempt to provide some discussion
5-316	A	9:28	9:31	These last two sentences have a lot of numbers which need a reference. Inserting "they find" after "Remarkably" would tie these results to Curry and Mauritzen (2005). [Melissa Bowen]	Noted
5-317	A	9:31	18:45	Much of this section (5.3) deals with "modes" (NAO, AMO, IOD, etc.) and their relationship with water mass properties. So there is a large amount of detail on the time evolution (decadal variability/trends in the modes and in properties of water masses. However, little attempt is made to link this variability to anthropogenic forcing. My recommendation is that this section could be significantly reduced to focus on signal	Noted. Introduction changed. Section 5.3 restructured.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				linked to global warming, and/or output from GCM experiments should be used to help explain why these signals should be considered in the IPCC report (see the following section on sea level as example of how this can be done). [Michael Alexander Alexander]	
5-318	A	9:33	19:4	My principal problem with Chapter 5 is that in locations where we have good historical measurements (Indian Ocean at 32 S, Atlantic Ocean at 25 N) we have seen long period oscillations (rather than trends) in water mass properties with freshening of Subantarctic Mode Waters at 32 S from 1936 to 1987 and then higher salinities from 1987 to 1995 to 2002 (Bryden, H. L., E. L. McDonagh and B. A. King. 2003. Changes in ocean water mass properties: Oscillations or trends? Science, 300, 2086-2088) and with warming at 25 N from 1957 through 1998 and cooling to 2004; we have observed a strengthening of the gyre circulation from 1987 to 2002 at 32 S (Palmer et al, 2004) and from 1992 to 2004 at 25 N with associated increase in thermocline oxygen concentration (McDonagh et al., 2005). The oscillations in the southern Indian Ocean SAMW properties were contrary to the HadCM3 simulations by Banks and Woods (2002) under anthropogenic forcing. In this chapter, there remains the impression that there are trends in water mass properties and decreases in gyre circulation, ventilation and oxygen concentration. The Banks and Wood paper is complimented (page 5-4, line 16, a sentiment I agree with even if it did not prove to be correct in its Indian Ocean application). The Bryden et al. paper is not referenced though it is generally cited by "antis" as evidence for oscillations rather than anthropogenic change. If I were an "anti" I would find much to criticise in the selective reporting in Section 5.3. [Harry Bryden]	Accept, section 5.3 is much revised. Bryden et al 2003 paper is referenced.
5-319	A	9:33		Section 5.3. This section summarizes a lot of interesting information, but contributes relatively little to the Executive Summary of the chapter because there are not that many well established changes in ocean circulation and water mass formation that might be related to Global Change. For this reason, I would recommend an effort to streamline and shorten the text where possible. [Gerrit Burgers]	noted: section restructured, interpretation moved to a new summary and most important results included in executive summary
5-320	A	9:33		Overall: it does not cover enough materials of ocean climate change (they are there and they should be the focus of this section) but too much emphasis is put on the NAO, i.e. too much of variability. There is too little of the Arctic, too much of Mediterranean. The whole section could be better focused on ocean heat uptaking and changes in the hydrological cycle. Some paragraphs are not very easy to read and understand. [Peili Wu]	noted: Others (Church) think not enough emphasis on natural modes, such as NAO. More Arctic included. Section better summarized.
5-321	A	9:37	9:40	Four "modes" of climate variability are listed ENSO, NAO, PDO and SAM and in the subsequent sentence these modes are said to "drive" the climate system. ENSO is clearly	noted: First paragraph rewritten. Interpretation moved to a summary

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				a coupled phenomena and the PDO is defined solely in terms of SST, thus these two features can not "drive" the ocean. I also believe that statements like these give the impression that we can explain almost all climate variability by a handful of patterns and some of these patterns are statistical measures of climate rather than a fundamental physical mode of the system. In addition, there are other climate patterns, such as the West Pacific pattern, Pacific North American pattern, that impact the ocean, while the AMO is mentioned in the following paragraph and the IOD later in the section. [Michael Alexander Alexander]	section.
5-322	A	9:37	9:44	This discussion merely discusses patterns of natural variability, but doesn't speak directly to the IPCC Assessment-relevant aspects of the needed ocean analyses. [Jerry Mahlman]	noted: see above.
5-323	A	9:37	9:44	The wording of this paragraph implies the ocean plays a purely passive role, even in ENSO (ie the ocean responds to the atmosphere, with no feedback). [Stephen R Rintoul]	Accept
5-324	A	9:37	9:38	This is a complicated but trivial way of stating something. All processes with water motion control the ocean's response. But how about warming due to an a decrease in the outgoing IR radiation, or freshening due to increased precipitation? [Hendrik M. van Aken]	Accept
5-325	A	9:38	9:40	I would add the Atlantic Multidecadal Oscillation to the mentioned modes. I would also add "(see section 3.6)" at the end of the sentence. [Roxana Bojariu]	Accept and now included in Atlantic section 5.3.2
5-326	A	9:38	9:38	Shorten text as "… the main modes of climate variability: El Nino Southern Oscillation (ENSO); etc." [Robert Marsh]	Accept
5-327	A	9:38	9:40	Please see Trenberth et al 2005 for the modes of variability established in a rigorous way: they include the NAM, cross reference also chapter 3: Trenberth, K. E., D. P. Stepaniak and L. Smith, 2005: Interannual variability of the patterns of atmospheric mass distribution. J. Climate, 18, 2812-2825. [Kevin Trenberth]	Noted: referencing chapter 3 now.
5-328	A	9:38		Is it resolved that NAO, ENSO PDO etc are independent of climate change. This sections suggests indirectly that they are. [Michael Tsimplis]	noted: rewritten.
5-329	A	9:39	9:39	You are not going to include the Northern Annular Mode (NAM) in this list? [R Allyn Clarke]	Accept. Cross-reference to chapter 3.
5-330	A	9:39	9:40	For consistency with Chapter 3, add after the NAO, the Northern Annular Mode (NAM), and replace PDO with PDO/IPO (Interdecadal Pacific Oscillation).	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Chris Folland]	
5-331	A	9:39	9:40	Citations for these "modes" are needed, as many readers will not be familiar with them. [Franklin Schwing]	Accept
5-332	A	9:40	9:44	« These modes drive the ocean, causing changes in … » and « examines the climate modes driving these changes. » I think these statements are too strong and I am not convinced that it is clearly established that main variability modes drive the ocean changes. [Pascale Delecluse]	noted: rewritten
5-333	A	9:40	9:40	Suggestion: These modes drive variability in the ocean and cause changes…. [Wilco Hazeleger]	noted: rewritten
5-334	A	9:42		add- While the modes are generally condsidered to be specific to a particular ocean basin, global atmospheric teleconnections link these modes of climate variability across oceans (Chiang et al. 2002, Schwing et al. 2003). Full citation- Schwing, F.B., J. Jiang, and R. Mendelssohn. 2003. Coherency of regime shifts between the NAO, NPI, and PDO. Geophys. Res. Lett. 30: 1406, doi:10.1029/2002GL016535., Chiang et al. already cited. [Franklin Schwing]	noted: rewritten
5-335	A	9:48	9:56	With all of the discussion of "modes"(See also Chapter 3), I would bet that the critical "new signal" is an increased poleward flux ofatmospheric water vapour from the warmed oceans. See also Page 10-11. I am betting that the final IPCC assessment product will de-emphasize this mode-think, and will choose to focus on the evidence for systematic anthropogenic change in the climate system. After all, we have still to understand how climate will warm and how it is radiatively forced. The detection of regional-scale secular "trends" is a very challenging problem, well beyond the scope of this Chapter. Because of this, I am glossing over the long, mode-oriented discussions that have the net effect of evading the assessment of onrushing climate warming. I don't see how this Chapter can duck this problem. Certainly, the Chapter 3 authors are almost certainly grappling with this, to their frustration. Mother nature is not always open to our quest for deep understanding. [Jerry Mahlman]	Accept. Streamlined and rewritten.
5-336	A	9:48	11:47	This very much written in terms of driving by the NAO. Other mechanisms are hardly mentioned. Although much work has been done internal variability and stochastically forced variability. [Wilco Hazeleger]	Accept
5-337	A	9:49	9:49	"decadal signals from the North Atlantic Oscillation (NAO)" : why decadal? NAO has several temporal scales not easily to be distinguished [Michel Rixen]	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-338	A	9:50	9:50	I don't think the Kerr paper is a peer reviewed article. Knight et al 2005 (already cited) would be a good reference here. (just published in GRL) [Richard Wood]	Accept
5-339	A	9:50		The Kerr (200) reference is questionable as it is not a research article, but rather a news report. Should preferably cite one of the original studies he mentions. [Frank Bryan]	Accept
5-340	A	9:50		Kerr, 2000 is not a solid reference for this statement [Pascale Delecluse]	Accept
5-341	A	9:50		Kerr 2000 is a news article not a research paper. Please see chapter 3 for full discussion of AMO. [Kevin Trenberth]	Accept
5-342	A	9:52	9:54	The subtropical gyre is always warm, and the subpolar gyre is always colder, irrespective of the NAO. Some reference should be added to relate warm and colder to something like the long term mean status of the Atlantic. [Hendrik M. van Aken]	noted: Reworded as anomalies
5-343	A	9:56	9:56	Add a reference (already cited): "…Deep Water (NADW) (Dickson et al. 2002)." [Robert Marsh]	Accept
5-344	A	10:0	11:	The presentation is done by vertical water masses rather than geographically. This is not optimal since the analysis is finally global and regions of water mass formation are more or less discussed . the discussion of the intermediate waters is interesting and refers to what is happening in the adjacent seas : the Mediterranean Sea and the Nordic Seas, which are discussed later. It may be possible to modify the structure : start with the surface, then the adjacent seas, and finally, their contributions to intermediate and deep waters ? twice. [Pascale Delecluse]	noted: section has been restructured, but we decided to retain the basin organization.
5-345	A	10:0	11:0	With all of the discussion of "modes"(See also Chapter 3), I would bet that the critical "new signal" is an increased poleward flux ofatmospheric water vapour from the warmed oceans. See also Page 10-11. I am betting that the final IPCC assessment product will de-emphasize this mode-think, and will choose to focus on the evidence for systematic anthropogenic change in the climate system. After all, we have still to understand how climate will warm and how it is radiatively forced. The detection of regional-scale secular "trends" is a very challenging problem, well beyond the scope of this Chapter. Because of this, I am glossing over the long, mode-oriented discussions that have the net effect of evading the assessment of onrushing climate warming. I don't see how this Chapter can duck this problem. Certainly, the Chapter 3 authors are almost certainly grappling with this, to their frustration. Mother nature is not always open to our quest for	noted: Same comment as 5-335.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				deep understanding. (From comment on p. 9, lines 48-56.) [Jerry Mahlman]	
5-346	A	10:1	11:47	This section needs writing in a clearer and less technical way, focused on the main messages for policymakers' advisors. The key stories are not easy to understand at present.Fig 5.3.2a is very difficult to see. [Chris Folland]	Accept.
5-347	A	10:3		what are "primarily warming tendencies"? [Stephen R Rintoul]	Reject. Don't understand problem. Now re-phrased in SOD 5.3.2.1.
5-348	A	10:6	10:9	I did not understand most of this sentence which requires much more discussion. Tropical SST where? What is the atmospheric variation? What is the meridional gradient mode and where is it? Rainfall variability where? Explain the Atlantic El Nino - not very familiar to many. [Chris Folland]	noted: See next suggestion from Joyce.
5-349	A	10:6		replace with this: "Quasi-decadal variations of tropical SST, induced by and perhaps coupled with atmospheric variability represents a superposition of the meridional gradient mode (Endfield, et al. 1999, Joyce et al., 2004) driven by off-equatorial changes in the trades, and cross-equatorial meridional winds (Moura and Shukla, 1981), and a zonal mode driven by zonal equatorial winds (Zebiak, 1993). The former is associated with rainfall variability in the boreal Spring (Ruiz-Barraddas et al., 2000, Wang, 2002) and the latter boreal summer rainfall variability. The zonal mode is thought to be a forced mode, while some aspects of the gradient mode may involve coupled atmospherie/ocean dynamics (Chang et al., 1997, 2001, Carton et al., 1996, Xie, 1999). The last manifestation ..." (as written) [Joyce Terry]	noted:use streamlined version of this contribution.
5-350	A	10:7	:10	writing that the meridional gradient mode and the equatorial mode are driven by rainfall variability is wrong. Both modes are associated with a redistribution of atmospheric cells, and thus a redistribution of precipitation but it is certainly not a driving force in equatorial region. A more careful reading of the referred literature is needed. [Pascale Delecluse]	Accept
5-351	A	10:7		This section introduces a number of new climate modes. Perhaps a centralised summary of known modes of variability would be useful. [Stephen R Rintoul]	noted: reduced amount of "mode" description, moved interpretation to a new summary.
5-352	A	10:7		The term "mode" is used incorrectly here, also line 9. It is not generally accepted to use the term Atlantic El Nino. [Kevin Trenberth]	Noted.
5-353	A	10:14	10:14	It would be appropriate to discuss South Atlantic variability here. Venegas et al. 1998 (J. Geoph Res., 103, 24.723-24.736) show a South Atlantic dipole in SST on interannual to	Accept (adding S. Atlantic here). noted: references etc.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				decadal time scales. Sterl and Hazeleger 2003 (Clim Dyn. 21, 559-571) show that this dipole is forced by latent heat fluxes related to changes in the subtropical high. Water masses along the South Equatorial Current subduct and pathways connect them with the tropical thermocline (Hazeleger et al. 2003, J. Phys. Oceanogr.33, 677-693). Also water mass anomalies can propagate along these pathways toward the north (Lazar et al. 2002 J. Geoph. Res., 107, 3104). [Wilco Hazeleger]	
5-354	A	10:16	10:24	Please see chapter 3 for changes in the NAO. [Kevin Trenberth]	Accept
5-355	A	10:16	10:47	Shouldn't the material here, especially on the AMO and salinity changes be linked to the THC discussion and consistent with it? [Kevin Trenberth]	Accept
5-356	A	10:16	10:16	This should be in the North Atlantic etc.." Thereis also a subtropical gyre in the South Atlantic! [Hendrik M. van Aken]	Accept
5-357	A	10:16		specify « northern » for the Atlantic subtropical gyre. [Pascale Delecluse]	Accept
5-358	A	10:16	:19	very difficult to read and understand. that first sentence needs to be rewritten. [Peili Wu]	Accept
5-359	A	10:19	10:24	The response of STMW to NAO forcing has been shown by Hazeleger and Drijfhout 1998 (J. Phys. Oceanogr. 28, 266-288). However, other mechanisms cannot be discarded here. The role of stochastic forcing in generating low frequency variability is highlighted by Hasselmann 1976 (Tellus 28, 473-485) and can even excite oscillatory modes as shown by Griffies and Tzipermann 1995 (J. Climate, 8, 2440-2453). Also for STMW the role of stochastic forcing has been shown (Hazeleger and Drijfhout 1999, J. Phys. Oceanogr., 29, 1772-1786). Finally, internally generated mechanisms may play a role as well such as shown by Delworth et al 1993 for the THC (J. Climate, 6, 1993-2011) and by Hazeleger and Drijfhout 2000 for STMW variability driven by eddies (J. Geoph. Res., 105, 13.965-13-979). [Wilco Hazeleger]	Noted, and references have been included in the SOD.
5-360	A	10:23	10:24	Cross refer to Chapter 3 for recent changes in NAO. [Chris Folland]	Accept
5-361	A	10:23	10:24	The existing text is vague and incorrect. I suggest "The NAO index increased to the mid-1990s and has somewhat declined since then (see http://www.cru.uea.ac.uk/~timo/projpages/nao_update.htm)." [referencing a website updating the NAO index] [Robert Marsh]	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-362	A	10:23	10:24	The NAO index increased to the early 1990s but has declined since then (Section 3.6.4). [David Parker]	Accept
5-363	A	10:23	10:24	replace with "index has increased to the early 1990s and has since begun to decline." [Joyce Terry]	Accept
5-364	A	10:26		What do "gulfstream waters" mean? "MOC brings gulfstream waters to the north Atlantic current" does not sounds right. I would put it something like: the upper limb of the MOC carries tropical/subtropical waters to the sub-polar high latitudes. [Peili Wu]	Accept
5-365	A	10:27	10:27	change "region and North Atlantic Current" to "region via the North Atlantic Current". [Michael Alexander Alexander]	Accept
5-366	A	10:27	10:27	The North Atlantic Current is formally part of the upper limb of the MOC, hence cannot "brings waters to itself". I suggest deleting the text "and North Atlantic Current". [Robert Marsh]	Accept
5-367	A	10:29	10:30	the impact of surface fluxes associated with the NAO is much greater than the 0.2ºC stated here, Its more like 0.5º-8ºC (Seager, R., Y. Kushnir, M. Visbeck, N. Naik, J. A. Miller(Velez), G. Krahmann, and H. M. Cullen, 2000: Causes of Atlantic Ocean climate variability between 1958 and 1998. J. Climate, 13, 2845-2862). [Michael Alexander Alexander]	Noted
5-368	A	10:34		In the absence of a basin wide array like RAPID, the Florida Current transport provides the best available long time series of the upper limb of the MOC. Baringer and Larsen, GRL, 28(16), 3179-3182, 2001 presents a long time series of this index and shows that it is significantly correlated with the NAO. [Frank Bryan]	noted
5-369	A	10:36	10:43	This paragraph repeats much of the information on the previous page (5-9 lines 48-56). [Michael Alexander Alexander]	Noted and the overlap in salinity changes has been removed between 5.2 and 5.3.
5-370	A	10:41		"quasi-decadal"?? [Kevin Trenberth]	Reject
5-371	A	10:43	10:43	In addition to the well-known freshening of the subpolar gyre, since the mid-1990s increasing salinity has been observed in the Atlantic inflow to the Nordic Seas (Hatun et al. 2005). This appears to be related to a change in the position of the North Atlantic Current, and may stabilise the MOC in spite of other changes (warming, other sources of freshening). I suggest the additional text (after line 43): "Since the mid-1990s, the salinity of Atlantic inflow to the Nordic Seas has increased to the highest values since observations started in 1948, largely due to a change in the shape of the subpolar gyre which allowed more warm water into the Nordic Seas (Hatun et al. 2005)." Reference:	Noted, text revised

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Hatun, H., A. B. Sando, H. Drange, B. Hansen, and H. Valdimarsson, 2005: Influence of the Atlantic subpolar gyre on the thermohaline circulation. Science, 309, 1841-1844. [Robert Marsh]	
5-372	A	10:45		AMO (?) [Hartmut Grassl]	noted: cross-reference chapter 3
5-373	A	10:45		"At multidecadal time scales, the AMO (Atlantic Multidecadal Oscillation) is seen in N. Atlantic basin changes in SST with time scales from 50-80 years. Amplitudes of temperature changes are 0.2 - 0.4 oC (Goldenberg et al., 2001) and have been associated in coupled numerical climate models with changes in the thermohaline circulation (Delworth and Mann, 2000; Jungclaus, et al., 2005; Knight et al., 2005), and with observed summer rainlfall variability (Enfield et al., 2001; McCabe et al., 2004). The phase of the AMO is such as to be a minimum in 1900-1920, 1970-1990, and positive in 1935-1955 and since 1995. Schlesinger and Ramankutty (1994), speculated that this mode could affect changes in global mean SSTs, for example the minimum in global heat content Fig. 5.2.1 from 1985-1990 could be due in part to the AMO. With coupled climate models all capturing this variability at about the same time scale, more will soon be learned about this important coupled natural variability in the climate system. [Joyce Terry]	noted
5-374	A	10:46		Define what you mean by "magnitude" [Vincent Gray]	Noted and paragraph now removed from SOD.
5-375	A	10:47	10:49	Mention the Knight et al (2005) study which supports this, though AMO warming started somewhat earlier from a preceding cold phase. [Chris Folland]	accepted
5-376	A	10:50	10:50	Reference to figure 5.2.2 is incorrect. I guess it should be 5.3.2. [Philippe Tulkens]	Accept
5-377	A	10:50	10:51	I do not undestanh ow Fig. 5.2.2., which shows the temperature change in the upper 1450 m can give information on changing properties of NADW. Possibly a wrong reference or a wrong figure? [Hendrik M. van Aken]	Accept: wrong figure reference
5-378	A	10:57		stopped around 1997 (Phillips and Joyce, 2005)... [Joyce Terry]	Accept
5-379	A	11:0	12:	Box 5.1 on THC: The thermohaline circulation is a coupled phenomenon and depends on surface fluxes. This box does not adequately discuss changes in precipitation (see chapter 3) or inferred P-E, and does not seem to account for the noted changes in salinity in lines 26-28 in the conclusion. Note also p 5-13 L 33-35. On lines 35-46 it discusses the weakening of the MOC, although this probably applies only till about 1990. Then it comes to completely	Noted, however the relation of deep salinity changes to surface flux changes is not obvious. Will be rewritten.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				contradictory conclusions in lines 49-50. The THC is missing in Figure 5.6.1. It should be reconciled with the material there. [Kevin Trenberth]	
5-380	A	11:0	12:	Box 5.1 on THC (continued): In lines 43-47 it deals with the AMO which has not been shown to be a mode, much less an oscillation. The evidence on changes in salinity and rainfall seems to support the idea of a change in MOC and perhaps AMO related to global warming, because the increased rainfall at high latitudes is associated with warming and increased water holding capacity of the atmosphere. Hence there is no basis for assuming a decrease in MOC from natural variability in the next decades as the observed decrease may not be natural. The Knight et al conclusion has no basis. Isn't it plausible also that the observed changes in salinity are associated with a slowdown in MOC and due to global warming? The comments here are not necessarily provable but they are plausible and there needs to be a more complete assessment of the THC as a coupled phenomenon and the discussion carefully worded. [Kevin Trenberth]	noted: Trenberth's discussion in chapter 3 refers to AMO, so we have not deleted it here. Now discuss contradictory evidence for changes in MOC.
5-381	A	11:0		The authors may want to consider moving the box discussing the thermohaline change earlier in the text, as it defines what the MOC is. Currently MOC is mentioned without being defined prior to page 5-11 [Michael Alexander Alexander]	noted: box is now moved to the end of section 5.3 to new summary (since it was expanded to include the southern hemisphere)
5-382	A	11:1	11:3	Reference to Curry et al. (2003) and Figure 5.3.1 seems more relevant in the preceding section on upper water property changes. [Robert Marsh]	Noted
5-383	A	11:6	11:6	I found Fig 5.3.1 confusing partly because the caption needs a lot of work. Need to explain "neutral density". The start to the caption of part C is unclear and the diagram itself is confusing. Time series diagrams should have x axes labelled on the left. [Chris Folland]	Noted, figure removed
5-384	A	11:8	11:23	This paragraph belongs more naturally with the discussion on the adjacent seas section 5.3.2.3 [Helene Banks]	Noted, text partly moved
5-385	A	11:10	11:10	Quantify "mid-depths". [Chris Folland]	Accept
5-386	A	11:11	11:13	Delete: In the last decade (1994–2003), a large warming (0.3 C) and salinity increase (0.6) were observed at Gibraltar (Millot et al., 2005) and of similar magnitude in the Bay of Biscay (Vargas- Yanez et al., 2002). I propose change by: [ALICIA LAVIN]	Noted, text modified
5-387	A	11:11	11:13	Intermediate waters down to the 1000 m depth in the mid-latitude eastern North Atlantic basin have warmed at very high rates during last decade. The reoccupation in 2002 of the	noted: see 386

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				eastern part of the WOCE A05 section (24.5° N), respect to the previous one in 1992 [Parrilla et al., 1994], revealed warming rates over 0.2°C/decade and as high as 0.4 °C/decade in some levels [Vargas-Yanez et al., 2004]. In the southern Bay of Biscay (?43° N), the monthly sampling of an standard section from 1993 onwards tracked warming rates from 0.2°C/decade to more than 0.3°C/decade in all levels, including the whole branch of East North Atlantic Central Water (ENACW) and, at least, the upper part and core of the Mediterranean Water (MW). MW warming in the Bay of Biscay, associated to a salt-increase of 0.05 decade-1, are probably related to the warming/salt-increase in both constituents of MW (ENACW and MOW) in the formation area (Gulf of Cadiz), with the former being the most important contributor [Gonzalez-Pola et al., 2005]. (Reference List in the corresponding place) [ALICIA LAVIN]	
5-388	A	11:12	11:13	Replace the (Vargas-Yanez et al., 2002) reference with a more recent reference (Gonzales-Pola et al., 2005). Reference: Gonzales-Pola, C., A. Lavin, and M. Vargas-Yanez , 2005: Intense warming and salinity modification of intermediate water masses in the southeastern corner of the Bay of Biscay for the period 1992-2003. J. Geophys. Res., 110, C05020, doi:10.1029/2004JC002367. [Robert Marsh]	Accept
5-389	A	11:18	11:20	Delete the sentence starting "For instance …" and modify the next sentence to start "Since the mid-1990s ...". Lines 18-20 discuss changes in Med Water, citing Rixen et al. (2005). Change in the Med Sea are more fully discussed in section 5.3.2.3 (where Rixen et al. is again cited). If the sentence is retained, WMDW should not be defined twice (p.11, line 18 & p.13, line 30). [Robert Marsh]	noted
5-390	A	11:21	11:23	In my opinion this is way too strong. I wish I had evidence that it were true, and in fact am trying to generate such evidence. The fact is that we don't really know how much changes in the Mediterranean just affect the properties of the water that is entrained in the overflows and how much actually goes towards changing the properties of the overflows themselves. I have seen no credible quantitative estimates of this, and the poor representation of overflows in the models renders them suspect. [Anand Gnanadesikan]	Accept
5-391	A	11:23	11:23	Should be noted parenthetically that water mass formation within the Mediterranean is discussed in more detail in section 5.3.2.3 [Melissa Bowen]	Accept
5-392	A	11:27	11:37	Paragraph can be dropped as this discussion of the short-term variations in the subpolar Atlantic are not very relevant for the IPCC. [Michael Alexander Alexander]	Reject: important finding about changes in regional salinity in area where IPCC modeling is vigorous and

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					directed
5-393	A	11:27	:37	I would like to see a discussion of where the additional freshwater comes from. [John Church]	noted: discussion of global salinity changes and assumed changes in atmospheric hydrological cycle that is implied by these changes will be improved
5-394	A	11:37	11:37	Add reference to the most recent (high resolution, realistic) model study which simulates the 1990s change in LSW formation and volume: "(Eden and Willebrand, 2001; Gulev et al., 2003; Marsh et al. 2005a)". Reference: Marsh. R., B. A. de Cuevas, A. C. Coward, A. J. G. Nurser, and S. A. Josey, 2005a: Water mass transformation in the North Atlantic over 1985-2002 simulated in an eddy-permitting model. Ocean Science 2, 1-18. [Robert Marsh]	Noted, and some references now in the text of SOD
5-395	A	11:39	11:47	Hatun et al (Science, vol 309, pp. 1841-1844 (2005) report increasing salinity in the Atlantic inflow to the Nordic seas in the past decade [David Parker]	Accept
5-396	A	11:39	:47	this paragraph needs improving. The second sentence reads awkward. The last sentence: "The freshening ...". Dickson et al. (2003, you have the reference already) don't find "convincing evidence yet of any significant, converted slowdown in the Atlantic overturning circulation". The Curry and Mauritzen (2005) paper should be mentioned here. They don't find any weakening of the MOC. It is noted in ref. (11) of Curry and Mauritzen (2005) that the 20% reduction was not persistent, may have stopped or even reversed. The paper by Wu et al. (2004, ref: Wu, P., R. Wood and P. Stott, 2004: Does the recent freshening trend in the North Atlantic indicate a weakening thermohaline circulation? Geophys. Res. Lett., 31,L02301,doi:10.1029/2003GL018584.) is probably relevant here. The changes in the Labrador Sea freshening should also be mentioned somewhere here. The recent freshening trend has stopped or even reversed. If you don't have a paper to quote, it is referenced in a paper by Wu et al. (2006) as personal communication from Igor Yahsyaev. (Wu, P., R. Wood, P. Stott and G.S. Jones, 2005: Deep North Atlantic freshening simulated in a coupled climate model. Progress in Oceanogr., to appear in a special issue edited by Yashayaev). [Peili Wu]	Noted
5-397	A	11:41	11:41	I don't think the link of the freshening NEADW/LNADW to the NAO is clearly established. Indeed Wu et al (2005) present a model simulation of these changes which is not dependent on the NAO. [Richard Wood]	Noted, however we have to report observations, not model simulations
5-398	A	11:41	11:41	Reference: Wu, P., R. Wood, P. Stott and G.S. Jones, 2005: Deep North Atlantic freshening simulated in a coupled climate model. Prog. Oceanogr. (submitted – Status:	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				under review but being chased for likely decision by December. A copy will be sent to the CLAs). [Richard Wood]	
5-399	A	11:41	11:41	Reference: Wu, P., R. Wood, P. Stott and G.S. Jones, 2005: Deep North Atlantic freshening simulated in a coupled climate model. Prog. Oceanogr. (submitted – Status: under review but being chased for likely decision by December. A copy will be sent to the CLAs). [Richard Wood]	Same comment as 398
5-400	A	11:42	11:43	The sentence that begins "In, contrast" is somewhat confusing as it states that the overflows have been stable but then goes on to indicate that there was a 20% change in the easternmost bank overflow. Isn't a 20% change large? Or is it that the volume transport through the Faroe channel is small so this 20% change does not impact the total of all of the overflows. [Michael Alexander Alexander]	Noted, text modified, and this overflow is only a small fraction of the total.
5-401	A	11:43	11:45	There are now several years more data since the Hansen et al paper was written, and I am not sure that the decreasing trend in the Faroe Bank Channel has continued. This is cited as Hansen Pers. comm. In the Curry and Mauritzen paper. May be worth checking with Bogi Hansen directly (presumably a grey literature report on the more recent observations would be admissible as the methods were peer reviewed). [Richard Wood]	Noted:
5-402	A	11:45	11:47	It would be good to make this text more consistent with the discussion in Chapter 9. The link of the freshening to a weakening MOC is not established or even widely believed, given that the freshening is at depth. Indeed see Wu et al. 2004 for a model simulation that reproduces the observed freshening over the past few decades but actually shows a slight strengthening of the MOC over the same period. Reference: Wu, P., R.Wood and P. Stott, 2004: Does the recent freshening trend in the North Atlantic indicate a weakening thermohaline circulation? Geophys. Res. Lett., 31, L02204 (status – published) [Richard Wood]	Noted, however chapter 5 is on observations
5-403	A	11:48	11:49	Suggested supplement for page 54 before [START OF BOX 5.1] by Walter Zenk Not only the coldest water masses of northern sub-polar regions contribute to the variability of the MOC of the whole Atlantic. Surface waters in high latitudes of the South Atlantic set the initial conditions for bottom water in the southern hemisphere. This extremely dense Antarctic Bottom Water (AABW) is formed by deep-reaching convection in the Weddell Sea (see 5.3.5.3). While it carries a time stamp of its variable formation process (Robertson et al, 2002) it spreads equatorward filling the deepest	noted: added to new discussion of S. Atlantic variability in Section 5.3.2.2.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				regions of the western basins of the South Atlantic. It enters the Brazil Basin through the narrow Vema Channel of the Rio Grande Rise at 31 S. Ongoing observations of the lowest bottom temperatures have revealed a slow but consistent increase of the order 2mK/a in the abyssal over the last 30 years. [INSERT attached new FIGURE 5.3.2a HERE] Fig 5.3.2a: {Fig could was not acpeted by this mask, must be send separately} Time series with error bar of the lowest potential temperature close to the bottom of the Vema Channel of the South Atlantic (31 S, 39 W), adopted from Hogg & Zenk (1997) and extended. References: Hogg, N & W Zenk (1997): Long-period changes in the bottom water flowing through Vema Channel. J. Geophys. Res., 102, 15639-15646. Robertson, R., Visbeck, M., Gordon, A. L., Fahrbach, E.(2002): Long-term temperature trends in the deep waters of the Weddell Sea, Deep-Sea Research II, 49, 4791-4806. [Walter Zenk]	
5-404	A	11:51	12:50	Given the interest in this topic, the box should be recast as a common question. The box also addresses the consequencs of a change in MOC. The question should be changed to reflect this topic, e.g. Is the THC (MOC) changing, and if so, what are the consequences? [Lenny Bernstein]	Noted, editorial decision by oversight committee of WG1.
5-405	A	11:51	12:52	Box 5.1: The standard of writing is close to that needed throughout chapter 5 which often falls below this standard. The sea level sections, though, generally reach this standard as well. [Chris Folland]	Noted
5-406	A	11:51	12:50	In this box, I would like to suggest to reference Wu et al. (Wu, P., R. Wood, and P. Stott (2004), Does the recent freshening trend in the North Atlantic indicate a weakening thermohaline circulation?, Geophys.Res. Lett., 31, L02301, doi:10.1029/2003GL018584) and Hu and Meehl (Hu, A., and G. A. Meehl (2005), Reasons for a fresher northern North Atlantic in the late 20th century, Geophys. Res. Lett., 32, L11701, doi:10.1029/2005GL022900.) and Hu et al. (Hu, A., G. A. Meehl, and W. Han (2005), Causes of a fresher, colder northern North Atlantic in late 20th century in a coupled model, Progress in Oceanography, submitted in April, 2005). All of these papers are coupled model based study of the THC variations during the 20th century. Hu and Meehl (2005) studied the freshening of the northern North Altantic using a coupled global model (Parallell Climate Model. They concluded that a reduced meridional freshwater divergence related to a weaker THC is the major cause of the freshening in this region. Hu et al (2005) further studied the cooling in the northern North Atlantic and found that	noted. Also mentioned by Wood in 5-402

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				the increased meridional heat divergence is the major cause of the cooling. The increased MHT divergence is related to an increased MHT at 65N and a decreased MHT at 45N. Wu et al (2004) using the HadCM3 found that the freshening of the northeast Atlantic deep water is related to the increased freshwater transport from the Arctic. [Aixue Hu]	
5-407	A	11:55		the inflow of the MOC is not suface water (at least at mid-latitudes). Is "densify" a word? [Stephen R Rintoul]	Accept: change to "upper ocean waters"
5-408	A	12:1	12:50	This is entirely a North Atlantic view of the thermohaline circulation and not consistent with present understanding of the global overturning, in which the Southern Ocean plays an important role, both by forming dense water and by converting deep water to intermediate water to close the global cell associated with NADW formation. [Stephen R Rintoul]	Accept, and the box 5.1 is now recognised as being almost exclusively about the Atlantic.
5-409	A	12:2	12:6	There is a redundancy compared with page 11, lines 8 to 23 [Michel Boko]	Accept
5-410	A	12:2	12:2	" , " in "surface waters, to mid-depth" can be omitted [kyung-ryul Kim]	Reject
5-411	A	12:8	12:19	Cross refer to the paleoclimate chapter here. [Chris Folland]	Accept
5-412	A	12:8	:9	I do not think that Rhamstorf has analyzed sediments cores and corals during the past 120000 years. An effort is needed to recognize the original data providers. [Pascale Delecluse]	Accept: use primary references
5-413	A	12:12	12:12	the words "on Dansgaard-Oeschger events" should be added after "Proxy data". This repetition from the previous sentence rules out possible confusions on the period described. [Philippe Tulkens]	Accept
5-414	A	12:18		the causes of the 8.2k event should be explained (rather than just saying that somebody has explained it) [Stephen R Rintoul]	noted: cross reference to chapter 6
5-415	A	12:21	12:24	Box 5.1: could include Bryden et al. (submitted to Science) results suggesting 30% decrease in THC. Is this supposed to be discussing model results? [Helene Banks]	noted: add Bryden, reduce modeling
5-416	A	12:21		projections with coupled ocean-atmosphere models do not agree on the future of the MOC. It still remains a difficult question because opposite effects contribute to its change (i.e. : more moisture in high latitudes contribute to freshen Nordic waters thus less convection, but more evaporation in the tropics has the opposite effect – Latif M.,	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Roeckner E., Mikolajewicz U. and Voss R., Tropical stabilisation of the thermohaline circulation in a greenhouse warming situation.2000 J. Climate, 13 :1809-1813.) [Pascale Delecluse]	
5-417	A	12:22	12:50	It would be preferable to write "anthropogenic forcing", rather than "anthropogenic warming and freshening", for as it is written in line 50, "…that is so far not possible to distinguish between natural and anthropogenic causes". [Michel Boko]	Reject: not "forcing", this is meant to be response. Box 5.1 has changed significantly and comment is now not relevant.
5-418	A	12:23	12:24	Instead of the final sentence on lines 23-24 on page 5-12, I would write more accurately and more hopefully "Direct observations of the MOC have been based on transatlantic hydrographic sections and monitoring systems to observe the MOC continuously are now being deployed (Srokosz, 2004)". The reference is: Srokosz, M. A. 2004. New experiment deploys observing array in N Atlantic to investigate Rapid Climate Change, EOS Trans. AGU, 85 (8), 78 & 83. [Harry Bryden]	Reject. Hydrographic sections do not provide direct MOC measurements because of the uncertainty in barotropic flows and results of this programm are not yet available..
5-419	A	12:23	12:23	What is "MOC index"? I am not familiar with it and it is not defined in the text. Delete the word "index". [Robert Marsh]	Accept
5-420	A	12:23	12:23	MOC index undefined - also the statement that direct observations of the MOC index do not exist is incorrect (see comments below) [Meric Srokosz]	Accept
5-421	A	12:23	12:24	An observational array to measure the MOC has recently (2004) been deployed in the North Atlantic - see Srokosz M.A. 2004 New experiment deploys observing array in N Atlantic to investigate Rapid Climate Change, Eos Trans. AGU, 85 (8), 78 & 83. (correction Eos Trans. AGU, 85 (12), 114) - this will provide measurements of the MOC 2004-2008 and funding is being sought to continue the measurements in the longer term as part of the UK Natural Environment Research Council (NERC) Rapid Climate Change programme (RAPID - see Srokosz M.A. 20003 Rapid climate change: scientific challenges and the new NERC programme, Phil. Trans. R. Soc. Lond. A, vol. 361, 2061-2078). This work is being done jointly with NSF and NOAA scientists. [Meric Srokosz]	Rejected, results of this programm are not yet available
5-422	A	12:23	12:24	An analysis of 5 trans-Atlantic hydrographic sections (taken in 1957, 1981, 1992, 1998, 2004 along ~25?N) shows that the MOC was 30% weaker in 1998 and 2004 compared to the previous years - see Bryden H.L., Longworth H.R. & Cunningham S.A. 2005 Slowing of the Atlantic Meridional Overturning Circulation at 26.5?N, accepted in principle by Nature. However, continuous measurements of the MOC strength are required to determine whether these "snapshots" represent a significant change in the MOC or are due	Bryden et al will be cited.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				to variability. The recently deployed array will provide the measurements to address this question. [Meric Srokosz]	
5-423	A	12:23	12:23	The MOC index needs to be defined. At 24N, we do have relatively direct observational estimates at 5 times in history (see Bryden et al paper referenced in my comment on p9 ll 6-7) – and there is good evidence that the hydrographic observations, or even just their endpoints, provide a good estimate of the MOC at that latitude [Hirschi et al GRL 2003, Baehr et al JMR 2004.]. The new observations discussed by Bryden et al. provide an undersampled timeseries, but they are pretty direct observations of the quantity of interest, so I think the overall assessment should include them alongside the better sampled but indirect estimates. [Richard Wood]	Accept
5-424	A	12:24	12:24	More is known about recent MOC variability and change than is stated here. I propose some more text: "A model hindcast of MOC strength at key hydrographic sections has revealed considerable variability on a range of timescales over 1985-2003, including a 20% reduction of MOC strength in the northeast Atlantic over the 1990s (Marsh et al. 2005b). Analysis of the MOC strength and structure at 25 degrees N, based on five occupations (1957, 1981, 1992, 1998, 2004), suggests a long-term reduction of around 30%, from 20 Sv in 1957 to 14 Sv in 2004 (Bryden et al. 2005)." Reference 1: Bryden, H. L., H. R. Longworth, and S. A. Cunningham, 2005: Slowing of the Atlantic Meridional Overturning Circulation at 25 degrees N. Nature, in press. Reference 2. Marsh, R., B. A. de Cuevas, A. C. Coward, H. L. Bryden, and M. Alvarez, 2005b: Thermohaline circulation at three key sections in the North Atlantic over 1985-2002. Geophys. Res. Lett., 32, L10604, doi:10.1029/2004GL022281. [Robert Marsh]	Reject: minimizing reference to modeling in this chapter. Accept: Bryden refernece
5-425	A	12:35	:50	The above comments are also relevant to changes of the MOC discussion here. The work by Vellinga and Wu (2004, ref: Vellinga, M. and P. Wu, 2004: Low-latitude freshwater influence on centennial variability of the thermohaline circulation. J. Climate, 17(23), 4498-4511.) is of interest here. Velling and Wu show multidecadal air-sea coupling and upper ocean salinity oscillation in the Atlantic as internal climate variability. The relationship between freshening originating from the tropics and MOC changes may be useful in understanding the current situations in observations. [Peili Wu]	Noted, the paper is of interest, but see 5-397
5-426	A	12:36		Page 5-12, line 36, change "would" to "could". [Harry Bryden]	Accept
5-427	A	12:36	:37	a model result needs a consensus from different models. Such an important result is expected from the present report but is yet to be done.	Noted, however three different models cited in the paper gave the same results

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Pascale Delecluse]	
5-428	A	12:38	12:38	What is "MOC index"? I am not familiar with it and it is not defined in the text. Delete the word "index". [Robert Marsh]	Accept
5-429	A	12:38	12:38	MOC index undefined [Meric Srokosz]	Accept
5-430	A	12:41	12:41	I would suggest replacing "natural and anthropogenic processes" with "natural variability and climate change processes". The aim here is more to see whether there is a climate change signal in the MOC observations than to detect an anthropogenic effect from a natural process. [Philippe Tulkens]	noted: retain the word "anthropogenic" however.
5-431	A	12:43	12:44	The AMO is primarily a northern hemisphere feature with little southern hemisphere signal (Enfield et al., 2001). Enfield et al. (2001) use only North Atlantic SST as an index for the AMO. [Robert Molinari]	noted: our new assessment in summary at end of section now discusses problems with AMO
5-432	A	12:45	12:47	The Knight et al paper only considers the part of the MOC variability that is connected with the AMO. A quadratic global warming trend was removed from the SST field before analysis – and of course we would expect that global warming component to have resulted in a weakening MOC. I think the sentence would be more correct if it said " … infer that the part of the MOC that is linked to the AMO has increased since 1970, and conclude that even without …". [Richard Wood]	Noted, text modified
5-433	A	12:46	12:46	Knight et al (2005) infer the increase in THC strength from a minimum around 1975-80. [Chris Folland]	Accept
5-434	A	12:49	12:50	"we conclude that … the MOC properties are changing" is both too strong and too vague. It is too strong given the weak statements in the preceding paragraphs ("suggest that" "model results suggest") and too vague in that the NAO/AMO effect is not mentioned. An alternative might be "In summary, decadal fluctuations in the MOC are probably closely related to decadal fluctuations in the NAO/AMO. Coupled ocean-atmosphere models suggest that the MOC will decrease in the 21th century as a consequence of global warming, but in the strength of the MOC during the 20th century no secular trend can be inferred from the available observations. [Gerrit Burgers]	Accept
5-435	A	12:49	12:49	At a recent Clivar workshop on the THC in Kiel there was much attention on changes in properties and in transport. The reduction of the overflow transport as observed in the 90s by Hansen et al 2001 (Nature, 411, 927) did not continue. So, we might observe a decadal cycle. Perhaps in this part of the report there could be more attention on the transport	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				changes as well (it is mainly on hydrographic properties now). In particular the Rhines and Hakkinen paper in Science is of relevancel. Even if it does not match some other observations, the discrepancy could be mentioned. [Wilco Hazeleger]	
5-436	A	12:49	12:49	Add text as follows: "… the MOC properties are changing, that the MOC may have slowed, but that …" [Robert Marsh]	Accept: assessment now in summary of section 5.3
5-437	A	12:49	12:50	Some progress has been made on attribution of some of the observed changes discussed in this section, and this is discussed in Chapter 9. I agree that the jury is still out on the detection and attribution of MOC changes, and would add that this is entirely consistent with most model simulations, which suggest that the anthropogenic MOC signal should only start to emerge from the other variability over the next few decades [see Fig. 10.3.14 which I think is currently incomplete]. One model at least (HadCM3) suggests that many of the trends of the last few decades (including a possibly increasing MOC at 48N) could be reversed over the next few, as the anthropogenic signal starts to dominate (see Wu et al 2004 reference in my comment on p11 ll 45-47). [Richard Wood]	Noted, Wu et al cited
5-438	A	12:49		what is meant by "the MOC properties"? I believe it is not only me having difficulty to understand that. Could that be replaced by something like: there are large MOC fluctuations, but ... [Peili Wu]	Accept: phrase more carefully
5-439	A	12:50	12:50	A comment here should be added that changes in the MOC may or may not translate to changes in meridional oceanic heat flux and that a distinction should be made between the two. [LuAnne Thompson]	noted
5-440	A	12:50	12:50	I would suggest replacing "natural and anthropogenic processes" with "natural variability and climate change processes". The aim here is more to see whether there is a climate change signal in the MOC observations than to detect an anthropogenic effect from a natural process. [Philippe Tulkens]	Same as comment 430
5-441	A	12:50	16:14	Cross refer up front to Chapter 3 for an introductory description of these Pacific phenomena. If possible, IPO aspects of the PDO should be mentioned, though I realise much less work has been done on the south Pacific on this time scale from an oceanographical perspective. [Chris Folland]	noted: improved referencing and overlap with chapter 3
5-442	A	12:54	13:48	While the variability in the Artic, Norwegian and Mediterranean sea are listed they are not linked to global warming issues. Thus this section can be shortened or dropped.	Reject: climate change is not necessarily always global warming

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael Alexander Alexander]	
5-443	A	13:0		This is a bit redundant with Chapter 4, and thus may be shortened without overall loss of content. [Jerry Mahlman]	noted: improved referencing and overlap with chapter 4
5-444	A	13:5		In order to explain the changes in the Greenland Sea and also the Arctic Ocean, a crucial component is the water exchange through the Fram Strait. No reference has been cited in the document. Although the observations are very limited, we can refer to papers showing the exchange such as Schauer et al. (2004). They observed the transport of the West Spitsbergen Current, which increased between 1997 and 2000 and could be responsible for the warming in the Eurasian Arctic. Reference/Schauer, U., E. Fahrbach, 2004: Arctic warming through the Fram Strait: Oceanic heat transport from 3 years of measurements. [Motoyoshi Ikeda]	Noted, paper referenced
5-445	A	13:5	:12	this paragraph needs expanding and clarification. It looks messy and confusing. The freshening at Faroe Bank Channel (Dickson et al. 2003) does not necessarily mean that "salinity in the Nordic seas has decreased markedly since the 1970s". Should the work by Hatun et al. (2005, Science) be quoted here? Is Hatun et al.(2005) consistent with what has been said? [Peili Wu]	noted. Reworded, Hatun reference included.
5-446	A	13:8		here it says NAO shift occurred in 1980s; earlier in chapter said 1960s [Stephen R Rintoul]	Noted. I can't find the earlier 1960s reference
5-447	A	13:8		Please do not use the term "AO" but use NAM instead. It is neither Arctic nor an oscillation. [Kevin Trenberth]	Accept
5-448	A	13:14	13:15	Please refer to chapter 4. [Kevin Trenberth]	noted: improved referencing and overlap with chapter 4
5-449	A	13:14		Are there any available illustrations showing the decline of sea ice cover, volume, etc. for the Arctic? [Franklin Schwing]	Accept: refer to chapter 4
5-450	A	13:16		"changing conditions for ice formation" increasing or decreasing ice formation? [Stephen R Rintoul]	Accept: increasing ice formation is what we mean
5-451	A	13:16		doesn't surface freshening cause an INCREASE in stratification? [Stephen R Rintoul]	Accept: thanks
5-452	A	13:17	13:17	Change "decrease" to "increase" [David Parker]	Accept
5-453	A	13:25	13:28	When I read these sentences, I am left with the message that changes in the air-sea heat fluxes and temperatures in the Mediteranean are dominated by the NAO. Is there no	The correlation to NAO is suggested by the referenced publication. No

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				evidence of a longer term change which seems to be visible in the figure? [R Allyn Clarke]	statements on longer term trends are available.
5-454	A	13:25	13:48	Although in few space, I have to acknowledge that the relevant signals of possible changes which ought to be reflected in properties of water masses in the Mediterranean Sea are well reported. Surprisingly, there is no mention of the evidence of remote effects of the EMT in the area of Strait of Sicily which, in my opinion, would better complement the observations of Rixen et al. (2005) and Millot et al. (2005) in the Western Mediteranean. All these studies document that the Mediterranean system is extrememely sensitive to impulsive variability. In this framework, therefore I would suggest citing the following references: [STEFANO VIGNUDELLI]	Noted, however space is too limited to report all regional issues of interest
5-455	A	13:25	13:48	1) Astraldi, M., G. P. Gasparini, A. Vetrano, and S. Vignudelli, Hydrographic characteristics and interannual variability of water masses in the central Mediterranean: A sensitivity test for long-term changes in the Mediterranean Sea, Deep Sea Resl, Part I, 49, 661– 680, 2002; 2) Gasparini, G.P., A. Ortona, G. Budillon, M. Astraldi and E. Sansone, 2005. The effect of the Eastern Mediterranean Transient on the hydrographic characteristics in the Strait of Sicily and in the Tyrrhenian Sea. Deep-Sea Research I 52 (2005) 915–935). [STEFANO VIGNUDELLI]	Continuation of comment 454
5-456	A	13:25		The material on the Med outflow (pg 5-11) is somewhat redundant with the material here on changes within the Med basin itself and could be consolidated and shortened. [Frank Bryan]	noted: will make sure overlap is as small as possible
5-457	A	13:27		Tsimplis and Josey 2001 GRL 28(5), 803-806paper have first documented the changing fluxes of E, P and oressure to the Mediterranean with the NAO. [Michael Tsimplis]	Accept
5-458	A	13:28	13:29	This sentence stating that marked changes in the thermohaline properties are seen throughout the Medditeranean is not particularly informative. [R Allyn Clarke]	Accept:
5-459	A	13:30	13:30	Lyons [David Parker]	Accept
5-460	A	13:31	13:35	Please refer to chapter 3 for observed temperature changes. [Kevin Trenberth]	noted: improved referencing and overlap with chapter 3
5-461	A	13:34	13:34	Add references : Bethoux J.P, Gentili B, Tailliez D., Raunet J.: Warming trend in the Western mediterranean Deep water. Nature, 347, 660-662, 1990; Bethoux J.P., Gentili B., : Functioning of the mediterranean sea: past and present changes related to freshwater input and climate changes.Journal Mar. System, 19, 33-47, 1999. Add also a general	Noted, however due to space limitations references have to be limited

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				reference for the Mediterranean Sea: Progress in Oceanography of the Mediterranean Sea. Special Issue Progress in Oceanography Vol. 44, 1999. [Renzo MOSETTI]	
5-462	A	13:37	13:37	Could the caption for the figure give a little more information on the depth range and horizontal extent of the various North Atlantic ocean temperature and flux signals displayed in this figure. Figure is also missing units for the heat flux anomalies. [R Allyn Clarke]	Noted, figure and caption revised
5-463	A	13:39	:48	This paragraph also need improving. I suggest the following changes to make it read better: Recent changes in the eastern Mediterranean has been dominated by the so-called "Eastern Mediterranean Transient (EMT)" (Roether et al. 1996, Klein et al. 2000, Gertman et al. 2005). The source of Eastern Mediterranean Deep Water (EMDW) has been switched from the Adriatic to the Aegean, probably due to a shift of the NAO (Tsimplis and Rixen 2002) that causes excessive surface heat flux in the Aegean (Wu et al. 2000, Demirov and Pinardi 2002, Rupolo et al. 2003). The EMT can be established due to the anomalous cooling over the Aegean through a salinity redistribution mechanism proposed by Wu et al. (2000). The Levantine Intermediate Water (LIW) has also been cooled between the later 1970s and mid 1980s (Brankart and Pinardi 2001). It is not clear whether the EMT is an uniquie event or a part of long term internal variability (Josey 2003, Pisacane et al. 2005). [Peili Wu]	noted: thanks for the suggestion for streamlining and focusing
5-464	A	13:41	13:48	The text states that 'A negative correlation of SST and NAO (Tsimplis and Rixen, 2002) established the "Eastern Mediterranean Transient" (EMT)…'. This misrepresents the general body of research into the EMT which so far has advanced various possible causes - salinity variations, wind forcing, extreme heat loss - for the generation of the EMT without coming up with a definite explanation (as discussed for example in the Introduction of Josey, 2003). It is definitely not well established that the cited SST-NAO correlation established the EMT and Tsimplis and Rixen (2002) do not argue that this is the case. The text should be rewritten to reflect a more balanced view of possible causes of the EMT . [Simon Josey]	noted
5-465	A	13:41	13:43	A negative correlation of SST and NAO (Tsimplis and Rixen, 2002) established the "Eastern Mediterranean Transient" (EMT) in the early 1990s...". This sentence has been modified from my original version. I would prefer "A negative correlation of SST and NAO (Tsimplis and Rixen, 2002) helped establishing (Demirov and Pinardi, 2002) the "Eastern Mediterranean Transient" (EMT) in the early 1990s... [Michel Rixen]	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-466	A	13:41	13:45	This section is not accurate. The causation of the EMT is yet unknown and the detailed mechanism has not been resolved although many of the characteristics are understood.. Maybe it should be rephrased as follows:A sudden change in the deep water formation region in the Eastern Mediterranean Sea from the North Adraitic to the Aegean Sea occured sometime between 1987 and 1995 (Roether et al., 1996). While the exact mechanisms are yet unreasolved it appears that changes in the heat flux anomalies (Josey 2003, Theocharis et al. 2003) coupled with oceanic changes (The LIWEX group 2001) and possibly long term changes in the heat and freswater forcing (Tsimplis and Josey 2001, Tsimplis and Rixen 2002, Boscolo and Bryden 200?) as well as changes in the Black Sea outflow (Zervakis et al., 2003) have all contributed to its generation. [Michael Tsimplis]	Accept
5-467	A	13:50		Section 5.3.3 Pacific Ocean : This section contains only a couple of lines about the South Pacific subtropical gyre (at the end of the intermediate water section). Recent work on this large expanse of ocean deserves mention in Section 5.3.3.1 (along the lines of the paragraph about southern gyre spin-up in the Indian Ocean on page 17, lines 31-37). Willis et al (2005) show a large part of the trend in global heat content occur in regions of the Southern Hemisphere subtropical gyres. Measured temperature changes of a degree over the top 800m of the Tasman Sea during the 1990s cannot be explained by any local mechanism suggesting the change is due to larger scale forcing (Sutton et al., 2005). Roemmich et al. (2005) finds the strength of the South Pacific subtropical gyre circulation has increased more than 20% over the 1990s, peaking in 2003, and subsequently declining. They link the spin up to an increase of Ekman pumping over the gyre due to an increase in the Southern Hemisphere annular mode (SAM). New references cited: PJH Sutton, M Bowen, D Roemmich, 2005: Decadal temperature changes in the Tasman Sea, New Zealand Journal of Marine and Freshwater Research, in press. (contact p.sutton@niwa.cri.nz for a copy); D Roemmich, J Gilson, R Davis, P Sutton, S Wijffels, S Riser, 2005: Decadal spin-up of the South Pacific subtropical gyre, Journal of Physical Oceanography, in press. (contact droemmich@ucsd.edu for a copy). [Melissa Bowen]	noted: need to increase discussion of S. Pacific, we have added references to Roemmich et al and Willis and a short paragraph in Section 5.3.3.1
5-468	A	13:54	13:54	I don't think PNA can be characterized as having "longer time scale" in comparison with ENSO, as in PDO case. [Roxana Bojariu]	Accept
5-469	A	13:54		does PNA really have longer time-scales than ENSO? [Stephen R Rintoul]	Accept
5-470	A	13:55	13:57	This sentence is inappropriate: delete. [Kevin Trenberth]	noted, text changed
5-471	A	13:55	13:55	About the variability of the gyre strength, one of our recent paper addressed an	Not included, our reference list is

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				investigation based on 50a data assimilation product. Here, I suggest to cite it as '9. Dongxiao Wang, Yun Liu and Dejun Gu, 2004, Gyre-connected variations inferred from the circulation indices in the Northern Pacific Ocean, in Earth Climate: The Ocean-Atmosphere Interaction, Geophys. Monogr. Ser., Vol. 147, Eds. C. Wang, S.-P. Xie, and J. A. Carton, pp. 319–328, AGU, Washington, D. C.' As we know this is only one paper upto now about the variabilities in three gyres in the Northern Pacific, i.e., subpolar, subtropical, and tropical gyres. [Dongxiao WANG]	already very long in this assessment.
5-472	A	13:57		"impact of anthropogenic forcing …must be closely followed". What does this mean? This first paragraph is confusing. [Stephen R Rintoul]	noted: see 5-470
5-473	A	14:1	14:4	This is thoughtfully stated and AR4-relevant. [Jerry Mahlman]	Noted
5-474	A	14:1	14:29	A lot or most of this material is inappropriate and should be deleted. It does not deal with observations (lines 1-13). The changes in ENSO and PDO are dealt with in chapter 3 and lines 18-22 are not correct and at odds with chapter 3: there is no evidence of another regime shift in the later 1990s. Any announcements of such were premature and seeing only the downside of the 1997-98 El Nino. There was a moderate El Nino in 2002. Lines 24-29 are not appropriate here but the topic is dealt with in chapter 3. [Kevin Trenberth]	noted: need to cross-reference with chapter 3
5-475	A	14:6	14:29	All of 5.3.3.1 is covered in either Chapter 3 or Chapter 9. I dnt think it needs to be reiterated here and doing so increases the chances of some inconsistency between the chapters. [Neville Nicholls]	accepted, ENSO material deleted
5-476	A	14:6	:29	I was surprised to find so little on ENSO, its decadal variability and its relation to climate change. [John Church]	see chapter 3 for ENSO issues
5-477	A	14:7	14:7	It would be preferable to write "anthropogenic forcing", rather than "anthropogenic warming and freshening", for as it is written in line 50, "…that is so far not possible to distinguish between natural and anthropogenic causes". [Michel Boko]	TSU mistake: this is comment 417 and does not apply to line 14:7
5-478	A	14:7	14:12	However, in both the GFDL models and many other realistic models with El Nino, changes are projected to be relatively small. See Oldenburgh et al., Ocean Science 1, 81-105, 2005, http://www.copernicus.org/EGU/os/os/1/81/os-1-81.htm [Anand Gnanadesikan]	noted: reducing this whole section because of overlap with chap. 3.
5-479	A	14:11		that precise magnitude" add: "that the precise [Hartmut Grassl]	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-480	A	14:13		add- The long-term trend in equatorial Pacific SST has contributed to an apparent 30-50% increase in the magnitude of recent El Niño events (Mendelssohn et al. 2005). Full citation- Mendelssohn, R., S.J. Bograd, F.B. Schwing, and D.M. Palacios. 2005. Teaching old indices new tricks: a state-space analysis of El Niño related climate indices. Geophys. Res. Lett. 32: L07709, doi:10.1029/2005GL022350. [Franklin Schwing]	noted: checked with chapter 3
5-481	A	14:15	14:7	Page 5.14 Lines 15-22. This part mentioned the so-called "regime changes" and I believe it is a mistake to include this discussion as there is way too much critical debate about the statistical techniques used to detect regime changes (see Rudnick and Davis, 2003) and a recent paper in Nature (Hsieh et al 2005) that demonstrates very clearly that there is insufficient non-linearity in the physical time series to support regime changes. Rudnick, D. L. and R. E. Davis. 2003. Red noise and regime shifts. Deep-Sea Research 50, 691-699. Hsieh, C., S. M. Glaser, A. J. Lucas and G. Suguhara. 2005. Distinguishing random environmental fluctuations from ecological catastrophes for the North Pacific Ocean. Nature 435, 337-340. [Howard Freeland]	noted: other chapters also using "regime shift"
5-482	A	14:21		Did La Nina really extend "at least to 2003"? I thought 2002 was an ENSO year. [Stephen R Rintoul]	Accept
5-483	A	14:24	14:19	The whole paragraph should be removed, in my opinion - it deals mainly with atmospheric observations, not oceanic ones. [Roxana Bojariu]	accepted
5-484	A	14:28		Was drought restricted to the northern hemisphere? [Stephen R Rintoul]	Noted, will be checked
5-485	A	14:31	14:57	In principle, this could be the cause of the "Levitus Bump" in his "globally averaged ocean data". It is worth investigating. [Jerry Mahlman]	Noted: text in section 5.2 is being strengthened on this aspect of the global heat content averages.
5-486	A	14:32	14:39	Comparison with similar signals in the Atlantic would provide some indication of the global nature of this type of variability. [Robert Molinari]	Noted:
5-487	A	14:36		Please use "climate shift" not PDO regime shift. In the tropics the 1976-77 shift is a singular event. [Kevin Trenberth]	Accept
5-488	A	14:37		"importance of tropical Pacific in climate change as described above" Was it described above? If so, I missed it. [Stephen R Rintoul]	Noted: this was the paragraph 14:24 to 14:29 that Bojariu wants us to remove

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-489	A	14:39	14:39	However, the change in the heat transport induced by the reduction of the Subtropical Cells is partly compensated by changes in the western boundary current transports (Hazeleger et al. 2004, J. Phys. Oceanogr., 34, 320-333 and McPhaden and Zhang 2004, Geoph. Res. Lett. 31) [Wilco Hazeleger]	Noted:
5-490	A	14:41	14:41	Drop "subpolar": 40N is south of the subpolar region. [Michael Alexander Alexander]	Accept
5-491	A	14:41	14:41	the region around 40 N" instead of "the subpolar region around 40 N [Roxana Bojariu]	Accept
5-492	A	14:41	14:43	The sited literature "Miller and Douglas, 2004" seems inappropriate here. The correct literature could be "Miller et al., 2004." Miller, A.J., F. Chai, S. Chiba, J.R. Moisan, D.J. Neilson, 2004: Decadal-scale climate and ecosystem interactions in the North Pacific Ocean. Journal of Oceanography, 60(1), 163-188. [Toshio Suga]	Accept
5-493	A	14:42		Not "precisely" – this is overstated. [Kevin Trenberth]	Accept
5-494	A	14:48	14:54	Should be deleted, this is not about observations. [Kevin Trenberth]	accepted
5-495	A	14:48	14:50	I don't like the style of asking questions in the text. Some people do, but the style should be standardised. [Philip Woodworth]	Accept
5-496	A	14:50	14:53	For consistency, the chapter should interprete observations in the context of model results throughout the chapter. [Helene Banks]	Noted: interpretation moved to last section of 5.3
5-497	A	14:54	14:54	This is a bit of a controversial statement and suggestive. I think it is not clear from the ensemble of IPCC models that we have now. There is a tendency to a more El Nino-like or PDO state, but if the "bad" models (those who simulate ENSO badly as in the GISS models) are disgarded hardly any changes is found in ENSO are found. I would leave this sentence out. [Wilco Hazeleger]	Noted: text removed
5-498	A	14:56	14:57	I don't like the style of asking questions in the text. Some people do, but the style should be standardised. [Philip Woodworth]	Accept and removed from SOD.
5-499	A	14:56	15:24	This set of paragraphs uses the modeling study of Barnett et al. (2005) to motivate why the change in the PDO may be due to anthropogenic forcing. However, the Barnett (which is instructive and quite relevant for the chapter as a whole) looked at basin-wide	Rejected: The reviewer is wrong. There is no reference at all to the work of Barnett et al. (2005).

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				averages, so it couldn't distinguish between the PDO while the Thompson and Wallace study examines variability in the Southern Hemisphere. In addition, transitions in the PDO happened earlier in the 20th century (~1925, 1947) so its unclear if the 1976 transition and the associated changes discussed here result from global warming. Furthermore, many of the changes in the 1990s to the present are associated with the second mode of variability and are thus not the PDO (at least as its classically defined, see Bond, N.A., J.E. Overland, M. Spillane, and P.J. Stabeno (2003): Recent shifts in the state of the North Pacific. Geophys. Res. Lett., 30(23), 2183, doi: 10.1029/2003GL018597.) [Michael Alexander Alexander]	Mis-numbered? noted: move discussion to new summary
5-500	A	15:1	15:24	Same comment as for previous line. [Robert Molinari]	noted:
5-501	A	15:1	15:41	Please see chapter 3 for decadal variability and especially see the paper by Deser et al. 2004, J Climate. This material should focus on what the changes were, the attribution is inappropriate and often wrong (e.g. line 19 attributing salinity changes to a stronger PDO is not right). What is the reference for lines 9 to 12? Please see Deser et al 2004 J Climate, 15 Aug issue. [Kevin Trenberth]	accepted, material focusses now on ocean observations
5-502	A	15:5	15:7	Page 5-15 lines 5-7 mention "…a shoaling of the halocline in the centre of the western subarctic gyre…." At this point it might usefully be mentioned that there are observations of shoaling of the mixed-layer depth throughout the eastern sub-arctic gyre, trends verified over 50 year time scales (see Freeland et al 1997 and Li et al 2005). The first suggestion that this was correct was by Polovina et al 1995, but they used temperature profiles alone and in the sub-arctic Pacific salinity is essential for monitoring mixed layers. The other papers did it right. Observations also exist of mid-winter MLD decline over all of the sub-arctic N. Pacific and the Bering Sea. This is important because it limits nutrient supply and potentially can have a direct impact on ecosystem dynamics. References:- Freeland, H.J. K.L. Denman, C.S. Wong, F. Whitney and R. Jacques. 1997. Evidence of change in the N.E. Pacific Ocean. Deep-Sea Res. 44(12), 2117-2129. Polovina, J.J., G.T. Mitchum and G.T. Evans (1995) Decadal and basin-scale variation in mixed layer depth and the impact on biological production in the central and North Pacific, 1960-88. Deep-Sea Research 42, 1701-1716. Li, Michelle, P. G. Myers and H. Freeland. 2005 An examination of historical mixed layer depths along Line-P in the Gulf of Alaska. Geoph. Res. Lett. 32(5), L05613 10.1029/2004GL021911. [Howard Freeland]	noted: and now referred to in Section 5.3.3.1

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-503	A	15:7		add- The transport of the Kuroshio Extension and North Pacific Current increased by 8% and expanded southward after 1976 (Parrish et al. 2000). Full citation- Parrish, R.H., F.B. Schwing, and R. Mendelssohn. 2000. Midlatitude wind stress: the energy source for climatic regimes in the North Pacific Ocean. Fish. Oceanogr. 9: 224-238. [Franklin Schwing]	Noted, and added to the text in this section.
5-504	A	15:12		the phrase "long-term trend is toward positive PDO" might be read as suggesting we expect this trend to continue. Is this the intention? [Stephen R Rintoul]	noted: rephrased
5-505	A	15:27	15:32	If you refer to decrease in dissolved oxygen, it could be due to a larger flux of polluted continental water discharge in the seas: industry activities, forest vegetation clearing could result in a larger amount of organic load, increasing the BDO of sea water., but necessary less ventilation, un less I don't understand what your are meaning. [Michel Boko]	noted: conflict in interpretation now indicated (Deutsch, Johnson and Grubrer vs. Ono)
5-506	A	15:32	15:32	"in the NPIWsouth of Hokkaido from 1970" : space missing [Michel Rixen]	Accept
5-507	A	15:32		a space is missing between "NPIW" and "south" [Hartmut Grassl]	Accept
5-508	A	15:43	15:43	Figure 5.3.4 is mislabeled as it shows total water column changes not just those below 4000m as indicated in the figure legend. [Robert Molinari]	Accept
5-509	A	15:45	15:57	This seems quite far removed from the upper ocean where we expect climate-warming-relevant signals to appear. [Jerry Mahlman]	Noted: we are looking at all changes. This change could be associated with a circulation change that might indeed be forced by anthropogenic signals upstream, perhaps in the tropics or southern ocean
5-510	A	15:45	15:51	The age given for Pacific bottom waters, 500 to 1000 years is not consistent with the deep changes noted between 1985 and 1999, paragraph should be rewritten. [Robert Molinari]	noted: rephrased.
5-511	A	15:48		This level of accuracy is incredible. To be that accurate it has to be in exactly the same spot at same time of year and I don't care how accurate the thermometer is, the location is surely not that good. [Kevin Trenberth]	Reject: accuracy is indeed this good. Objective mapping is used.
5-512	A	15:49		The latter comment has to be seen in the light of more recent comparable observations in the Pacific by Fukasawa et al., 2004, see Chap 5, pg 15, line 49 [Walter Zenk]	Noted:

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-513	A	15:51		The Gille (2002) results refer to changes in 900 m depth. It is not appropriate to link these to changes in the bottom 1000 m of the North Pacific (very different density classes). [Stephen R Rintoul]	Accept:
5-514	A	15:55		Please check these values. [Kevin Trenberth]	Reject: values are correct
5-515	A	16:0	17:	Section 5.3.4: I suggest major revisions. A lot of it is based on models and not observations and is inappropriate. Delete lines 18-23. Figure 5.3.5 deals with changes in SST which is the province of chapter 3. The IOD is not an established mode and there is a lot of literature disputing what is written here, especially by Neville Nicholls et al.: see lines 52-54. Much of what is here (lines 35-57) should be in chapter 3 (except chapter 3 does not believe much of it: most of what is here was ENSO not IOZM). Also p 5-17 lines 4 to15: that was ENSO not IOZM. Figure 5.3.7 is a model result and inappropriate here. What evidence is there that it is real? [Kevin Trenberth]	noted: section rewritten
5-516	A	16:0		Section 5.3.4: I was surprosed to find so little on the monsoons, their decadal variability and their relation to climate change. [John Church]	see chapter 3 for monsoon issues
5-517	A	16:9	16:10	These comments are not appropriate. [Kevin Trenberth]	Reject: (assuming this comment is really about the Japan Sea), based on actual observations of new ventilation Statement on PDO change removed.
5-518	A	16:16	16:27	Drawing the line between Chapter 3 and 5 may not always be easy but this section seems to belong to Chapter 3. Note that HADISST is correctly HadISST. [Chris Folland]	noted: this whole section rewritten
5-519	A	16:16	17:37	The first paragraph and associated figure, which examine the basin-wide SST warming over the Indian Ocean are very relevant for the IPCC report (although they may be examined in another chapter). However, no attempt is made to understand the processes leading to this warming or if the warming is due to anthropogenic forcing. The remaining discussion of the circulation, the Indian Ocean dipole, etc. is a confusing mix of topics that are not relevant for the IPCC report and should be dropped. [Michael Alexander Alexander]	noted: section rewritten
5-520	A	16:16	17:38	Section 5.3.4 on the Indian Ocean is differently structured from the other sections. It focusses on the Indian Ocean Zonal Mode. The other sections are more an assessment on the observed changes, here it is more on changes associated with the mode of variability. [Wilco Hazeleger]	noted: Indian Ocean section is largely rewitten, and the coupled modelling and IOZD have been much diminished.
5-521	A	16:16		Section 5.3.4. The signature of SST increase since 1977 in the Indian ocean is very clear	Noted: section rewritten and overlap

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				(nearly 2 degrees warming) (cfr. p 67, figure 5.3.5). However, an impact of such warming on the monsoon regime is apparently not detected (cfr comments in section 5.3.5 and Chapter III, section 3.7.1). The absence of an identified link may require a comment in the text. [Philippe Tulkens]	with other chapters removed, so this comment might not be used
5-522	A	16:16		Section 5.3.4. The signature of SST increase since 1977 in the Indian ocean is very clear (nearly 2 degrees warming) (cfr. p 67, figure 5.3.5 and see p 3-12, section 3.3.2.3 L. 21 to 35). However, an impact of such warming on the monsoon regime is apparently not detected (cfr comments in section 5.3.5 and Chapter III, section 3.7.1). The absence of an identified link may require a comment in the text. [Philippe Tulkens]	same comment as 521
5-523	A	16:16		Section 5.3.4. In the actual data, can no contribution from global warming be detected in data records? The dynamics of monsoon is extremely complex and not fully understood. Though, if the trend in Indian ocean SST observations has an impact on the monsoon regime, it could be emphasized. If there is no noticeable impact, it could be noted and explained [Philippe Tulkens]	noted: global warming signature is part of section 5.2. Monsoon impacts has been removed or reduced.
5-524	A	16:16		Section 5.3.4. Consistency in this section should also be checked with page 3-59, section 3.7.1 on monsoon observations. [Philippe Tulkens]	noted: overlaps with other chapters will be removed or checked
5-525	A	16:18	16:27	This has potential to have secular regional climate change implications. [Jerry Mahlman]	Noted
5-526	A	16:18	16:18	Actually, in the beginning of 'Indian Ocean' section, it is better to introduce a little about the observation analysis of the decadal to interdecadal variabilities in SST in this basin. For example, an ealier paper in 199: 71. Dongxiao Wang, Guoxiong Wu, Jianjun Xu, 1999, Interdecadal variability in the tropical Indian Ocean and its dynamic explanation, Chinese Science Bulletin, 44(17), 1620-1626. [Dongxiao WANG]	Reject: section rewritten to de-emphasize modes and emphasise the sparse observational results within the oceans.
5-527	A	16:18		start the assessment on the Indian ocean first by the region itself : the monsoon and the IOZM, then teleconnections would be discussed (not only the Sahel, but also Mediterranean-European climate ex : M. P. Hoerling1 , J. W. Hurrell2, T. Xu1, G. T. Bates1 and A. S. Phillips, Twentieth century North Atlantic climate change. Part II: Understanding the effect of Indian Ocean warming. Climate Dynamics, DOI: 10.1007/s00382-004-0433-x, Volume 23, 391 – 405). The relations between IOZM, ENSO, monsoon is complex and may not be stationary in time. Indeed the mid 70's seem to have modified the links between the variability modes. Another interesting analysis has been made by : Terray, P., S. Dominiak, and P. Delecluse, 2004 : Role of the southern	noted: section rewritten. Climate modes emphasis will be greatly reduced. Wijffels reference welcome.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Indian ocean in the transition of the monsoon-ENSO system during recent decades,. Clim. Dyn., DOI: 10.1007/s00382-004-0480-3.The Indian ocean discussion is particularly poor on hydrography but to my knowledge, a revised analysis of the water masses in the Indian ocean has not been recently published. Most of the discussion is on the impact of the Indian ocean SST on other regions and little is said about the Indian ocean itself, and its driving mechanisms. A discussion on the ITF could also bring an interesting opening on inter-ocean links. See, for instance :Wijffels, S., and G. A. Meyers, 2004. An Intersection of Oceanic Wave guides: Variability in the Indonesian Throughflow Region, J. Physical Oceanography, 34,1232-1253 [Pascale Delecluse]	
5-528	A	16:35	16:37	The independent existence of the Indian Ocean dipole has been challenged. This should be mentioned here. [Chris Folland]	Noted: section rewritten and IOD is now not discussed, and the emphasis is on the oberservations within the oceans.
5-529	A	16:47	16:50	If this stays in Chapter 5, cross refer to the relevant part of Chapter 3 on East African rainfall. [Chris Folland]	Noted: paragraph should has been removed
5-530	A	16:48	16:49	…is usually strongly correlated….(Figure 5.3.6b)… [David Parker]	Noted: but paragraph is being removed
5-531	A	16:52	16:53	there is a significant debate goning on whether……this sentence may be correct as "A significant debate is going on wheter…." [kyung-ryul Kim]	Accept: but all rewritten
5-532	A	16:52	17:37	A really nice new study on this is Song, Vecchi and Rosati, (subm. J. Climate) which shows that the linkage between these two phenomena can be attributed to a common triggering phenomenon in (westerly wind anomalies in the Pacific) if these are long enough, they trigger anomalies in both the Indian and Pacific, but if they are only found in one part of the triggering season, they only trigger one event. I will get Scott to send a copy of the manuscript. [Anand Gnanadesikan]	Noted: section rewritten
5-533	A	16:57	16:57	The Fischer et al. (2004) appears to be wrong. In the text the citation in the context of Indian Ocean Zonal Mode and ENSO, while in the References, Fischer et al. (2004) is a paper about the boundary circulation at the exit of the Labrador Sea. [Robert Marsh]	Accept; but section rewritten
5-534	A	16:57		Use "may be associated" instead of "is either associated", to make the fact that it could be either/or more clear. [Neville Nicholls]	Accept: but section rewritten
5-535	A	17:0		almost free of the hydrography! But I was reminded here of Morner's paper suggesting a	section rewritten

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				recent sea level fall in the Maldives and my refutation of that. Anny can see if they are worth mentioning in the sea level section. Both are in Global and Planetary Change - my refutation is in Woodworth, P.L. 2005. Have there been large recent sea level changes in the Maldive Islands? Global and Planetary Change (in press). [Philip Woodworth]	
5-536	A	17:1	17:37	Again, one should have to make the case as to how these regional variations are related to anthropogenically forced secular climate change. [Jerry Mahlman]	section rewritten
5-537	A	17:4	17:15	This needs to be cross refered with Chapter 3 on the Indian Monsoon rainfall. [Chris Folland]	section rewritten
5-538	A	17:5	17:7	The sentence starting with Latif et al. (1999) is not clear to understand [kyung-ryul Kim]	section rewritten
5-539	A	17:7	17:7	…rainfall anomalies when the observed Pacific SST anomalies were also applied. [David Parker]	section rewritten
5-540	A	17:8	17:8	Change "ensemble mean" to "composite" (the usual word when dealing with observations) [David Parker]	section rewritten
5-541	A	17:19	17:21	The text and figure 5.3.6c are not consistent as the text talks about conditions after 1993 and the chart ends in 1994. [Robert Molinari]	section rewritten
5-542	A	17:26		EEIO not defined [Stephen R Rintoul]	section rewritten
5-543	A	17:27	17:29	This sentence does not follow the previous text and is impossible to understand. [Chris Folland]	section rewritten
5-544	A	17:27	17:29	The sentence starting with The effect of PDO is not clear to understand [kyung-ryul Kim]	section rewritten
5-545	A	17:27	17:27	Figure 5.3.7 would be more informative if ENSO years were marked. [Robert Molinari]	section rewritten
5-546	A	17:31	17:33	The sentece is not clear to understand [kyung-ryul Kim]	section rewritten
5-547	A	17:36	17:36	Change "Thus there is" to "Thus there seems to be". [Trevor McDougall]	section rewritten
5-548	A	17:36	17:37	There is a serious issue of undersampling in time here, and I think it is too bold to discuss interdecadal oscillations when we could be looking at aliasing of large interannual variability. At the very least this caveat to the interpretation should be prominently stated (here and possibly in a number of other places in the chapter). The results are also	section rewritten

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				discussed in Ch 9 p 43. The paper by Stark et al. 2005 discusses the difficulties in interpreting these observations. Reference: Stark, S., R. A. Wood and H. T Banks, 2005: Re-evaluating anthropogenic climate change in Indian Ocean water masses. J. Climate (submitted – status: undergoing final minor revision, acceptance anticipated before deadline. Nathan Bindoff has a copy). [Richard Wood]	
5-549	A	17:37		I suggest adding a reference to Bryden et al. (2003) for the final statement on page 5-17, line 37 just to get it mentioned so the Chapter cannot be criticised for leaving out 'contrary' results. [Harry Bryden]	noted: will include actual oceanographic results in the newly written section
5-550	A	17:39	17:39	Add words of "and Circulations" after the 5.3.5 title [Zhaoqian Dong]	Accept
5-551	A	17:39	18:2	The discussion of Subantarctic Mode Water enteirely ignores the most recent research which is documented in the following papers: 1. Bryden, H.L., E. McDonagh and B.A. King, 2003: Changes in Ocean Water Mass Properties: Oscillations or Trends? Science, 300, 2086-2088. 2. McDonagh, E.L., H.L. Bryden, B.A. King, R.J. Saunders, S.A. Cunningham and R. Marsh, 2005: Decadal Changes in the South Indian Ocean Thermocline. J. Climate, 18, 1575-1590. The implication of these results on previous conclusions of anthropgenic climate change is discussed in: Stark, S. Wood, R.A. Wood and H.T. Banks, 200?, Re-evaluating the Causes of Observed Changes in Indian Ocean Water Masses. J. Climate, accepted pending final revisions. These results clearly document that in 2002 Indian Ocean Subantarctic Mode Water was saltier than in 1987 reversing the generally accepted fresehning trend seen throughout the Indo-Pacific. This section is the only one to have been sampled again but it is unclear why these results have been omitted from the report. The sentence beginning on line 52 is no longer justified in the light of the new observations and gives a false impression of present understanding of SAMW properties. Withtin the report itself the omission may lead to confusion as the new results are included in chapter 9 on page 43. [Sheila Stark]	noted: papers 1&2 referenced (reference 2 had always been included in the Indian Ocean Section and Oscillation discussed albeit briefly). Now strengthened.
5-552	A	17:39		Why is this section restricted to "water masses"? [John Church]	Accept: change title
5-553	A	17:39		Section 5.3.5: I found little discussion on potential changes in the Southern Ocean overturning circulation and indicators of any change.	noted: the observational literature has very little on the indicators of change in

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[John Church]	Overturning circulation.
5-554	A	17:42	17:50	Clear and good analysis! [Michel Boko]	Noted
5-555	A	17:42	17:43	SAMW is not the thick mixed layer in my opinion, but the thick layer (or low PV) that is subducted. [Wilco Hazeleger]	agreed and adopted.
5-556	A	17:52	17:53	This appears to ignore the results of McDonagh et al 2005 and Bryden et al 2003, which show that the SAMW was saltier on isopycnals in 1995 and 2002 than it was in 1987. Either there has been a reversal of the freshening trend, or the interannual variability is so strong that we can say nothing about interdecadal trends with the temporal sampling available. [Richard Wood]	This fact is referred to in the text. Note that the part of the water column that reversed in trend is the shallower or lighter varieties of mode water. The global avaerage for this water mass is still of cooling and freshening.
5-557	A	17:52		This comment ignores the Bryden, McDonagh and King (Changes in ocean water mass properties: Oscillations or trends?, Science, 300, 2086-2088, 2003) results showing salinification of SAMW [Helene Banks]	Noted, paper referenced, see above comment.
5-558	A	17:52		The presentation and discussion of the water mass changes in the Indian basin in an isopycnal framework, while other basins are presented as isobaric changes could be confusing. References discussing isopycnal changes for other basins are availbale (e.g Arbic and Owens, J. Clim, 14, 4091-4108, 2001; Vargas-Yanez et al, GRL, doi:10.129/2003GL019308, 2004). The authors should either present the changes in both reference frames for all basins (providing an opportunity to discuss mechanisms that have been identified through the comparison of isobaric vs. isopycan changes), or stick to one or the other. [Frank Bryan]	noted: we have tried, but the published results are in mixture of coordinates systems, and incomplete, thus making it difficult to translate between coordinate systems.
5-559	A	17:52		How well are these changes known? What is the error bars? Isn't sampling an issue? [Kevin Trenberth]	noted: the error bars are in the publications, and also in the results presented in section 5.2.
5-560	A	17:54	17:54	The sited literature "Bindoff and McDougall, 1992" is incorrect and could be replaced with "Bindoff and Church, 1992" or "Bindoff and McDougall, 1994." Bindoff, N.L., and J.A. Church, 1992: Warming of the water column in the south-west Pacific. Nature, 357, 59-62. [Toshio Suga]	Accept
5-561	A	18:0		Some of the figures are extremely poor to even understand what they show. [Daniel Lluch-Belda]	Accept: figures need to be improved

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-562	A	18:0		Again, relevance is the key here for this Assessment chapter. [Jerry Mahlman]	noted: improving relevance and assessments
5-563	A	18:7	18:9	Does this result mirror the SSTs, e.g. in HadISST? [Chris Folland]	noted: text changed
5-564	A	18:9	18:9	Add a degree symbol between the 40 and S [Zhaoqian Dong]	Accept
5-565	A	18:9	18:9	"40S is larger than" : add a 'degree' sign [Michel Rixen]	Accept
5-566	A	18:15		"increase in the hydrological cycle": this is inappropriate as it is not clear what it means. Stick to the facts wrt inferences about changes in precipitation. [Kevin Trenberth]	Accept
5-567	A	18:16	18:16	Is there a reference on the changes in precip in the south in scenarios of climate change (or cross-reference to other chapter)? [Wilco Hazeleger]	noted:
5-568	A	18:18	18:18	Label the vertical axis in Fig 5.3.8 [Chris Folland]	Accept
5-569	A	18:18		Figure 5.3.8 The caption does not explain what the solid dots, open circles, crosses, and arrows represent. [Melissa Bowen]	Accept: Figure has been completely revised.
5-570	A	18:23		I believe the Aoki results refer to changes near where the CDW layer outcrops, while text reads as if all the CDW between 35S and 60S is warming. [Stephen R Rintoul]	Accept and text has been revised.
5-571	A	18:25	18:45	Should add discussion about changes in the Antarctic Oscillation, which is a huge signal. [Anand Gnanadesikan]	noted: section rewritten, look at SAM variations if useful
5-572	A	18:28		Can this also be given as a % change? [Kevin Trenberth]	Reject: ocean salinity is a large number and percentages don't mean much
5-573	A	18:40	18:45	Can the trend in ACC transport by attributed to the changes in wind stress associated with the Southern Annular Mode? [Wilco Hazeleger]	noted: actually we report that there is no trend in the strength of the ACC
5-574	A	18:41	18:41	Need to have uniformity within and across all chapters on these units: m3/sec or Sv? [Chris Folland]	Accept:
5-575	A	18:44		the number of independent observations needed to measure the ACC at Drake Passage has been discussed explicitly by Hughes and Meredith. Also it has been shown to capable of monitoring the ACC by means of Antarctic sea level measurements, see Meredith, M.P. and Hughes, C.W. 2005. On the sampling timescale required to reliably monitor interannual variability in the Antarctic circumpolar transport.	Accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Geophysical Research Letters, 32, L03609, doi:10.1029/2004GL022086. [Philip Woodworth]	
5-576	A	18:45		I believe there is a manuscript by Cunningham et al. on changes in the ACC. [John Church]	Noted and referred to in 5.3.5.3
5-577	A	18:46		Add a title such as 5.3.6 ARCTIC OCEAN and description under the title [Zhaoqian Dong]	Reject: removing section
5-578	A	18:47	18:47	Change "5.3.6 Summary" into "5.3.7 Summary" [Zhaoqian Dong]	Reject: removing section
5-579	A	18:47	19:4	This paragraph discusses the Southern Ocean only, so I think 5.3.5.4 would be the right section number. In any case, the paragraph reads to me like a tutorial on the role of the Southern Ocean in the global circulation – a nice tutorial, but I don't think it is a summary of the material that has gone before, nor is it really addressing 'IPCC questions'. I think the paragraph could be removed. [Richard Wood]	Accept: removing section
5-580	A	18:47		5.3.6 is a summary of 5.3.5, not of 5.3 as a whole as implied by the numbering of the section. [Harry Bryden]	Accept: removing section
5-581	A	18:47		Section 5.3.6. Summary. For some reason, this is now a summary of section 5.3.5 only. I recommend that this section is reworked into a summary of the main findings of section 5.3. This new section 5.3.6 should have about the same length as the present one. Some of content of the present section 5.3.6 may be moved into section 5.3.5. [Gerrit Burgers]	Accept: removing this little section and adding general summary
5-582	A	18:47		The contents of Section 5.3.6 does not seem a summary of Section 5.3 and thus does not fit the section title "Summary." Since the whole paragraph of Section 5.3.6 seems to express the significance of the Southern Ocean in the global ocean circulation and its changes, it could be appropriate for the begining of Section 5.3.5. I would suggest that this paragraph be inserted between Lines 40 and 41 on Page 17, and then the emptied Section 5.3.6 be deleted. [Toshio Suga]	Accept: removing this little section and adding general summary
5-583	A	18:49	19:5	In the coupled models with low diffusion as well as ocean-only models with low diffusion, the Southern Ocean is the dominant location where NADW is transformed into light water (Hirst, Ocean Modelling, 1999; Gnanadesikan et al., Global Biogeochemical Cycles, 2004; Gnanadesikan et al., GFDL's CM2 Global Coupled Climate Models: The baseline ocean simulation in press, J. Climate) While this disagrees with the Sloyan and Rintoul model (it is after all, an inverse model) solution, it is consistent with the idea that	Reject: model results are not appropriate for the chapter

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				relatively little deep water upwells in the Pacific. In climate models the Southern Ocean formation of AAIW/AAMW and MOC are not separate! [Anand Gnanadesikan]	
5-584	A	19:0		The section on biogeochemistry is important and includes already important material. It is well written. There are, however, omissions that should be covered in addition. A page allocation of 10 percent or 3 pages only to this section appears to be too small in comparison with the other sections of the chapter. The section does not cover oceanic observations on other important GHGs. In particular for N2O there is a rich literature available both on the distribution in the interior ocean as well as on the air-sea exchange. There are probably also a few observational paper out there that address CH4 leakage from clathrates. Ocean acidification is an extremly policy-relevant issue that needs to be treated carefully in this report and is to be covered in the TS and SPM and Synthesis Report. The section on pH changes and on changes in the saturation horizon of aragonite must be kept and slightly extended. [Fortunat Joos]	Noted: N2O and CH4 will be referred to Chapter 7. pH is extended by 1 sentence.
5-585	A	19:0		The overall section 5.4 Ocean Biogeochemical Changes is rather poor with respect to the other sections. The section could be inserted (and it is also partly overlapped) in Chapter 7 of this report dealing with ecosystem impact. Several studies here not reported also deal with higher trophic levels (change in species composition, migration of exotic species and so on). [Renzo MOSETTI]	Noted. Links with physics are strenghtened. Ecosystems belong to WGII.
5-586	A	19:6	21:22	I was surprised to see essentially no discussion of interannual variability in carbon fluxes and uptake. I was also surprised that I did not see any discussion of the role of watermass formation on the transport of carbon into the ocean interior and how changes in circulation might affect that. I realize space is limited, but it would be nice to have a little more in depth discussion of how all the different peices fit together. [Christopher Sabine]	Noted. Interannual variability is in Ch7. Links to ocean physics are strenghtened.
5-587	A	19:6	21:57	Why is there no question box in this section? (e.g. similarly to the sea level and physical properties one would expect a question like "is the biogeochemistry of the ocean changing ?") [STEFANO VIGNUDELLI]	Editorial decision
5-588	A	19:6	23:0	This entire discussion goes directly to the documentation of the CO2 problem, in the atmosphere, and how its uptake in the ocean affects the ocean's biology and its chemistry. These phenomena are at the heart of the non-climatic implications of the continuing addition of CO2 into the atmosphere. [Jerry Mahlman]	Noted.
5-589	A	19:6		There is a considerable body of work on estimating changes in ocean ventialtion rates	Accepted. Links with physics are

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				based on observed passive tracer data, particularly CFCs that has not been addressed in this chapter. These studies complement the discussion of changes in water mass proeprties and provide the basis for a number of the biogeochemical studies metntioned in section 5.4. Examples include: Doney et al, GRL, 25, 1399-1402, 1998; Mecking et al, DSR I, accepted; Watanabe et al, GRL, 28, 3289-3292.2001. [Frank Bryan]	strenghtened.
5-590	A	19:6		No mention of nutrients is made in this section, although they are referred to in the Introduction of the section, and are important with respect to primary production, which influences C drawdown. They are affected by temperature, freshwater inflow, and other climate change signals. I recommend that a summary of nutrient changes be added to the chapter. [Franklin Schwing]	Accepted (in missing sections).
5-591	A	19:15	19:15	The word "of" should be between "penetration" and "anthropogenic". [Melissa Bowen]	Noted, text modified
5-592	A	19:15		should read "downward penetration of anthropogenic carbon" [Frank Bryan]	Noted, text modified
5-593	A	19:15		downward penetration of … [Peter Gent]	Noted, text modified
5-594	A	19:15		penetration anthropogenic" add: "penetration of anthropogenic [Hartmut Grassl]	Noted, text modified
5-595	A	19:19	21:57	This is a general comment on the biogeochemistry section. I miss the synthesis with the physical changes, although this is one of the main objectives of this section here. Can there be an assessment on the solubility pump at the end of the section that relates the discussed changes in the hydrography to the chemical changes (in particular with respect to NADW and SAMW formation and perhaps the outgassing at low latitudes)? [Wilco Hazeleger]	Accepted (in missing sections)
5-596	A	19:19		Section 5.4.2. The role of dissolved organic matter (DOM) in climate change should be reported. DOM represents one of the largest exchangeable carbon reservoirs on earth. The global dissolved organic carbon pool (DOC) in the oceans is estimated to be similar to the concentrations of atmospheric CO2 (Siegenthaler U. and Sarmiento J.L., 1993. Atmospheric carbon dioxide and the ocean. Nature 365, 119-125.), as consequence net oxidation of only 1% of the seawater DOM pool within 1 year would be sufficient to generate a CO2 flux larger than that produced annually by fossil fuel combustion (Hedges J.I., 2002. Why dissolved organic matter. in Biogeochemistry of marine dissolved organic matter, edited by D.A. Hansell and C.A. Carlson, pp. 1-33, Academic Press, London, 2002). Siegenthaler and Sarmiento (1993) reported that ocean is a significant sink for anthropogenic carbon dioxide, taking up about a third of the emission arising from fossil-	Rejected. There is little information on **changes** in DOM. Labile DOM is a small pool.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				fuel use and tropical deforestation. Their calculations needed of a 'missing sink', that they hypothesized to be located in terrestrial biosphere. Recently Avril (Avril B. 2002. DOC dynamics in the north-western Mediterranean sea (DYFAMED site). Deep-Sea Research II 49, 2163-2182) reported that vertical dissolved organic carbon (DOC) fluxes may represent a large part of the 'missing sink' of atmospheric carbon in the oceans. He calculated that where strong winter mixing occurs, and assuming a DOC availability during the mixing period, the global DOC flux to deep waters, due to convective mixing, could be close to 2.0 Gt C y-1. Thus, DOC more than POC represents the main pool in which carbon is sequestrated in deep waters. DOC in the Mediterranean Sea has been reported to be strictly linked to water masses, so changes in termohaline circulation will surely affect also DOC distribution, with consequences on the role of oceans as a sink of CO2 (Santinelli, C. et al., 2002. Vertical distribution of dissolved organic carbon (DOC) in the Western Mediterranean Sea in relation to the hydrological characteristics, Deep-Sea Res. I, 49(12), 2203-2219; Seritti et al., 2003. Relationships between hydrological properties and dissolved organic carbon (DOC) in the Ionian Sea (winter 1999). Journal of Geophysical Research Oceans, 108, C9, 8112, doi:10.1029/2002JC001345). [Chiara Santinelli]	
5-597	A	19:21	20:39	This section appears not sufficiently described if compared to the others. I also observe that in the carbon subsection, there is no reference to the organic matter pool. It is well known that the largest reservoir of organic carbon in seawater is in a dissolved form. Recent studies in open oceans have shown that this fraction (referred as dissolved organic carbon (DOC)) plays an important role in the marine carbon cycling (e.g., see Hansell, D. A., 2002, DOC in the global carbon cycle. In: Hansell, D. A., Carlson, C. A. (Eds.), Biogeochemistry of Marine Dissolved Organic Matter, Academic, San Diego, California, pp.685-715). The consistency with changes in physical properties is well demonstrated as well (e.g. see Hansell, D. A., Carlson, C. A., Suzuki, Y., 2002. Dissolved organic carbon export with North Pacific Intermediate Water formation. Journal Geophysical Research 16, doi:10.1029/2000GB001361). [STEFANO VIGNUDELLI]	Rejected. There is little information on **changes** in DOM. Labile DOM is a small pool.
5-598	A	19:22	19:23	"most accurate method". I disagree with the notion that the O2/N2 method is the most accurate one. It has significant uncertainties due to an ill constrained ocean outgassing flux. It's an extremely elegant and very important method, and but I don't think that one can claim that it is the most accurate one. For example, using our ocean inversion approach, we believe that we can constrain the oceanic uptake of anthropogenic CO2 to within about +/- 0.3 Pg C yr-1 (this includes biases from data and models) (Mikaloff-Fletcher et al., in press, Jacobson et al., submitted). This doesn't have to be discussed here, since I think chapter 7 needs to provide an overview of the different methods available to estimate the oceanic uptake of anthropogenic CO2 and come up with a consensus number	Accepted. Carbon budget in Ch7.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				based on different methods, all with different strengths and problems. Futhermore, one needs to be very careful with the words here. "The global air-sea CO2 flux" refers to the total exchange flux, including the ~0.6 PgC yr-1 outgassing from river fluxes, while the subsequent discussion is really about the uptake of anthropogenic CO2. Therefore, the above needs to be changed to "The global uptake of anthropogenic CO2 from the atmosphere". (see also subsequent comments about the separation of natural and anthropogenic) [Nicolas Gruber]	
5-599	A	19:22	19:28	The section title and first sentence imply that they are talking about net CO2 flux, but I believe that the specific examples they give refer to anthropogenic CO2 flux. Please clarify. [Christopher Sabine]	Noted. Text and section title clarified.
5-600	A	19:22	22:22	I am wondering why there is no discussion of model results here. I believe that these observations provide strong constraints on the ocean models that are later used to make projections about future oceanic uptake of anthropogenic CO2 etc. For example, Matsumoto et al. (2004) showed that only a fraction of all models that participated in the OCMIP-2 project are actually consistent with the available CFC, radiocarbon, and ant. CO2 constraints. [Nicolas Gruber]	Rejected, belongs to Ch7.
5-601	A	19:23		Double the error figures to 95% confidence : 2.05±1.0 [Vincent Gray]	Rejected. This is not a statistical error.
5-602	A	19:24	19:28	"This is consistent with…" This discussion belongs to chapter 7, where it is currently largely missing. This section should focus on changes in this number [Nicolas Gruber]	Noted. Text remains but is reduced.
5-603	A	19:24	19:28	There are many other studies that tried to estimate the net uptake of anthropogenic CO2 by the ocean (again, this should be done in chapter 7) [Nicolas Gruber]	Noted. Budget numbers strenghtened in Chapter 7.
5-604	A	19:30	19:38	What is the point of this paragraph? The impression it leaves is that direct pCO2 measurments are unreliable because of potential changes in physics and biology. I think the direct measurements are the most reliable measure of change. The models and CFC approaches also have issues with physics and biology. I would like to see a clear statement of what has been seen. If it can not be extrapolated to a global number then so be it. [Christopher Sabine]	Accepted. Text clarified.
5-605	A	19:31		The units of the trend in air/sea fluxes should be PgC yr^-2. [John Church]	Rejected. The number refers to the flux, not to the trend. Text clarified.
5-606	A	19:33	19:33	McNeil et al. Although these authors reported different numbers for the 1980ies and	accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				199ies, their estimate is really based on a "model" and not stricly on data. Their result was obtained by first estimating the age distribution of the ocean using the CFC data from the WOCE survey and then deconvolving this distribution with the atmospheric CO2 history. Once you know this age distribution, you can estimate the oceanic uptake rate for any given period in the past (assuming that the circulation hasn't changed). Therefore the McNeil et al. result needs to be put into the same category as forward or inverse models of the anthropogenic CO2 uptake and shouldn't be mixed with the MLR based methods. [Nicolas Gruber]	
5-607	A	19:34	19:35	Change to….during the past few decades, although significant regional differences have been observed (Lefevre et al., 2005; Takahashi et al., 2005; Feely etal., 2005). [Richard Feely]	Noted
5-608	A	19:35	19:35	Lefevre should have a accent (') on the second "e" as on line 38 [Philippe Tulkens]	Noted
5-609	A	19:37		because there is … [Peter Gent]	Noted
5-610	A	19:37		correct: "there are large" to "there is large" [Hartmut Grassl]	Noted
5-611	A	19:38	19:38	Change to… Lefevre et al., 2005; Feely et al., 2005). [Richard Feely]	Noted
5-612	A	19:38	19:38	This is is self-serving, but I suggest to add Gruber et al. (2002) for Bermuda and Keeling et al. (2004) for HOT. In Keeling et al. (2004), we showed that it is not simply E-P driving the results (the magnitude of the salinity changes that can be induced by E-P are much smaller than the observed changes), but also large-scale ocean circulation changes, in this case associated perhaps with the PDO. [Nicolas Gruber]	Noted. Text reduced. Reference to Gruber et al included but Keeling et al no more relevant.
5-613	A	19:41	19:43	I think that the distinction between anthropogenic and non-anthropogenic changes in DIC needs to be disucssed in more detail. I believe that it is very critical to do this up-front, as many people have difficulty making this distinction, and the discussion comes back to this later. For example, it is not entirely clear what the MLR methods are actually estimating. [Nicolas Gruber]	Accepted. Text and section title clarified.
5-614	A	19:44	19:47	At the end of this sentence it would be important to acknowledge the Southern Ocean observations aswell by adding "and 1500m in the Southern Ocean (McNeil et al., 2001)". [Ben McNeil]	Accepted
5-615	A	19:46	19:46	Change to… Peng et al., 2003; Sabine et al., 2004b). Sabine, C.L., R.A. Feely, Y.W. Watanabe, and M.F. Lamb (2004): Temporal evolution of the north Pacific CO2 uptake rate. J. Oceanogr., 60(1), 5–15.	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard Feely]	
5-616	A	19:47	19:47	Sabine et al. 2004 did not look at changes between WOCE and GEOSECS…I think you mean Sabine et al.'s (1999) Indian Ocean paper. [Christopher Sabine]	Accepted.
5-617	A	19:49	20:14	This section relies on very old studies. Was this not covered in the TAR? If so, has there been any advance? Need to try to avoid including older studies, just for completeness. [Neville Nicholls]	Accepted.
5-618	A	19:49	20:2	Chen and Millero (1979) is one of the original references for the technique together with Brewer. Also there were many more criticisms of the Chen technique before Gruber et al. 1996. [Christopher Sabine]	Noted. Text reduced.
5-619	A	19:53	19:54	I don't think this paragraph adds anything at all to the report and could be reduced significantly / deleted. The old pre-formed DIC methodology of Chen (1979) should be referenced but not described in detail. The global estimate of 90+/-18PgC between 1750 and 1978 is not needed given the new C* estimate and the decadal based estimates between 1980-2000. I would suggest cutting out most of this paragraph. [Ben McNeil]	Accepted. Text is reduced.
5-620	A	20:2		5.5. This is a general comment about the treatment of sea level. Whereas, in the TAR, most sea-level issues were addressed in a single chapter (Chapter 11), they are here fragmented through several chapters and within chapters. I appreciate that there may be no way to change this at this stage, but it will make the AR4 considerably less useful to a sea level scientist compared with the TAR. [John Hunter]	Editorial decision
5-621	A	20:8	20:8	A few words on how the earlier estimates were obtained and their reliability is justified. [Robert Molinari]	Noted
5-622	A	20:9	20:9	"consistent with direct measure" I don't understand this sentence [Nicolas Gruber]	Noted, text modified
5-623	A	20:11	20:14	The potential bias estimated by Matsumoto and Gruber (2005) is actually +7% and not +10%. I also suggest to mention other studies that have looked at this issue, I.e. Hall et al., Primeau and Hall, Waughn et al., etc. Waughn et al., in their abstract presented at ICDC7, computed an anthropogenic CO2 inventory that is actually somewhat larger than that reported by Sabine et al. (2004). How can this be given that Hall et al. found a much smaller inventory for the Indian Ocean than Sabine et al. (1999). I don't know the answer yet, but I do know (unfortunately) that the results of the DC* method depends quite a bit on how and who applies it (there is some subjectivity involved). In summary, I think that it is premature to decide that the Sabine et al. (2004) inventory has to be reduced by about 10 Pg C.	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Nicolas Gruber]	
5-624	A	20:12	20:12	I think it is overstating the case to say that it has been proven that the global uptake is over estimated by 10%. Most GCM's suggest that the C* estimates under estimate the uptake. I would say that there are many uncertainties and biases that still have to be quantified, but it is not clear what the magnitude or direction of these biases really are. [Christopher Sabine]	Accepted
5-625	A	20:14	20:14	other errors. Now it gets complicated (see discussion by Keeling (2005) and Sabine and Gruber (2005)). If you want to be thorough, you have to bring up the distinction between the net air-sea CO2 balance and the oceanic uptake of anthropogenic CO2. Ocean warming per se, for example, does not impact the uptake of anthropogenic CO2 much, but it leads to a release of natural carbon from the ocean, i.e. it impacts the net air-sea balance of CO2. That's why it is important to distinguish anthropogenic CO2 and natural CO2 changes clearly up front. It's actually interesting to point out that the O2/N2 method can only determine the net air-sea CO2 flux, but not separate it into an anthroogenic and natural component. [Nicolas Gruber]	Accepted
5-626	A	20:16	20:16	Fig caption. Is this measure made throughout the depth of the ocean? [Chris Folland]	Noted
5-627	A	20:20	20:20	In my opinion, an additional figure showing more details of the anthropogenic CO2 distribution would be well justified. I think the fact that we have been able to map out the three-dimensional distribution of anthropogenic CO2 in the ocean is an important achievement and deserves more visibility. Sacrificing one of the many sealevel figures for this seems well justifiable. [Nicolas Gruber]	Accepted
5-628	A	20:20	20:21	How is the potential uptake calcualted? Does it account for changes in the Revelle Factor of the deep ocean? Is there a reference for this? [Christopher Sabine]	Noted, text removed.
5-629	A	20:23	20:24	Figure 5.4.2. Please, give a better quality figure [Renzo MOSETTI]	done
5-630	A	20:23		Figure 5.4.2 This figure is unreadable. It needs to be higher quality. [Melissa Bowen]	done
5-631	A	20:25	20:27	This paragraph should be deleted as it adds absolutely no value to the report at all along with the statistical insignificance of presenting an estimate of 28+/-26PgC for the anthropogenic DIC inventory between 1978 to 1994. Comparing two pre-industrial, indirect and independent methods to estimate the decadal anthropogenic inventory (which are subject to many biases - especially the Chen method) is really flawed. The MLR and CFC methods explicitly estimate the decadal inventory between 1980-89 to be 15+/-3PgC	Noted. Text reduced.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				and between 1990-99 to be 18+/-4 PgC (McNeil et al., 2003) which have been assessed for the biases and found to be small (Matear et al, 2003). What is the goal of referring to out of date, erroneous and invalidated estimates such as the Chen (1979) method. [Ben McNeil]	
5-632	A	20:29		I think it is worth pointing out that the largest zonally-integrated accumulation of anthropogenic CO2 is occurring in the Southern Ocean. [Stephen R Rintoul]	Rejected. This is a global estimate.
5-633	A	20:30	20:30	Do you mean "still the same fraction within the uncertainty"? [Chris Folland]	Noted. Text clarified.
5-634	A	20:30		Double the error figures to 95% confidence : 0.36±0.18 [Vincent Gray]	Rejected. This is not a statistical uncertainty.
5-635	A	20:30		Double the error figures to 95% confidence : 0.42±0.14 [Vincent Gray]	Rejected. This is not a statistical uncertainty.
5-636	A	20:35	20:35	Table 5.4.1. This is an important table, and it likely deserves a bit more discussion. It seems to me, though, that this is most effectively done in chapter 7 [Nicolas Gruber]	Noted. Table remains here, but links to Ch7 are improved.
5-637	A	20:35		Should be careful about how this is discussed. The bulk of the decrease in uptake fraction is simply due to the fact that the ocean has had longer to come into equilibrium with the 1750-1994 emissions than to any slowdown either due to chemistry or circulation change. This should be stated explicitly. [Anand Gnanadesikan]	Accepted. Text clarified.
5-638	A	20:35		Double all the error figures to 95% confidence in Table 5.4.1 [Vincent Gray]	Rejected. This is not a statistical uncertainty.
5-639	A	20:35		Table 5.4.1: 3rd row: I did not find these numbers in Chapter 7. If they are observationally based it would seem to me to make more sense for the relevant literature to be cited in this chapter. If they are model based then it is probably worth elaborating the reference to chapter 7 so that the reader understands the origin of the numbers immediately, and knows exactly where to look for more detail. [Martin Manning]	Noted. Links to Ch 7 improved.
5-640	A	20:36		indicate units (PgC) in body of table or caption. [Frank Bryan]	Accepted
5-641	A	20:41	20:47	A few references should be inserted in 5.4.2.3. There was a nice Royal Society report on acidification last July. I understood from that report that changes in pH since 1750 are inferred from carbon chemistry and are not from observations. Is that what "is estimated to have caused" means in lines 43-44, page 5-20? [Harry Bryden]	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-642	A	20:41		Section 5.4.2.3: Similar content can be found in Box 7.1 and Section 7.3.2.2.3. I suggest that this section be merged into those parts in Chapter 7, or at least there should be a reference to those in this section. [Michio KAWAMIYA]	Rejected. Observations belong here.
5-643	A	20:41		Section 5.4.2.3. This section is very brief and figure 5.4.3 is not very informative. I would suggest some elaboration on the possible consequences of ocean acidification (if it it not done elsewhere in the draft report). A reference for this could be cited: Orr J.C et al. "Anthropogenic ocean acidification over the twenty-first century and its impact on calcifying organisms", Nature, Vol 437, 29 September 2005. [Philippe Tulkens]	Noted. 1 sentence is added. Links to Ch7 improved.
5-644	A	20:42	20:42	Do you mean "dissolved CO2 forms a weak acid"? [Chris Folland]	Accepted
5-645	A	20:45		Double the error figures to 95% confidence : 28±52 [Vincent Gray]	Rejected. this is not a statistical error.
5-646	A	20:47		These changes should be compared with inferred historical changes. [John Church]	Accepted
5-647	A	21:1	21:1	Fig 5.4.3: Not easy to understand all the axes and how they relate to the plots, or some of the units. [Chris Folland]	Noted. Figure improved.
5-648	A	21:1	21:2	Figure 5.4.3. Please, give a better quality figure [Renzo MOSETTI]	Done.
5-649	A	21:1		Figure 5.4.3 This figure is unreadable. It needs to be higher quality. [Melissa Bowen]	done
5-650	A	21:3	21:3	Add Orr et al. Nature, 437, 681-686, 2005. [Anand Gnanadesikan]	Rejected. Orr et al is a modelling study.
5-651	A	21:3		Section 5.4.2.4: Similar content can be found in Box 7.1 and Section 7.3.2.2.3. I suggest that this section be merged into those parts in Chapter 7, or at least there should be a reference to those in this section. [Michio KAWAMIYA]	Rejected. Observations belong here.
5-652	A	21:10	21:21	A more detailed explanation of the causes of the shoaling process would make the significance of this clearer to a policymaker. [Chris Folland]	Accepted
5-653	A	21:10		The aragonite under-saturation should emphasise changes in the Southern Ocean, the only area where surface waters become undersaturated in aragaonite (relevant since the organisms forming aragonite shells live in the surface ocean). [Stephen R Rintoul]	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-654	A	21:11	21:11	Change to……Sarma et al., 2002; Chung et al., 2003; Feely et al., 2004). Chung, S., K. Lee, R.A. Feely, C.L. Sabine, F.J. Millero, R. Wanninkhof, J.L. Bullister, R.M. Key, and T.-H. Peng (2003): Calcium carbonate budget in the Atlantic Ocean based on water column inorganic carbon chemistry. Global Biogeochem. Cy., 17(4), 1093, doi: 10.1029/2002GB002001. [Richard Feely]	noted
5-655	A	21:15		a dot is missing after "al" [Hartmut Grassl]	Noted
5-656	A	21:17	21:21	The uncertainties in the GEOSECS alkalinity numbers are about the same size or larger than the changes Sarma presented. I would add a caveat to this statement saying that additional studies are needed to verify whether the changes reported by Sarma are seen in other areas. [Christopher Sabine]	Noted. Text revised.
5-657	A	21:20	21:21	This sentence is incorrect....Sarma et al. (2002) stated that half of alkalinity change is accompanied with processes which also change AOU, and they indicated both the change in biological activity and the change in water residence time as candidate of such processes. [Tsuneo Ono]	Accepted. Text modified.
5-658	A	21:21	21:21	"due to biological activitiy" This is not clear and needs to be explained. I haven't seen the publication, but it is really that clear whether this trend is of biological origin or could it reflect changes in ocean ventilation/mixing? [Nicolas Gruber]	Accepted. Text modified.
5-659	A	21:24	21:54	This seems like a very superficial coverage of oxygen changes. What might these oxygen changes imply for the carbon cycle? [Christopher Sabine]	Accepted. Links to physics and carbon improved.
5-660	A	21:25	21:29	I would start the oxygen section with an intro paragraph that highlights the great value of oxygen for detecting ocean bgc changes --> see EOS paper by Joos et al. (2003). I also think that a link should be made to the atmospheric oxygen method for the partitioning of the ocean/land sinks. [Nicolas Gruber]	Noted. Text improved, but link to O2/N2 bethod belongs in Ch7.
5-661	A	21:26	21:26	which are only of the order of 10-6 .. [Philip Woodworth]	Accepted
5-662	A	21:31	21:32	How does this compare to mean oxygen concentrations? [Chris Folland]	Rejected, the mean is not relevant.
5-663	A	21:31	21:36	I doubt that the decadal variations found by Garcia are very relevant in the context here. The variations are very small compared to the oxygen content of the thermocline or the	accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				observed changes in the thermocline. From the rapid air-sea exchange rate of O2, one expects and this is indeed observed that the surface waters are always close to equilibrium with the atmosphere and that oscillation reflect changes in the solubility. I suggest to reduce the text of this and the following paragraph on surface O2 changes, put the surface changes quantitatively into context with the overall observed O2 changes, and switch the order of the text first presenting changes in the thermocline and the deep. [Fortunat Joos]	
5-664	A	21:31	21:31	umol/kg" : the "u" should be replaced by the greek symbol "miu [Philippe Tulkens]	accept
5-665	A	21:31		correct: umol/kg" to "µmol/kg" [Hartmut Grassl]	accept
5-666	A	21:36		correct: "are though to be" to "are thought to be" [Hartmut Grassl]	Noted
5-667	A	21:39		… with the local [Peter Gent]	Noted
5-668	A	21:39		with local the" change to: "with the local [Hartmut Grassl]	Noted
5-669	A	21:42		equilibrates quickly with the … [Peter Gent]	accepted
5-670	A	21:46	21:54	"consistent with changes in ventilation". This section could be strengthened by a stronger and more explicit connection to the discussion in the physical parts of the chapter. Interestingly, though, some of the arguments about "decreased ventilation" in the physical parts (e.g. in section 5.3.3.4) are actually based on oxygen (this would cause a circular argument) [Nicolas Gruber]	Accepted
5-671	A	21:48	21:51	On page 5-21, lines 48-51, the same sentence is repeated. [Harry Bryden]	ok
5-672	A	21:48	21:51	Repitition. [Chris Folland]	ok
5-673	A	21:48	21:52	This sentence is repeated. [Peter Gent]	ok
5-674	A	21:48	21:48	There must be some reduplication here. [Tsuneo Ono]	ok
5-675	A	21:48	21:52	This paragraph contains an almost-repeated sentence. [David Parker]	ok
5-676	A	21:49	21:51	correct to: "µmol/kg"	accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Hartmut Grassl]	
5-677	A	21:52	21:52	Do you mean reductions in ventilation? [Chris Folland]	Accepted, text clarified.
5-678	A	22:0		Section 5.5: Overall this section contains appropriate material and explains the results well. Congratulations. However, I suggest this section needs a little further revision - it reads as a sum of pieces contributed by individuals. While each of the sections are generally OK they do not fit together as well as they could. There are also significant similarity from some of the results presented and the reconstructions of Church et al. (2004). Highlighting these simlarities would enhance the assessment and emphasise a growing concensus on at least some aspects of the sea level rise issues. [John Church]	This section has been entirely rewritten to make it more homogeneous
5-679	A	22:0		Section 5.5 Sea level is very important for this report. The suggestion is to make more clear the conclusion and the role of the various elements. The description is very detailed but sometimes one get lost. [Renzo MOSETTI]	Accepted; text modified
5-680	A	22:0		This is a very long section of "Introductory remarks", since this was covered in the TAR. [Neville Nicholls]	Accepted; text modified
5-681	A	22:1	23:2	Introductory Remarks for Sea Level section seems to be too lengthy. Most explanations in Introductory Remarks is also re-explained in the following sections [kyung-ryul Kim]	Accepted; text modified
5-682	A	22:1		Section 5.5. Maybe it should be made clear what is global sea level rise and how should it be calculated. Is it relative or absolute sea level the one we are interested in? [Michael Tsimplis]	Accepted; text modified
5-683	A	22:1		A statement on how long we should be observing to get an estimate of sea level trends will help putting into context the estimates from altimetry. [Michael Tsimplis]	Noted
5-684	A	22:2	22:2	I read the sea level section as an interested non-expert, and my comments below are therefore form a user's perspective. I found it an interesting and useful summary of the knowns and unknowns, which gave me the information I wanted. I thought the level of detail was somewhat inhomogeneous, and suggest in detailed comments below a few areas that I felt might be shortened or lengthened. [Richard Wood]	Accepted; text modified
5-685	A	22:2		5.5. This section should include a definition of the quoted uncertainties -- are the error bars +/- one standard deviation, or are they more like limits, such as 95-percentile values? [John Hunter]	Noted
5-686	A	22:7		Could cross-reference to WG2 assessment, especially Chapter 6 which is looking at the	Beyond the scope of this section

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				impact issue. [Robert Nicholls]	
5-1205	B	22:7		change "Besides" to "Furthermore" [William Richard PELTIER]	Done
5-687	A	22:10		It should also be pointed out that correctly reproducing sea level would be a major verification of climate model simulations of climate change. At present it would appear that our understanding is deficient since they can not yet adeqautely reproduce observed sea level change. [John Church]	Noted, however the chapter is about observations, not about models
5-688	A	22:12	22:12	"6.5.7" should read "6.3.3", I believe. [John Hunter]	OK
5-1206	B	22:12		reference should be to Section 6.3.3 NOT to Section 6.5.7 [William Richard PELTIER]	OK
5-689	A	22:14	22:15	Sea level referred to the geoid is not necessarily absolute as the geoid changes with time as a result of changes of the Earth's shape (e.g. through GIA and other crustal phenomena) and also through (e.g. decadal) changes in the ocean circulation. [Neil White]	Accepted; text modified
5-690	A	22:14	:19	Here and later in the section (p23, l30-34; p24, l1), care needs to be taken with the reference frame issues. My understanding is that tide gauges measure sea level relative to the crust. If GIA is the only land motion, then the tide gauge data, corrected for the commonly used GIA, measures sea level relative to the geoid (and not the centre of the earth). This is the sea lvel measurement required for estimating changes in ocean volume. A further correction is required to estimate sea level relative to the centre of the earth. Altimeters measure sea level relative to the centre of the earth and need to be corrected (as in Tamisea et al.) to measure relative to the geoid. For estimating sea level change associated with a changing ocean volume, we want sea level relative to the geoid. A further difficulty is that altimeter biases are corrected with tide gauge data which may measure sea level relative to the crust, or the geoid if corrected for GIA and there are no other crustal motions, or the centre of the earth if corrected by GPS or DORIS. [John Church]	Accepted; text modified
5-1207	B	22:14		a reference of sea level change to the geoid is NOT absolute as the geoid is itself a dynamic surface specified relative to the centre of mass of the planet [William Richard PELTIER]	Accepted; text modified
5-691	A	22:15	22:15	I think "or to the geoid" should be omitted here, as it is not clear what is meant by "the geoid" (see comments to page 22, lines 18-19 of this Section). [John Hunter]	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-692	A	22:15		Is it true that present satellite altimetry provides absolute sea level? I thought that there was a necessity to intercalibrate with tide-gauges. [Michael Tsimplis]	Satellite altimetry provides sea level measurements wrt the geocenter. In this sense, it is qualified of 'absolute' (i.e., independent of crustal motions). The need for tide gauge calibration is to correct for onboard instrumental drifts (radiometer drift, etc.). To date this calibration is limited by the crustal motion contamination to the tide gauge records.
5-693	A	22:16	22:17	I'm surprised at the statement that the the "vertical land motion is sufficiently well known…" - even where there are long GPS records the uncertainty in vertical velocities is ~1mm/year or so - close to the magnitude of the signal we are trying to measure. [Neil White]	Accepted; text modified
5-694	A	22:18	22:19	I do not agree with the statement "when comparing with climate-related contributions, only absolute sea level is considered". There are really four types of sea level and not two. The first two are as defined in this section ("relative": relative to the Earth's crust, and "absolute": relative to the centre of the Earth).The other two may be derived by firstly using a model of GIA to calculate what (a) tide gauges would have observed, and (b) what satellites would have observed, if there had been no ANTHROPOGENIC forcing of sea level. Among other things, this requires that we separate the recent melting of ice into one component which we consider to be "natural" (i.e. a recovery from the last glaciation) and another component which we consider to be anthropogenic (e.g. the recent melting of mountain glaciers). If we then subtract (a) from the tide gauge record and (b) from the satellite record we get two estimates of "anthropogenic" sea level rise. These two estimates, although almost always considered to be the same, are actually spatially different (although this difference may not be important), due to the effect of anthropogenic forcing on GIA. I believe that is important that this type of discussion is included in this section, to counter the many misunderstandings related to this issue. [John Hunter]	Accepted; text modified
5-695	A	22:21	22:26	While I agree that these two classes represent the most significant contributions to global sea level variations, motion of the solid earth effecting the volume of the ocean basins will also contribute to global absolute sea level change. See section 5.5.4.2. It is not clear to me if a sentence should be added here, or if lines 28-30 should be altered (separate comment.) [Mark Tamisiea]	Accepted; text modified
5-696	A	22:21	22:26	Shouldn't this be "Three classes of processes…"? Changes in ocean basin shape will	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				affect absolute GMSL, won't they? [Neil White]	
5-697	A	22:23		Why not give the word "eustatic" here, as the opposite of "steric"? Is there reason not to use it, perhaps. But the word IS in common usage and I wonder why it is avoided here. [David Vaughan]	It was decided not to use the term 'eustatic' because in the literature, it has different meaning (thus avoid confusion)
5-698	A	22:28	22:30	These paragraphs ignore the effect of crustal motion mentioned in 5.5.4.2. Strictly speaking, line 30 is correct only for relative sea level, but not the absolute sea level lines 18-19 make clear are important to climate studies. [Eric Leuliette]	Accepted; text modified
5-699	A	22:28	22:30	This paragraph appears to refer to all of 5.5.4; e.g., geoid changes are not mentioned in 5.5.4.1. However, motion of the crust (and thus changes in the ocean basins) due to GIA contributes to the global mean absolute sea level change. (Note, however, that there is no contribution to the global mean relative sea level change.) I am not sure of the best way to alter this paragraph. Perhaps a parenthetical comment at the end of the second sentence, such as (except for crustal motion due to GIA). Another possibility is to alter the previous paragraph (separate comment.) However, it cannot be said that Section 5.5.4 does not have a contribution to global mean absolute sea level change. [Mark Tamisiea]	Accepted; text modified
5-700	A	22:29	22:29	"Geoid change" is a very imprecise term. For me at least, it is not clarified by lines 8 to 17 on page 31 of this section. Which "geoid" is referred to here? The geopotential surface which would most closely match mean sea level if there were no anthropogenic forcing? The geopotential surface which most closely matches present sea level? The problem here is not just that the geoid changes in time, but that the actual geopotential level (i.e. the value of the potential) which is referred to as "the geoid" changes as the ocean volume changes and the crust deforms. If the word "geoid" is to be used here, a formal and understandable definition is require up front. [John Hunter]	Accepted; text modified
5-701	A	22:29	22:29	The cross-reference should probably be Section 5.5.4, rather than 5.5.4.1. [Eric Leuliette]	OK
5-702	A	22:30	22:30	To which type of sea level does "global mean" refer? (see comments to Chapter 5, page 22, lines 18-19, and line 29). [John Hunter]	Accepted; text modified
5-703	A	22:30	22:30	Is there a reference for the truth of this statement? [Michael Tsimplis]	Accepted; text modified
5-704	A	22:30	22:30	I'm not convinced that this statement ("These processes have however no contribution in terms of global mean") is true???	Accepted; text modifie

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Neil White]	
5-1208	B	22:30		the rate of change of geoid height due to the GIA process does NOT have zero mean [William Richard PELTIER]	Accepted; text modified
5-705	A	22:32	22:42	I liked the idea of a summary of the TAR conclusions, but as a non-expert I found this paragraph slightly too compressed to be clear. Some specific suggestions for clarification follow. [Richard Wood]	Accepted; text modified
5-706	A	22:34	22:35	Does the change in terrestrial water storage that results from human activities exclude the effects of anthropogenic climate change? This wan't clear to me. [Richard Wood]	YES
5-707	A	22:35		I suggest you also include (here and elsewhere) the value from the TAR if terrestrial storage issues are not included - 1.05mm/yr, marginally larger than the revised estimate in the AR4 of 0.9 mm/yr. [John Church]	Noted
5-708	A	22:36	22:36	"These contributions" need to be defined, all that is discussed previously in this paragraph terrestrial water storage. [Robert Molinari]	Accepted; text modified
5-709	A	22:36	22:36	"The sum of these contributions…" Not clear which contributions are being summed. [Richard Wood]	Accepted; text modified
5-710	A	22:36		sea level fall [John Church]	OK
5-711	A	22:37		see comment page 2, line 43-47 [Philip Woodworth]	Accepted; text modified
5-712	A	22:38	22:39	" … low compared to observations. In effect, the observed value…" Observations of what? Having read the whole chapter, I now assume you are referring to the imbalance between the estimates of individual terms and the directly observed total sea level rise. But that was not clear to this non-expert reader at this stage in the text. [Richard Wood]	Accepted; text modified
5-713	A	22:42	22:42	Are the scenarios listed as in the second to last sentence possible resolutions to Munk's "Enigma", rather than the "Enigma" itself? Munk argues that constraints derived from perturbations to the Earth's rotation eliminate other possible under-estimations in TAR (such as melt from ice sheets and glaciers.) It should be noted that Munk listed a third possible explanation, which may be relevant. [Mark Tamisiea]	Accepted; text modified
5-714	A	22:42	22:42	as the 'Enigma' [Philip Woodworth]	OK

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-715	A	22:42		English -- "an Enigma" [Robert Nicholls]	OK
5-1209	B	22:42		The sentence beginning"Munk(2002)---" should be changed to " Douglas and Peltier(2002) have referred to this as a "puzzle" and Munk(2002) as an "enigma". The missing reference is: Douglas, B.D. and W.R. Peltier, 2002. The puzzle of global sea level rise. Physics Today, March issue, 35-40. [William Richard PELTIER]	Noted
5-716	A	22:44	22:52	Please ensure that the bias introduced by the GIA 'contamination' reported in Chapter 6, p. 19, ll. 21-24 (see below), is considered in the discussion of current rates of sea-level rise. [Donald Forbes]	Noted
5-717	A	22:44	22:52	This paragraph is out of place and should be deleted here, it should be in exec summary. The discussion on "an acceleration or…decadal variability" is a false one as there is an acceleration but it may not continue or it may have been enhanced by the Pinatubo cooling, as discussed elsewhere. [Kevin Trenberth]	Accepted; text modified
5-718	A	22:45	22:45	This sentence could mention that Jason has contributed to the altimetry record starting in 2001. [Eric Leuliette]	Accepted; text modified
5-719	A	22:45		Uncertainty needed in the 3 mm/yr [Michael Tsimplis]	Noted
5-720	A	22:48		What is near-global? How much in error could we be? [Michael Tsimplis]	Noted
5-721	A	22:49		The term "first" is not appropriate here. There were global observational estimates in the TAR. [John Church]	Noted
5-722	A	22:50		If sea level is highly non-uniform how can we consider the tide-gauge estimates as estimates of the global mean? [Michael Tsimplis]	Noted
5-1210	B	22:51		The sentence beginning "Direct estimates---" is contradicted by the Summary results in Figure 5.5.10 which shows that there is still an unexplained contribution to the observed rate of sea level rise of ~1.0 mm per year. [William Richard PELTIER]	Accepted; text modified
5-723	A	22:52	22:52	Do you mean "… can be explained by these two processes"? [Richard Wood]	Accepted; text modified
5-724	A	22:54	22:54	"… of present-day sea level rise". Presumably this means "…of sea level changes over the past n years"?	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Richard Wood]	
5-725	A	23:0	24:0	I have read the Sea Level section carefully and came away with a conclusion that was surprising to me: I found this Section to be very precisely written with a very careful consideration of quantitative throughness and accuracy in all parts of the sea-level rise challenge. The deliberations are very focussed on improved accuracy, and very transparent in what they know and what they are still investigating. Where their confidence is low on particular parts of the problem, they lay out very carefully what they do not know, and what they do not understand. Clearly, sea-level rise is not a totally solved problem, but their methodologies are precise and quantitative. I was also impressed by their evaluation of higher-order terms and their uncertainties, as well as their growth, and sometimes lack therof, of understanding sea-level rise at a deep, quantitative level. With 6 chapters still to review, so far the sea-level part of chapter 5 is the most advanced, accurate, and quantitative at a highly focussed level of research focus. My thanks and congratulations to the sea-level research community for a highly commendable contribution to this critical AR4 IPCC process. [Jerry Mahlman]	Accepted
5-726	A	23:4		My overall remark concerns a better attention to numerical measures of accuracy and uncertainty. All results cited in the text about "sea level change" should report uncertainties in the form of % confidence limits. Are we sure that not-scientific readership (e.g. policy-makers) know the meaningful of "confidence limits"? e.g., that 95 % means that there is a 1 in 20 chance that the result might fall outside limits. In some cases an effort has been made to provide confidence limits, but also for homogeneity it should be adopted for all the results cited in the text. [STEFANO VIGNUDELLI]	Accepted
5-727	A	23:7	:19	This paragraph contains a mixture of global and regional estimates. Suggest it be restructured to differentiate between the different estimates. [John Church]	Accepted; text modified
5-728	A	23:9		a map with the geographical distribution of the tide-gauges used for the estimates would be useful. [Michael Tsimplis]	Rejected
5-729	A	23:11	23:11	"Woodworth" spelled incorrectly. [John Hunter]	OK
5-730	A	23:11	23:13	Aparrently on the two studies mentioned there is a suggestion that the error in the estimate has been reduced from 0.5 mm/yr to 0.3 mm/yr. However, I cannot see this can be possible by just adding a few years of data. I think this reduction is unjustified and is caused by stating least square errors only.	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael Tsimplis]	
5-731	A	23:11	23:11	I think the Plag paper in the Phil Trans volume should be mentioned here also. [Philip Woodworth]	Accepted; text modified
5-732	A	23:11		should be Woodworth [Frank Bryan]	Accepted; text modified
5-733	A	23:11		Woodworth [John Church]	Accepted; text modified
5-734	A	23:11		Double the error figures to 95% confidence : 1.7±0.4 [Vincent Gray]	okj
5-735	A	23:11		Spelling "Woodworth" [Robert Nicholls]	OK
5-736	A	23:13		Double the error figures to 95% confidence 1.8±0.6 [Vincent Gray]	ok
5-1211	B	23:13		The estimate of 1.8+-0.3 mm per yr for the global rate of rsl rise published by Church et al.(2004) is essentially identical to that published by Peltier (2001) who obtained 1.84+-0.35 mm per year for the same quantity. This fact should be acknowledged here in order to increase the confidence of the reader that independent analyses are now leading to the same result. Otherwise one might be forgiven for having the impression that the authors of Church et al.(2004) wish to use the IPCC Report for establishing an undeserved priority. [William Richard PELTIER]	Accepted; text modified
5-1212	B	23:14		Has the paper by Church et al. (2005) actually been accepted for publication? I do not believe that a rational case can be made for ths conclusion. [William Richard PELTIER]	Yes it is
5-737	A	23:16	23:16	"-" missing in superscript to "yr". [John Hunter]	OK
5-738	A	23:18	23:19	Does this mean GIA is included or excluded? Since it is real it should be dealt with explicitly. The text is confusing in this regard. [Kevin Trenberth]	Accepted; text modified
5-1213	B	23:18		following "---(Proshutinsky et al. 2004)" insert the phrase ", the same as the global rate inferred by Peltier(2001) as well as that for the US east coast by Peltier(1996)." The new reference is: Peltier, W.R., 1996. Global sea level rise and glacial isostatic adjustment: an analysis of data from the east coast of the North American continent. Geophys. Res. Lett. 23, 717-720. [William Richard PELTIER]	Accepted; text modified
5-739	A	23:19		With the same GIA model?	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Michael Tsimplis]	
5-1214	B	23:22		following the phrase "---best estimate loies nearer to 2 than to 1 mm per yr" insert the phrase " as exlicitly concluded in Douglas and Peltier (2002)". There is no reason for refusing to cite the Douglas and Peltier paper as this was at least as thoroughly refereed as any journal article. [William Richard PELTIER]	Accepted; text modified
5-740	A	23:24	23:26	The underestimate of adjusted rates mentioned by Peltier (2001) stems from applying a linear trend to the geologic data, not from GIA being the only geological process involved. [Vivien Gornitz]	Accepted; text modified
5-741	A	23:24	23:28	I did not understand this discussion at all until I read Peltier (2001). I then discovered that the effect described by Peltier depends on the length of time over which one averages the GIA signal (in this case 4000 years or 1000 years), rather than on the method of estimating the GIA (i.e. by model or by geological inference). The connection between these two is that estimates of GIA by geological methods are generally over the longer time scale (i.e. thousands of years). This should be explained in the text. [John Hunter]	Accepted; text modified
5-742	A	23:28	23:28	this is correct but it doesn't account for all the lower European rates in general. This topic is returned to below, some connection could maybe be made. [Philip Woodworth]	Accepted; text modified
5-743	A	23:36	24:8	The altimeters measure 60N to 60 S. Are there any estimates on the uncertainty introduced by this limitation? Should that be added to the stated uncertainty? [Michael Tsimplis]	This could be done using ENVISAT but only for the past 4 years
5-744	A	23:36	24:19	Satellite altimetry is capable of providing absolute sea level measurements with the necessary spatial coverage in open seas. The recent Topex/Poseidon and Jason-1 missions have brought an improvement in many of the error sources that overwhelmed the estimation of sea level rates from previous missions. With the enhancement of the data record, estimating global sea level changes has been one of the important tasks. Unlikely using tide gauges, an accurate knowledge of the long-term crustal variations, one of the fundamental problems with this kind of instruments for the detection of the long-term signal related to climate change, is not more necessary. Unfortunately, the future of satellite altimetry is uncertain. At present, only one altimeter mission (Jason-2) is approved. Therefore, the satellite altimetry topic needs to be emphasized and supported more because it is now a mature technology in detecting sea level changes and therefore capable of providing a valuable data set complementing traditional tide gauges. [STEFANO VIGNUDELLI]	Noted
5-745	A	23:37	23:47	This text read to me like an advert and a tutorial for T/P and Jason. I think it could be	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				shortened with advantage for the IPCC reader. [Richard Wood]	
5-746	A	23:39	23:39	paradigm? standard? [Philip Woodworth]	OK
5-747	A	23:45	23:45	"10 day" should be "10-day". [Chiu-Ying LAM]	OK
5-748	A	23:46	23:46	"10 day" should be "10-day". [Chiu-Ying LAM]	OK
5-749	A	23:48	23:48	Again, is this "absolute" (i.e. relative to the centre of the Earth) or adjusted for GIA ? (although I note that Figure 5.5.1 states "GIA effects included"). [John Hunter]	Accepted; text modified
5-750	A	23:49	23:52	How can there be a rise and fall of global mean sea level due to the ENSO event? The water must stay somewhere, so globally averaged it should be neutral. [Douwe Dillingh]	ENSO produces a temporary increase of the central Pacific temperature, hence of thermal expansion
5-751	A	23:49	23:49	I ended up slightly confused about whether we are discussing absolute or relative sea level rise here. P 22 ll 15-16 state that the altimeter measures absolute sea level, Fig. 5.5.1 suggests that inverted barometer and GIA corrections have been included. What exactly does the 3.1 mm/yr refer to? [Richard Wood]	Accepted; text modified
5-752	A	23:49		In the figure it looks more like 10 mm. [John Church]	Accepted; text modified
5-753	A	23:49		Double the error figures to 95% confidence +3.1±0.8 [Vincent Gray]	accepted
5-754	A	23:50		It was an El Nino event not an ENSO event. [Kevin Trenberth]	Accepted; text modified
5-755	A	23:55		care must taken" add: "care must be taken [Hartmut Grassl]	OK
5-756	A	23:57	23:57	care must be taken [Douwe Dillingh]	OK
5-757	A	24:7		Double the error figures to 95% confidence 3.1±0.8 [Vincent Gray]	accepted
5-758	A	24:12	24:19	Figure 5.5.2 I'm pretty sure this reflects the signature of the PDO (or just of the big 97/98 ENSO) and of the Indian Ocean Zonal Mode. I think this should be mentioned since we know that there is decadal ENSO-like variability (Zhang et al. 1997, J. Clim., 10, 1004-1020). Cross referencing to other chapter might help here. [Wilco Hazeleger]	Noted, relation of sea level pattern to climate modes is discussed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-759	A	24:12	24:19	Presumably this paragraph and Fig. 5.5.2 are discussing best-fit linear trends at each point? If so, some indication of the significance of the regressions would be useful. I don't know how much to believe the maps. [Richard Wood]	Noted
5-760	A	24:13	24:14	I disagree with the statement "most investigators assumed uniform sea level change". For example, Douglas (1991 and 1997) divided the world into 9 regions into which he distributed around 20 tide gauge records. He then averaged the trends in each region and then averaged the results over all regions. If he believed that the sea level change was uniform, he would not have attempted to achieve uniform coverage in this way -- he would have simply averaged all the tide gauge records using equal weightings. It should also be noted that analyses of tide gauge records have generally involved much longer records (typically 80 years in length) for trend estimation than is used for satellite altimeter data (which is only around 12 years long) -- this is an implicit acceptance that sea level trends must vary spatially as the temporal variations observed in a single tide gauge record could not physically be representative of the world as a whole (there just isn't that much expansion or extra water to go around). [John Hunter]	Accepted; text modified
5-761	A	24:13		Note Church et al. (2004) is the only study that did not make this assumption. [John Church]	Accepted; text modified
5-762	A	24:14	24:14	perhaps this could be reworded "…for the first time clear evidence…". The evidence of regional variability has been around for a while, but I don't think anyone took much notice until they started looking at the maps from T/P! [Neil White]	Accepted; text modified
5-1215	B	24:14		add the following sentence. "This inference is essentially identical to the earlier result of Peltier (2001) based upon a relatively small number of esoecially long tide gauge recordings." [William Richard PELTIER]	Accepted; text modified
5-763	A	24:16	24:16	Before "It is also worth…", insert "Using T/P and Jason-1 satellite data, it was found that between 1993 and 2003, the annual mean sea level in the South China Sea rose at a rate of 7.4 ± 2.4 mm per year (Wong et al. 2003)." This is to provide information on mean sea level rise in the South China Sea which is a major basin rimmed by a number of very populous countries. The full reference is given in comment # 10. It can also be accessed online at http://www.weather.gov.hk/publica/reprint/r556.pdf. [Chiu-Ying LAM]	Noted
5-764	A	24:17	24:17	In figure 5.5.2 there are also blue area's in the Atlantic Ocean. [Douwe Dillingh]	Noted
5-765	A	24:17	24:17	Figure 5.5.3 must be figure 5.5.2	OK

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Douwe Dillingh]	
5-766	A	24:17	24:17	"Figure 5.5.3" should read "Figure 5.5.2". [John Hunter]	OK
5-767	A	24:17	24:17	Figure 5.5.3 should be Figure 5.5.2. [Chiu-Ying LAM]	OK
5-768	A	24:17	24:17	Should refer to Figure 5.5.2 not 5.5.1. [David Parker]	OK
5-769	A	24:17	24:17	My version of the figure shows some significant areas of sea level fall in the Atlantic – especially the area around the North Atlantic Current, which could be dynamically important. The figure reference here should I think be to 5.5.2. [Richard Wood]	Noted
5-770	A	24:17	24:17	5.5.3 should be 5.5.2 [Philip Woodworth]	ok
5-771	A	24:17		Figure 5.5.2, not 5.5.3? [Peter Gent]	OK
5-1216	B	24:17		replace the word "Besides" with the word "Furthermore". Also the reference should be to Figure 5.5.2 NOT 5.5.3 [William Richard PELTIER]	Accepted; text modified
5-772	A	24:21	24:55	Should be compared or integrated with 5.2.2. [Kevin Trenberth]	Noted
5-773	A	24:32	24:44	I would recommend strengthening the caveats about results from Church et al. [2004]. These results are only valid if the assumption that the EOFs based on nine years of altimetry data span the variability of the tide gauge data. There are good reasons to believe that interdecadal variations are missed in this analysis. [Eric Leuliette]	Accepted ; tests need to be done
5-774	A	24:34	24:36	I think this sentence should be clarified. I assume that it means that the satellite data is too short to provide information on decadal variability and can only provide information on interannual variability -- the spatial variability of which has to be used to infer the spatial variability of the the multidecadal signals observed by the tide gauges. [John Hunter]	Accepted
5-775	A	24:34	24:35	As noted p 5-29 l 43-44 this is likely distorted by the 1997-98 event and shortness of record for determining EOFs. [Kevin Trenberth]	Accepted
5-776	A	24:40		two data sets for both regional and global average trends (January [John Church]	Accepted
5-777	A	24:41	24:42	The method of reconstruction used by Church et al. [2004] used a "eustatic" pattern (or	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				"EOF 0") in addition to the patterns found from altimetry. This pattern has the effect of averaging sea level from the tide gauges, so it is unlikely that the use of the reconstruction provides a better estimate of the long-term trend than Douglas [1991] and Holgate and Woodworth [2004]. [Eric Leuliette]	
5-778	A	24:41	24:42	The 1.8 +/- 0.3 mm yr^-1 actually comes from Church et al (2004). The figure (5.5.3) is from Church et al. (2005) which probably won't be published in time for this assessment (is the cutoff time the end of this year?). Figure 12 from Church et al. (2004) would be more appropriate at this point anyway. [Neil White]	Accepted; text modified
5-779	A	24:42	24:42	Figure 5.5.3 is from a different paper. Figure 5.5.3 should be Fig. 12 of Church et al. 2004. [John Hunter]	Accepted; text modified
5-780	A	24:42		Double the error figures to 95% confidence 1.8±0.6 [Vincent Gray]	ok
5-781	A	24:43	24:43	It would be helpful if the result of Holgate and Woodworth (2004) was given here (1.7 +/- 0.2 mm/year). [John Hunter]	Accepted
5-782	A	24:43	24:44	The reference to "see Figure 5.5.3" appears to be spurious. [John Hunter]	Accepted
5-783	A	24:44	24:44	the fig 5.5.3 is just the Church et al figure. I understood that Anny was going to make a combined figure with the Holgate and Woodworth numbers, as the text here implies. [Philip Woodworth]	Done
5-784	A	24:48	24:48	I do not understand what is meant by "(and the implied global correlations)". [John Hunter]	Accepted
5-785	A	24:52	24:52	Should "north-western" here be "north-eastern", given the colours on Fig 5.5.2? [Trevor McDougall]	Accepted; text modified
5-1217	B	24:54		following the phrase "---the US east coast" insert the words " as first discussed in detail be Peltier(1996) who demonstrated this to be due to the impact of the proglacial forebulge collapse during the glacial isoststic adjustment process". [William Richard PELTIER]	Noted
5-786	A	25:2	25:4	Maul and Martin (Maul, G.A. and D.M. Martin (1993), Sea level rise at Key West, Florida, 1846-1992: America's longest instrument record. Geophys. Res. Lett., 20, 1955-1958.) also hints at an acceleration.This reference may be more appropriate further down (e.g. where Donnelly et al is mentioned). [Neil White]	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-787	A	25:14	:16	I do not understand the implication of this sentence. [John Church]	Accepted
5-788	A	25:22		In the North Adriatic Sea, a comparison between Trieste and Venice tide-gauges has been made for separating subsidence from eustacy contribution. In the period 1896-2002 the sea level trend recorded in Venice is 2,50 mm/year and in Trieste 1,19 mm/year: the different rate corresponds to the subsidence of Venice area (Carbognin, Teatini, Tosi , Journal of Marine Systems 51, 2004,pp. 345-352) [Pierpaolo Campostrini]	Accepted
5-789	A	25:22		correct: "North American" to "North America" [Hartmut Grassl]	OK
5-790	A	25:22		and this para - the reader will be perhaps a but puzzled by the 0.4 mm/year per century on line 23, and the 0.012 mm/year/year on line 27 which is 3 times larger (it'd be good to quote them both I think as mm/year per century). Also as well as Woodworth et al 1999, please add Woodworth 1999: Woodworth, P.L. 1999. High waters at Liverpool since 1768: the UK's longest sea level record. Geophysical Research Letters, 26 (11), 1589-1592. However, the accelerations are in fact very similar. In Ekman (I think - I have not checked) and my papers the 'acceleration' mentioned is the quadratic coefficient (c) in a least squares fit to a + bt + c t**2 (as the text of my papers at least makes clear) whereas the acceleration of Church et al is d2h/dt2 = 2c. See what I mean? I think it is important to make clear that these acceleration rates are consistent. [Philip Woodworth]	Accepted
5-791	A	25:23	5:23	is the unit given for tha acceleration rate of 0.4 mm/year correct ? I believe it should be in mm/(year)*2 as on line 27 [Philippe Tulkens]	OK
5-792	A	25:23		Wrong reference: should be "Woodworth (1999)" on the Liverpool sea-level record? [Robert Nicholls]	corrected
5-793	A	25:25	25:28	The reconstruction used in Church and White [2005] is based on the assumption that the EOF patterns from altimetry span the variability from 1870-2000. The error estimate on the acceleration in their submitted paper does not take in account the uncertainty arising from missing, interdecadal variability. I believe that Church and White [2005] is still in review, so perhaps this sentence will need to be revised to reflect their final published results. [Eric Leuliette]	Accepted; Tests need to be done
5-1218	B	25:25		has the Church and White (2005) paper been accepted. I have the impression that it has not. [William Richard PELTIER]	YES
5-794	A	25:27		the acceleration should be 0.013	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[John Church]	
5-795	A	25:27		Double the error figures to 95% confidence: 0.012±0.012 [Vincent Gray]	OK
5-796	A	25:30	25:38	This part should include a reference to Sivan, Lambeck, Toueg, Raban, Porath and Shirman, 2004, Earth and Planetary Science Letters, 222, 315-330, which described sea-level observations based on coastal water wells in Israel, dating from the 1st to the 13th century AD. [John Hunter]	Noted
5-797	A	25:31		Archaeological data has also been studied in Venice to extrapolate the rsl trend from AD200 to 1900. Ammerman A et al (1999) Sea-level change and the archaeology of early Venice. Antiquity 73 303-312. [Pierpaolo Campostrini]	Noted
5-798	A	25:33	25:35	I think the point should be made that Lambeck et al. derived the "100 +/- 53 years before present" from linearly extrapolating back the "presently" observed sea level to the sea level observed 2000 years ago. However, given that there is little direct observational data on sea level prior to 1900, and none prior to 1700, it seems quite feasible that we are now observing the upward part of a multi-centenial cycle which happened to be at the same level in 2000 BP as it was around 1900. This is similar to the present arguments about the global temperature "hockey stick" -- the more smooth we assume that pre-20th century variations were, the more unusual the 20th century appears to have been! On the other hand, the results of Donnelly et al. (2004) indicate that relative sea level in Connecticut increased quite smoothly from 1300 to 1850, suggesting that any long-term variation in pre-industrial sea level had a period of, at least, many centuries. [John Hunter]	Noted
5-799	A	25:35	25:35	I searched Lambeck et al. (2004) for confirmation that "present" is the year 2000, but could not find it -- is this a pers. comm.? [John Hunter]	Noted
5-800	A	25:40	25:51	I found the 'data archaeology' paragraph at the end of 5.5.2.1.4 to be quirky. It looks like an invitation for the next IPCC report to have an extensive chapter on paintings and their mould patterns. [Harry Bryden]	Accepted; text modified
5-801	A	25:40	25:51	This paragraph does not add any quantitative information on global lone term accelerations and thus can be deleted. [Robert Molinari]	Accepted; text modified
5-802	A	25:40	25:51	As important as data archaeology is, I am not sure that an entire paragraph in an IPCC report is the place for this argument. [Neville Nicholls]	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-803	A	25:40	25:51	I hate to suggest this for such an interesting paragraph, but I feel it could be shortened. It reads (e.g. lines 40, 44-45, 50-51) rather like a plea for research, which I think is beyond our remit. [Richard Wood]	Accepted; text modified
5-1219	B	25:40		In my opinion the entire paragraph beginning "The importance---" should simply be deleted as it adds nothing of value for the urposes of the AR4. [William Richard PELTIER]	Accepted; text modified
5-804	A	25:47	25:51	It is not relevant the reference for Venice due to the local process of subsidence. [Renzo MOSETTI]	Accepted; text modified
5-805	A	25:51		The statistical treatment of sea level data can produce different inferences, as explored by Carbognin et al (Carbognin, Teatini, Tosi, 2004) and Butterfield in the case of Venice. Butterfield R. (2005) Interpretation of relative sea-level change in Venice in Fletcher and Spencer (Eds) Flooding and Environmental Challenges for Venice and its Lagoon: State of Knowledge, Cambridge University Press. [Pierpaolo Campostrini]	Noted
5-806	A	25:53		Section 5.5.2.2 was interesting, though I was not sure of its purpose in the chapter as a whole. The goal seems to be mainly to expose the issues in constructing and interpreting regional sea level timeseries, but because it is based on examples an actual regional assessment is not given, so the value that it adds to the global maps such as Fig. 5.5.2 was not clear. For me the section could be shortened. [Richard Wood]	Accepted; text modified
5-807	A	25:53		5.5.2.2 etc. I'm a bit uneasy why these regions were selected somewhat apparently arbitrarily. Why is there not a NE Atlantic region? There are important papers showing links between sea level and NAO e.g. Wakelin, S.L., Woodworth, P.L. Flather, R.A. and Williams, J.A. 2003. Sea-level dependence on the NAO over the NW European Continental Shelf, Geophysical Research Letters, 30(7), 1403, doi:10.1029/2003GL017041, 2003. Woolf, D.K., A.G.P. Shaw and M.N. Tsimplis. 2003. The influence of the North Atlantic Oscillation on sea level variability in the North Atlantic Region. The Global Atmosphere and Ocean System, 9(4), 145-167. see also Andersson paper (Tellus) referenced in Wakelin et al. I don't have the exact reference to hand, and Jevrejeva S., Moore, J.C., Woodworth, P.L. and Grinsted, A. 2005. Influence of large scale atmospheric circulation on European sea level: results based on the wavelet transform method. Tellus, 57A, 183-193. [Philip Woodworth]	Accepted; text modified A section on NE Atlantic is added
5-808	A	25:54		An area where sea level is well understood is the N.Sea. There are important results there in relation to the influence of the NAO on sea level variability. Maybese results should be	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				included as the also explain at least partly the lower trends found in European tide gauges. See for example Tsimplis et al., 2005, Phil Transactions of Roy. Soc. London, 363, 1329-1358 and references therein. [Michael Tsimplis]	
5-809	A	25:56	25:56	The vulnerability of small Pacific Islands (e.g. Tuvalu) to sea level rise is more than "potential"! [Neil White]	Accepted; text modified
5-810	A	26:1	26:24	Since "The extent to which the Mediterranean can have long-term sea level variability different from the global ocean remains an open question" and only a few records are available this section can be deleted. [Robert Molinari]	Accepted; text modified Section deleted
5-811	A	26:1	26:24	It should be emphasized that the Mediterranean is a complicated basin, with many different scales interacting within each other, and that, probably, around 15 years are not enough to conclude about the sea level trends. Not all of the observed patterns can be related to real sea level trends, but in some cases might reflect changes in the sea circulation. An example is the significant sea level drop observed in the Ionian Basin which might corresponds to a change in the regional circulation that took place in 1997-1998 (Pujol and Larnicol, 2005, Mediterranean Sea EKE variability from 11 years of altimetric data. MERCATOR model capability to reproduce it. Geophysical Research Abstracts, Vol. 7, 01919, 2005 SRef-ID: 1607 7962/gra/EGU05-A-01919 European Geosciences Union 2005). [STEFANO VIGNUDELLI]	Accepted; text modified Section deleted
5-812	A	26:4	26:5	I do not understand how "oceanic circulation (inclusive of changes in intermediate and deep water formation)" affects sea level -- although I can see how steric variations, addition of water mass, winds and atmospheric pressure (the latter two of which should probably be included in this sentence) can. I think this sentence needs recrafting. [John Hunter]	Accepted; text modified
5-813	A	26:8		In addition the Black Sea has shown sharp sea level rise even when the Mediterranean was going down. This is apparently related to reduced evaporation (Tsimplis et al. 2004, JGR) [Michael Tsimplis]	Noted
5-814	A	26:8		reword Only a few, good quality Mediterranean sea level records exist which are approximately a century long. These are .. [Philip Woodworth]	Accepted; text modified Section deleted
5-1220	B	26:8		change "spanning to" to " that extend from" [William Richard PELTIER]	OK

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-815	A	26:17		correct: "T/P at the Eastern" to "T/P in the Eastern" [Hartmut Grassl]	OK
5-816	A	26:25		I have a recent paper Tsimplis et al., 2005 published in GRL which essentially shows that the whole of the Med was going down to the NAO influence (mainly pressure increase) but that in the 1990s the Adriatic started going up faster than the west Med. The speculation is that that was an EMT effect. It is up to you whether you wish to add this or not. Anny has the reference.. [Michael Tsimplis]	Accepted; text modified Section deleted
5-817	A	26:27	26:31	A lot of this chapter is a bit dull in reading. I think this is because of the style, and because more words are used than necessary to express a finding. This is one example in this paragraph. Starting sentences with "It was found…" is pretty boring, especially when this sort of thing is as common as it is in this chapter. And this is even more of a problem when you start the previous sentence by listing the data that was analysed. Why not replace the first three sentences here with something like "Arctic Ocean sea level exhibits pronounced decadal variability corresponding to variability in the NAO (Proshutinsky et al., 2004; Hughes and Stepanov, 2004)." This is much shorter, simpler, and clearer (in my opinion). There are many examples thru the chapter where this sort of editing would redcue teh length dramatically, and improve clarity. [Neville Nicholls]	Accepted; text modified
5-818	A	26:32	26:32	I wonder why the RELATIVE sea level rise is given here -- is this the only one available from these studies? It would be better if a GIA-adjusted value was given. [John Hunter]	Accepted; text modified
5-1221	B	26:33		folowing "--- has ben 1.85 mm per yr" add ", a value in accord with the global and US east coast estimates reported by Peltier(2001) and Peltier(1996) respectively) when the raw tide gauge rates were coorected using the GIA estimated rates based upon the ICE-5G(VM2) model of Peltier (2004; see SEction 6.3 for further discussion of this latest model of the GIA process." [William Richard PELTIER]	Accepted; text modified
5-819	A	26:36	26:36	…the COASTAL Arctic seas…? [David Parker]	OK
5-820	A	26:43	26:47	This is related to the above comment for page 26, line 32: the effect of GIA does not appear to have been accounted for in this calculation.In the Arctic Ocean, Peltier's models seem to indicate a GIA of order 0.5 mm/year, which would, I think, close the budget without resorting to melting ice. [John Hunter]	Accepted; text modified
5-821	A	26:50	26:53	You don't need to describe ENSO. [Neville Nicholls]	OK

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-822	A	27:0		comment: "poor" [Hartmut Grassl]	OK
5-823	A	27:4	27:4	extra "Church et al." [Melissa Bowen]	OK
5-824	A	27:5	27:5	The value of 0.9 mm/year has become 1.0 mm/year in a revised version of Church et al., 2005b. [John Hunter]	OK
5-1222	B	27:6		I do not understand the sentence "However,---". What should be said here is " However, Peltier (2002) and peltier et al. (2002)has demonstrated that these rates are biased down by the influence of the global isoststic adjustment process by approximately 0.4 mm per yr. The climate related contribution to the 50 year records from this region is therefor best approximated by 2.0 mm per yr." These references are as follows: Peltier, W.R., 2002. On eustatic sea level history, Last Glacial Maximum to Holocene. Quat. Sci. Rev. 21, 377-396. Peltier, W.R., I. Shennan, R. Drummond and B. Horten, 2002. On the post glacial isoststic adjustment of the British Isles and the shallow viscoelastic structure of the Earth. Geophys. J. Int. 148, 443-475. [William Richard PELTIER]	Accepted; text modified
5-825	A	27:15	27:16	This appears to be a strange place to state that "all error estimates are one standard deviation". Does this apply only to the current paragraph or to some larger part of the text? The status of uncertainty estimates should be defined in a much more "global" and consistent way (e.g. for Section 5.5., for Chapter 5 or for the whole AR4 WG1 document). [John Hunter]	corrected
5-826	A	27:15		Double the error figures to 95% confidence 1.3±0.8 [Vincent Gray]	accepted
5-827	A	27:17	27:19	Isn't this statement true for altimetric records too? [Michael Tsimplis]	Noted
5-828	A	27:17		Double the error figures to 95% confidence 1.9±0.8 [Vincent Gray]	Ok
5-829	A	27:23	27:24	details about how to obtain the Hunter (2004) report would be better put in the references [Melissa Bowen]	accepted
5-830	A	27:24	5:24	The web link given should be given with the reference of Hunter (2004) not in the body of the text. [Philippe Tulkens]	Accepted
5-831	A	27:24	27:24	I am not sure if this is a valid reference for the purposes of AR4, but anyway the URL is now obsolete; the correct present one is: http://www.antcrc.utas.edu.au/~johunter/	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[John Hunter]	
5-832	A	27:25	27:25	"contaminated by subsidence". It would be useful to describe whether the subsidence is an artifact in the data set or if subsidence is a geological effect that adds up to the climate related sea-level rise. Of course, sea level rise needs to be quantified accurately and corrections for subsidence are necessary. However, if subsidence is a real effect that is measured in addition to sea level rise, it should preferably be note as such. [Philippe Tulkens]	Accepted
5-833	A	27:26	27:26	The figure of 1.2 +/- 0.8 mm/year refers to what was called in the reference a "less cautious estimate", which treated low sea levels caused by ENSO as data outliers which were rejected. It would be better to either quote the "cautious" estimate of 0.8 +/- 1.9 mm/year on its own, or to quote both the "cautious" and the "less cautious" estimates. This comment is related to the another one for the same line. [John Hunter]	Accepted
5-834	A	27:26	27:26	It should be noted that Church et al. (2005b) included an updated estimate of sea level rise for Tuvalu, using better survey control and a slightly longer record; this is 2.3 +/- 1.6 mm/year. This comment is related to the another one for the same line. [John Hunter]	Accepted
5-835	A	27:26		Double the error figures to 95% confidence 1.2±1.6 [Vincent Gray]	OK
5-836	A	27:29	28:8	So what is the conclusion -- mean sea-level rise is a good surrogate for rises in extremes, or do we need more work, and if so what? [Robert Nicholls]	Noted
5-837	A	27:29		Section 5.5.2.3: This section requires a definition of terms, such as "high water" and "surge". For example, what is "high water? -- is it the predicted or the observed maximum tide? Is "high water" the sum of the predicted high water and the surge? What is a "tidal residual"? [John Hunter]	Accepted
5-838	A	27:29		section 5.5.2.3 changes in extreme sea level It should be made clear that "Changes in extreme surges can be due to soil subsidence, sea level rise and changes in storminess, so that their response to climate change depends on a combination of factors with strog regional characteristics ". Possible location for this text, page 27, line 31 [Piero Lionello]	noted; text modified
5-839	A	27:29		section 5.5.2.3 changes in extreme sea level In my view, It is important to include the "Venice" case in this section. Venice has a very long observational record (not fully explored yet) and storm surges are a frequent and	The restriction on the length of the section makes it difficult to include the example of Venice. In Europe, there are

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				increasing problem. Venice is a good examples of how it is important to disentangle the various causes of increase in extreme surges, as in this case local subsidence, sea level rise and changes of storminess contribute simultaneously. A study of mine considered the period 1940-2000. The reference is: Lionello P. (2005), Extreme surges in the Gulf of Venice. Present and Future Climate in "Venice and its lagoon, State of Knowledge" C.Fletcher and T.Spencer Eds., Cambridge University Press, Cambridge UK, 59-65. A description of this study is the following: "The analysis of extreme storm surges recorded in Venice in the period 1940-2000 (Lionello, 2005) shows a positive significant trend, which is produced by the combination of sea level rise and local ground subsidence. If the effect of these two factors is subtracted, the time series presents very large inter-decadal oscillations, which prevent the identification of an eventually present residual trend due to changes of storminess." Possible location for this text is at end of paragraph, page 28, line1. [Piero Lionello]	longer records for surge studies. Text modified
5-840	A	27:41	28:8	This section is full of sentences that first say so and so analysed these data, followed by a sentence saying so and so found this. This is another example of the sort of edit this chapter needs - Just say so and so found this trend at Liverpool from 19??-19??. [Neville Nicholls]	Accepted; text modified
5-841	A	27:41		p27, last para - there are errors in this which I pointed out in an email to Unnikrishnan. I must admit all these high waters makes for confusion! line 44 should read: On the other hand, values of [DROP annual maximum high water and] annual maximum surge at high water ... [Philip Woodworth]	Accepted; text modified
5-842	A	27:50		p50 residuals [Philip Woodworth]	accepted
5-1223	B	27:57		insert the word "to" following the word "due" [William Richard PELTIER]	accepted
5-843	A	28:0		Section 5.5.3: Like section 5.2.2, there are major omissions from this section, including the work with the SODA analysis, the Stammer and ECCO analysis and the many more regional analyses. All of these studies are an important component of steric sea level variation studies. [John Church]	Accepted; text modified
5-844	A	28:4	28:4	I think "extreme high water" here means "maximum [John Hunter]	noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-845	A	28:7	28:7	It should be noted that the situation regarding IOD and extremes in the Indian Ocean is not as simple as presented here. Woodworth and Blackman (2004) found only one instance of a site within the Indian Ocean which correlated with IOD, while there are at least 7 sites in the Pacific which correlated with IOD. [John Hunter]	Accepted; text modified
5-846	A	28:13	28:13	Presumably you mean " … global thermosteric sea level change would parallel ..." [Richard Wood]	Accepted; text modified
5-847	A	28:13		thermal expansivity. ok? [Philip Woodworth]	Accepted
5-848	A	28:16	28:16	I think the statement "if the equation of state of sea water were linear" strictly means "if the specific volume of seawater varied linearly with salinity" (this is not what is normally understood by the "equation of state" which is generally expressesd as density being a function of salinity, temperature and pressure). [John Hunter]	Since both density and specific volume are inversely proportional and vary with T and S in the ocean only by a few permille, a linear approximation for one of them is very nearly coincident with a linear approximation for the other (with changed sign of the coefficients).
5-849	A	28:23	28:28	I do not really understand this paragraph at all. If the equation of state is "linear" (in the sense given by my comment to page 28, line 16) then volume is conserved on adding fresh water to seawater. This paragraph seems to be describing some intermediate step (perhaps the case of the fresh water on its own with sufficient salt added to bring its salinity to an ocean value), which clearly involves the added water changing volume . This change of volume is clearly compensated (exactly for the case of a linear equation of state) by the transfer of salt out of the original ocean water. However, considering the original fresh water as an entity on its own, to which you can just add salt (without any mixing of actual water) seems completely artificial. I feel that this paragraph should either be clarified or omitted. There is a related comment to page 30, lines 35-36. [John Hunter]	Accepted; text modified
5-850	A	28:23	:28	While this is correct as stated, I believe the wording could be improved. [John Church]	Accepted; text modified
5-851	A	28:23		Antonov paper and p31 Wadhams and Munk - it is a pity that these papers are buried in the text in this way when the salinity changes COULD explain the enigma! They deserve more discussion. [Philip Woodworth]	Noted: We agree with the reviewer. Although there is substantial uncertainty associated with the estimate of global salinity variability (due to sampling limitations, e.g., lack of data) this section should emphasize that the "enigma" is not really an enigma. We simply do not have good enough

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No.	Batch	Page:line From	Page:line To	Comment	Notes
					estimates of some of the components that make up the freshwater balance of the earth system to accurately determine the sea level budget.
5-852	A	28:30	28:34	This paragraph is confusing, I think mainly because of the phrase "the barotropic circulation (or equivalently the level of no motion)". Firstly, I think the "barotropic circulation" is ill-defined -- is it just the depth-averaged circulation, or is it the circulation that would be caused by barotropic dynamics (i.e. where isopycnal surfaces are parallel with isobaric surfaces or, alternately, where density is only a function of pressure)? If "barotropic" should be replaced by "depth-averaged" in this paragraph, then I am not clear how the depth-avaraged flow is related to the "level of no motion" -- isn't the depth-averaged velocity derived by intregrating the horizontal density gradient once in the vertical, up from the layer of no motion, to obtain the velocity profile, and integrating again over depth to obtain the total transport, and finally dividing by the range of depth to get the depth-averaged current? I think that this paragraph needs a bit of thought, although I think I know what it is getting at. [John Hunter]	Accepted; text modified
5-853	A	28:30	28:34	I didn't see the point of this paragraph, apart from the first sentence. If some assessment can be made of dynamical contributions to regional change, that would be useful. [Richard Wood]	Accepted; text modified
5-854	A	28:36	28:49	The difference between thermosteric rise from Levitus 2000 and from Levitus 2004 is striking. Good to see tht Willis et al is like the new Levitus estimate, but what is the cause of the 2 Levitus products? Is this just the interpolation? This could be made more clear. [Wilco Hazeleger]	Noted: See response to Comment 5-858
5-855	A	28:42		and elsewhere. The discussion refers to a rate of rise as if it were constant, but it is not. [Kevin Trenberth]	Accepted; text modified
5-856	A	28:43	28:43	"Pentadal" is an appallingly imprecise word and should (presumably) be replaced by "5-yearly". The word "pentadal" is not even in the Oxford English Dictionary. In addition, the OED defines the noun "pentad" as EITHER a period of 5 DAYS or a period of 5 YEARS (both meanings having been used in climatology). [John Hunter]	Noted: Pentadal is frequently used in the meteorological (for 5-day period) and oceanographic literature (5-year period). We will insert "(5-year)" after the first use of "pentad". Accepted; text modified
5-857	A	28:48		Double the error figures to 95% confidence 0.38±0.08, 1.8±0.4 [Vincent Gray]	OK
5-858	A	28:53	29:48	Figures 5.5.6, 5.5.7 and 5.5.9 all show results from Levitus et al (2000), which are incorrect. They should be updated to show results from Levitus et al (2005).	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Incidentally, the caption to fig 5.5.6 says 2004 for these results, which should be 2005. [Peter Gent]	
5-859	A	29:1	29:11	and Table 5.5.1. The mean rate of 1.55 should not be used but rather an assessment should be done of the 3 studies and the best value assumed: presumably the 1.6 value? Also, shouldn't an adjustment be made for omission of effects below 700 m? Please use 95% error bars not rms. [Kevin Trenberth]	Accepted; text modified
5-860	A	29:1		Double the error figures to 95% confidence 1.6±0.6 [Vincent Gray]	OK
5-861	A	29:6	29:6	The error bar is +/- 0.10 in Table 5.5.2 and on page 35 line 16. Given the methodological or "structural" uncertainty giving a range from 1.2 to 1.8 in Table 5.5.1, the wider error bar is probably wiser. The use of error bars should be consistent throughout the Chapter, i.e. always use 1 sigma or always use 95% confidence limits. [David Parker]	Accepted
5-862	A	29:6		Double the error figures to 95% confidence 1.53±0.6 [Vincent Gray]	OK
5-863	A	29:9	29:10	In Table 5.5.1 on page 5-29, I was struck by the fact that the steric sea level change over 50 years 1955-2003 of 17.3 mm (=0.36 x 48 years) was almost exactly the same as that from 1993-2003 of 17 mm (=1.7 x 10 years). Was there effectively no change in steric sea level from 1955 to 1993 and all of the change is really over the last 10 years? [Harry Bryden]	Accepted; text modified
5-864	A	29:9		Double all the error figures in Table 5.5.1 to 95% confidence [Vincent Gray]	OK
5-865	A	29:11	29:16	The Levitus et al (2000) results are known to be wrong. They overestimate the ocean heat uptake by at least 50%. The corrected estimates are in Levitus et al (2005). Therefore, references to work based on the (2000) results should be omitted, in order to avoid confusion. [Peter Gent]	Accepted; text modified
5-866	A	29:11	29:11	This is the first place that Lombard et al. (2005a) appears in this Chapter -- what was their estimate of sea level rise? [John Hunter]	Noted, text modified
5-867	A	29:11		The Levitus et al (2000) paper is superseded by the Levitus et al (2005) paper due to processing errors. Any discrepancy identified with the early paper is now irrelevant and need not be discussed here. [Frank Bryan]	Accepted; text modified
5-868	A	29:20	29:44	I expect in this paragraph a discussion of the similarities and differences of Figs. 5.5.1 and	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				5.5.5, and perhaps Fig.5.5.3. [Gerrit Burgers]	
5-869	A	29:20	29:45	This paragraph (and its four associated figures) seems like a long-winded way to say that things are complicated. I didn't feel any interpretation or synthesis was given. Also there is overlap with some of the earlier discussion in section 5.5.2. I think there is scope for considerable shortening here.The sentence at lines 41-45 is an important caveat to the methods described in 5.5.2.1.2, and I think the caveat would be better placed within that section. [Richard Wood]	Accepted; text modified
5-1224	B	29:20		Should the reference not be to Figure 5.5.6 rather than to 5.5.5? [William Richard PELTIER]	Accepted; text modified
5-870	A	29:25	29:25	"(1950-2000)" is inconsistent with "1950 to 1998" in the caption of Figure 5.5.7 on line 5 of page 5-81. [Chiu-Ying LAM]	Accepted; text modified
5-871	A	29:26		Sentence read alone could imply a global mean sea level drop. should read: ... often with rates of magnitude that of the global mean sea level rise. [Frank Bryan]	Accepted; text modified
5-872	A	29:26	:28	Note the reconstruction of trends from Church et al. (2004) for the 1955 to 1998 period (rather than 1950 to 2000 as shown in the paper) is very similar to the pattern in Fig 5.5.7. We will email the figure separately to Anny Cazenave. [John Church]	OK
5-873	A	29:27	29:27	"40-year period" should be "50-year period" in line with the text on line 25. [Chiu-Ying LAM]	Accepted
5-874	A	29:27	29:27	The previous discussion in this paragraph is relative to the 50-year period, 1950-2000, is the 40-year period given in this sentence an error, if not the 40-year period needs to be defined. [Robert Molinari]	Accepted; text modified
5-875	A	29:28	29:28	Replace "the 1990s" by "1993 to 2003" to be consistent with Figure 5.5.8. [Chiu-Ying LAM]	Accepted; text modified
5-876	A	29:32		Note that the first EOF for the Steric data looks very similar to the first EOF used by Church et al. (2004) [John Church]	OK
5-877	A	29:33		The Church et al 2004 study should also be cited here. [Kevin Trenberth]	OK
5-878	A	29:36	:37	it is written that a regime change was observed, originating in the Atlantic ocean. Is there a reference for this result ?	Noted, text changed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Pascale Delecluse]	
5-879	A	29:37	29:37	What is the evidence that this behaviour "originates in the Atlantic Ocean"? [John Hunter]	Noted, text changed
5-880	A	29:37		What is the evidence for this behaviour originating in the Atlantic. [John Church]	Noted, text changed
5-881	A	29:39		Note the tide gauge reconstruction also show an almost identical trend for this period. [John Church]	OK
5-882	A	29:41	29:42	I don't understand why the spatial pattern of sea level trends is likely not a long-lived feature. Why can't future trends be similar to the trends of the recent past? [Peter Gent]	Because EOF analyses of thermal expansion over the past 50 years show important decadal variability of the spatial patterns, with strong dependance on SOI, PDO, NAO, etc
5-883	A	29:41	29:44	I think this statement is a bit strong. Firstly, one could argue that, although Figures 5.5.7 (1950-1998) and 5.5.8 (1993-2003) look quite different quantitatively, for the Pacific and Indian Oceans the patterns for 1950-1998 and 1993-2003 are similar but of opposite sign -- in other words, the SAME EOF is dominant in these regions but the sign of the temporal multiplier has changed. Secondly, even though Figures 5.5.7 and 5.5.9 look similar, this doesn't mean that 5.5.7 is dominated by the first EOF (in fact, the captions to the maps in 5.5.9 indicate that the first mode only account for 14.9% and 27.8% of the variance in the two cases). Therefore there are clearly other important EOFs in the 1950-1998 data. By the same token, the pattern for 1993-2003 in Figure 5.5.8 most probably contains a number of important EOFs., which may not be visually obvious in 5.5.8, but which would be detectable by EOF analysis and may become dominant at other times. [John Hunter]	Accepted
5-884	A	29:41	29:44	(continued) I therefore see no reason why analysis of satellite altimeter data from 1993-2003 should not detect sufficient EOFs (even though they may not be the dominant EOFs during 1993-2003) to enable satisfactory reconstructions at other times. [John Hunter]	
5-885	A	29:41	29:42	Has anyone ever claimed that the trend pattern over the T/P period is long-lived??? [Neil White]	OK
5-886	A	29:41	:44	Note that the reconstructions do not use the pattern of sea-level rise for the altimetery decade. Rather they use the global correlations of sea level time series as expressed in EOFs. The first EOFs used (from the altimter data) is very similar to the first EOF of the Levitus and Ishii data. The reconstructions in fact give quite different pattterns to the 1990s trend over longer periods. [John Church]	This was clear!

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-887	A	29:41		"driven by" is incorrect. It is an association (and mutual adjustment). [Kevin Trenberth]	OK
5-1225	B	29:41		The sentence beginning "Moreover, this suggests---" is important and seems to strongly undercut the utility of the recent attempts to infer the existence of an acceleration of the global rate of sea level rise that have relied upon this methodology. [William Richard PELTIER]	Accepted; text modified
5-888	A	29:42	29:42	Change T/P to "satellite altimetry" or include Jason. [Eric Leuliette]	Accepted; text modified
5-889	A	29:42		Characterizing the pattern as "not a long-lived feature" could be interpreted to imply that it will not persist far into the future, whereas I believe the authors mean that it does not explain changes far back in the past. This sentence should be clarified. [Frank Bryan]	Future changes will probably be quite different!
5-890	A	29:43	29:43	Minor point: The pattern in Figure 5.5.2 is based on 12 years of data, not 10. [Eric Leuliette]	OK
5-891	A	29:46	29:48	Questoion marks have been added to the insert position of figures 5.5.8 and 5.5.9. I do not hope that this implies that these figures will be skipped, since the illustrate quite clear some of the methodological problems in the estimates of sea level rise, due to the existence of multi-year (pentadal to decadal) veriability, as is also stressed in the following pararaph. [Hendrik M. van Aken]	Noted, will not be skipped
5-892	A	30:1	30:1	Nowhere did I read about thermal expansion being an increasingly important component of sea level rise in te coming decades and centuries, as the mimportance of the melting glacers decreases. [Trevor McDougall]	Noted, the chapter is about previous decades, not about the future
5-893	A	30:2	30:2	There is only one figure about future sea level rise (namley Figure 5.1). This seems rather light on. What about figures of sea level rise under variuous greenhouse gas forcing scenarios. Do they occur in different chapters of the WG1 report? [Trevor McDougall]	Accepted. Sea level box includes contributions from Chapter 10 where future scenarios are discussed.
5-894	A	30:3	30:8	Same comments as before; eliminate references to the Levitus et al (2000) results. [Peter Gent]	OK
5-895	A	30:3	30:16	I am in disagreement here. Miller and Douglas average over very large areas and smooth so much that there is no thermosteric signal. In essence they have repeated the same error that Cabanes et al have made by averaging oceanic features. In Tsimplis and Rixen 2002, a paper published before the whole dispute started there is a comparison of thermosteric sea level change in Lagos estimated from Medatlas and the local tide gauge. The correlation is good. Miller and Douglas chose a tide gauge further north (Cascais and one	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				in Tenerife) and averaged all the avrious and shifting signals out. Moreover, if the NW Atlantic tide-gauges are not representative of the NA region (because of the gulf stream) how can they be considered as estimates of global sea level rise, they only describe the narrow band between the western edge of the Gulf Stream and the American coasts. [Michael Tsimplis]	
5-896	A	30:3	30:16	This paragraph appears to be representing a disagreement within the literature. It would be helpful to the user if you could provide either a synthesis, or a definite statement that this is an unresolved issue, and an assesment of what that implies. [Richard Wood]	Noted, para removed
5-897	A	30:9	30:16	There are too many "blow by blow" descriptions of the type, A did this and found that. This was confirmed by B. Just say things like: "Sea level rise in the second half of the 20th cenyury was mostly due to water mass added to the oceans, because inferred steric sea level rise is too low (by a factor of 3) to account for this rise (Miller and Douglas, 2004; Lombard et al, 2005b). Many similar examples thru the chapter. [Neville Nicholls]	Accepted; text modified
5-898	A	30:13	:16	I am not yet convinced by this argument. Indeed the Arctic example quoted on page 36, line 44 is counter evidence. [John Church]	Accepted; text modified
5-899	A	30:18		If the promise of ARGO for future estimates of heat content change are discussed, then so should other emerging observing systems such as gravimetry missions (GRACE, GOCE). See for example, Jayne et al, JGR, 107, doi:10.1029/2001JC001274, 2002. [Frank Bryan]	Accepted; text modified
5-900	A	30:25	30:25	As noted before, the term "geoid" is not well-defined. [John Hunter]	OK
5-901	A	30:25	30:25	sealevel" should read "sea level [Philippe Tulkens]	ok
5-902	A	30:30	30:46	I had trouble with Section 5.5.4.1 on page 5-30. To the opening of the section "Changes in ocean circulation are a consequence of changes in atmospheric forcing", I suggest adding "which are caused by changes in ocean circulation". Without some kind of linkage or uncertainty, I immediately wanted to know what the changes were in atmospheric forcing that had caused the non-uniform distribution of steric sea level changes. The steric changes are "attributed to changes in ocean thermal structure", but what atmospheric changes caused the changes in ocean thermal structure? The section makes it appear that we know what causes the non-uniform changes without giving any details. [Harry Bryden]	Accepted; text modified
5-903	A	30:30	30:39	Again, it would be useful if some assessment were made of the importance of dynamical	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				changes for regional sea level change. Maybe none is available on a wide scale, but I am thinking here, e.g. of the results of Hakkinen and Rhines 2004 based on T/P data – is this a purely thermosteric signal? [Richard Wood]	
5-904	A	30:32	:33	I think this is also true for the 1955-98 trends. I will email Anny Cazenave a plot of reconstructed trends for this period. [John Church]	OK
5-905	A	30:33	30:33	I think "5.5.3" should read "5.5.2". [John Hunter]	OK
5-906	A	30:35	30:36	Antonov (2002) has apparently "shown that the halosteric effect can be quite significant at a regional scale" -- but does it allow for the compensation noted on page 28, lines 23-28? [John Hunter]	Accepted; text modified
5-907	A	30:37	30:39	On page 5-30, lines 37-39, I understood the Stammer et al results were based on an assimilation of all available observations into a model run backwards and forwards repeatedly to get best fit to the observations. Is it fair to label this assimilative model as an independent confirmation of the observations? [Harry Bryden]	To some extent it is, however assimilation results usually (and specifically in the cited case) also reflect model errors.
5-908	A	30:38	30:39	Do you really mean density changes arising from wind forcing or steric height changes? [R Allyn Clarke]	Accepted; text modified
5-909	A	30:38	30:39	I do not see the point of the last sentence in this paragraph. [John Hunter]	Accepted; text modified
5-910	A	30:41	:46	Note the trends in atmospheric pressure are large enough to bias regional (and global averaged trends if care is not taken with the analysis or the trends are ignored - see church et al. 2004) [John Church]	Accepted; text modified
5-911	A	30:44	30:44	I think "On a time mean, regional changes ….." could be replaced by "Long-term-averaged regional variations …..". [John Hunter]	Accepted; text modified
5-912	A	30:48	30:49	I am not sure of the precise meaning of the sentence that begins: "Including the glaciers and ice caps…" Are the estimated contributions to sea level rise, the contributions from all the globes glaciers and ice caps, or only those associated with Greenland and Antartica? [R Allyn Clarke]	Comment refers to p.31. Text modified
5-913	A	30:49	31:27	does the whole process leave the ocean basins volume unaffected? [Michael Tsimplis]	Accepted; text modified
5-1226	B	30:54	:57	It is a significant mistake to connect the fall of sea level in the far field of the ice sheets	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				solely to th migration of water into the regions of forebulge collapse surrounding the regions in which deglaciation occurs. As pointed out in Peltier(1998) there is also significant migration of water from the interiors of the ocean basins into the regions peripheral to the far field continents, regins that experience downwards flexure of the crust due to the application of the off shore water load. This might be covered by inserting the phrase in line 56 fllowing the words "peripheral bulges" the phrase " of both the ice sheets and the far field contnents (Peltier, 1998)". The additional reference is: Peltier, W.R., 1998. Postglacial variations in the level of the sea: Implications for climate dyanmics and solid Earth geophysics. Rev. Geophysics 36, 603-689. [William Richard PELTIER]	
5-1227	B	31:1		following the word "planet" insert the phrase " as first described in detail in Wu and Peltier(1984)". [William Richard PELTIER]	Noted, text modified
5-914	A	31:4	31:4	Please change "signal on" to "contribution to". I also believe " - to the crust -" should be removed. The rest of the document refers to relative sea level with out this clarification. [Mark Tamisiea]	Noted
5-1228	B	31:4		delete the hyphen following the word "relative" [William Richard PELTIER]	Noted
5-915	A	31:5		Note that these models are also constrained by paleo data. [John Church]	Noted
5-916	A	31:8	31:15	I think this discussion should be combined with the discussion of page 22, lines 14-19, with reference to my comments to page 22, lines 15, 18-19, 29 and 30. [John Hunter]	Accepted; text modified
5-917	A	31:16	31:16	Please change "-0.3" to "-0.30". [Mark Tamisiea]	OK
5-918	A	31:16	31:16	from a plausible [Philip Woodworth]	OK
5-919	A	31:16		from plausible suite" add: "from a plausible [Hartmut Grassl]	OK
5-1229	B	31:16		following "---(Peltier, 2001)" insert the words " for the ICE-4G(VM2) model of Peltier (1994, 1996) and a value of -0.36 mm per yr for the ICE-5G(VM2) model of Peltier (2004). See Section 6.3 for further discussion." [William Richard PELTIER]	Noted, however due to space limit we cannot give all interesting detail
5-1230	B	31:16		delet the entire phrase "---with results from a plausible suite of viscosity models---((tamisea et al.(2004)". The reference to the Tamisea et al. paper cannot be referenced as this paper was not accepted for publication. The flaw in the analyses presented was that	The paper is in press

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				the authors accpeted models as plausible that could not fit the constraints provided by the large number of Holocene rsl records available globally. [William Richard PELTIER]	
5-920	A	31:20	31:27	If there is a 0.5mm/yr uncertainty due to GIA should that be added to the 0.3 mm/yr uncertainty stated earlier [Michael Tsimplis]	Noted, however the 0.5 number is local/regional and the other number is global.
5-1231	B	31:20		the sentence beginning " Unfortunately, uncertainties in GIA predictions---" and the references contained should be entirely deleted. The problem with these analyses is that the models that span the range suggesting errors in the GIA corrected globally averaged rate of rsl rise as large as 0.5 mm per yr are not constrained to fit the extremly large data base of available Holocene rsl histories. Theyare therefor NOT models that can be legitimately employed to correct tide gauge observed secular sea level trends. NOTE: I have no idea who has contributed Section 5.5.4.2 but it is hpelessly self serving of a small cabal of interests.Our document should not promote what is demonstrably bad science. [William Richard PELTIER]	Accepted; text modified
5-921	A	31:23	31:25	I do not fully understand the two sentences from "However ….." to " ….. far field.". Firstly, is this referring to the GIA effect of 20th century changes in ice volume (i.e. the difference between the spatial fields of the "two estimates of anthropogenic sea level rise" noted in my comments for page 22, lines 18-19)? Secondly, there seem to be much more obvious reasons why "GIA-corrected rates are to be expected" -- as discussed at length in Section 5.5.3 -- these need not have a "GIA-related" explanation at all. [John Hunter]	Accepted; text modified
5-922	A	31:23	31:23	Please remove "to be". [Mark Tamisiea]	oK
5-923	A	31:25	:27	Comment on these results. [John Church]	Noted
5-924	A	31:26	31:26	Please remove the parentheses: "sources". [Mark Tamisiea]	OK
5-925	A	31:31		Section 5.5.5.1: Suggest that the accuracy of the global average salinity estimates should be discussed. Personally I do not believe these are a strong contraint on mass added to the oceans. [John Church]	Noted
5-926	A	31:32		Is there a reference for the truth of this statement or is it a matter of belief? [Michael Tsimplis]	Noted, reference cited
5-927	A	31:36		Antonov paper and p31 Wadhams and Munk - it is a pity that these papers are buried in the text in this way when the salinity changes COULD explain the enigma! They deserve	Noted: The importance of these papers is that they point out that eustatic

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				more discussion. [Philip Woodworth]	effects could plausibly "close" the budget for sea level during the past 100 years. I do not believe we can say much more than this.
5-928	A	31:37		Precise assumption is a strange usage. [Neville Nicholls]	OK
5-1232	B	31:37		delete "precise assumption on" and replace with "A precise estimate of" [William Richard PELTIER]	OK
5-929	A	31:39	31:41	I know what this sentence means but the grammar is strange. [John Hunter]	OK
5-930	A	31:40	31:41	Wadhams and Munk estimate 0.6 +/- 0.18 mm/yr, but concede that there are large uncertainties in their estimate. I'm not sure what that means. How "large" are the uncertainties: +/- 0.18? Or are does the large uncertainty refer to the "known unknowns" mentioned on line 42? They have already assumed error bars for the sea ice melt so presumably that uncertainty is included in the +/- 0.18. This kind of problem is widespread and of course needs a pragmatic solution here. I think the sentences could be rephrased to make it clear what is meant (but can't suggest how without reading the Wadhams and Munk paper). [Richard Wood]	Noted, text modified
5-931	A	31:40		The sentence should read "Wadhams and Munk (2004) estimate the rate of sea level rise attributable to fresh water input as 0.6 +/- 0.18 mm/yr. [Frank Bryan]	Text rewritten
5-932	A	31:40		estimate to 0.6" drop: "to [Hartmut Grassl]	Ok
5-933	A	31:40		to be 0.6 [Philip Woodworth]	Ok
5-934	A	31:42	:44	Note that this residual is similar to the glacier and ice cap contribution (Chapter 4). [John Church]	Ok
5-935	A	31:44	31:44	it is factor 2 smaller but it is about the glacier and ice cap rate which is nice? [Philip Woodworth]	Noted
5-936	A	31:47	31:51	compare with chapter 4. [Kevin Trenberth]	Ok
5-937	A	31:49		Double the error figures to 95% confidence 0.43±0.12 [Vincent Gray]	accept
5-938	A	31:50	31:50	The estimated errors for 1961-1998 given here do not agree with Table 5.5.2.	accept

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Mark Tamisiea]	
5-939	A	31:50		Double the error figures to 95% confidence 0.88±0.26 [Vincent Gray]	accept
5-940	A	31:53	32:13	These two paragraphs contain an incorrect information concerning Greenland Ice Sheet contribution to sea level rise. The paragraphs should be modified considering (a) satellite analysis (Johannessen et al., Science Express, 20 October 2005, 10.1126/science.1115356) showing an average Greenland ice thickenning of about 5 cm per year; (b) decreasing summer temperature at the Summit if the Greenland Ice Sheet (Chylek, Box and Lesins, Global warming and the Greenland Ice Sheet, Climatic Change, 63, 201-221, 2004) and the fact that global warming affects dicetly only a small fraction of Greenalnd Ice Sheet margins (Chylek and Lohmann, Ratio of Greenland to global temperature change: Comparison of observations and climate model results, Geophysical Research Letters, 32, doi:10.1029/2005GL023552, 2005) while the rest is dominated by North Atlantic Osciallation. [Petr Chylek]	Accepted; text to be modified
5-941	A	31:53	32:4	There seems to be a little double counting here due to the "1990s rate" (which is based on altimeter observations) including the contribution due to "long-term imbalance from earlier climate change", and these two rates being later added together. I can't see any way around this, but at least it should be admitted. However, I may well have misunderstood what is being done here, in which case theings need to be clarified. [John Hunter]	Accepted; text tob e modified
5-942	A	31:53	32:4	It should be indicated whether the uncertainties quoted here are limits or standard deviations (from table 5.5.2, they appear to be limits). [John Hunter]	accept
5-943	A	31:53	:57	The agreement with Chapter 4 is not transparent here. This stresses the need for a table at the end of Chapter 4 with the cryospheric contributions listed. I think the relevant section number on line 55 should be 4.8.2 and it is not clear of the Antarctic contribution listed here includes the glaciers from the Antarctic peninsula or not. [John Church]	Accepted; text to be modified
5-944	A	31:54	31:55	The figure given for the Antactic contribution to sea level of - 0.2 to -0.0 does not correspond exactly to the figure given in Chapter 4 (p. 39 L 24 to 27). They nearly match though. Identical figures or a figure with an uncertainty range should be given. [Philippe Tulkens]	Accepted; text modified
5-945	A	31:54	32:3	I found this block of text hard to follow. It might need breaking up into smaller sentences. [Trevor McDougall]	Accepted
5-946	A	31:54		There are problems with consistency in Chapter 5. For instance, here it states: "during	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				1993–2003... the Antarctic ice sheet may have had a net mass gain" but in Table 5.5.2, 0.0+/-0.1mm/yr is given for the same period, and Chapter 4 is cited. The exectuive summary, bullet 12, gives a combined ice sheet number that seems not to be consistent with either of these. Please work with Chapter 4 to achieve consistency within and between chapters. [Michael Oppenheimer]	
5-947	A	31:55	31:55	The cross reference should be to Section 4.6. Figure 5.5.10 is symmetric about zero: see also Chapter 4 and my comment on Chapter 5 page 3 line 9. [David Parker]	Accepted; text modified
5-948	A	31:55	31:55	The reference to section 4.7 does not seem to be correct. Should it be section 4.8.2 in the First Order Draft [Philippe Tulkens]	Accepted; text modified
5-949	A	31:57	31:57	Please be more specifc about "earlier climate change". Which epoch? [Trevor McDougall]	Noted
5-950	A	32:0		Section 5.5.5.3.2: no numbers are given here but a number is quoted in the Summary. [John Church]	Accepted
5-951	A	32:1	32:1	"1990s" should read "1993-2003". [John Hunter]	OK
5-1233	B	32:1	:4	The ppublished ice sheet modelling studies provide no useful estimate on the extent to which continuous melting of Antarctica or Greenland following the last glacial interglacial transition could be contributing to the modern rate of global sea level rise. In Peltier (2002) and Peltier et al.(2002) referenced above, it was shown that Holocene rsl data from islands in the tropical Pacific Ocean provide a strong constraint upon the extent to which the onserved modern rate of rsl rise could contain such a contribution. Even a rate of 0.1 mm per yr would be sufficient to produce a visible midfit of the global GIA model to the observational constrints. [William Richard PELTIER]	Noted
5-952	A	32:4	32:4	I do not get this result for Antarctica. The "former" term lies between -0.2 and 0 mm/year, while the "latter" term lies between 0.1 and 0.4 mm/year, yielding a range for the sum of -0.1 and 0.4 mm/year. [John Hunter]	noted
5-1234	B	32:6		The paragraph beginning " Constraints---" sould simply be deleted as the papers referenced are hopelessly speculative and ill conceived. Even a contribution from present day Greenland melting of 0.2 mm per year eustatic equivalent over the past 100 years has a sufficiently strong impact uppon polar wander sped and direction as to be ruled out by the available space geodetic oservations. However, a larger contribution from Antarctic melting can be tolerated if the mass loss is coincentrated near the south pole of rotation as	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				this will influence only the length of day constraint, one that may continue to be met by an appropriate modification of the viscosity in the deepest mantle of the Earth. In Peltier (1998) formal inverse theory was shown to enable a current rate of Antarctic ice sheet melting of 0.5 mm per year to be easily tolerated by the earth rotation constraints solely me increasing the viscosity of the deepest mantle such that it has the form of the VM3 model therein described. If the chapter 5 team really wants to discuss the possible use of earth rotation data to constrain the rate of polar ice sheet melting then it should be obliged to reference this work. Reference to the Peltier(1998) paper was provided above. [William Richard PELTIER]	
5-953	A	32:9	32:12	The statement that Mitrovica et al., 2001, disagrees with altimetric observations is true. However, listing a possible improvement to this analysis without discussion of possible problems with the altrimetric results prejudices the reader. [Mark Tamisiea]	Noted
5-954	A	32:10		Double the error figures to 95% confidence +0.54±0.26 [Vincent Gray]	OK
5-955	A	32:11	:13	I gather there is a recent Mitrovica and Wahr EPSL paper that is relevant here. [John Church]	OK; the paper is now quoted
5-956	A	32:12	32:12	remain in stead of remains [Douwe Dillingh]	OK
5-957	A	32:12	32:13	The studies cited believe that they have properly accounted for GIA. To be less argumentative, I think "remains inconclusive because they are" should be removed. [Mark Tamisiea]	Accepted; text modified
5-958	A	32:12		these approaches remain … [Peter Gent]	OK
5-959	A	32:15		Section 5.5.5.3: The title of this section ("Land hydrology: natural variability in land water storage") is confusing, as it includes a section on anthropogenic change. I would think that simply "Land hydrology" would be an adequate title. [John Hunter]	Title changed
5-960	A	32:38	32:41	Unclear. Precipitation anticorrelated to thermosteric sea level implies less precipitation in warm climate. Are you trying to say that warm ocean -> more precipitation over land -> compensation of the thermosteric sea level rise because more water is stored on land? [David Parker]	YES
5-961	A	33:0		table 5.5.2: This table should be reformatted to parallel columns for the two different time periods. Also, on line 11, 1.0 should be 0.9 mm/yr. Also, suggest this table and the discussion should contain estimates of the terrestrial storage. Also suggest this table should contain the model estimates so that the reader can easily see all information at	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				once and thus assess the undertaninding and the reliability of the projections. [John Church]	
5-962	A	33:0		Table 5.5.2: I am not entirely clear how this should compare with Table 11.10 of the IPCC TAR. As I understand, Table 11.10 of the TAR separated the 1910-1990 sea level rise due to Greenland and Antarctica into three components: "Greenland - 20th century effects", "Antarctica - 20th century effects" and "Ice sheets - adjustment since LGM". In Table 5.5.2, 20th century sea level rise due to Greenland and Antarctic is separated into two components: "Greenland ice sheet 20th century" and "Antarctic ice sheet 20th century". Allowing for a small difference in the respective periods, the sum of the three terms from Table 11.10 of the IPCC TAR is equivalent to the sum of the two terms from Table 5.5.2. Whether my interpretation is right or wrong, I think it is important that sufficient explanation is provided to allow Table 11.10 of the IPCC TAR to be compared with Table 5.5.2. [John Hunter]	Noted
5-963	A	33:0		Table 5.5.2: the error noted for page 5-32, line 4 ("-0.2 to 0.4 mm/year" should read "-0.1 to 0.4 mm/year") has been carried across to row 9 of the Table, where "0.1 +/- 0.15" should read "0.15 +/- 0.12" (assuming the uncertainties in page 5-32, line 4, are limits of error). [John Hunter]	Accepted; text modified
5-964	A	33:0		Table 5.5.2: The sum of terms (3+5+7+9) is incorrect using the figures in the Table (it should be 0.93 +/- 0.28 ~ 0.9 +/- 0.3). However, using the correct values for row 9 (0.15 +/- 0.12; see earlier comment for this Table), the sum of terms (3+5+7+9) becomes 0.98 +/- 0.27 ~ 1.0 +/- 0.3 (i.e. the value quoted in the Table). This should all be checked. [John Hunter]	Accepted; text modified
5-965	A	33:0		Table 5.5.2: The sources given for rows 7 and 9 should be "Section 5.5.5.2", rather than "5.5.2.2". [John Hunter]	Accepted; text modified
5-966	A	33:0		Table 5.5.2: The terrestrial storage term should be included both in this Table and in Figure 5.5.10, as was done in the TAR. Otherwise, there is no indication at all as to whether the budget balances. [John Hunter]	This contribution is too poorly known to be included in the budget
5-967	A	33:0		Table 5.5.2: This table is difficult to read with a number of alternate rows representing 1961-1998 and 1993-2003. Either two separate tables or different columns for different period should be employed. [John Hunter]	Accepted; text modified
5-968	A	33:0		Section 5.5.6: This very important Section is marred by a hotpotch in the way that uncertainties are given, some being expressed as "A - B" and others being expressed as "C	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				+/- D", with little indication of whether the uncertainties are standard deviations or limits, and whether uncertainties have been added linearly or in quadrature. This needs cleaning up. [John Hunter]	
5-969	A	33:0		Table 5.5.2: Under sources, 5.3 should be 5.5.3 and 4.7 should be 4.6. [David Parker]	Accepted; text modified
5-970	A	33:4	33:5	This sentence needs to be better explained or it is wrong as expressed [Pierpaolo Campostrini]	Accepted; text modified
5-971	A	33:9		Infiltration can also occur from irrigation. [John Church]	Accepted; text modified
5-972	A	33:9		Add to line 9 and to references: Gornitz, V., 2001. Impoundment, Groundwater Mining and Other Hydrologic Tranformations. In: Sea Level Rise, History, and Consequences, B.C. Douglas, M.S. Kearney, and S.P. Leatherman, eds. Academic Press. P. 97-119. [Vivien Gornitz]	Accepted; text modified
5-973	A	33:10		…storing more water. However, losses may also occur through seepage and evaporation. Infiltration would raise the water table if porous and permeable rocks surrounding the reservoir were underlain by impermeable rocks. [Vivien Gornitz]	Accepted; text modified
5-974	A	33:15	33:15	"1990's" should read "1993-2003" (and certainly shouldn't have an apostrophe!). [John Hunter]	Accepted; text modified
5-975	A	33:19	33:40	In Fig 5.5.10 the error bars are 95% but not in Table 5.5.2 or text: please use the 95% values. In Table 5.5.2 where is GIA? [Kevin Trenberth]	Accepted; text modified
5-976	A	33:21	33:21	1990's should read "1993-2003". [John Hunter]	OK
5-977	A	33:21	33:21	I am not sure where the ">= 75%" comes from. The ratio of the central values is 2.6/3.1 = 0.84, and taking the "minus one standard deviation" divided by the "plus one standard deviation" values gives 2.4/3.5 = 0.69. [John Hunter]	Accepted; text modified
5-978	A	33:22	33:22	It would be helpful if "3.1 +/- 0.4 mm/year" was referenced to section (5.5.2.1.1). [John Hunter]	Accepted; text modified
5-979	A	33:22		Double the error figures to 95% confidence 3.1±0.8, 0.5±0.8 [Vincent Gray]	OK
5-1235	B	33:22		Presumably the T/P rate quoted as 3.1+- 0.4 mm per yr includes the GIA correction of peltier (2001) of -0.3 mm per yr? If so this should be explicitly stated. As noted in Section 6.3 of the FOD the correction for the ICE-5G(VM2) model is slightly larger, i.e. -0.36	Accepted; text modified

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				mm per yr. [William Richard PELTIER]	
5-980	A	33:26	33:26	It would be helpful if "1.8 +/- 0.3 mm/year" was referenced to section (5.5.2.1.2) . [John Hunter]	Accepted; text modified
5-981	A	33:26		Double the error figures to 95% confidence 0.8±0.6 [Vincent Gray]	OK
5-982	A	33:27	33:30	Here it talks about earlier values being smaller than more recnet values. This is unusual and confusing. Best to talk of more recent values as beeing larger than the earlier values. [Trevor McDougall]	Accepted; text modified
5-983	A	33:30	33:30	0.3 to 1.4 rather than 0.4 to 1.5 in Figure 5.5.10. [David Parker]	Accepted; text modified
5-984	A	33:30		With around half of the sea level rise still unexplained, this still leaves us with a sea level "Enigma" (Munk, 2002). [Vivien Gornitz]	Accepted; text modified
5-985	A	33:30		Double the error figures to 95% confidence; 0.9±0.8 [Vincent Gray]	OK
5-986	A	33:32	33:35	If terrestrial water storage is invoked as an explanation for the discrepancy, it would have to be adding ~0.9 mm/yr to SLR--which is not very likely. [Vivien Gornitz]	Noted
5-987	A	33:33	33:34	I do not agree that "the discrepancy is similar for the two periods" -- the ratio of the central values is 0.9/0.5 ~ 2. [John Hunter]	Noted, text changed
5-988	A	33:37		Table 5.5.2 I think there should be 2 tables side-by-side showing the rates for 2 periods, rather than have one under the other for each term as now. It is hard to read now. Also I don't like Church et al being shown as the only, or main, source of the 1.8 mm/year. You can at least refer back to the previous text in this chapter. [Philip Woodworth]	Noted, table changed accordingly
5-989	A	33:39	33:40	Table 5.5.2: lines 3 Thermosteric: it has a large rms error ?; same for 9,10 Anctartic ice sheet ? It is not clear the sources for the table values. [Renzo MOSETTI]	Accepted; text modified
5-990	A	33:39		Table 5.5.2, line 2: Sea level for this period can also be estimated from the tide gauge data. Suggest this number should be added with both estimates corrected for GIA. [John Church]	Accepted; text modified
5-991	A	33:39		Double all the error figures in Table 5.5.2 to 95% confidence [Vincent Gray]	OK

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-992	A	34:1	34:46	This section (5.5.7) makes me feel uncomfortable. The purpose of the section is apparently to assess the models' usefulness for projecting future sea level rise. Four of the models (or at least their 20th Century runs) are considered 'wrong' and excluded because they do not reproduce the global mean surface temperature change over the 20th Century. OK. But some of the remaining model runs produce a higher than observed thermosteric sea level rise over 1961-98, and a lower than observed trend since 1993. Both these errors are explained away by the fact that many of the model runs did not include volcanic forcing. The question I am left with is "What do we conclude from all this reagrding the reliability of model sea level projections?" Are the 4 excluded models to be excluded from the sea level projections in chapter 10, with the remaining models considered suitable? (I don't think this was actually done in Chapter 10). If so why is correct simulation of surface temperature trend considered more important than correct simulation of thermosteric sea level, in choosing which models to believe for sea level? Did the 4 models that were excluded on the basis of their 20th Century surface air temperature simulations include volcanic forcing? If volcanic forcing were added I would expect this to reduce the surface temperature rise, possibly making some of the excluded models consistent with the surface temperature observations and some of the included ones inconsistent. Overall it was very unclear to me what the significance of the results was. The results are worth discussing, but I feel they need some careful interpretation here. Actually, I think the whole section would fit more logically in Chapter 9 as that chapter is about using models to understand observed climate changes. (I will also flag this in my comments to Chapter 9, which only has a very brief paragraph on this topic at present). [Richard Wood]	Section has been dropped, may appear in chapter 9.
5-993	A	34:15	34:16	Again, there is a hotpotch in the way that uncertainties are given -- why am I expected to compare "0.32-0.99" with "0.35 +/- 0.22"? [John Hunter]	accept
5-994	A	34:16	:17	I think volcanic forcing can only be part of the answer. I think the volcanic contribution over this period is about -0.2 mm/yr. IE the upper end of the model range becomes 0.79 mm/yr still larger than 0.57 mm/yr (0.35+0.22). [John Church]	subsection moved to chapter 9
5-995	A	34:16		Replace "range" with "95% accuracy " [Vincent Gray]	see 5-994
5-996	A	34:16		Double the error figures to 95% confidence; +0.35±0.44 [Vincent Gray]	see 5-994
5-997	A	34:17		This is a linear value: the real value is about 0.7 K per century or 0.75 K through 2004. [Kevin Trenberth]	see 5-994

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-998	A	34:20	34:21	I do not understand what is meant by a variability of "0.1 - 0.6" -- does this mean that the smallest range of variability for any model was +/- 0.1 mm/year and the largest variability for any model was +/- 0.6 mm/year? [John Hunter]	see 5-994
5-999	A	34:20	34:20	range of 1.2 to 1.8 mm/year (Table 5.5.1) [David Parker]	see 5-994
5-1000	A	34:20		Insert at beginning "95% accuiracy" [Vincent Gray]	see 5-994
5-1001	A	34:20		Double the error figures to 95% confidence 1.0-1.8 mm per yr [Vincent Gray]	see 5-994
5-1002	A	34:21	34:22	Why the reference to Section 5.3 regarding models? [David Parker]	see 5-994
5-1003	A	34:24	:27	Note Church and White (2005, submitted) estimate that the recent volcanic eruptions offset about 0.005 mm yr-2 of the acceleration that would otherwise be present. [John Church]	see 5-994
5-1004	A	34:25		PCM3 [John Church]	see 5-994
5-1005	A	34:29	34:29	I don't think "G&IC" has been defined -- presumably it is "glaciers and ice caps". [John Hunter]	see 5-994
5-1006	A	34:29	34:29	There is an extra "mm yr^-1" in the middle of this sentence (after "1961-1975") [Neil White]	see 5-994
5-1007	A	34:29	:34	I think this section should be rephrased using the numbers elsewhere in this chapter and chapter 4. Also, what are the implications of the discrepancies. Given these discrepancies, what should we make of the smaller predictions of future glacial contributions in Chapter 10 compared with the TAR? The growing discrepancy of the AOGCM results (0.3 to 0.7 mm/yr) would suggest some deficiency in the modelling of glacier melting and raises concern about discrepancies for the 21st century. [John Church]	see 5-994
5-1008	A	34:36	34:39	The reason for the discrepancy should be explicitly repeated. [Chris Folland]	Ok
5-1009	A	34:36		What are the implications of the growing discrepancy between the AOGCM estimates and the observations. [John Church]	Model estimates will no longer be discussed in chapter 5.
5-1010	A	34:37	34:38	Here I says "This is not surprising because the observational and model-based estimates of the terms are roughly similar." I thought you had just finnished telling us that there is an unexplained amount of sea level rise in the historical record. What am I missing?	See 5-1009

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Trevor McDougall]	
5-1011	A	34:37		Insert "95% accuracy" before "rates" [Vincent Gray]	See 5-1009
5-1012	A	34:41		It must be noticed that the time series of yearly mean sea level presents wide oscillations and a cospicous increase during the last decade,. These oscillations can mask the trend which is necessary to be long enough for being statiscally significant. To consider only 50 years is not enough, and satellite data are available from a too little time for being significant for understanding the trend. From a tide-gauge analysis over a century (1896-2002), the rate of the North Adriatic eustacy is 1.19 mm/yr (Carbognin et al, 2004). [Pierpaolo Campostrini]	noted
5-1013	A	34:42		0.013mm/yr^2 [John Church]	ok
5-1014	A	34:43		Double the error figures to 95% confidence ; 0.012±0.012 [Vincent Gray]	ok
5-1236	B	34:43		Another reference to the dubious Church and White (2005) paper. I will be very surprised if tis paper were accepted for publication. [William Richard PELTIER]	Paper has been accepted for publication
5-1015	A	34:44	34:45	It is true that a acceleration in sea-level rise is easier to detect in models. But given the larger range in model derived sea level rise, both for the 1961-1998 and for thre 1993-2003 period, is is questionable whether one should rely on models. In the beginning of cgapyter 5.5.7 it is stated that the reliability of models has to be demonstated. Keep it to that, and either remove the sentence of lines 44-45, or add that an improvement of the model perfomanceis required in order to use models for that purpose. [Hendrik M. van Aken]	See 5-1009
5-1016	A	34:50	34:50	I think this QACC is a nice summary for the non-specialist, but have a few detailed comments. I noticed that the figure is not referenced in the text. [Richard Wood]	noted
5-1017	A	34:52	34:53	Add some references here. A ~3 thousand year sea level trend was not previously discussed. [Vivien Gornitz]	some references have been added
5-1018	A	34:52		What error bar would you assign to this? We have already said that there is significant spatially variability. How does this couple with the comparison with geological evidence? [Michael Tsimplis]	Error bar will be discussed
5-1019	A	34:53		Insert "about" after "than" [Vincent Gray]	Ok
5-1020	A	34:53		Question 5.1 (Comment from only David Fahey only, since David Wratt is a Review	text changed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				Editor for this chapter): Suggest replacing the term 'onset of acceleration' to something more simple in the opening paragraph. [David & David Wratt & Fahey]	
5-1021	A	35:0		Section 5.6: I like this section - it is a real attempt at synthesis/assessment. Suggest some of the rest of the chapter should aim for a similar style. [John Church]	Ok
5-1022	A	35:3	:5	I suggest that this is not the appropriate way to discuss the acceleration. The detection of the volcanic signal and the detection of a long term acceleration suggests different ways of discussing the differences between the post 1993 period and the longer period - see for example the end of Church et al. (2005) [John Church]	Noted, text changed
5-1023	A	35:3		Double the error figures to 95% confidence 3.1±0.8 [Vincent Gray]	ok
5-1024	A	35:10	35:11	The simple statement that differential sea level rise around the world is mostly due to non-uniform thermal expansion needs to be expanded. At least two additional questions should be addressed. (1) What causes non-uniform thermal expansion? and (2) Are the areas of the world that are currently experiencing faster than average sea level rise, likely to experience faster than average sea level rise in the future? A policymaker might have a very different response to an 0.5 meter sea level rise to 2100 than to a 2 meter sea level rise over the same timeframe. [Lenny Bernstein]	Noted, these issues are discussed in the chapter but due to length restrictions not in the FAQ.
5-1025	A	35:10	35:11	The statement that differential sea level rise around the world is mostly due to non-uniform thermal expansion rasies at least two additional questions that should be disucssed this section. (1) What causes non-uniform thermal expansion? and (2) Are the areas of the world that are currently experiencing faster than average sea level rise, likely to experience faster than average sea level rise in the future? Policymakers might have a different response to an 0.5 meter sea level rise to 2100 than to a 2 meter sea level rise over the same timeframe. [Jeffrey Kueter]	Noted, see 5-1024
5-1026	A	35:10	35:11	The concept of a non-uniform rise in sea level is somewhat counter-intuitive and the intended audience for the Questions might well ask - "what is holding up the higher sea levels in some places? Why doesn't the water just flatten out?" Can the authors provide a simple answer to this? [Martin Manning]	Non-uniform sea level is related to ocean circulation, non-uniform change in sea level are related to ocean circulation changes
5-1027	A	35:10	35:10	The word 'hydrographic' may not be understood by the target audience. [Richard Wood]	noted
5-1028	A	35:10	35:11	'Spatial variability of sea level rates is mostly due to non-uniform thermal expansion'. At	Noted, of course thermal expansion can

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				the very least I think this sentence is misleading. Given that we see large areas where the sea level has fallen, I think there must be a substantial contribution from redistribution of heat by circulation chages (e.g. more El Ninos, as discussed elsewhere in the chapter). [Richard Wood]	have two signs (redistribution of heat is correct but only another word for the same).
5-1029	A	35:13	35:20	I think this discussion of the discrepancy between the budget calculations and the observed sea level rise needs careful phrasing for the non-specialist. How about (to replace the last sentence): "There is a discrepancy of about 0.9 mm/yr between calculations of sea level rise based on these individual contributing processes, and estimates based on direct sea level observation. While this discrepancy has been reduced since the TAR [true?] it shows that some limitations remain in our current understanding of sea level change." [Richard Wood]	noted
5-1030	A	35:13		Suggest "improved' rather than "reasonable" - I still do not think it is acceptable! [John Church]	Noted, text changed
5-1031	A	35:19	35:20	Isn't the 0.9 mm/yr is the residual between the observed sea level rise and the sum of all the potential sources of that rise, not just thermal expansion? [R Allyn Clarke]	Noted, text changed
5-1032	A	35:19	35:19	Again, for consistency, "0.2 - 0.9" should be expressed something like "0.6 +/- 0.3". [John Hunter]	ok
5-1033	A	35:19		add: "for the recent 50 years" at the end of the line [Hartmut Grassl]	accept
5-1034	A	35:20	35:20	..thermal expansion plus land-ice contribution… [David Parker]	Noted
5-1035	A	35:21		I didn't see a reference to the figure for Question 5.1? [Peter Gent]	Will be corrected
5-1036	A	35:26	35:28	I think this statement should be qualified: "Most patterns of observed changes…" There are plenty of changes shown in this chapter for which no explanation has been offered. For those offered, some need more evidence before they can be treated as consistent with ocean circulation. [Melissa Bowen]	Noted
5-1037	A	35:28		In figure 5.6.1 there is an indication that equatorial upwelling has increased. This is not documented in the body of the text, nor am I aware of any studies that do so. [Frank Bryan]	Noted
5-1038	A	35:30		Figure 5.6.1 This figure is almost unreadable. Increasing the size of the font would help. [Melissa Bowen]	Noted, figure has been changed
5-1039	A	35:32	35:43	Although it is true that heat content has increased since 1955, some discussion is needed	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				on the dependency of this increase on the start year of the trend. Going back in time only some 10 to 15 years, based on SST, suggests that trends would be significantly different. [Robert Molinari]	
5-1040	A	35:32	:43	Sugggest add some comments about property changes [John Church]	Noted, CFC changes included
5-1041	A	35:33		delete: "the" in (south of the 45) [Hartmut Grassl]	ok
5-1042	A	35:42	35:43	Where is this sentence specifically supported earlier in the Chapter? [David Parker]	Noted, text modified
5-1043	A	35:42	:43	I recall little discussion of this issue in the text. [John Church]	Noted, see above
5-1044	A	35:42		to be precise, there is little evidence of changes in the transport of the MOC in either hemisphere; there is evidence of changes in water properties in the North Atlantic [Stephen R Rintoul]	disagree, there is evidence for changes though not necessarily for trends
5-1045	A	35:45	35:45	It is the subduction of carbon dioxide not carbon that has lead to the acidification. [R Allyn Clarke]	Noted
5-1046	A	35:45		Need to include a statement on the impacts of the change in calcium carbonate and aragonite saturation horizons. [Stephen R Rintoul]	Reject, impacts are not task of this chapter
5-1047	A	35:51	35:51	5.2.6d [David Parker]	ok
5-1048	A	35:55	35:55	"we are confident" needs to be reexepressed in the agreed uncertainty language. Thus "it is very likely"…. depending on assessed confidence. [Chris Folland]	ok
5-1049	A	35:55		On page 5-35, line 55, the "we are confident" statement makes me ask what we know about changes in the atmospheric hydrological cycle over the ocean. A link to Chapter 3 may be in order. But my impression from Chapter 3 is that there has been little work done on changes in E-P over the ocean. It is a rather large gap in this IPCC report in my opinion and a "we are confident" statement is asking for a rebuttal. [Harry Bryden]	Noted, indeed the E-P over the ocean is poorly known.Will be reworded
5-1050	A	35:55		change: "such as 24" to "such at 24" [Hartmut Grassl]	ok
5-1051	A	35:56		Northern Hemisphere [Peter Gent]	ok
5-1052	A	36:1		the phrase "it would seem likely" does not sound very scientific. What is the evidence? [Stephen R Rintoul]	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-1053	A	36:2	36:2	"precipitation minus evaporation" is better. [Chris Folland]	ok
5-1054	A	36:6	36:6	Suggest "The transfer of heat anomalies into the ocean…" (since many heat anomalies are caused by reduced heat loss). [Richard Wood]	Noted
5-1055	A	36:7		Delete "mainly" [John Church]	Noted
5-1056	A	36:9		that showed the strongest [Peter Gent]	Ok
5-1057	A	36:12	36:12	The river run-off contribution is not clear in previous sections and it is not reported also in Table 5.5.2. [Renzo MOSETTI]	Noted
5-1058	A	36:14	36:15	Tis the cooling in the western equatorial Pacif and the warming in the eastern equatorial Pacific consistent with the very nice Figure 5.5.8. It may be, but it would be good to guide through this seeming contradiction. [Trevor McDougall]	Yes, is consistent (decadal changes have different pattern as multidecal changes)
5-1059	A	36:14		east to west gradient in the [Peter Gent]	ok
5-1060	A	36:14		shows that strong" add. "shows that the strong"; correct: "gradients" to "gradient [Hartmut Grassl]	ok
5-1061	A	36:15	33:17	The statement "This decrease in the equatorial temperature gradient is consistent with the increased frequency and duration of El Nino over this same period" does not belong in this Summary section 5.6 and should be omitted, because it does not correspond to a part of the main text. Of course, the authors of Chapter 5 may decide to discuss changes in ENSO variability in section 5.3.3 or in a special subsection with appropriate references, and to summarize the main findings in the Summary section 5.6 in a paragraph that discusses changes in ocean circulation. [Gerrit Burgers]	Noted, text modified
5-1062	A	36:17	36:20	This change in surface currents is not an observation, but is a deduction using the simple geostrophic balance, I think. The reader should be told it is a deduction, not an observation. [Trevor McDougall]	Noted
5-1063	A	36:17	36:17	"this same period". The period referred to should be specified. I suppose it is 1950-2000 [Philippe Tulkens]	Noted
5-1064	A	36:18	36:43	On page 5-36, lines 18-19, the statement that "the Antarctic Circumpolar Current is slightly stronger" conflicts with page 5-18, lines 42-43. And I cannot remember or find	Noted, will be re-evaluated

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				any text in the Chapter that backs up the accompanying statement that "the North Atlantic subtropical gyre has strengthened". [Harry Bryden]	
5-1065	A	36:18		I don't recall seeing the changes in ACC strength or the North Atlantic subtropical gyre discussed earlier. It should be introduced somewhere before the summary and references given to support the statement. [Stephen R Rintoul]	accept
5-1066	A	36:22	36:22	Replace "The increase" with "The rate of increase". [Trevor McDougall]	ok
5-1067	A	36:24		Although this claim that CO2 uptake is slowing makes physical sense, as explained above, we don't actually have evidence that its true- it's claims like this that should be taken out- I think Chris Sabine will now agree with this. [Anand Gnanadesikan]	noted
5-1068	A	36:26	36:28	decadal variations due to climate modes such as PDO and NAO" instead of "decadal variations such as "PDO and NAO [Roxana Bojariu]	ok
5-1069	A	36:30	36:30	Delete "the" from "All of the these observations…" [Toshio Suga]	ok
5-1070	A	36:32		the statement that the changes in the ocean are "in response to changed ocean surface conditions" seems hard to argue with, but not very useful. What caused the changed ocean surface conditions? The chapter needs to conclude with a clear statement of how these changes should be interpreted: is it climate change? natural variability? or maybe we cannot tell (and so should say so). [Stephen R Rintoul]	Attribution of changes is not task of chapter 5
5-1071	A	37:0		References: references of "submitted" and "to be submitted papers" should be avoided. [Renzo MOSETTI]	ok
5-1072	A	37:50	37:53	Reference is repeated - the first one has the wrong year. [Peter Gent]	ok
5-1073	A	37:50	37:53	Duplicate and/oir incomplete reference [Michel Rixen]	ok
5-1074	A	37:50	37:51	This literature does not exist and should be deleted. [Toshio Suga]	ok
5-1075	A	37:52	37:52	climate-change" should be "climate change [Toshio Suga]	Ok
5-1076	A	38:30	38:32	The final statement on lines 30-32 on page 5-38 that "spatial distribution of the changes is broadly consistent with the large scale ocean circulation and that these changes are in	Noted, text throughout the chapter has been changed to state the connection

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				response to changed ocean surface conditions" is an appealing way to end the Chapter but it is vague and unsupported by the text of the Chapter (see comments above about Section 5.5.4.1). Perhaps more needs to be added to the text to back up the sweeping statements at the end of the Synthesis section. [Harry Bryden]	more clearly.
5-1077	A	40:27		Schröder" with umlaut or "oe [Walter Zenk]	ok
5-1078	A	41:22	41:24	Gonzalez-Pola, C., A. Lavin, and M. Vargas-Yanez, Intense warming and salinity modification of intermediate water masses in the southeastern corner of the Bay of Biscay for the period 1992-2003, J. Geophys. Res., 110, C05020, doi:10.1029/2004JC002367, 2005. [ALICIA LAVIN]	Noted
5-1079	A	42:41	42:42	Josey & Marsh now published - JGR vol. 110 C05008 doi:10.1029/2004JC002521 [Meric Srokosz]	ok
5-1080	A	43:0		Atlantic Climate Pacemaker for Millennia Past, Decades Hence? [Peili Wu]	Noted
5-1081	A	43:0		Richard A. Kerr, Science 1 July 2005; 309: 41-43 [DOI: 10.1126/science.309.5731.41] (in News Focus) [Peili Wu]	Noted, not original publication
5-1082	A	43:3		also check the reference for Kerr (2000). is it not this noe? [Peili Wu]	Noted, reference removed
5-1083	A	46:20	46:21	Parrilla, G., A. Lavin, H. Bryden, M. Garcia, and R. Millard, Rising temperatures in the Subtropical North Atlantic Ocean over the past 35 years, Nature, 369(6475), 48-51, 1994. [ALICIA LAVIN]	noted
5-1084	A	49:31	49:33	Vargas-Yanez, M., G. Parrilla, A. Lavin, P. Velez-Belchi, and C. Gonzalez-Pola, Temperature and salinity increase in the eastern North Atlantic along the 24.5 degrees N in the last ten years - art. no. L06210, Geophys. Res. Lett., 31, L6210, doi:10.1029/2003GL019308, 2004. [ALICIA LAVIN]	Noted, paper referenced
5-1085	A	49:40	49:41	Visbeck citation incomplete [Michel Rixen]	ok
5-1086	A	50:11	50:11	In association with comment # 5, insert "Wong, W.T., K.W. Li, and K.H. Yeung, 2003: Long term sea level change in Hong Kong. HKMetS Bulletin, 13(1/2), 24-40." [Chiu-Ying LAM]	Noted
5-1087	A	51:3		This is very useful background information which removes earlier doubts about the techniques used in analysing ocean heat content.	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[David Parker]	
5-1088	A	51:5	:42	Suggest most of this is unnecessary [John Church]	noted , but comment disagrees with preceeding comment
5-1089	A	51:13		Infilling can reduce amplitude of anomalies depending on how it is done. L 5-29: what about missing data? [Kevin Trenberth]	Will be addressed
5-1090	A	51:17	53:47	Suggest most of this is unnecessary [John Church]	Noted, text has been shortened
5-1091	A	51:24	:27	The important difference with the Willis et al. estimate is the inclusion of altimeter data. [John Church]	Noted
5-1092	A	51:25	51:25	"two-scale" - does this mean anisotropic? [Chris Folland]	Noted, text modified
5-1093	A	51:37	51:39	Something wrong with the sentence? [Michel Rixen]	Noted
5-1094	A	51:38		available as a function … [Peter Gent]	ok
5-1095	A	51:45	52:15	Suggest this is an important issue and should be discussed in the text. However, I remain unconvinced by the last sentence. [John Church]	Noted
5-1096	A	51:47	51:47	"several AOGM simulations forced" : AOGM or AOGCM? [Michel Rixen]	AOGCM
5-1097	A	52:14	52:15	But what about discrepancies on decadal variability in Church et al 2004 and the direct thermal measurements? Doesn't this suggest that some decadal variability may be spurious? [Kevin Trenberth]	Will be addressed
5-1098	A	52:15	52:15	is indeed real" : We can add here a sentence saying that "Moreover, a very similar change occured in the early 1980s in the Mediterranean (Rixen et al 2005). [Michel Rixen]	Noted
5-1099	A	52:45		Most potential biases … [Peter Gent]	Don't understand comment
5-1100	A	52:52	52:52	Here and elsewhere, maybe in a number of Chapters. I am not sure "pentad" is the right word for a five year period. It is often used in meteorology for a 5 day period. "Pentade" I believe has been used in the past for a 5 year period. The correct usage should be clarified with dictionary experts. [Chris Folland]	Noted, my dictionary said "group of five".
5-1101	A	53:9	54:12	Altimeters are complex instruments which must be calibrated so as to provide accurate	Noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				measurements. The rise signal represents only a few mm/yr and the data record is short enough fro which its interpretation requires extreme caution. Any attempt at estimating sea level trends is subject to the issue of the record length as well as the time interval chosen, with the result to produce different signs and values at a same site. We also need to know more about the influence of potential drifts of the altimetric system components (i.e., altimeter, orbit, geophysical corrections, system reference, etc.) and it is evident that the order of magnitude of the sea level rate requires millimeter-control on these parameters. The satellite observations are validated not only by the rigorous screening they successfully passed, but also by the network of corroboratory in situ instruments. This network represents an independent system to evalute quality of satellite-derived sea levels as well as to monitor drifts and bias of the altimeter system over time. [STEFANO VIGNUDELLI]	
5-1102	A	53:9	54:12	In my opinion, a bit more evidence of the results of these comparisons is needed for better gaining confidence with the altimeter-derived sea level change estimates. It might be provided in a form of a table or alternatively expanding the final part of the subsection. [STEFANO VIGNUDELLI]	Noted, however space limitations prevent a more detailed discussion
5-1103	A	53:26	53:27	I don't understand this comment on the inverted barometer correction. Surely you would still apply this correction even if the altimeter coverage was global because the altimeter global sampling is slow relative to the development and movement of meteorological systems. [R Allyn Clarke]	Will be clarified
5-1104	A	53:38		What is the basis for the claimed "measurement accuracy"? [Vincent Gray]	Will be clarified
5-1105	A	53:46		What is the probability of this error? [Vincent Gray]	Will be clarified
5-1106	A	54:1	:9	Suggest this is important and should be included in the text. [John Church]	accept
5-1107	A	54:14	:33	Suggest most of this is unnecessary [John Church]	Noted, has been shortened
5-1108	A	55:0		The caption should indicate that the 3 curves are arbitrarily shifted by 150 m for display purposes. [Michael Alexander Alexander]	ok
5-1109	A	55:8	55:8	(Pahlow and Riebesell (2000) => Pahlow and Riebesell (2000) [Tsuneo Ono]	ok
5-1110	A	55:14	55:17	This sentence implies that Freeland et al. (1997) concludes the cause of nutrient change as	Accepted, text clarified.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				the change of plankton composition, but in the same page line 50-51, we recognize that Freeland et al. (1997) show the decrease of surface mixing. These two statement might conflict each other. In my impression, Freeland et al. (1997)'s suggestion is that the decline of surface mixing (or decline of MLD) is the primary cause of nutrient change, and change in biology is the consequence of nutrient changes rather than a cause of it. [Tsuneo Ono]	
5-1111	A	55:16	55:17	Watanabe et al. (2003) =>Watanabe et al. (2005)? [Tsuneo Ono]	Noted
5-1112	A	55:24	55:35	Many other studies come to mind: McGowan et al., 1998, Roemmich and McGowan, 1995, Falkowski and Wilson, 1992, just to mention a few. [Nicolas Gruber]	Rejected. This belongs to WGII. Reference to WGII is added.
5-1113	A	55:24		Section 5.4.5. We should extend this section more: If the signal is global it is important because it affects carbon cycle directly, but change in biology is very important problem by itself, even that the signal is limited to basin-scale or regional scale. I, therefore, think that we should make short description here for each reported biological change in each region, such as those reported by Beaugrand and Reid, 2003, Chavez et al.,2003, Karl, 1999, and Hirawake et al., 2005. [Tsuneo Ono]	Rejected. This belongs to WGII. Reference to WGII is added.
5-1114	A	55:30	55:30	"associated with warming and decrease iron deposition" Particularly the latter argument is very tentative. I don't think we have good evidence that the latter occurred. I therefore suggest to use words like "suggested to be associated". [Nicolas Gruber]	Accepted.
5-1115	A	55:31	55:34	Chavez et al., 2003 and Karl, 1999 is nominated as only references of the North. Pac. biology change, but both of these two address changes in subtropical North Pacific. I recommend to add one more reference, Chiba et al. (2004), which reports long-term biological change in subarctic North Pacific region....as this sentence says that our spatiotemporal coverage is limited, we should refer so as to we can get as large spatio-temporal coverage as possible. [Chiba, S., T. Ono, K. Tadokoro, T. Midorikawa and T. Saino, 2004: Increased stratification and decreased lower trophic productivity in the Oyashio region of the North Pacific - a 30-year retrospective study -. Journal of Oceanography, 60(1), 149-162.] [Tsuneo Ono]	Noted.
5-1116	A	55:41	55:43	This reference changed to : Watanabe, Y. W., H. Ishida, T. Nakano, and N. Nagai, 2005: Spaciotemporal decreases od nutrients and chlorophill-a in the surface mixed layer of the western North Pacific from 1971 to 2000. Journal of Oceanography, 61(5), 1011-1016. [Tsuneo Ono]	Accepted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-1117	A	55:42	55:42	one could add Gruber et al. (2002) for NAO [Nicolas Gruber]	Accepted.
5-1118	A	56:0	61:	Figures 5.2.1, 5.2.2, 5.2.3 and 5.2.6 need their sources referenced within the captions. [Harry Bryden]	accepted
5-1119	A	56:0		This is an important figure and should be defined. For example these must be interannual anomalies of heat content since negative heat is not possible (in reality). What period was the base for the reference used in the calculation? Explanatory text is needed in the figure caption and in the body of the document. [Joyce Terry]	Heat content is by definition an anomaly. We can add more explanation in the figure captions as suggested by the reviewer.
5-1120	A	56:0		Figs. 5.2.1 and 5.2.2: At first glance, Fig. 5.2.1 does not seem to be consistent with Fig. 5.2.2, as Fig. 5.2.1 shows the deeper ocean warming faster than the 0-700 m layer, which is certainly not the case as seen in Fig. 5.2.2. [Kevin Walsh]	Fig 5.2.2 represents the zonal average of liner trends of heat content. Fig. 5.2.1 shows the global integral. Also convection can bring relatively warm, salty water to depth relatively quickly from relatively small surface regions and when this water advects horizontally the deeper layers can warm while upper layers cool.
5-1121	A	56:6		Redraw Fig 5.2.1 with TWO standard error error bars, giving 95% confidence [Vincent Gray]	Accepted, has been done
5-1122	A	57:0	57:0	I do not understand this figure. These are sections…where are the linear trends? [Christopher Sabine]	These are linear trends of the zonally integrated heat content by 1-degree latitude belts as a function of depth.
5-1123	A	58:0		It is not made clear what the timeserie denote; description and vertical axis [Douwe Dillingh]	Vertical axis is given in then figure caption. Can be added to figures themselves.
5-1124	A	58:0		Fig. 5.2.3 Define EOF. Empirical orthogonal functions. Label X and Y axes. Label units of X and Y axes. What is the difference (e.g., input?) in the four graphs? Does chapter text define 1 through (e.g.,versus) 4 EOFs? [Melinda Marquis]	See responses to Comment 5-1123. Also, these are standard presentation formats.
5-1125	A	58:0		Figure 5.2.3. This is a good Figure but the version in Levitus et al 2005c, which is now fully published, is even better as it is in colour. The polarity is reversed in 3 of the 4 panels in Levitus et al. 2005c. I know that this is arbitrary, but it will be less confusing to readers if Figure 5.2.3 matches Levitus et al 2005c exactly.	This figure has bee removed.

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[David Parker]	
5-1126	A	59:0		It is hardly to believe that only 29% is explained by EOFs 1-4; it seems much more in this figure. [Douwe Dillingh]	This figure has been removed.
5-1127	A	60:0		Fig. 5.2.5 Add ", respectively" to end of second sentence of caption. [Melinda Marquis]	O.K.
5-1128	A	60:5		Redraw Fig 5.2.5 with error bars twice the value of the standard error, for 95% confidence [Vincent Gray]	We will redo the error estimates.
5-1129	A	61:0	61:0	I do not understand this figure. These are sections…where are the linear trends? [Christopher Sabine]	These are linear trends of the zonally averaged salinity by 1-degree latitude belts as a function of depth.
5-1130	A	61:5	61:7	Legend of Figure 5.2.6. The average salinity of the oceans should be given in order to give some indications on the magnitude of the observed salinity changes. [Philippe Tulkens]	We have to assume that readers have some familiarity with then ocean or we are at risk of producing a "textbook" as some reviewers have suggested we are doing.
5-1131	A	62:0		Figure 5.2.7 [cited in section 5.2.4 on p8]. Please repeat in the caption of this figure (or of Figure 5.2.1) that 14.5 10^22J corresponds to 0.2 W/m^2 over this period. [Gerrit Burgers]	We can add this information.
5-1132	A	62:0		Fig. 5.2.7 Add units to X axis. What does the term "reduce" in "heat required to reduce Antarctic" mean, e.g., compared to the term "melt" in heat required to melt" mountain glaciers, NH sea-ice, and Arctic perennial sea-ice volume? If "reduce" means "melt," use the term "melt" consistently throughout graphic. [Melinda Marquis]	Units are already in the figure at the top of the figure. We should change "reduce" to "melt".
5-1133	A	62:0		Fig. 5.2.7. Should be "Heat required to melt Arctic perennial sea-ice volume from 1955-1998", since the authors do not mean the entire volume of Arctic perennial sea ice. [Kevin Walsh]	We agree.
5-1134	A	62:1		Figure 5.2.7. The figures given should be updated for the next draft and include the latests estimates given in the AR4-FOD 2, 3 and 4. [Philippe Tulkens]	We agree
5-1135	A	63:0		Fig. 5.3.1 Improve quality of lettering on graphics. In caption, explain better what graphic (c) is showing, e.g, Y axis is height of what. Define E-P. [Melinda Marquis]	Will be removed
5-1136	A	63:3	63:8	take small "a b c " instead of cap letters	noted

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				[Walter Zenk]	
5-1137	A	63:8	63:8	Figure 5.3.1: winter outcrop is only shown in panel A [Wilco Hazeleger]	noted
5-1138	A	63:8	63:8	in A and C." should be "in A. C, [Toshio Suga]	Noted
5-1139	A	64:0		5.3.2 Graphics are fuzzy. Add label (a) to top graphic. Please improve quality of graphics, including lettering on graphics. Add Y axis label and units to top graphic. [Melinda Marquis]	Quality will be improved
5-1140	A	64:6	64:6	change "clockwise areound the gyre" to "westward" [David Parker]	noted
5-1141	A	64:6	64:6	shown in a)" should be "shown in (a) [Toshio Suga]	ok
5-1142	A	65:5		Replace "Times series" with "Time series" [Vincent Gray]	ok
5-1143	A	65:7	65:7	Lyons [David Parker]	ok
5-1144	A	66:0	66:0	The figure caption says this is a plot of temperature changes below 4000 m, but it looks like a plot of the full water column and the pattern of the changes look very curious. Some additional explination of the overall patterns of change would be helpful. [Christopher Sabine]	Noted, figure will be removed
5-1145	A	66:0		Caption error? The figure shows the temperature change from the surface to the bottom of the ocean not just below 4000 m. Perhaps the authors wished to highlight the warming below 4000 m? [Michael Alexander Alexander]	See 5-1144
5-1146	A	66:2	66:3	Change text in Figure 5.3.4 from "Potential temperature (IPTS-68) (1999-1985)" to "Potential temperature (IPTS-68) (1985-1999)". [Chiu-Ying LAM]	See 5-1144
5-1147	A	66:5	66:7	This caption to Figure 5.3.4 seems weird. The figure shows a whole crossection (ie all depths) of changes but the caption talks about "temperature below 4000 metres". [Trevor McDougall]	See 5-1144
5-1148	A	66:6		If the "measurement accuracy is only one standard error double it to -.002°C to give 95% confidence [Vincent Gray]	ok
5-1149	A	67:0	67:0	Please explain this linear trend…it is not clear from the figures. [Christopher Sabine]	noted
5-1150	A	68:0		Figure 5.3.6. Middle and bottom panels are incorrectly labeled "(a)" and "(b)",	figure will be removed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				respectively and should be labeled "(b)" and "(c)", respectively. [Toshio Suga]	
5-1151	A	69:0	69:	Figure 5.3.7: I find this figure not very helpful [Wilco Hazeleger]	figure will be removed
5-1152	A	69:0		Fig. 5.3.7 Add labels and units to X and Y axes to top and bottom graphic. Add reference in caption to bottom graphic. [Melinda Marquis]	figure will be removed
5-1153	A	70:0	70:	Figure 5.3.8: what do the arrows and the circles mean in the figure? [Wilco Hazeleger]	Will be clarified
5-1154	A	70:0		Fig. 5.3.8 Add labels and units to Y axis and units to X axis.. [Melinda Marquis]	figure will be removed
5-1155	A	71:0	71:0	The quality of this figure is very poor. Also, the units should be mol/m^2 not umol/kg [Christopher Sabine]	figure improved
5-1156	A	71:0		Fig. 5.4.1 Seems that this graphic would be more helpful if it were in color, rather than gray scale. Add labels and units to X and Y axes. Add label and units to color bar. [Melinda Marquis]	ok
5-1157	A	71:4	71:4	Legend of Figure 5.4.1 : "umol/kg" : the "u" should be replaced by the greek symbol "miu" [Philippe Tulkens]	ok
5-1158	A	71:5	71:5	Figure 5.4.1: write out DIC (dissolved inorganic carbon) [Wilco Hazeleger]	Noted
5-1159	A	72:0	72:	Fig 5.4.2: quality [Michel Rixen]	ok
5-1160	A	72:0	72:0	It is very difficult to read this figure. [Christopher Sabine]	Noted, figure changed
5-1161	A	72:0		Figure 5.4.2: The concentrations in the deep Atlantic are shown to be negative. Please check. [Nicolas Gruber]	Noted
5-1162	A	72:1		Figure 5.4.2. The resolution of the graphic is too low, it is not readable. [Philippe Tulkens]	ok
5-1163	A	72:1		Figure 5.4.2. This figure does not add much information to the text. If the chapter should be shortened, this figure could be dropped. [Philippe Tulkens]	Noted. Better figure provided.
5-1164	A	73:0	73:	Fig 5.4.3: quality [Michel Rixen]	ok

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No.	Batch	Page:line From	Page:line To	Comment	Notes
5-1165	A	73:0	73:0	It is very difficult to read this figure. [Christopher Sabine]	ok
5-1166	A	73:0		Figure 5.4.3: I suggest to add the atmospheric pCO2 trend as well to strengthen the connection between the oceanic and atmospheric trends. Note that Gruber and Sarmiento, 2002 should read Gruber et al. (2002) --> Science article. One could also add Keeling et al. (2004) for the HOT site. [Nicolas Gruber]	Rejected. Figure will be reduced.
5-1167	A	73:0		Fig. 5.4.3 Clarify left top versus left bottom -- what is each graph showing. [Melinda Marquis]	Noted.
5-1168	A	73:1		Figure 5.4.3. The resolution of the graphic is too low, it is not readable. [Philippe Tulkens]	ok
5-1169	A	73:8	73:8	Wrong Reference. Change Takahashi et al., 2005 to Feely et al., 2005. Feely, R.A., T. Takahashi, R. Wanninkhof, M.J. McPhaden, C.E. Cosca, S.C. Sutherland, and M.-E. Carr (2005): Decadal variability of the air-sea CO2 fluxes in the equatorial Pacific Ocean. J. Geophys. Res. [Submitted]. [Richard Feely]	ok
5-1170	A	74:0		Fig. 5.4.4 Add labels and units to X and Y axes. Add label and units to color bar. [Melinda Marquis]	Noted
5-1171	A	74:1		Figure 5.4.4. The figure layout should be improved, the axis values are difficult to read. [Philippe Tulkens]	Noted
5-1172	A	75:0		Figure 5.5.1. I believe the phrase "GIA effects included" is confusing. Perhaps "GIA effects removed". It is also not clear if the GIA contribution is removed in the quoted rate or from all of the data shown in the plot. [Mark Tamisiea]	Will be redone
5-1173	A	75:0		Figure 5.5.1. The slope listed on this figure is 3.1 but the actual slope plotted is 2.9 (3.2 over 11 years). [Kevin Trenberth]	Noted, will be corrected
5-1174	A	75:5	75:5	"compute" should be "computed". [Chiu-Ying LAM]	ok
5-1175	A	75:5		Amend 3.1±0.4 in Fig 5.5.1 to 3.1±0.8 to reflect 95% confidence limits [Vincent Gray]	noted
5-1176	A	76:0		Fig. 5.5.2 Add labels and units to X and Y axes. [Melinda Marquis]	Ok
5-1177	A	76:5	76:5	Fig 5.5.2 Caption : why not simpler -> "Sea level change (mm/yr) over 1993-2005" [Michel Rixen]	Noted
5-1178	A	77:0		Figure 5.5.3: This is from a different paper to one referred to in the text. Figure 5.5.3	Noted, figure will be changed

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				should be Fig. 12 of Church et al. 2004. [John Hunter]	
5-1179	A	77:0		Fig. 5.5.3 Add "s" to "area" in last sentence of caption. [Melinda Marquis]	Ok
5-1180	A	77:6	77:10	Fig 5.5.3 Caption: are the offset 150 and 300 just there to distinguish the time series? [Michel Rixen]	Noted
5-1181	A	77:10		Rejoice! We have two standard deviations shown in Fig 5.5.3, but where else in the whole chapter? [Vincent Gray]	Noted
5-1182	A	78:0		Fig. 5.5.4 Clarify Y axis, e.g., *sea-level* height. [Melinda Marquis]	Noted
5-1183	A	79:0		Fig. 5.5.5 Clarify Y axis: e.g., *sea-level* steric height. [Melinda Marquis]	Noted
5-1184	A	79:6		Redraw Fig 5.5.5 showing two standard errors for 95% confidence [Vincent Gray]	Will be done
5-1185	A	80:0		Figure 5.5.6: it would be useful to include the results of Ishii et al. (2005) on this plot, although I realise that this plot is extracted from Willis et al., 2004. [John Hunter]	Will be done
5-1186	A	80:0		Figure 5.5.6: it seems from the text that "Levitus et al. (2004)" should read "Levitus et al. (2005a)". [John Hunter]	Ok
5-1187	A	81:0		Figure 5.5.7: it would be clearer to indicate that the depth range was (presumably) 0-500m, rather than just stating "500m". [John Hunter]	Ok
5-1188	A	81:0		Fig. 5.5.7 Add labels and units to X and Y axes. [Melinda Marquis]	For standard global plots this is unnecessary.
5-1189	A	81:1	81:6	The spatial maps of Figure 5.5.7 are VERY VERY different to those of Figures 5.5.2 and 5.5.8. What is going on here. Can all three figures be belived? I think that more discussion of these very different spatial patterns in certainly needed. [Trevor McDougall]	This is correct, the reasons will be better explained in the next draft.
5-1190	A	81:5		Note that the figures indicate the trend from 1955 to 1998 but the captions says 1950 to 1998. [John Church]	Noted
5-1191	A	82:0		Fig. 5.5.8 Add labels and units to X and Y axes. [Melinda Marquis]	See 5-1188
5-1192	A	83:0		Fig. 5.5.9 Add labels and units to X and Y axes. Better to label part (a) at the top versus	See 5-1188

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No.	Batch	Page:line From	Page:line To	Comment	Notes
				part (b) at the bottom, and refer to both in caption. [Melinda Marquis]	
5-1193	A	84:0		Figure 5.5.10 [cited in section 5.5.6 on p33]. As you probably have noticed already, the numbers for the sum of climate-related terms in Table 5.5.2 at p33 do not correspond to the numbers indicated in Fig. 5.5.10. [Gerrit Burgers]	Noted, will be corrected
5-1194	A	84:0		Figure 5.5.10: The bars for the glaciers and the total are misplotted in both panels. Suggest the panels should also contain the terrestrial storage terms. [John Church]	Will be redrawn
5-1195	A	84:0		Figure 5.5.10: These figures would be easier to read if a vertical line was drawn to indicate the 0.0 mm/year rate of sea level rise. [John Hunter]	Noted
5-1196	A	84:0		Figure 5.5.10: This may be nit-picking, but here (and probably elsewhere) "sea level rise" is used to denote "rate of sea level rise". In this case, the meaning is clear from the units (mm/yr), but in other places, it may not be so obvious. [John Hunter]	Noted
5-1197	A	84:0		Figure 5.5.10: The terrestrial storage term should be included. [John Hunter]	Noted
5-1198	A	84:8		At last two standard errors. But where else in the Chapter? [Vincent Gray]	Noted
5-1199	A	85:0		Fig needs more space [Walter Zenk]	noted
5-1200	A	85:0		Figure 5.6.1: Where is the THC in this figure? Also what period is it for? [Kevin Trenberth]	Only the robust features of change are indicated in figure, THC change is not robust
5-1201	A	85:1		Figure 5.6.1. This figure is an extremely useful summary figure [Philippe Tulkens]	Noted
5-1202	A	86:0		I do not think this figure is referred to. [John Church]	Will be changed
5-1203	A	86:5		I feel strange to see a future projection in Figure 1 because the chapter is discussing observations. [Kiminori Itoh]	Sea level box is in collaboration with chapter 10
5-1204	A	86:7		Q 5.1, Fig. 1 Fix typo: Chpater -> Chapter. [Melinda Marquis]	Ok

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