

IPCC Working Group I Fourth Assessment Report

Expert and Government Review Comments on the Second-Order Draft

Chapter 5

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Batch AB (15 June 2006)

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| | | From | To | | |
| 5-1 | A | 0:0 | | The Chapter is focussed and of good length. This is the chapter where sea level is most appropriately dealt with. It would help consolidate the body of the WG1 report if discussion on sea level was confined to this chapter to the extent possible. In some sections of this chapter it was noted that there was conflict in the use of the terms 'trend' and 'change'. [Govt. of Australia (Reviewer's comment ID #: 2001-260)] | Thanks |
| 5-2 | A | 0:0 | | I find chapter 5 consistent with the other observation chapters of the SOD. In my opinion, the chapter was considerably improved, compared with the SOD. I've especially enjoyed the well done and suggestive figures. [Roxana Bojariu (Reviewer's comment ID #: 24-10)] | Thanks |
| 5-3 | A | 0:0 | | Alongside the chapter it becomes clear that observed variations in variables such as: heat contents, salinity, and sea level, are strongly consistent with known characteristics of circulation of the ocean at a large scale. [Govt. of Chile (Reviewer's comment ID #: 2005-7)] | Thanks |
| 5-4 | A | 0:0 | | It is a contribution the information provided related with measurement techniques and estimation of oceanic parameters, which allow the development of studies of variations in seasonal and decadal timescales. [Govt. of Chile (Reviewer's comment ID #: 2005-9)] | Thanks so much |
| 5-5 | A | 0:0 | | This chapter will allow to assess several of the observations presented regarding changes in oceanic parameters, considered as indicators of climate change. It will also, will allow to update antecedents presented in the TAR and to point out some of the most frequent questions related with the role of the ocean and climate change. [Govt. of Chile (Reviewer's comment ID #: 2005-10)] | Agree, thanks |
| 5-6 | A | 0:0 | | Finally, this chapter is a big contribution for identification of some of the oceanic parameters that may be useful to detect climate change, particularly changes in temperature and salinity. [Govt. of Chile (Reviewer's comment ID #: 2005-11)] | Agree, thanks. |
| 5-7 | A | 0:0 | | Ok [Tiziano Colombo (Reviewer's comment ID #: 46-13)] | No change necessary. |
| 5-8 | A | 0:0 | | This chapter provides an excellent overview but I have some concern that key points may be lost in the technical detail. There are a number of cases in which the interested but non-expert reader will have trouble following some aspects of the science. I recognize that the CLAs and LAs have made a significant effort to pull the material together in a coherent manner and I would encourage them to continue to try to make the text as clear and accessible as possible. [Donald L. Forbes (Reviewer's comment ID #: 72-1)] | Will be accepted through the chapter text |

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| 5-9 | A | 0:0 | | Missing ? Reference to J.E. Hansen et al., Science, 308, 1431 (2005). It does appear in a later chapter. [Govt. of France (Reviewer's comment ID #: 2010-46)] | Taken into account: chapter primary focii is the observations and so not included. |
| 5-10 | A | 0:0 | | I'm sorry to have been slow responding. I read Chapter 5 early and was actually rather pleased with how most of my earlier comments had been met and dealt with. I think this is a good chapter and have only one comment of a detailed nature, essentially except for one concern i'd be happy to see the report issued as it stands. [Howard Freeland (Reviewer's comment ID #: 75-1)] | Thanks and noted. |
| 5-11 | A | 0:0 | | ?It is really strange to ignore chemical tracers such as CFCs, which have been used to show ages of water masses. [Motoyoshi Ikeda (Reviewer's comment ID #: 113-1)] | Accept. Slightly more reference to CFCs in section 5.3 now. Already included in section 5.4. They are not fully ignored. |
| 5-12 | A | 0:0 | | Suggest including more discussion of better characterized embedded shorter period trends to balance discussion of trends computed over long periods. Readers will concentrate on the long-term trends which, when considerable shorter-term variability is present, will be strong functions of the conditions at the start and end of the record and not indicative of important changes on shorter time scales. This comment reflects some of the specific comments received on this chapter concerning the statistical analysis to extract trends from a record containing strong fluctuations at various time scales. [Govt. of United States of America (Reviewer's comment ID #: 2023-311)] | Taken in consideration for the new version of the chapter |
| 5-13 | A | 0:0 | | Use of "likely" and other terms reflecting certainty or confidence of a statement in the chapter are inconsistently applied. There are numerous instances where formal terms of certainty or confidence defined elsewhere in the assessment, in particular, the Technical Summary, have been used to qualify a statement in an informal and inappropriate sense for the assessment. Recommend that the authors conduct a global search and evaluation for consistent use of these terms throughout the volume. These terms include, but are not limited to: "likely", "caused", "confidence", "attribution". [Govt. of United States of America (Reviewer's comment ID #: 2023-312)] | Accept. 2 instances in section 5.3 were changed to "possibly" or "probably" since indeed they were not intended to be formal measures of confidence. The term "very likely" was retained in Box 5.1 however. Agree – will be done in most cases when it is possible |
| 5-14 | A | 0:0 | | Chapter 5 is supposed to focus on results from observations, but frequently went beyond the summary of recent observations in the literature into explanations and discussions of attribution. Strongly recommend removing these discussions, or if appropriate, moving them to Chapter 9. Also strongly recommend a substantial shortening of the Chapter 3, 4, and 5 bundle in order to make them more even in presentation, as well as more focused, and improve the ease of reading. [Govt. of United States of America (Reviewer's comment ID #: 2023-313)] | Accept. Some reviewers requested more, not less, attribution. So we are striving for a balance, with a minimum of attribution. Not fully agree. Since the Ocean chapter is the first time appearing in IPCC as a separate chapter, some |

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| | | | | | baseline means should be given. |
| 5-15 | A | 0:0 | | Recommend a thorough review of the terminology associated with large-scale coherent patterns of the atmosphere (such as AMO) throughout Chapters 3, 4, and 5 to improve the consistency in the discussion. [Govt. of United States of America (Reviewer’s comment ID #: 2023-314)] | Taken into account. Terms in 5.3 were consistent with definitions in Chapter 3. Agree – will do. |
| 5-16 | A | 0:0 | | I am still worried by the use of 1.5+/-0.5 mm/year instead of 1-2 mm/year as reported by the TAR for sea level trend, and possibly other examples. The former implies that 2.5 is possible at 2-sigma which is not what the TAR meant. [Philip Woodworth (Reviewer’s comment ID #: 295-1)] | Taken into account in revision of introduction. Now very close to the actual TAR words. |
| 5-17 | A | 0:0 | | The FOD contained on page 25, lines 40-51, a relevant paragraph describing data archaeology (Port Arthur time series in particular). That seems to have been dropped in the SOD. Can something like that be put back in? [Philip Woodworth (Reviewer’s comment ID #: 295-5)] | We have focussed on global changes and regional changes for selected regions. The earlier discussion on data archeology has not been brought in again because it did not fit in this concept. |
| 5-18 | A | 1:1 | 36:32 | It reads much better than the previous version. Thus well done! Only major worry is an impression left that everything is based on the last decade which ofcourse weakens the whole argument about climate change I view of the recent arguments about hockey stick statistics. I am also uncomfortable with the certainty that the heat content estimates are reliable because when different people do it with the same data they get consistent answers. Surely if there are not enough observations our certainty must be lower than that claimed. However, it would be very difficult to disprove the statement for the same reasons. [Michael Tsimplis (Reviewer’s comment ID #: 268-12)] | Taken into account: the revised chapter will include a greater discussion of decadal variability and greater emphasis on long term results. |
| 5-19 | A | 1:22 | | Table of Contents of this Chapter includes the main subjects related with oceans and their importance in climate change, mainly on interaction between ocean and atmosphere, as well as identification of variations of some indicators considered as climate change markers in seasonal and decadal timescales. [Govt. of Chile (Reviewer’s comment ID #: 2005-5)] | Noted, no changes necessary. |
| 5-20 | A | 1:26 | 1:38 | I think the revised structure here is good, especially the section 5.3.6 which draws together results from the global and regional analyses. However the reasoning behind it only became clear to me after I had got as far as 5.3.6. I suggest this could be clarified by adding the word 'Regional' at the start of the title of section 5.3, and adding a short introductory sentence at the start of 5.2 to explain the roles of 5.2 and 5.3. [Richard Wood (Reviewer’s comment ID #: 294-1)] | Accept. Title changed for 5.3. Some structural change of sections 5.3.1 and 5.3.6 in response to comment 5-645. |
| 5-21 | A | 2:1 | 2:1 | You should begin by telling us what you mean by confidence limits. I suspect that they are only one standard deviation. They should be two standard deviations to comply with the standard requirement for 95% confidence. I propose to double all your confidence | Confidence intervals are 95% nearly everywhere in the text of this draft. We will increase the clarity of the |

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| | | | | limits accordingly. [VINCENT GRAY (Reviewer's comment ID #: 88-558)] | confidence intervals used throughout. As a result of Bergen LA4 the confidence intervals will be 90% intervals. |
| 5-22 | A | 2:1 | 3:14 | The Executive Summary has not picked up on the same set of Robust Findings and Key Uncertainties found in the Technical Summary. It would assist the reader if this were so - and if each point in the Summary was referenced to the body of the text. [Govt. of Australia (Reviewer's comment ID #: 2001-263)] | Accept, and robust findings and Executive summary will agree. |
| 5-23 | A | 2:1 | 3:14 | It would be helpful if each item in the Executive Summary had references to the relevant subsections in the chapter. [John Hunter (Reviewer's comment ID #: 112-31)] | Reject. The executive summary is ordered in the same way as the sections, and we believe the material can be found easily without explicit reference. |
| 5-24 | A | 2:1 | | Executive Summary paragraphs present appropriately the contents of the whole chapter. [Govt. of Chile (Reviewer's comment ID #: 2005-6)] | Noted and thanks. |
| 5-25 | A | 2:3 | 2:6 | The aim of the AR4 is not its consistency with the TAR, per se. I think the statement have to be rephrased. [Roxana Bojariu (Reviewer's comment ID #: 24-11)] | Noted, emphasis of TAR results reduced |
| 5-26 | A | 2:3 | 2:6 | Here and elsewhere, it's a pity this key number cannot be updated since the TAR [Chris Folland (Reviewer's comment ID #: 71-161)] | We agree, however no change possible: no new estimates exist of this variable since the TAR, due to insufficient data. |
| 5-27 | A | 2:3 | 2:3 | Replace "risen" with "periodic behaviour" [VINCENT GRAY (Reviewer's comment ID #: 88-559)] | Reject: no reason given, and the data don't support this view. |
| 5-28 | A | 2:4 | 2:5 | Replace ."14.5 x 10 to the 22 Joule" on line 4 to "1998" on line 5" with " 10x 10 to the 22nd Joule from 1955 to 1980; followed by a fall of 6x10 to the 22nd Joule from 1980 to 1990; and a rise of 8 x 10 to the 22nd K\Jpule from 1980 to 2005." 190 5-190 560 [VINCENT GRAY (Reviewer's comment ID #: 88-559)] | Rejected- We have computed the increase in heat content using the linear trend. This is standard procedure. |
| 5-29 | A | 2:5 | 2:5 | Delete "This amount represents an average warming of" [VINCENT GRAY (Reviewer's comment ID #: 88-561)] | Rejected- We see no reason to delete this and the Reviewer gives no reason why this should be done. |
| 5-30 | A | 2:6 | 2:6 | Insert after "ocean" "can vary in temperature over time by as much as" [VINCENT GRAY (Reviewer's comment ID #: 88-562)] | Rejected- We see no reason to make this change and the Reviewer gives no reason why this should be done. |
| 5-31 | A | 2:8 | 2:9 | the paragraphs 1 and 2 could be combined. [Roxana Bojariu (Reviewer's comment ID #: 24-12)] | Accepted |
| 5-32 | A | 2:9 | 2:9 | Replace "longer-term" by "periodic" | Rejected- We see no reason to make |

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| | | | | [VINCENT GRAY (Reviewer's comment ID #: 88-563)] | this change and the Reviewer gives no reason why this should be done. |
| 5-33 | A | 2:18 | 2:25 | Suggest add year from which these observations of warming and freshening are made from. [Govt. of Australia (Reviewer's comment ID #: 2001-261)] | Taken into account: an exact period is hard to specify over the mix of studies assessed in this analysis. |
| 5-34 | A | 2:18 | 52:18 | typo error: "Deep Waters waters" should be corrected as " Deep Waters". [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-7)] | Accept: |
| 5-35 | A | 2:18 | | Add AABW as stated later under 5-3-2-2 -deep circulation: "Southern Ocean Mode Waters, Upper Circumpolar Deep Waters, and even Antarctic Bottom Water are warming." Mode Water with capital letters as Deep Water. [Walter Zenk (Reviewer's comment ID #: 301-3)] | Reject: while there is wamring of some components of AABW the spatial scale of this warming is not great enough to warrant addition into the exec. Summar |
| 5-36 | A | 2:20 | 52:20 | typo error: "accompagnied" should be corrected as "accompanied" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-8)] | Accept |
| 5-37 | A | 2:21 | 2:22 | The statement about the Mediterranean Sea is misleading. The warming of the Mediterranean Sea is a product of the mid-1990s. Painter and Tsimplis (2005) and Tsimplis and Rixen (2002) have shown that in many regions the upper waters of the Mediterranean have been cooling between the 1960s and the 1995. Moreover the plots in Rixen et al. clearly show that the increase is a product of the last decade. The increases in the T of the deep waters are accompanied with increases in S and are caused by the inceased S linked reduced freshwater provision either due to the NAO or river damming. Because S increases deep water can be formed at higher (deep water) temperatures (Bethoux, Rohling and Bryden Tsimplis and Baker and many others have described this mechanism) even if these higher, for the deep waters, T are caused by lower near surface temperatures. The statement is misleading because there is a major argument for the global ocean that sea level is partly going up due to thermal expansion.Thus the statement that the Mediterranean Sea is warming would imply that sea level is going up. This is clearly not the case for the Mediterranean over the past decades but only during the1990s. In general, the treatment of the Mediterranean Sea is misleading. You've made a choice to remove the sea level variability for the Mediterranean basin which is probably the best understood deep basin in the world both in circulation and inrespect of contribution to sea level processes. I can understand such a decision if it is based on the acceptance that the local processess have been dominating the sea level in the basin. For the report to be accurate it must be clearly stated that the Mediterranean Sea level is dominated mainly by local / regional processes and that the thermal expansion or the salinification of the water below sill level cannot play a role in the sea level variability of the basin. [Michael Tsimplis (Reviewer's comment ID #: 268-1)] | Statement on Mediterranean deleted from exec summary |
| 5-636 | B | 2:21 | 2:21 | here and elsewhere: The expression "ventilation" means different things to different | Taken into account. Change in |

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| | | | | people. As used here, it means something like "rate of water renewal" , or "strength of circulation". When talking about O2 changes, one also needs to consider the actual "ventilation" process, which is the exchange of oxygen across the air-sea interface and the transport out of the mixed layer into the permanent thermocline. In a recent paper, Deutsch et al. (2005) referred to the former as "circulation" and the latter as "ventilation". I am not arguing here that this use necessarily needs to be adopted, but that perhaps the use of the word "ventilation" should be used more carefully. Why not writing here "a reduction in the rate of renewal of subtropical thermocline waters"? [Nicolas Gruber (Reviewer's comment ID #: 307-1)] | wording in SOD 18:21 in response. What we mean is indeed "rate of water renewal", which is not synonymous with strength of circulation. We mean the amount of mixed layer water that reaches interior isopycnals, whether measured by oxygen or CFC content, etc, or by physical measures of the circulation. These separate measures don't always give the same result, but both are commonly referred to as ventilation. We do not wish to rule out one of the two separate methods of assessment. |
| 5-38 | A | 2:24 | 2:25 | ?The document states that the NADW has been freshened significantly. However, nothing is mentioned about the fact that the lower part of the NADW used to come from the Greenland Sea through the Irminger Sea but has been significantly reduced. This event should be mentioned also, since it may cause the deeper convection in the Labrador Sea by reducing the density at depth below 2000 m. [Motoyoshi Ikeda (Reviewer's comment ID #: 113-2)] | Rejected. We have not found evidence for changes in the transport balance of overflows. Freshening of the NADW is consistent with freshening in the Greenland Sea and other locations in the northern North Atlantic. |
| 5-39 | A | 2:24 | 2:25 | it is stated that the North Atlantic subpolar gyre is cooling (with increased convection). This statement is valid till 1994/95. Since 1995 the convection generally ceased and produced lighter water masses [Monika Rhein (Reviewer's comment ID #: 212-1)] | Accept. Comment amended. |
| 5-40 | A | 2:24 | 2:25 | The statement is in conflict with Figure 3.2.9b, which shows a warming in the Atlantic subpolar gyre over the period since 1979. The statement must be modified accordingly: There is a cooling on the long time scale, but recently that cooling has turned into a warming. [Andreas Sterl (Reviewer's comment ID #: 253-3)] | Taken into account. Comment amended. |
| 5-41 | A | 2:27 | 2:28 | The statement is not informative in its own. Perhaps is better to combine it with paragraph 5. [Roxana Bojariu (Reviewer's comment ID #: 24-13)] | Taken into account. Comment made clearer and more accurate, remains a separate point. |
| 5-42 | A | 2:27 | | What "indirect evidence" on "considerable decadal MOC variability" do you have? That is not really supported by the following sections. It is small in models, and the Bryden et al. thing is just erroneous, how can you put so much weight on the result of an application of an outdated old-fashioned method? Even the main author is now backpadding on this | Text changed, we use "significant" instead of "considerable". |

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| | | | | stuff! [Friedrich Schott (Reviewer's comment ID #: 228-1)] | |
| 5-43 | A | 2:27 | | I think the Bryden et al 2005 observations are about as direct an estimate of the MOC as we will ever get, and suggest adding "Direct and..." at the start of this sentence. [Richard Wood (Reviewer's comment ID #: 294-2)] | Accept. Remove word "indirect" |
| 5-44 | A | 2:28 | 2:28 | "but" opposes the decadal variability and the long-term trend, which in fact are complementary [Pascale DELECLUSE (Reviewer's comment ID #: 58-1)] | Accept. |
| 5-45 | A | 2:31 | 2:31 | specify "reduced rates of subtropical ventilation" [Pascale DELECLUSE (Reviewer's comment ID #: 58-2)] | text changed, no reference to ventilation in exec summ |
| 5-637 | B | 2:31 | 2:31 | consistent with reduced rates of ventilation". See comment above with regard to "ventilation". I am also not so sure what is meant by "consistent with". Reduced oxygen is ALWAYS consistent with reduced rates of "ventilation", but this doesn't mean that it is the main cause. Why not saying so? e.g. write something like "most likely driven by the ..." [Nicolas Gruber (Reviewer's comment ID #: 307-2)] | Accepted |
| 5-638 | B | 2:32 | 2:33 | "changes in deep ocean nutrients are indicative of changes in biological activity". This statement is inconsistent with the above statement that the majority of the ocean interior oxygen changes are due to ventilation. One of the characteristics of circulation driven changes in oxygen is a concomitant change in nutrients. Therefore, it can't be circulation in one case and biology in the other case. The problem is that there exist very few observational studies, but conceptionally there is little doubt about this. [Nicolas Gruber (Reviewer's comment ID #: 307-3)] | Accepted – "and in deep ocean nutrients" removed. |
| 5-46 | A | 2:35 | 2:35 | Replace "118±19" by "118±38", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-564)] | Rejected – in the SOD all confidence intervals were 95%. In the final draft the confidence interval are changed to 90% |
| 5-639 | B | 2:35 | 2:35 | add "inorganic" in front of carbon. We don't know whether organic carbon has changed or not. This is one of the big unknowns. [Nicolas Gruber (Reviewer's comment ID #: 307-4)] | Accepted, word inorganic added |
| 5-47 | A | 2:37 | 2:37 | Replace "42±7" by "42±14", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-565)] | Rejected – see comment 5-46 |
| 5-48 | A | 2:37 | 2:37 | Replace "37±7" by "3714", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-566)] | Rejected – – see comment 5-46 |
| 5-49 | A | 2:42 | 2:43 | I would remove this statement because to identify the consistency with TAR is not the goal of AR4, by itself. [Roxana Bojariu (Reviewer's comment ID #: 24-14)] | Accepted, see earlier comment |

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| 5-50 | A | 2:42 | 2:42 | Replace "1.8±0.5" by "1.8±10", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-567)] | Rejected: – see comment 5-46 |
| 5-51 | A | 2:42 | 2:46 | Is there really any evidence at all for a previous period in the last few centuries having a rate of sea level rise as large as 3.1 mm/yr--on what possible basis is this decade long record being judged possibly just a natural variation. Is it not really unusual to be having a rate anywhere near this large given the tide gage record? There is no allowance being made that the late 20th century warming is just a natural variation, so why is the rise in sea level, given the rather smooth increase noted in Figure 5.5.1, being suggested as possibly natural. Does not the presumption have to be that this is human-induced, even if all terms cannot be explained for the past 50-100 years? Going back to page 26, line 54ff I see some mention of past high rates, but this was from weaker data sets and it is not clear that there was a physical basis for expecting the rates to be real rather than an artifact. [Michael MacCracken (Reviewer's comment ID #: 152-259)] | Taken into account: further analysis suggests that 3.1 mm/yr has been observed at several times during the last 50 years. Attribution is the task of Chapter 9. |
| 5-52 | A | 2:42 | 2:44 | Here, and elsewhere in this chapter, rates tend to be quoted per year; in particular, the sea-level rise is quoted in mm/year. In comment #80 I suggested quoting the sea-level rise in mm/decade, to be consistent with the earlier quotation of temperature rises in terms of K/decade. A decade seems a more natural time span than a year in discussing the sorts of observed change that are discussed here. [Adrian Simmons (Reviewer's comment ID #: 242-87)] | Rejected: our preferred style is per year. |
| 5-53 | A | 2:43 | 2:43 | Replace "1.5±0.5" with "1.5±1.0", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-568)] | Rejected: already confidence interval |
| 5-54 | A | 2:44 | 2:44 | Replace "3.1±0.8" by "3.1±1.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-569)] | Rejected: already confidence interval |
| 5-55 | A | 2:48 | 2:48 | Replace "0.4±0.1" by "0.4±0.2", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-570)] | Rejected: already confidence interval |
| 5-56 | A | 2:48 | 2:48 | "is is 0.4±0.1 mm" should be "is 0.4±0.1 mm". [Chiu-Ying LAM (Reviewer's comment ID #: 139-5)] | Accepted |
| 5-57 | A | 2:49 | 2:49 | Replace "1.6±0.6" by "1.6±1.2", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-571)] | Rejected: already confidence interval |
| 5-58 | A | 2:53 | 2:54 | Paragraph 11 may be combined with paragraph 10. [Roxana Bojariu (Reviewer's comment ID #: 24-15)] | Structure has been changed |
| 5-59 | A | 2:53 | 2:54 | You might consider including the ice melt contribution to sea level rise for the last 50 years in bullet #11 in addition to the currently cited 1993-2003 period. This would maintain consistency with bullets #10 and 12. [Brian Soden (Reviewer's comment ID #: 245-5)] | The ice melting rates are results of chapter 4, are not cited in final draft exec summary |
| 5-60 | A | 2:53 | | point 11 should give the contribution of loss of mass from glaciers, ice caps, etc. for the | see previous comment |

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| | | | | last 50 years for the point made on the next page (3:1) to be illustrated more clearly. For the last 10 years, the differences between the observed value (3.1) and the two components (2.8) are not large. [David Rind (Reviewer's comment ID #: 214-41)] | |
| 5-61 | A | 2:53 | | Point 11 should give the contribution of loss of mass from glaciers, ice caps, etc. for the last 50 years for the point made on the next page (3:1) to be illustrated more clearly. For the last 10 years, the differences between the observed value (3.1) and the two components (2.8) are not large. [Govt. of United States of America (Reviewer's comment ID #: 2023-315)] | see previous comments |
| 5-62 | A | 2:54 | 2:54 | Replace "1.2±0.6" by "1.2±1.2", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-572)] | Rejected: – see comment 5-46 |
| 5-63 | A | 2:56 | 2:57 | Unclear sentence; "smaller than the observed value" of what? Should "estimated" be before "contribution"? [Govt. of Australia (Reviewer's comment ID #: 2001-262)] | Accept: |
| 5-64 | A | 2:56 | 3:2 | This bullet is potentially confusing and the reference to missing contributions for the past 50 years and the budget being closed over the last decade creates ambiguity. The Executive Summary should strive for simple language that makes very clear points (an objective that the chapter team has achieved in most other bullets). [Donald L. Forbes (Reviewer's comment ID #: 72-2)] | Taken into account: executive summary revised |
| 5-65 | A | 3:1 | 3:1 | Replace "2.8±0.8" by "2.8±1.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-573)] | Rejected: already confidence interval |
| 5-66 | A | 3:4 | 3:6 | The fact that sea level is not uniform is not intuitively obvious. Adding the statement from Pg 5-35, lines 1-2 "Spatial variability of sea level (rise) rates is mostly due to non-uniform thermal expansion." would provide an answer to the obvious question in many readers' minds. [Lenny Bernstein (Reviewer's comment ID #: 20-57)] | Noted. However, we feel the executive summary should focus on the results and not on explanations which are given in the main text. |
| 5-67 | A | 3:4 | 3:6 | The fact that sea level is not equal around the globe is not intuitively obvious to readers who do not work in the area. An explanation of why this is so, such as appears on Pg 5-35, lines 1-2, should be added. [Jeff Kueter (Reviewer's comment ID #: 137-54)] | See previous comment |
| 5-68 | A | 3:4 | :6 | The fact that sea level is not uniform is not intuitively obvious. Adding the statement from Pg 5-35, lines 1-2 "Spatial variability of sea level (rise) rates is mostly due to non-uniform thermal expansion." would provide an answer to the obvious question in many readers' minds. [Govt. of United States of America (Reviewer's comment ID #: 2023-316)] | see previous comments |
| 5-69 | A | 3:12 | 3:14 | The statement is not informative. In my opinion, it should be deleted. | Reject- the fact that the changes in |

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| | | | | [Roxana Bojariu (Reviewer's comment ID #: 24-16)] | various parameters are consistent with ocean circulation characteristics is an important result. |
| 5-70 | A | 3:12 | 3:14 | : patterns of salinity change have to be consistent with atmospheric circulation also (maybe even predominantly). [David Rind (Reviewer's comment ID #: 214-42)] | Noted- this is now expressed in the exec summary |
| 5-71 | A | 3:12 | 3:15 | I found this sentence hard to understand in isolation from the chapter text. It could be misinterpreted to mean that the observed changes could be natural in origin. How about "The patterns ... are broadly consistent with what would be expected in a warming world, given established understanding of the large scale ocean circulation" [Richard Wood (Reviewer's comment ID #: 294-3)] | Noted. However, we have observational evidence only for ocean changes. The attribution of these changes is discussed in chapter 9.. |
| 5-72 | A | 3:12 | :15 | Why can't changes in heat content and salinity, for example, described as consistent with known characteristics of ocean circulation also be consistent with characteristics of surface energy fluxes as well? [Govt. of United States of America (Reviewer's comment ID #: 2023-317)] | Noted: they are consistent, but we don't have observational evidence of this consistency. |
| 5-640 | B | 3:14 | 3:14 | "consistent with known characteristics of the large-scale ocean circulation". This sentence is unclear. Was it meant to include "changes in ocean circulation". Otherwise, I read this sentence as that the changes in bgc, etc are consistent with the "mean" circulation. [Nicolas Gruber (Reviewer's comment ID #: 307-5)] | Noted: you have understood the intent of this bullet. The changes in bgc are indeed consistent with the way the mean circulation works, |
| 5-73 | A | 3:50 | 4:12 | At the end of this paragraph a reference is made to heat increase in observational estimates in ocean models. I think the word observational should be removed or it needs to be explained what is meant by it. One could say, ocean models constrained by ocean data. In any case, those results are not observational. Moreover, the statement is obsolete, since recent model results from ECCO and SODA are actually larger than Levitus estimates and still larger than Willis et al. A recent paper on ECCO 1 deg. ocean synthesis is in press (Koehl, Stammer and Cornuelle, JPO, in press) and could be used now as reference for much better results than the cited 4 year old (obsolete) results from a 2 degree model that are being used in the report. [Detlef Stammer (Reviewer's comment ID #: 251-1)] | Accepted, "observational" removed and new paper taken into account. |
| 5-74 | A | 4:1 | 8:46 | In sections 5.1 and 5.2 where 'trend' has been used, in most instances this should be replaced by 'change', to be consistent with terminology through the document. [Govt. of Australia (Reviewer's comment ID #: 2001-264)] | Noted. We have now inserted a remark in the introduction that calculation of a linear trend does not imply that the signal is consistent with a linear trend. |
| 5-75 | A | 4:3 | 4:3 | Delete "change" [VINCENT GRAY (Reviewer's comment ID #: 88-574)] | Rejected- The reviewer has given no reason to make this change. |
| 5-76 | A | 4:7 | 4:7 | remove "e.g. related to" [Melissa Bowen (Reviewer's comment ID #: 28-1)] | Accepted |

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| 5-77 | A | 4:21 | 4:22 | Here it is stated that more than half the ocean warming is found in the upper 300 m, an increase of 0.037 C/decade. On page 2, line 6, it is stated that the average warming of the entire upper 3000 m is 0.037 C over the period 1955-1998, or 0.0088 C/decade. 300 x 0.037 is not more than half of 3000 x 0.0088, so these two statements are not self-consistent. To say "almost half" seems to be correct. [Danny Harvey (Reviewer's comment ID #: 101-28)] | Accepted- text will be modified. |
| 5-78 | A | 4:22 | 4:22 | Correct "the upper 3000 m" (not 300 m). Also, I previously asked for specification "0.037 deg C per decade". I now understand this is incorrect. The correct statement is "0.037 deg C" - i.e., this is the total mean ocean warming since the late 1950s. I apologise for causing this confusion. [Robert Marsh (Reviewer's comment ID #: 163-1)] | noted, sentence deleted, section rewritten |
| 5-79 | A | 4:22 | 6:3 | Page 4 (line 22) refers to 300 m and 0.037 deg C/decade. Page 6 (lines 1-3) refers to 3000 m and 0.037 deg over period of 1955-1998. I'm just checking here: The similarities of these numbers (300 m and 3000 m; 0.037 deg C and 0.037 deg C) are coincidental, right? No typos here, right? I checked the TAR (page 35, Figure 7a, because the TAR is referred to on pages 4 of Ch. 5 AR4 SOD); the TAR, Fig. 7a refers to "global ocean (to 300 m depth) heat content increase since 1950s equal to 0.04 deg C per decade." [Melinda Marquis (Reviewer's comment ID #: 162-11)] | Noted-stement has been removed |
| 5-641 | B | 4:25 | 4:27 | Add "inorganic" in front of carbon [Nicolas Gruber (Reviewer's comment ID #: 307-6)] | Accepted |
| 5-80 | A | 4:27 | 4:27 | add "in" between "changes" and "ocean" [Melissa Bowen (Reviewer's comment ID #: 28-2)] | Accepted |
| 5-81 | A | 4:27 | 4:27 | typo error: "on changes ocean" --> "on changes in ocean" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-9)] | Accepted |
| 5-82 | A | 4:30 | 4:30 | Replace "1.5±0.5" with "1.5±1.0", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-575)] | Rejected: – see comment 5-46 |
| 5-83 | A | 4:30 | 4:30 | Delete "average for the". You have given a total, not an average. [VINCENT GRAY (Reviewer's comment ID #: 88-576)] | Rejected: – see comment 5-46 |
| 5-84 | A | 4:34 | 4:34 | remove extra comma [Melissa Bowen (Reviewer's comment ID #: 28-3)] | Accepted |
| 5-85 | A | 4:34 | 4:34 | suppress " ,," [Pascale DELECLUSE (Reviewer's comment ID #: 58-3)] | Accepted |
| 5-86 | A | 4:37 | 4:38 | I would suggest that a statement that semi-enclosed seas are not included in this assessment is needed. [Michael Tsimplis (Reviewer's comment ID #: 268-2)] | Noted. We have added a remark in the introduction explaining that the chapter deals mainly with the global ocean, however a few regional changes are |

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| | | | | | discussed because they are of particular interest. |
| 5-87 | A | 5:0 | | I am told that Greg Johnson (PMEL) et al. have computed a new heat storage curve for 0-700m that actually shows a decrease since 2003, cónztrary to your fig. 5.2.1. That decrease still exists if the Argo profiles are all taken out, so it does not appear due to sampling/method change. A puzzling observation. Its distribution in the world ocean apparently does not offer any obvious clues on what its cause might be. [Friedrich Schott (Reviewer’s comment ID #: 228-2)] | Noted- Recent work by Levitus et al. and Willis as well as Johnson all confirm a decrease in ocean heat content since 2003, we have extended the figure 5.1 to cover the recent years |
| 5-88 | A | 5:7 | :28 | In sentence 5 it is stated that results for this section are based on WOD2001, yet in sentence 28 results to 2003 are given. This implies another database was used and this should be explained. [Govt. of United States of America (Reviewer’s comment ID #: 2023-318)] | Accepted- As noted by Levitus et al. 2005a) additional data were added to WOD01 to extend the time series to 2003. Text will be modified. |
| 5-89 | A | 5:9 | 5:12 | I wonder why all instruments for measuring temperature are mentioned here. I suggest to skip this sentence and - like it is done for salinity - refer only to the appropriate references. For temperature those are already given in the previous sentence. [Monika Rhein (Reviewer’s comment ID #: 212-2)] | Rejected- Ocean heat content and the thermosteric component of sea level change are of primary importance to this chapter and so we include the various instrument types used to measure temperature. |
| 5-90 | A | 5:15 | 7:38 | The authors have overlooked reference to Hansen et al. (2005) (Earth's energy imbalance: confirmation and implications, Science, 308, 1431-1435). In their Fig. 2, Hansen et al. show a close match between the measured and calculated heat uptake by the ocean for the past decade, based on coupled climate model simulations using the observed changes is radiative forcing of the climate system. Their calculated 0.85 W/m2 global energy imbalance at TOA is the rate at which the oceans are warming at the present time. This paper is an important closure analysis which shows that the measured change in ocean heat content is in complete agreement with climate model simulations of decadal climate change forced with observed radiative forcing changes. After all, it is the atmosphere that is supplying the ocean heat input, and it is important to know how the rate of heat input to the ocean has been changing in the past, and whether this rate of heat input is consistent with the observed radiative forcing changes. The 0.85 W/m2 is also the unrealized radiative forcing still in the pipeline. [Andrew Lacis (Reviewer’s comment ID #: 138-7)] | Work (unpublished by Levitus et al. Willis, and Ishii) documents that ocean heat content has decreased substantially since 2003. Thus, closure has not been documented for the past 50 years. |
| 5-91 | A | 5:17 | 5:39 | I would find it more satisfying if the principal differences between the three gloabla analysis was revealed in the body of the text and not just in the Appendix. The important point to be made is that while Levitus (2005a) only uses the insitu sub surface ocean temperature profiles, Ishii(2006) also uses SST observations times mixed layer depth climatology to estimate the heat content of the mixed layer portion of the profiles. | Rejected- We feel this material is more appropriate in the Appendix. The World Ocean Database 2005 now includes the WOCE southern |

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| | | | | Willis(2004) adds southern hemisphere profiles from the WOCE profilers, adding data from a chronically undersampled region. [R Allyn Clarke (Reviewer's comment ID #: 43-1)] | hemisphere profiler data and the Levitus updated heat content analyses for this Assessment will be based on WOD05. We shall note the inclusion of SST data by Ishii surface layer. |
| 5-92 | A | 5:18 | 5:18 | The text introduces "time series of ocean heat content", but in fact Figure 5.2.1 appears to show annual anomalies (relative to some long-term mean). Although the Levitus et al. (2005) paper describes the quantity likewise, I prefer a more specific terminology such as "time series of annual anomaly in ocean heat content" [Robert Marsh (Reviewer's comment ID #: 163-2)] | Noted. We have now explicitly stated that the heat content is an anomaly (deviation from average over a particular period). |
| 5-93 | A | 5:21 | 5:35 | There is some redundancy in this paragraph; "...there is good agreement", "...show very good agreement...", "consistent with each other...". [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-10)] | Rejected- We believe this agreement is important and we choose to emphasize it. |
| 5-94 | A | 5:22 | 5:22 | Replace "an overall trend of increasing" with "a periodic variability in " [VINCENT GRAY (Reviewer's comment ID #: 88-577)] | Rejected- The Reviewer gives no reason for making this change. |
| 5-95 | A | 5:24 | 5:24 | Delete "on this trend" You cannot draw a "trend" through such irregular data [VINCENT GRAY (Reviewer's comment ID #: 88-578)] | Rejected- We disagree with the reviewer that the data are so irregular that a trend can not be computed. |
| 5-96 | A | 5:25 | 5:25 | The rms difference between the three time series of heat content is expressed in both Joules and as a heat flux. Expression as a heat flux is superfluous and confusing. How is it computed and what does it mean in context of the long-term warming? Dividing by seconds per year and world ocean area, I get a different number (1.35 W per sq. m). I recommend that the expression of rms difference as a heat flux is better explained or removed. [Robert Marsh (Reviewer's comment ID #: 163-3)] | Rejected- Expressing the r.m.s. both in joules and in W/square meter is done because there is interest in the scientific community in seeing results in both sets of units. |
| 5-97 | A | 5:28 | 5:28 | Insert after "use". "From 1955 to 1970 there was no change. From 1970 to 1980 there was a rise of 10x10 to the 22 Joules. From 1980 to 1993 there was a fall of 5x10 to the 22 Joules". [VINCENT GRAY (Reviewer's comment ID #: 88-579)] | Rejected- We believe the text is clear as written. |
| 5-98 | A | 5:28 | 5:30 | Are the reported trends, the rate of heat content change? If so, the units should include time (-1). [Franklin SCHWING (Reviewer's comment ID #: 230-8)] | Rejected- Time is included in the unit of watts (joules per second). |
| 5-99 | A | 5:28 | :30 | Are the reported trends, the rate of heat content change? If so, the units should include time (-1). [Govt. of United States of America (Reviewer's comment ID #: 2023-319)] | Rejected- Time is included in the unit of watts (joules per second). |
| 5-100 | A | 5:34 | 5:35 | There clearly remains a range of views on this question in the community. This is seen by | Noted- It is the responsibility of Ch 5 to |

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| | | | | contrasting this sentence with the para in Ch 9 (p 9-40, ll 39-47). I think the different views are now adequately represented across the report as a whole, though I think it would be preferable to acknowledge the range of opinion explicitly in both chapters, e.g. by some text like: "While some studies have noted the potential importance of choice of infilling method in poorly sampled regions (Gregory et al. 2004, Achuta Rao et al. 2005), the consistency of the Levitus et al, Ishii et al and Willis et al. analyses adds confidence to their use in climate change studies". I know there has been much discussion of how to present this, and if what we have is the best that can be achieved, I think it is OK. [Comment also sent to Ch 9] [Richard Wood (Reviewer's comment ID #: 294-4)] | discuss ocean observations and the responsibility of Ch 9 to review all chapters and summarize the evidence for climate change. |
| 5-101 | A | 5:34 | | The reader is referred to the Appendix to get a sense of the time-dependent biases that may affect the changes and variations of the ocean heat content data sets. However in the Appendix no assessment of these biases is provided. Rather the reader is told that the similarity between the large less well-calibrated data used to derive ocean heat content changes and the specific research voyage data gives credibility to the ocean heat content data sets. Yet the reference quality data sets in Section 5.3 focus on circulation and water masses, not heat content. It would seem appropriate to discuss any general assessment of time-dependent biases, or comparison of independent observing systems, as has been done with other data sets, e.g., tropospheric temperature, precipitation. Given that the community has not been able to do this yet, the authors should be more explicit about the lack of our ability to ascribe errors in the trends due to time dependent biases due to changes on observing methods (as opposed to analysis methods). [Govt. of United States of America (Reviewer's comment ID #: 2023-320)] | Noted. We have attempted to use all available data. While the results do have some uncertainty, we are confident of the overall trends. Section 5.3 focusses (among other things) on temperature changes which are the basis for heat content estimates. The data distribution in the observing is discussed in the appendix. |
| 5-102 | A | 5:35 | | can we really 'note' this if it is the first time it is being presented to us. I'd suggest to drop the "note that" phrase. [David Rind (Reviewer's comment ID #: 214-43)] | Accepted |
| 5-103 | A | 5:35 | | Can we really 'note' this if it is the first time it is being presented to us? Drop the "note that" phrase. [Govt. of United States of America (Reviewer's comment ID #: 2023-321)] | Accepted |
| 5-104 | A | 5:38 | 5:39 | Replace "increasing trends" with "periodic behaviour" [VINCENT GRAY (Reviewer's comment ID #: 88-580)] | Rejected- The Reviewer gives no reason for making this change. |
| 5-105 | A | 5:43 | 5:44 | Is this statement true for the Ishii (2006) analysis. They do use SST data. The statement does apply to the Levitus analysis so that the following statements comparing the SST trends with the Levitus (2005a) surface trends is correct. [R Allyn Clarke (Reviewer's comment ID #: 43-2)] | Accepted- We will clarify these statement because Ishii does use SST data. |
| 5-106 | A | 5:43 | 5:49 | It is noted in this paragraph that the ocean heat-content time series shown in FIGURE 5.2.1 is consistent with SST estimates. The consistency in fact appears to go farther than | Noted. Atmospheric observations and SST are discussed in chapter 3, and not |

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| | | | | <p>this. Atmospheric humidity estimates over the tropical ocean from ERA-40 (Uppala et al., 2005) show good correlation with SST over the period for which satellite observations are available, and in particular show low values in 1985/86, consistent with a minimum then in the ocean heat content estimates. The minimum at this time is seen also in the global-mean water vapour content in the ERA-40 analyses, and also in the global mean mass of the (moist) atmosphere as captured in the ERA-40 surface pressure analyses (Trenberth and Smith, 2005). There is less consistency, perhaps not surprisingly, as regards smaller amplitude fluctuations, but the features of a decline in values from the early to mid 1980s, and a subsequent rise, are common to the various estimates of ocean heat content and atmospheric humidity.</p> <p>[Adrian Simmons (Reviewer's comment ID #: 242-88)]</p> | task for this chapter. |
| 5-107 | A | 5:43 | :49 | <p>This paragraph cites a correlation as evidence for validity of the subsurface ocean data, but surely there must be some lag in the transport of heat, and this is never discussed or shown. It is of most interest to show the low frequency relationship, not the high frequency as related to the trends discussed in Chapter 5</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-322)]</p> | Taken into account: We will change the text to make clear that SST and temperature data immediately beneath the ocean surface are correlated, not the deep temperatures (where there would be a lag as the reviewer points out). |
| 5-108 | A | 5:43 | :44 | <p>Why aren't SST data used in heat content estimates? An explanation is needed.</p> <p>[Govt. of United States of America (Reviewer's comment ID #: 2023-323)]</p> | Noted- Ishii does use the product of a synoptic measurement of SST and the climatological mixed layer depth in his analysis. This does not seem to add much to the estimates of heat content because of the similarity between the Ishii and Levitus et al. estimates. In addition it is not clear that this is a suitable procedure in all ocean regions. |
| 5-109 | A | 5:46 | 5:46 | <p>Provide time period over which this correlation was calculated.</p> <p>[Govt. of Australia (Reviewer's comment ID #: 2001-265)]</p> | Accepted |
| 5-110 | A | 5:47 | 5:47 | <p>I think this is the total number of ICOADS obs since c. 1850. What is the number over the period of the main calculations in this chapter?</p> <p>[Chris Folland (Reviewer's comment ID #: 71-162)]</p> | Taken into account: text revised. The number is 134 million |
| 5-111 | A | 5:54 | 5:55 | <p>Is it possible to say whether the problem with the post 1998 deep data availability is because the stations were in fact not occupied in the post WOCE period or that the data collected is delayed in moving through the Data system.</p> <p>[R Allyn Clarke (Reviewer's comment ID #: 43-3)]</p> | Noted- It is not possible to state this. |
| 5-112 | A | 5:54 | | <p>should be "there ARE not..."</p> <p>[Danny Harvey (Reviewer's comment ID #: 101-29)]</p> | Accepted |

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| 5-113 | A | 6:6 | 6:6 | Replace "the linear" by "a pseudo-linear". There is no evidence that the change is linear. [VINCENT GRAY (Reviewer's comment ID #: 88-581)] | Rejected- We believe there is a linear trend and have fit a linear trend to the data. |
| 5-114 | A | 6:6 | :7 | There are trends in Gulf Stream position as shown by Joyce et al. and Molinari which cause large changes in heat content in the region of the separated boundary current. The trends in North Atlantic heat content could be related to these shifts and this should be indicated in the text. [Govt. of United States of America (Reviewer's comment ID #: 2023-324)] | Noted. While it is possible that there is a connection between changes in Gulf Stream and the Atlantic heat content, these have not been investigated in detail in the literature. Note that the basin integral of N. Atlantic heat content is fairly represented by a linear trend, and there is no evidence that the Gulf stream position exhibits a linear trend over the same time eperiod. |
| 5-115 | A | 6:8 | 6:8 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-582)] | Rejected- We believe there is a linear trend and have fit a linear trend to the data. |
| 5-116 | A | 6:9 | 6:9 | "The warming trend of Indian Ocean SST is significant after 1950s, and there is abrupt warming in 1976, 1986 and 1996 respectively (Yang and Ding, 2006)." Should be add after the word of "belt", for it shows the latest study result on this topic. [Govt. of China (Reviewer's comment ID #: 2006-48)] | Noted. SST observations are discussed in chapter 3, therefore there are no references to publications referring to SST changes in our chapter. |
| 5-117 | A | 6:9 | 6:9 | Insert after "warmed" "since 1955" [VINCENT GRAY (Reviewer's comment ID #: 88-583)] | Accepted. |
| 5-118 | A | 6:11 | 6:11 | There look to be some regions in Fig. 5.2.2. where heat content has decreased slightly in the Southern Ocean - south of the South Atlantic and also New Zealand. [Chris Folland (Reviewer's comment ID #: 71-163)] | Accepted- Text will be modified. |
| 5-119 | A | 6:12 | 6:13 | Can you back out how much of the Pacific Ocean and perhaps the World Ocean trends can be accounted for by ENSO and the PDO? The heating / cooling patterns across the tropical Pacific superficially look like an ENSO response. Does this pattern result in a net heating or cooling in the Pacific or in the global ocean? Figure 5.2.3 suggests net cooling. [R Allyn Clarke (Reviewer's comment ID #: 43-4)] | Noted- The Reviewer should consult the references cited. |
| 5-120 | A | 6:12 | 6:13 | This sentence does not read well. [Chris Folland (Reviewer's comment ID #: 71-164)] | Accepted- Text will be revised |
| 5-121 | A | 6:12 | 6:12 | has the abbreviation "PDO" been defined at this stage in the text? It is later in lines 46-47 [Govt. of United Kingdom (Reviewer's comment ID #: 2022-20)] | Accepted- PDO will be given in full. |
| 5-122 | A | 6:15 | | Fig. 5.2.2. The origin of this figure is Levitus et al. (2005a) according to the Legend. However I could not find this figure in the original paper, nor in the axiliary material | Noted-This figure is based on updates from the results presented by Levitus et |

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| | | | | available via Internet. The reference seems to be erroneous. [Hendrik M. van Aken (Reviewer's comment ID #: 273-1)] | al. (2005a) and as such is acceptable in the IPCC Assessment. |
| 5-123 | A | 6:17 | 6:17 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-584)] | Rejected- The Reviewer gives no reason for making this change. |
| 5-124 | A | 6:25 | 6:25 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-585)] | Rejected- The Reviewer gives no reason for making this change. |
| 5-125 | A | 6:27 | 6:28 | Give a reference to the East Atlantic Oscillation links. CH3 should discuss the East Atlantic Oscillation as an atmospheric phenomenon, but I think it does not. Please raise this with CH3 and then cross refer to CH3 for more details . [Chris Folland (Reviewer's comment ID #: 71-165)] | Noted. Chapter 3 has decided not to discuss the East Atlantic Oscillation. We continue to use the term "East Atlantic Pattern" because there are important publications relating this pattern with ocean changes. |
| 5-126 | A | 6:30 | | ... evaporative regions increasing SALINITY in almost all ocean basins. ... [Detlef Stammer (Reviewer's comment ID #: 251-2)] | Accepted: |
| 5-127 | A | 6:33 | | Fig. 5.2.3 These figures cannot be found in the original reference (Levitus et al., 2005a) nor in the auxiliary material. Only panel c looks like figure 2 in Levitus et al., but differs in detail. Apparently the reference is erroneous, or the authors have used unpublished material. [Hendrik M. van Aken (Reviewer's comment ID #: 273-2)] | Noted-This figure is based on updates of the results presented by Levitus et al. (2005a). Caption has been changed accordingly. |
| 5-128 | A | 6:36 | 6:42 | If Figure 5.5.1 is to be referenced here then it needs to be brought forward to this section; also, as written, it is unclear how this description of sea-level variations links to "heat content"; PDO needs to be spelt out & this last sentence does not seem to follow. [Govt. of Australia (Reviewer's comment ID #: 2001-266)] | Accepted- The text is being moved. |
| 5-129 | A | 6:36 | 6:42 | This paragraph seems out of place here. The link between sea level variation and heat content changes could be better demonstrated. I tried to look at Wong et al (2006) but it is not yet available on the J. Clim site. [R Allyn Clarke (Reviewer's comment ID #: 43-5)] | Accepted- The text is being moved. |
| 5-130 | A | 6:37 | 6:37 | Figure 5.5.1 is discussed out of order. It should be brought forward in the figure sequence. [Melissa Bowen (Reviewer's comment ID #: 28-4)] | Accepted- The text is being moved. |
| 5-131 | A | 6:37 | 6:42 | Firstly, the correct figure reference should be 5.5.2, since the signal discussed is not visible from the figure presently referenced. Secondly, it is not clear that changes in heat content should be related to changes in surface height. At least this has not been established in this chapter. Perhaps this would be appropriate: else jumping back and forth between sea level and heat content/storage makes little sense. Later in this chapter one sees that other factors (freshwater content) can affect sea level, so if this material is to stay, a stronger link between the ENSO signals in heat and sea level is needed. | Accepted- The text is being corrected and moved to the sea level section. |

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| | | | | [Terrence Joyce (Reviewer's comment ID #: 122-1)] | |
| 5-132 | A | 6:37 | | In this version, it is Figure 5.5.2 instead of Figure 5.5.1 [Gerrit Burgers (Reviewer's comment ID #: 34-1)] | Accepted. |
| 5-133 | A | 6:38 | 6:40 | The sentence on ocean heat storage and net radiation is strangely worded, but can be fixed as follows. change "ocean heat storage (Wong et al., 2006)." to "ocean heat storage. This has been confirmed by satellite radiation budget data (Wong et al., 2006)." [Bruce Wielicki (Reviewer's comment ID #: 287-5)] | Accepted- sentence has been deleted, text rewritten |
| 5-134 | A | 6:39 | 6:40 | Cross referal to the radiative forcing chapter may be needed here. Do you mean the changes in TOA are consistent with changes of ocean heat storage? Over what period? [Chris Folland (Reviewer's comment ID #: 71-166)] | Accepted- sentence has been deleted, text rewritten |
| 5-135 | A | 6:44 | 6:47 | Seems to be a partial repeat of last sentence of previous paragraph. [Govt. of Australia (Reviewer's comment ID #: 2001-267)] | Accepted- The text in this section is an attempt to further explain the large interdecadal variability observed in Fig 5.2.1 which has been the source of some controversy. We will modify the text in this section. |
| 5-136 | A | 6:44 | 6:44 | Add "global" in front of heat content. [Chris Folland (Reviewer's comment ID #: 71-167)] | Accepted. |
| 5-137 | A | 6:49 | 6:54 | For completeness, it would be instructive to compare Fig 5.2.4 with what SST showed. [Chris Folland (Reviewer's comment ID #: 71-168)] | Noted- This will be done in a manuscript to be submitted for publication. At this point in time including this figure in Ch 5 would result in too many other changes having to be made to the chapter. For space reasons, figure 5.2.4 has been omitted in the final version. |
| 5-138 | A | 6:49 | 6:53 | here the ocean heat content variability before 1981 is dicussed. This should have been part of the TAR. The fact, that warming and cooling occurs basinwide can also be seen in Fig. 5.2.2I suggest to skip this and skip figure 5.2.4. or enhance the discussion by incorporating newer data. I found no reference to figure 5.2.4. in the following text [Monika Rhein (Reviewer's comment ID #: 212-3)] | Noted. A considerable amount of pre-1981 data has been added to the database used to compute the ocean heat content fields so it important to review the entire heat content time series. We have removed Fig. 5.2.4 but kept the discussion. |
| 5-139 | A | 6:49 | 6:50 | why present the later time period first in each case, if the text says 'first warmed then cooled'? [David Rind (Reviewer's comment ID #: 214-44)] | Noted- We are removing Fig. 5.2.4 and will modify the text. |

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| 5-140 | A | 6:49 | 7:7 | Please explain to me and the reader why unforced changes in heat content can be explained at all? In one sense, internal variability should just redistribute heat content, not change it. So where is the heat going/coming from that is represented by changes shown in Fig. 5.2.4 and discussed here? Otherwise the links between changes in heat content and external forcing, central to IPCC are less compelling. Since, as is mentioned on line 17 of the same page, the ocean accounts for 90% of the observed heat gain, it is unlikely the atmosphere or solid earth is playing a role in heat content conservation during ENSO. So what is happening? [Terrence Joyce (Reviewer's comment ID #: 122-2)] | Accept – we now discuss that the causes of variability are not well understood |
| 5-141 | A | 6:49 | :50 | Why present the later time period first in each case, if the text says 'first warmed then cooled'? [Govt. of United States of America (Reviewer's comment ID #: 2023-325)] | Noted- We are removing Fig. 5.2.4 and will modify the text. |
| 5-142 | A | 6:53 | 6:54 | Not quite sure why this reference to changes in thermosteric anomaly are in this section on Heat content changes? In this paper, this is a quantity computed from the T & S climatologies that were used for the heat content estimates. The earlier part of this paragraph are discussing changes from one pentad to another; these lines refer to changes over 3 years but does not indicate if these were global changes or associated with a particular ocean basin. [R Allyn Clarke (Reviewer's comment ID #: 43-6)] | Accepted- Part of the text is being corrected and moved to the sea level section. Noted-Thermosteric anomaly is computed by subtracting a synoptic observation from a climatological mean value. We are not sure what the author's point is. |
| 5-143 | A | 6:56 | 7:1 | Figs. 5.2.4a and 5.2.4b. These figures cannot be found in the original reference (Levitus et al., 2005a) nor in the auxiliary material. Apparently the reference is erroneous, or the authors have used unpublished material. [Hendrik M. van Aken (Reviewer's comment ID #: 273-3)] | Noted-These figures are based on the results presented by Levitus et al. (2005a) and as such are acceptable in the IPCC Assessment. However, for other reasons these figures are being deleted. |
| 5-144 | A | 7:9 | 7:35 | Table 5.2.1 indicates the increase in the oceans' energy content is not linear. From 1961-2003, the increase was 0.33 J/yr, whereas from 1993-2003, the increase was 0.76 J/yr. It seems you might want to state explicitly that the oceans' energy content change (increase) was more than twice as great in the 1993-2003 period than in the 1961-2003 period. [Melinda Marquis (Reviewer's comment ID #: 162-12)] | Noted- The table is being deleted but we believe it is clear that such variability exists. |
| 5-145 | A | 7:10 | 7:11 | Why have both Table 5.2.1 and Figure 5.2.5? They give essentially the same message. The figure is nice for powerpoint presentations, however, the table gives more information. I would include one or the other but not both. [R Allyn Clarke (Reviewer's comment ID #: 43-7)] | Accepted-Table 5.2.1 is being deleted. |
| 5-146 | A | 7:10 | | under SPM: Axes do not show consistent lables. Se -> Sea . Shown quantity and their | Probably fig. 5.2.5 meant? Caption and |

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| | | | | units should be more cloerly stated. Missleading: 1961-90 --> 1961-1990 would be better. [Walter Zenk (Reviewer's comment ID #: 301-2)] | figure have been modified. |
| 5-642 | B | 7:13 | 7:13 | "heat of fusion" What is this? Not defined. [Nicolas Gruber (Reviewer's comment ID #: 307-8)] | Rejected- This is a standard term of physics and its meaning is essentially defined in the text. |
| 5-147 | A | 7:18 | 7:20 | Is this an assertion or is it supported by recent literature on climate change detection and attribution. Don't statements like this belong in the chapter on detection and attribution, not here. [R Allyn Clarke (Reviewer's comment ID #: 43-8)] | Rejected- This is not an assertion and is supported by the data in Table 5.2.1 and Fig.5.2.5. W believe this comparison is so important that it should be included in this chapter. |
| 5-148 | A | 7:20 | 7:22 | This seems a narrow view of what is truly important to the climate system. Sea ice and ice caps are involved in the earth's energy budget in other ways than purely changes in heat storage, eg changes in surface fluxes and changes in land topography. This statement doesn't seem to re4ally belong in this chapter. [R Allyn Clarke (Reviewer's comment ID #: 43-9)] | Noted- The text is a statement of fact. We are not denying the importance of the cryosphere in the earth's energy budget. |
| 5-149 | A | 7:34 | 7:35 | Table 5.2.1. The top line is better listed as"upper" ocean heat content. [Chris Folland (Reviewer's comment ID #: 71-169)] | Noted- This Table is being removed. |
| 5-150 | A | 7:34 | 7:34 | right-hand-column should be W m-2 [Philip Woodworth (Reviewer's comment ID #: 295-2)] | Noted- This Table is being removed. |
| 5-643 | B | 7:34 | 7:34 | Table 5.2.1: The uncertainty in the energy change over the past decade look wrong (last column) CHECK [Nicolas Gruber (Reviewer's comment ID #: 307-7)] | Accepted: the errors are too small but correct in the corresponding figure. |
| 5-151 | A | 7:34 | | table - J increase from 93-03 do not think this is a mystery, although you don't imply this. Crowley et al GRL 2003 used an ebm to calculate about a 0.1 X !)**22J / yr increase in ocean heat content after the Pinatubo perturbation subsided. This would be approximately consistent with observations and isa noise free prediction because there is no noise in the ebm. ref is doi:10.1029/2003GL017801, 203, para. 14 [Thomas Crowley (Reviewer's comment ID #: 51-1)] | We do not understand the reviewer's comment. This chapter is based on observations. |
| 5-152 | A | 7:36 | 7:36 | an intensification of the earth's hydrological cycle" instead of "an increase in the earth's hydrological cycle [Roxana Bojariu (Reviewer's comment ID #: 24-19)] | Probably a comment on p. 36:7 Accept |
| 5-153 | A | 8:0 | 18: | Some of section 5.3 belongs in section 5.2 [Govt. of Australia (Reviewer's comment ID #: 2001-268)] | accepted – we have now all global discussion in 5.2 and the regional discussion in 5.3 |
| 5-154 | A | 8:7 | 8:7 | Probably should say 'weak' rather than 'weaker' since there is no indication of which other | Accepted- We will modify the text but |

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| | | | | freshening you are comparing this to. [R Allyn Clarke (Reviewer's comment ID #: 43-10)] | perhaps in a slightly different way. |
| 5-155 | A | 8:15 | 8:26 | Is the analysis and interpretation of this section supported by published literature or are these conclusions being made in this assessment. If the latter, then a figure showing the distribution of E-P over the global ocean would be a useful and necessary support to the argument. [R Allyn Clarke (Reviewer's comment ID #: 43-11)] | Noted- We agree it would be a valuable adjunct to include a figure of E-P but we do not have this information. Surface fluxes considered in chapter 3 to which we now refer . |
| 5-156 | A | 8:16 | 8:16 | add "in salinity" after "increasing". [Chris Folland (Reviewer's comment ID #: 71-170)] | Accepted. |
| 5-157 | A | 8:16 | 8:16 | Insert "salinity of" after "with the." [Melinda Marquis (Reviewer's comment ID #: 162-13)] | Accepted. |
| 5-158 | A | 8:17 | 8:17 | replace "consistent" by "which is consistent" or "in consistency " [Pascale DELECLUSE (Reviewer's comment ID #: 58-4)] | Accepted |
| 5-159 | A | 8:18 | | typo on addition [Thomas Crowley (Reviewer's comment ID #: 51-2)] | Accepted |
| 5-160 | A | 8:18 | | It is important to note that changes in ocean fresh water transport may also be a significant contributor. I believe there are a few papers 'in the works' which suggest that this is an important term. [Richard Wood (Reviewer's comment ID #: 294-5)] | Accepted. |
| 5-161 | A | 8:19 | 8:20 | The text refers to 'much of the water column' and the 'whole water column'; however, data has only been presented for the upper 500 metres. Does the 'whole water column' mean the whole upper 500 metres are there analysis extending over the whole water column or at least the upper 3000 m. If so, this work should be explicitly referenced. [R Allyn Clarke (Reviewer's comment ID #: 43-12)] | Accepted- We now add the reference to the original work that shows the changes at depths greater than 500 m. |
| 5-162 | A | 8:20 | 8:20 | The phrase "the salinity over" can be deleted. [Chris Folland (Reviewer's comment ID #: 71-171)] | Accepted. |
| 5-163 | A | 8:22 | 8:22 | "freshwater" misspelled [Melissa Bowen (Reviewer's comment ID #: 28-6)] | Accepted. |
| 5-164 | A | 8:24 | 8:24 | suppress "with with" [Pascale DELECLUSE (Reviewer's comment ID #: 58-5)] | Accepted. |
| 5-165 | A | 8:24 | 8:24 | typo error: "with with a larger" --> "with a larger" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-11)] | Accepted. |
| 5-166 | A | 8:31 | 8:32 | "at a rate of 0.5 and 1 Wm ⁻² from 1993-2003" - is this the overall rate over this time period or a rate per year? Needs to be made clear. [Govt. of Australia (Reviewer's comment ID #: 2001-269)] | This will be clarified, also using additional reference to Gulev et al. (2006, J. Climate, in press) |
| 5-167 | A | 8:31 | 8:31 | better is "the ocean surface has been heated..." | accept |

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| | | | | [Chris Folland (Reviewer's comment ID #: 71-172)] | |
| 5-168 | A | 8:31 | 8:33 | can one really have high levels of confidence for a value that can't be directly observed and is the net result of a sum of other positive and negative numbers? [David Rind (Reviewer's comment ID #: 214-45)] | accept, reformulated |
| 5-644 | B | 8:31 | 8:31 | "between 0.5 and 1 W m ⁻² ": This statement is inconsistent with the data presented in table 5.2.1, which suggests a much smaller uncertainty in the net anomalous heat flux into the ocean [Nicolas Gruber (Reviewer's comment ID #: 307-9)] | accept, reformulated |
| 5-169 | A | 8:31 | :33 | Can one really have high levels of confidence for a value that can't be directly observed and is the net result of a sum of other positive and negative numbers? [Govt. of United States of America (Reviewer's comment ID #: 2023-326)] | See comment 5-168 |
| 5-170 | A | 8:32 | 8:32 | I would replace 'from the ocean's heat budget' with 'from changes in the ocean's heat content'. To me, reference to ocean heat budgets implies the classical attempts to estimate the net air-sea flux in order to determine the change in heat storage and heat transport through ocean currents. [R Allyn Clarke (Reviewer's comment ID #: 43-13)] | noted, text reformulated |
| 5-171 | A | 8:38 | | I do not think that it is right to say that "significant interannual to decadal variability in the Atlantic MOC has been reported in models". My reading of model results is that they typically show not more than +/-2Sv at these periods (e.g. Eden and Willebrand (2001), to name just one of those papers, from one of your main authors even). Furthermore, the new result of Stammer and Koehl (2006) with the ECCO assimilation also shows this moderate range of variability in the 10-20% domain, rather than your statement. [Friedrich Schott (Reviewer's comment ID #: 228-3)] | Noted. This depends entirely on the precise meaning of "significant". We have used the word roughly in the sense "clearly different from zero", i.e. not "important" or "large". However, at this place, the para has been shortened and reference to the variability has been deleted. |
| 5-172 | A | 8:39 | 8:39 | Add Knight et al (2005) (already in the refs) after Stammer et al, 2003. [Chris Folland (Reviewer's comment ID #: 71-173)] | Noted. Text has been shortened, reference to the paper appears later. |
| 5-173 | A | 8:40 | | when you say that a 30% decrease of heat transport "has been reported" it sounds like this is a fact and your low-confidence disclaimer on 1.45 does not really compensate. [Friedrich Schott (Reviewer's comment ID #: 228-4)] | Accept, text has been shortened. |
| 5-174 | A | 8:41 | | however" serves as a coordinating conjunction here, and should be preceded by a semicolon and followed by a comma. Check all other uses of "however" [Danny Harvey (Reviewer's comment ID #: 101-30)] | Accept, text shortened. |
| 5-175 | A | 8:44 | 8:45 | Is there evidence for the statement about inverse models? If so I think a reference is needed, otherwise I think it should be deleted. [Richard Wood (Reviewer's comment ID #: 294-6)] | Accept, text changed |
| 5-176 | A | 8:45 | 8:46 | Do you mean that the differences between different times are comparable to the | Accept, text changed and shortened |

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| | | | | uncertainty in the individual observations? Or that because of undersampling in time you can't say much about the trends? The preceding sentences suggest you are mainly discussing the latter, in which case I suggest changing "changes" to "trends" in this last sentence, for improved clarity. [Richard Wood (Reviewer's comment ID #: 294-7)] | |
| 5-177 | A | 8:48 | 8:51 | Note that we are not interested by the climatological freshwater transport (which is a quantity difficult to estimate) but by its changes that we are far from correctly evaluating [Pascale DELECLUSE (Reviewer's comment ID #: 58-6)] | Accepted, text changed and shortened. |
| 5-178 | A | 8:50 | | can't really have less agreement when the previous number cited came from only one calculation. [David Rind (Reviewer's comment ID #: 214-46)] | accept, discussion of heat transport changes deleted |
| 5-179 | A | 8:50 | | Can't really have less agreement when the previous number cited came from only one calculation. [Govt. of United States of America (Reviewer's comment ID #: 2023-327)] | See comment 5-178. |
| 5-180 | A | 9:0 | | With regard to the Northern North Atlantic Ocean, this chapters leans heavily on the work, published by the WODC group around Sid Levitus. The subpolar gyre is cooling, and freshening Indeed they have access to their huge data base. These statements were also made in sections 5.2.2. and 5.2.1. However that is only the long-term trend (1955-2003). In the last 15 years that trend seems to be reversed. According to the Ocean Climate Status Summary (IAOCSS), published annually since 1988 by the ICES, the trends, mentioned in the IPCC draft report have been reversed since 1995. The summary of the 1998/999 IAOCSS summarized on the Atlantic Subpolar gyre: "In general 1998 was a year in which ocean temperatures around the North Atlantic were warmer than the long term average, and most areas show a warming trend". [Hendrik M. van Aken (Reviewer's comment ID #: 273-4)] | Accept. We thank the reviewer for bringing the ICES reports to our attention. We now are much more careful about the last 10 years in the subpolar region compared with the long-term trend. The reversal of trends can also be seen in the Yashayaev data to which we refer, and we do refer now to the ICES paper |
| 5-181 | A | 9:0 | | (continued) In the 2004/2005 report this had changed to "In almost all areas of the eastern and western North Atlantic during 2004, temperature and salinity in the upper layers remained higher than the long-term average, with new records set in numerous regions." These statements suggest that the long-term trends, published by the Levitus Group, do not tell the whole story. [Hendrik M. van Aken (Reviewer's comment ID #: 273-5)] | See above comment. |
| 5-182 | A | 9:0 | | The references on the North Atlantic Ocean seem to me a little one-sided. For the discussion of the climatic changes in the northern North Atlantic Ocean publications by the ICES are dearly missed. The IAOCSS reports are annually produced by specialists of the ICES Working group on Oceanic Hydrography. Electronic versions of the reports can be found on the WEB at: http://www.soc.soton.ac.uk/JRD/ICES_WGOH/iaocss.php . Since 2003 these are official ICES publication in the ICES Cooperative Research Report | Accept. We have added the ICES reference. |

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| | | | | Series. Additionally I can mention the overviews of decadal oceanic variability in the peer-reviewed publication series ICES Marine Science Symposia. I refer to volumes 185, 195, and 219, for an overview of the oceanic variability in the North Atlantic Ocean and adjacent seas since 1970. [Hendrik M. van Aken (Reviewer's comment ID #: 273-6)] | |
| 5-183 | A | 9:15 | 9:15 | Correct "Northern Annular Mode" (not Northern Annual Mode). [Robert Marsh (Reviewer's comment ID #: 163-4)] | Accepted, however text has been modified. |
| 5-184 | A | 9:15 | 9:15 | typo error: "Northern Annual Mode" --> "Northern Annular Mode" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-12)] | see above |
| 5-185 | A | 9:15 | 9:15 | Shouldn't "Northern Annual Mode" be "Northern Annular Mode"? [Neil White (Reviewer's comment ID #: 286-1)] | Accepted See comemnt 5-183 |
| 5-186 | A | 9:16 | | since the TAR, a new important mode has been discovered, the Indian Ocean Zonal Dipole Mode (IOZDM). Accordingly, I propose adding a sentence after (SAM):"A new mode of interannual ocean-climate variability has been discovered since the TAR, the Indian Ocean Zonal Dipole Mode (IOZDM)." [Friedrich Schott (Reviewer's comment ID #: 228-5)] | Noted.. However, the definition of "modes" is given in chapter 3, therefore we have not done this here. |
| 5-187 | A | 9:23 | | Cancel ", it provides...change." - irrelevant. In addition I miss "Insert figure 5.22" and other insert locations. The text appears somewhat inconsistent. [Walter Zenk (Reviewer's comment ID #: 301-4)] | Accepted, sentence has been deleted. Don't understand the figure comments. . |
| 5-188 | A | 9:29 | 17:21 | At the start of each of the regional assessments in this section it would be useful to indicate when useful observational records start. [Govt. of Australia (Reviewer's comment ID #: 2001-272)] | Rejected. We have been careful to cite the periods over which each variation is observed. Since we are not really able to look at long-term trends in most of the regional properties/currents, information comes from very specific experiments or sets of observations, unlike those of section 5.2. Therefore no general dates can be given. |
| 5-645 | B | 9:29 | 17:21 | I found ~7 pages of description of the various changes in the different ocean basins not particularly enlightening. The ensuing summary is excellent, in comparison. I therefore strongly suggest to shorten the descriptive part and strengthen the integration part. One may take different positions, but in my opinion a pure list of changes in every little part of the corner doesn't leave me as impressed than a good summary figure that puts all these changes together and demonstrates the consistency. I also find not all of the figures well motivated. For example, 5.3.2 doesn't look all that interesting. | Taken into account. We have not completely restructured the section, but we have given simple summary statements at the end of most subsections.. Figure 5.3.2 has been deleted. |

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| | | | | [Nicolas Gruber (Reviewer's comment ID #: 307-10)] | |
| 5-189 | A | 9:29 | | The statements in this section probably deal only with the North Atlantic Ocean North of the Azores. That is the region where the NAO reflects the dominant forcing of the ocean. In the equatorial region one has to reckon with the Atlantic Equatorial Dipole. And certainly the South Atlantic Ocean will hardly feel any effects of the NAO. [Hendrik M. van Aken (Reviewer's comment ID #: 273-7)] | Taken into account. More inclusive language now. Chapter 3 has rejected the equatorial dipole mode, so we cannot refer to it. |
| 5-190 | A | 9:30 | 9:30 | NAO is not a part of NAM, but another way of viewing it. I would replace "the NAO which is part of the Northern Annular Mode" with "the NAO/NAM". [Roxana Bojariu (Reviewer's comment ID #: 24-17)] | Noted. Chapter 3.6.4 discusses the fine points of differences between NAO and NAM, so it is not accepted that they are identical. Since most ocean publications use "NAO", we have kept this here. |
| 5-191 | A | 9:32 | 9:33 | It would be useful (given the following descriptions of oceanic changes linked with the NAO), to reiterate here what the NAO is and what high and low NAO values mean in terms of the atmospheric circulation. [Govt. of Australia (Reviewer's comment ID #: 2001-270)] | NAO is described in Chapter 3. We have referenced to that chapter and modified the text |
| 5-192 | A | 9:35 | 9:35 | Replace "linear trends" with "changes". They are not "linear" [VINCENT GRAY (Reviewer's comment ID #: 88-586)] | accept, used "long-term" trend instead |
| 5-193 | A | 9:37 | 9:38 | Why is this consistent with positive NAO? A reference (or cross-reference to another part of the report) would be useful here. [Richard Wood (Reviewer's comment ID #: 294-8)] | Accepted – Added Dickson et al. (1996) and Hatun et al. (2005) references |
| 5-194 | A | 9:41 | 9:42 | fig. 5.3.1 refer to the subpolar region and the reference to the figure should be put after ... in the subpolar region.. and not after .. in the subtropical region. The figure does show a trend towards freshening in the Labrador Sea as it is stated in the text, but only till 1995/96. Since then, the salinity increased in part of the Labrador Sea (Fig. 5.3.1.) This should be mentioned in the text. [Monika Rhein (Reviewer's comment ID #: 212-4)] | Accept, and include reference to Figure 5.2.6 also. The recent increase in salinity is now discussed. |
| 5-195 | A | 9:42 | 9:42 | should refer to Figure 5.2.6 not 5.3.1 [Melissa Bowen (Reviewer's comment ID #: 28-7)] | Accept |
| 5-196 | A | 9:42 | 9:42 | I am confused by this sentence. What are "global tendencies for fresher and saltier regions"? [Brian Soden (Reviewer's comment ID #: 245-6)] | Accept – wording clarified |
| 5-197 | A | 9:44 | 9:44 | An additional reference, either to a paper or another figure, is needed to support the statement that the N Atlantic changes in salinity are "deeper and more pronounced than in any other region". [Govt. of Australia (Reviewer's comment ID #: 2001-271)] | Accept. Changes in 5.3 structure have been made to place the Curry and Wong salinity figures at beginning of section. |

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| 5-198 | A | 9:44 | 9:44 | add "and are" before "deeper" [Melissa Bowen (Reviewer's comment ID #: 28-8)] | Accept |
| 5-199 | A | 9:47 | 9:47 | I am not sure what upper 200-300m refers to as Fig 5.2.2 refers to the upper 700m according to its caption. [Chris Folland (Reviewer's comment ID #: 71-174)] | Accept, text changed |
| 5-200 | A | 9:47 | | This referenced figure shows heat content, not temperature at 200-300m depth as discussed in the text. I think the correct figure reference should be changed to Fig. 5.2.3 [Terrence Joyce (Reviewer's comment ID #: 122-3)] | Accept |
| 5-201 | A | 9:52 | 9:52 | What is meant by " the level of a model hypothesis"? That there are no observations to support it, only model simulations? If so, perhaps one could say that the observations are "inadequate to confirm or refute model simulations of decadal variability". [Brian Soden (Reviewer's comment ID #: 245-7)] | Accept, text changed |
| 5-202 | A | 9:54 | 9:56 | I hope this quote about a South Atlantic dipole is correct. A generally better known SST dipole which should be added to the discussion is that between the tropical North and tropical South Atlantic. This varies quite strongly in a stochastic way on interannual time scales (e.g. Folland et al, 2001) and coherently on decadal time scales (Chang et al, 1997). It affects the position and convergence into the ITCZ and other aspects of climate. Folland, C.K., Colman, A., Rowell, D.P., and M.K. Davey, 2001: Predictability of North East Brazil rainfall and real-time forecast skill, 1987-1998 J. Climate, 14, 1937-1958; Chang, P., J. Link and L. Hong, 1997: A decadal climate variation in the tropical Atlantic Ocean from the thermodynamic air-sea interactions. Nature, 385, 516-518 [Chris Folland (Reviewer's comment ID #: 71-175)] | Noted. Chapter 3 has however decided explicitly to not mention this dipole mode except in quotes for its influence on African rainfall. We have agreed with chapter 3 not to introduce modes that they are not defining. |
| 5-203 | A | 10:2 | | Add "At decadal time scales an active role of the Subtropical Cells (STCs) in affecting equatorial upwelling and SST has been documented in model studies (Kroeger et al., 2005).Ref: Kröger J., A. J. Busalacchi, J. Ballabrera-Poy, P. Malanotte-Rizzoli (2005), Decadal variability of shallow cells and equatorial sea surface temperature in a numerical model of the Atlantic, J. Geophys. Res., 110, C12003, doi:10.1029/2004JC002703. 418 5-418 6 [Friedrich Schott (Reviewer's comment ID #: 228-175)] | Reject. This is a nice paper but purely modeling studies don't belong in this chapter. |
| 5-204 | A | 10:8 | 10:8 | "high (low) temperatures" - water temperatures? [Govt. of Australia (Reviewer's comment ID #: 2001-273)] | Accepted |
| 5-205 | A | 10:8 | | is there a specific reason why low volumes are associated with warmer water? Is it due to lack of vertical mixing? Some explanation should be stated, perhaps just a few words in the sentence. [David Rind (Reviewer's comment ID #: 214-47)] | Accepted. Text changed |
| 5-206 | A | 10:8 | | Is there a specific reason why low volumes are associated with warmer water? Is it due to lack of vertical mixing? Some explanation should be stated, perhaps just a few words in | Accepted. See – 5-204 |

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| | | | | the sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-328)] | |
| 5-207 | A | 10:9 | 10:11 | This sentence may need some rephrasing as the NAO has decreased in the last decade (CH3, Fig 3.6.6) [Chris Folland (Reviewer's comment ID #: 71-176)] | Taken into account. Sentence has been modified based on Kwon and Riser results that show primary dependence is on NAO. |
| 5-208 | A | 10:13 | 10:22 | I recommend further citation of recent papers on relationships between: the NAO and a transport index for the Gulf Stream / North Atlantic Current (Curry and McCartney 2001); the NAO and eddy kinetic energy associated with the same current system (Penduff et al. 2004). Extra suggested text at the end of the paragraph: "Analysis of long hydrographic time series in the central Labrador Sea and at Bermuda suggests that Gulf Stream and North Atlantic Current transport co-varies with the NAO index, reaching record levels in the mid 1990s (Curry and McCartney 2001). This relationship is supported by evidence from satellite altimetry and eddy-resolving model simulations for corresponding intensification of indicate that the eddy kinetic energy field correspondingly intensified (Penduff et al. 2004)". Reference 1: Curry, R. G., and M. S. McCartney (2001). Ocean gyre circulation changes associated with the North Atlantic Oscillation. J. Phys. Oceanogr. 31, 3374-3400. Reference 2: Penduff, T., Barnier, B., Dewar, W. K., and J. J. O'Brien (2004). Dynamical response of the oceanic eddy field to the North Atlantic Oscillation: A model-data comparison. J. Phys. Oceanogr., 34, 2615-2629. [Robert Marsh (Reviewer's comment ID #: 163-5)] | Noted. We have referred to the Penduff et al-paper and discussed the relation of Gulf Stream and NAO. |
| 5-209 | A | 10:13 | | I would like to include a paragraph as: In most areas of the North Atlantic in the last years (until 2004), temperature and salinity in the upper layers reminded higher than the long-term average, with new records set in several regions (ICES 2005). The northern North Atlantic and Nordic Seas continued (until 2005) to be much warmer and more saline than the long-term mean. [Alicia M. Lavín (Reviewer's comment ID #: 141-1)] | Noted. We have referred to the ICES paper. |
| 5-210 | A | 10:14 | 10:14 | correlated" might be a better word than "coordinated" [Melissa Bowen (Reviewer's comment ID #: 28-9)] | Reject. Correlated has a specific definition, and implies certain error bars. Not always calculated. |
| 5-211 | A | 10:17 | 10:20 | It would be helpful to spell out the direction of the changes by adding that the FC transport is positively correlated to the NAO and that a higher NAO results in a more northerly GS position. [Melissa Bowen (Reviewer's comment ID #: 28-10)] | Accept. |
| 5-212 | A | 10:19 | 10:19 | Correlated in which direction? [Chris Folland (Reviewer's comment ID #: 71-177)] | Accept, fixed, same as 5-211. |
| 5-213 | A | 10:19 | :22 | The NAO and Gulf Stream transport are strongly anti-correlated. Bryden et al. only found | Accept. Thank you. Corrected |

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| | | | | strengthening of the eastern limb of the gyre (i.e., the Florida Current transport has stayed relatively stable over the past 40 years), a weakness in their argument for gyre changes that should be noted. [Govt. of United States of America (Reviewer's comment ID #: 2023-329)] | reference to Baringer and Larsen results. Curry and McCartney show increased Gulf Stream transport with positive NAO. See comment 5-208. |
| 5-214 | A | 10:27 | | what are GSAs associated with (caused by)? [David Rind (Reviewer's comment ID #: 214-48)] | Reject. Already said in this paragraph. |
| 5-646 | B | 10:35 | 10:37 | The following sentence overstates the conclusions reached by myself and Bob Marsh in our recent paper: 'About two-thirds of the freshening is due to an increase in precipitation associated with a climate pattern known as the Eastern Atlantic Oscillation and the remainder due to GSAs (Josey and Marsh, 2005).' Specifically:a) We used the term 'can be explained by' rather than 'is due to' as it more accurately reflects the conclusions we were able to draw; b) We did not ascribe the remainder to GSAs. Furthermore, c) the climate pattern we described is generally referred to as the East Atlantic Pattern and has never, to our knowledge, been termed the Eastern Atlantic Oscillation. For accuracy the sentence should be replaced with the following text: 'About two-thirds of the freshening may be explained by an increase in precipitation associated with a mode of climate variability known as the East Atlantic Pattern (Josey and Marsh, 2005).' | Accept, more equivocal language, removal of GSA statement, and change East Atlantic Pattern. |
| 5-215 | A | 10:36 | 10:37 | Re-word text as "... the East Atlantic Pattern (Josey and Marsh 2005)." [Robert Marsh (Reviewer's comment ID #: 163-6)] | Accepted. |
| 5-216 | A | 10:37 | 10:37 | East (not Eastern) Atlantic Oscillation: Cross refer to CH3 as above. This section is written as if the ref on p6 above to the East Atlantic Oscillation had not already been made. [Chris Folland (Reviewer's comment ID #: 71-178)] | Accepted- Text will be modified. See also previous comment5 |
| 5-217 | A | 10:38 | 10:38 | "subpolar storage anomaly" - need to explain a bit more what this is. [Govt. of Australia (Reviewer's comment ID #: 2001-274)] | Accept. text changed |
| 5-218 | A | 10:42 | 10:49 | can one explain, perhaps by using MW changes from the next paragraph, why this reversal occurred? [David Rind (Reviewer's comment ID #: 214-49)] | Taken into account. Most likely due to change in Labrador Sea Water, not Med Water (based on input from contributing author Terry Joyce, but there are no publications at this point) |
| 5-219 | A | 10:43 | 12:24 | The construction of these two paragraphs "intermediate and deep circulation..." then "adjacent seas..." is not optimal and there are some redundancies in the described | Accept. (structure of this section has been changed) |

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| | | | | features. As it i said in the lines that "marked changes in NADW...reflect changes in source waters in the Nordic seas, Labrador sea and mediterranean sea", why not start with these seas, and then deduce the changes observed in NADW ? T [Pascale DELECLUSE (Reviewer's comment ID #: 58-7)] | |
| 5-220 | A | 10:54 | 11:1 | Intermediate waters in the mid latitude eastern North Atlantic keeps warming (Bryden et al, 1996) and during the last decade (1992-2002) the rates are more than 0.2°C/decade with 0.4°C/decade at some levels (Vargas-Yañez et al, 2005). In the southern Bay of Biscay (44° N, 4°W), similar warming rates were observed through the thermocline (0.32°C/decade) and into the core of the MW (0.2°C/decade) and also salinity increases in 0.05/decade from 1992 to 2003. (González-Pola et al., 2005). [Alicia M. Lavín (Reviewer's comment ID #: 141-3)] | Accept, text changed |
| 5-221 | A | 11:7 | 11:17 | Cross refer to the NAO behaviour in Fig 3.6.6 of CH3. Is this behaviour consistent with that expected from Fig 3.6.6? [Chris Folland (Reviewer's comment ID #: 71-179)] | Accept. Added NAO behavior to sentence, also since part of the cited references. |
| 5-222 | A | 11:9 | | which decades did this occur in? [David Rind (Reviewer's comment ID #: 214-50)] | Accept and corrected LSW convection onset year, added reference. |
| 5-223 | A | 11:9 | | A reference is being made to dense water sinking to the sea floor and thereby driving the MOC. I think there is hardly any water really sinking to the sea floor. It does sink to depths of 2000-3000 m in the North Atlantic (the main branch of the MOC), but it does not sink to the bottom. [Detlef Stammer (Reviewer's comment ID #: 251-3)] | Reject. Can't tell which sentence being referred to. In any case, the renewed overflow waters do reach the sea floor |
| 5-224 | A | 11:9 | | Which decades did this occur in? [Govt. of United States of America (Reviewer's comment ID #: 2023-330)] | Accept. See comment 5-222. |
| 5-225 | A | 11:11 | 11:16 | Here it is stated that the intensity of deep convection and the production of LSW decreased from 1994 to 2004 with a short interruption in 1999-2000. add Kieke et al., 2006 as a reference to this sentence. Kieke et al., 2006 quantified the LSW formation rates and could show that in 1998-1999 the formation a different mode of LSW than in 1988-1994 compensated for the lack of formation of classical LSW, and in 2000-2001 the formation rate decreased by 50%, . : Kieke, D., M. Rhein, L.Stramma, W.M. Smethie, D.A. Lebel, and W. Zenk,2006, Changes in the CFC inventories and formation rates of Upper Labrador Sea Water, 1997 - 2001. J.Phys.Oceanogr. 36, 64-86 [Monika Rhein (Reviewer's comment ID #: 212-5)] | Accept, reference Kieke 2006 added |
| 5-226 | A | 11:12 | | How can you say that the production of LSW decreased from 1994-2004 "with a brief interruption in 1999-2000", meaning that there was in fact enhanced convection in 1999-2000? Your own fig. 5.3.1 shows continued warming in 1999-2000 and so do our moored stations there. I know of no reference claiming this convection. | Taken into account. Convection was weak before and after that year according to Yashayaev and Clarke; convection was the same in the |

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| | | | | [Friedrich Schott (Reviewer's comment ID #: 228-7)] | previous years and 1999-2000 and then reduced, according to Kieke et al. (see comment 5-225). Paragraph reworded to be clearer |
| 5-227 | A | 11:20 | 11:29 | There is now additional evidence for long-term changes in overflow water transport in the region between the overflow sills and the subtropics (Kieke and Rhein 2006, submitted August 2003). I recommend an additional penultimate sentence inserted at line 26: "Further south, in the Irminger and Labrador Seas, analysis of historical hydrographic data suggests that the combined overflow transport doubled from the mid-1950s to the 1980s, then declined back to the 1950s level by the 1990s (Kieke and Rhein (2006)).". Reference: Kieke, D., and M. Rhein (2006). Variability of the overflow water transport in the western subpolar North Atlantic, 1950-97. J. Phys. Oceanogr., 36, 435-456. [Robert Marsh (Reviewer's comment ID #: 163-7)] | Reject. Difficulty to interpret baroclinic transport changes. |
| 5-228 | A | 11:23 | 11:24 | it is stated, that the transport of the overflow water masses is relatively stable and that there is no 'clear' variability in the overflow transport in the Denmark Strait. I suggest to skip the last part of the sentence and modify the first one. Macrander et al. (2005) showed that the interannual variability of the overflow transport is in the order of 30%. : Macrander, A., U. Send, H. Valdimarsson, S. Jonsson, and R.H. Kaese, 2005, Interannual changes in the overflow from the Nordic Seas into the Atlantic ocean through Denmark Strait. Geophys. Res. Lett. 32, L06606, doi: 10.1029/2004/GL021463 [Monika Rhein (Reviewer's comment ID #: 212-9)] | Accept. The Macrander reference is now included again, indicating 30% variability. |
| 5-229 | A | 11:25 | | The eastern overflow has in fact increased again since 2001, and B. Hansen is working on a paper (pers. comm. to me) taking everything back he said back then about "day after tomorrow" having already arrived (Science 2004). I suggest adding after "...of the total": Recently, the eastern overflows have returned to the earlier levels. [Friedrich Schott (Reviewer's comment ID #: 228-8)] | Accept. Wording changed |
| 5-230 | A | 11:28 | 11:29 | Curry and Mauritzen (2005) do not report MOC weakening, as implied by the text - in fact they apply hydraulic theory to vertical density profiles, and claim no change (yet) in overflow transports. The sentence should be revised for clarification: "... may offset or even prevent the weakening expected through multi-decadal surface freshening up to 1995 (Curry and Mauritzen 2005)." [Robert Marsh (Reviewer's comment ID #: 163-8)] | Accept |
| 5-231 | A | 11:31 | 11:35 | Earlier, these Bryden results were characterised as not being robust with respect to the changes in the meridional heat flux (page 8, lines 40-42). Are these changes in layer volume transport also not robust? [R Allyn Clarke (Reviewer's comment ID #: 43-14)] | Accept. Caveat language added, additional reference for softness of results added. |

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| 5-232 | A | 11:31 | 11:35 | ?If the previous comment is taken, a reader would find consistency with the statement that the southward transport of the deep portion of the NADW has decreased. [Motoyoshi Ikeda (Reviewer's comment ID #: 113-3)] | Taken into account. Previous statement has been changed, as measured overflow transports have now increased again. See previous comment 5-231 |
| 5-233 | A | 11:33 | | How can you state for a fact "Southward transport...has decreased from 15Sv to 7Sv" when you know it is just a hydrographic analysis with a subjectively selected reference level? There is absolutely NO other evidence supporting this! Such a statement does not belong into a serious scientific document [Friedrich Schott (Reviewer's comment ID #: 228-9)] | Accept. See 5-231 |
| 5-234 | A | 11:34 | | Change slowdown to decrease. [Govt. of United States of America (Reviewer's comment ID #: 2023-331)] | Accept. |
| 5-235 | A | 11:40 | 11:41 | Antarctic Bottom Water is usually not formed by deep reaching convection, but by processes on the shelf (ice formation and brine release) and under the ice shelf with subsequent mixing with ambient water in the circumpolar current [Monika Rhein (Reviewer's comment ID #: 212-6)] | Accept. Drastic text reduction had resulted in this misstatement. Thank you for finding it. There was also a misstatement introduced in this paragraph that implied that the source waters of AABW come only from the South Atlantic. This has also been fixed.. |
| 5-236 | A | 11:41 | 11:41 | chapter 5.3.5.3. which is mentioned here as a reference to the Weddell Sea reports transports from the Circumpolar Current and does not mention the Weddell Sea. Change to 5.3.5.2 [Monika Rhein (Reviewer's comment ID #: 212-7)] | Accept. |
| 5-237 | A | 11:42 | | To be consistent replace "year" by "yr". [Walter Zenk (Reviewer's comment ID #: 301-5)] | Accept |
| 5-238 | A | 11:43 | | freshening is misspelled. [Detlef Stammer (Reviewer's comment ID #: 251-4)] | Accept. |
| 5-239 | A | 11:46 | | repeats the comment from 11:20. [David Rind (Reviewer's comment ID #: 214-51)] | Accept. Sentences deleted. |
| 5-240 | A | 11:46 | | Repeats the statement from page 11, line 20. [Govt. of United States of America (Reviewer's comment ID #: 2023-332)] | Accept. See 5-240. |
| 5-241 | A | 11:55 | | Latif et al maintain .. is not a hansom wording. Maybe insist? Or suggest? | This is actually line 13:40. Wording has |

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| | | | | [Detlef Stammer (Reviewer's comment ID #: 251-5)] | been changed |
| 5-242 | A | 12:7 | 12:8 | These lines are a repeat of the text found in lines 2-4, in the previous paragraph. [R Allyn Clarke (Reviewer's comment ID #: 43-15)] | Accept. Deleted 12:2 to 4. |
| 5-243 | A | 12:7 | :14 | The apparent inconsistency between a decrease in Nordic Sea subsurface salinity and increase in inflow salinity from the Atlantic needs to be explained. [Govt. of United States of America (Reviewer's comment ID #: 2023-333)] | Accept. Wording changed in light of the comments above by Lavin and von Aken that drew our attention to the ICES reports and a different approach to incorporating Hatun et al. (2005). |
| 5-244 | A | 12:20 | 12:20 | "redirection of river runoff" - presumably man-made? This needs to be made clear. [Govt. of Australia (Reviewer's comment ID #: 2001-275)] | Taken into account. Wording clarified. Not anthropogenic, but rather due to decadal shift in the winds. |
| 5-245 | A | 12:20 | 12:24 | was this 'redirection' purposeful, an anthropogenic effect that changed the convection noticeably in the Arctic? Is it known why the water subsequently freshened? [David Rind (Reviewer's comment ID #: 214-52)] | Taken into account. See 5-244. |
| 5-246 | A | 12:20 | :24 | Was this 'redirection' purposeful, an anthropogenic effect that changed the convection noticeably in the Arctic? Is it known why the water subsequently freshened? [Govt. of United States of America (Reviewer's comment ID #: 2023-334)] | Accept. Same as 5-244, 5-245. |
| 5-247 | A | 12:26 | 12:26 | Figure 5.3.2 does not support the following statement of "coordinated changes" between the Mediterranean and the Atlantic. The Atlantic curve shows continuing increase accelerated after 1990s the Mediterrean shows a no-change situation up to late 1970s around 1980 (anto-correlated with a dip in the Atlantic curve) and then a rise in the 1990s. Thus The only period that they seem to behave the same is the 1990s and even there the rise is much sharper in the Atlantic despite the low pass filter applied. Giving emphasis to the agreement within the last decade is opening the assessment to criticism that interprets decadal variability as climate change. I would argue that the long term trend in the Atlantic curve is the one linked to climate change and that as this des not show up in the Mediterranean Sea it demonstrates the regional character of the basin. [Michael Tsimplis (Reviewer's comment ID #: 268-3)] | Accept. Figure is now deleted. |
| 5-248 | A | 12:26 | | Fig 5.3.2. The figure has changed quite substantially from the initial figure in the first draft. I am not sure the two curves should be on the same plot (unless there would be some sort of comparison...). I would find a figure comparing the WMDW and the North Atlantic (Levitus) heat content change more appealing. Sending a message to the policy makers that "Global changes affect some regional areas in consistent ways" makes more sense in my opinion. [Michel Rixen (Reviewer's comment ID #: 215-1)] | Accept. Figure is a now deleted. |
| 5-249 | A | 12:29 | 12:29 | please add the following ref after Hurrell. Xoplaki et al. 2003 and/or Xoplaki et al. 2004: | Reject. These are purely atmospheric |

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| | | | | Xoplaki, E., González-Rouco, J.F., Luterbacher, J., and H. Wanner, 2003: Mediterranean summer air temperature variability and its connection to the large-scale atmospheric circulation and SSTs. <i>Clim. Dynam.</i> , 20, 723-739. Xoplaki, E., González-Rouco, J.F., Luterbacher, J., and H. Wanner, 2004: Wet season Mediterranean precipitation variability: influence of large-scale dynamics, <i>Clim. Dynam.</i> , 23, 63-78 [Jürg Luterbacher (Reviewer's comment ID #: 151-6)] | diagnostics and do not refer to in situ ocean, and not appropriate for this chapter. |
| 5-250 | A | 12:29 | 12:29 | Double closing parenthesis in the first reference [Michael Tsimplis (Reviewer's comment ID #: 268-4)] | Accept. |
| 5-251 | A | 12:36 | 12:36 | apart from Luterbacher et al. 2004, please add Xoplaki et al. 2005 who support those findings for the other seasons. Xoplaki, E., Luterbacher, J., Paeth, H., Dietrich, D., Steiner N., Grosjean, M., and Wanner, H., 2005: European spring and autumn temperature variability and change of extremes over the last half millennium, <i>Geophys. Res. Lett.</i> , 32, L15713. [Jürg Luterbacher (Reviewer's comment ID #: 151-7)] | Reject. See 5-249. |
| 5-252 | A | 12:41 | 12:45 | References to support these statements? [Govt. of Australia (Reviewer's comment ID #: 2001-276)] | Accept, text rewritten and statement modified |
| 5-253 | A | 12:48 | 13:47 | I would add reference to the following interesting work: Lucarini V., Calmanti S., Artale V. 2005: Destabilization of the thermohaline circulation by transient changes in the hydrological cycle <i>Climate Dynamics</i> (2005) 24: 253-262. In this work it is shown that it is possible to define a robust separation between slow and fast regimes of surface hydrological forcing to the Thermohaline circulation of the Atlantic Ocean. Such a separation between slow and fast regimes is obtained by singling out an estimate of the critical rate of increase for the anomalous forcing. The critical rate of increase is of the same order of magnitude of the ratio between the typical intensity of the hydrological cycle and the advective time scale of the system. Basically, if the change of the forcing is faster than the estimated critical rate, the system responds similarly to the case of instantaneous changes of the same amplitude. Specularly, if the change of the forcing is slower than the critical rate, the behavior of the system resembles the response to quasi-static changes of the same amplitude. [Teresa Nanni (Reviewer's comment ID #: 186-6)] | Reject. Modeling paper rather than ocean observations. |
| 5-254 | A | 12:48 | 13:47 | What are the criteria for confining text to a box? I thought it might be for "tutorial", background-type text. But the content of Box 5.1 is very important - a welcome inclusion in this chapter - is the box the right place for it? After all, its conclusion is very important, as shown but its inclusion in the Exec summary. [Govt. of United Kingdom (Reviewer's comment ID #: 2022-23)] | The box has been chosen because the MOC change is considered to be very important and of particular public interest. |
| 5-255 | A | 12:50 | :51 | All the dense waters of the MOC doesn't sink to the seafloor (e.g., Labrador Sea Water); sentence should be corrected. | Accept. Change to "abyssal ocean" |

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| | | | | [Govt. of United States of America (Reviewer's comment ID #: 2023-335)] | |
| 5-256 | A | 12:53 | 12:53 | L.D. Talley" should be "Talley [Melissa Bowen (Reviewer's comment ID #: 28-11)] | Accept. |
| 5-257 | A | 12:54 | 12:54 | "densify" - not sure this is a word? [Govt. of Australia (Reviewer's comment ID #: 2001-277)] | Accept. |
| 5-258 | A | 13:3 | 13:3 | "decrease" - add "in intensity"? [Govt. of Australia (Reviewer's comment ID #: 2001-278)] | Accept. Change to "decrease in transport" |
| 5-259 | A | 13:5 | | Again, I would suggest the Bryden et al. method is fairly direct (albeit with uncertainties remaining over the method) [Richard Wood (Reviewer's comment ID #: 294-9)] | Accept. Agree that statement that "only indirect estimates exist" is incorrect. |
| 5-260 | A | 13:11 | 13:11 | "changes in " before "ocean circulation"? [Govt. of Australia (Reviewer's comment ID #: 2001-279)] | Accept. |
| 5-261 | A | 13:17 | 13:17 | upper km is there a number missing before "km"? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-21)] | Accept. Change to "kilometer" since this is not a unit. |
| 5-262 | A | 13:19 | 13:19 | Change "considerable variability" to "unknown variability" - the RAPID programme will provide the first estimates of Atlantic MOC variability. [Meric Srokosz (Reviewer's comment ID #: 250-9)] | Noted, "considerable" changed to "significant" |
| 5-263 | A | 13:19 | | Add "There can also be large errors in that calculation of the MOC rate changes due to the subjective choice of reference level for determining geostrophic currents" [Friedrich Schott (Reviewer's comment ID #: 228-10)] | Noted. Paragraph is greatly shortened so no details about errors are given, however the assessment of Bryden's results reflects these errors. |
| 5-264 | A | 13:21 | 13:21 | missing word between "observed changes" and "water mass formation" [Pascale DELECLUSE (Reviewer's comment ID #: 58-8)] | Accept. |
| 5-265 | A | 13:21 | 13:21 | typo error: "observed changes water mass" --> "observed changes of/in water mass" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-13)] | Accept. |
| 5-266 | A | 13:21 | 13:21 | ... observed changes IN water mass formation ... [Brian Soden (Reviewer's comment ID #: 245-8)] | Accept. |
| 5-267 | A | 13:21 | 13:21 | is there a word missing between "changes" and "water"? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-22)] | Accept. |
| 5-268 | A | 13:21 | | should be "changes IN" [Danny Harvey (Reviewer's comment ID #: 101-31)] | Accept. |
| 5-269 | A | 13:21 | | It is not true that "indirect evidence from observed changes... the subpolar North Atlantic ...reveals changes of the MOC". You have absolutely NO evidence for this claim! The eastern outflows have increased again, and the decrease of deep salinities is totally compensated by cooling, so no decadal decrease in meridional density in the subpolar basin. | Accept. Changed to "inconclusive" |

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| | | | | [Friedrich Schott (Reviewer's comment ID #: 228-11)] | |
| 5-270 | A | 13:24 | 13:29 | 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:1029/2005GL022900.) found the weaker MOC is responsible for the freshening of the subpolar North Atlantic which agrees with Latif et al (2006), but opposite to Wu et al. (2004). [Aixue Hu (Reviewer's comment ID #: 110-3)] | Reject. Model results being removed from the box in favor of stronger statements about difficulties of interpreting the actual observations. |
| 5-271 | A | 13:24 | 13:24 | "... periods (a few years) it is unclear" ... Perhaps the specific period could be stated? [Brian Soden (Reviewer's comment ID #: 245-9)] | Accept. Overall statements changed though. |
| 5-272 | A | 13:28 | 13:28 | "freshening" misspelled [Melissa Bowen (Reviewer's comment ID #: 28-12)] | Accept |
| 5-273 | A | 13:28 | 13:28 | correct "freshening" [Pascale DELECLUSE (Reviewer's comment ID #: 58-9)] | Accept |
| 5-274 | A | 13:33 | 13:33 | I suggest to add the following text after and convection have been weaker. 'The formation rate decreased by 50% in 2000-2001 compared to the mean of 1970-1997 and the water formed is less dense (Kieke et al., 2006). [Monika Rhein (Reviewer's comment ID #: 212-8)] | Reject, too detailed for assessment |
| 5-275 | A | 13:34 | 13:35 | A reference to a Schott et al (2004) publication cannot be used to explain trends over a period extending to 2005, as is stated in the text. It is entirely possible that the Bryden et al result (from measurements in 2004) is consistent with results from Schott et al, since the latter result covers a period BEFORE the Bryden et al finding. That said, I doubt this is so, but the text needs to be changed anyway. [Terrence Joyce (Reviewer's comment ID #: 122-4)] | Accept. (This was a typo – it should have read over the period 1993-2001.) |
| 5-276 | A | 13:37 | 13:41 | This paragraph emphasizes a modelling result, which may be particular, not reflecting a consensus from several other models.. I would recommend to suppress it. [Pascale DELECLUSE (Reviewer's comment ID #: 58-10)] | Accept. Delete paragraph. |
| 5-277 | A | 13:37 | 13:37 | Change "concluded" to "inferred" - there is no observational evidence that the SST is related to the MOC therefore conclusions drawn from model-based relationships must be treated with extreme care. The Bryden et al. result is a genuine observation not an inference like Knight et al., Latif et al. [Meric Srokosz (Reviewer's comment ID #: 250-10)] | Taken into account. Paragraph deleted. |
| 5-278 | A | 13:43 | 13:47 | Here you harp again on that Bryden "result" that is based on an antiquated method, and that is supported BY ZERO OTHER EVIDENCE!!. How can you close this important section by such a false statement? Your meek disclaimer does not make it go away! [Friedrich Schott (Reviewer's comment ID #: 228-12)] | Accept. Our assessment has now changed. |
| 5-279 | A | 13:43 | :47 | Remove "structural" and just cite as "uncertainties". | Accept. |

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| | | | | [Govt. of United States of America (Reviewer's comment ID #: 2023-336)] | |
| 5-280 | A | 14:4 | 15:16 | Can you comment on whether any of the South Pacific changes are consistent with the IPO extension of the North Pacific PDO variability (e.g the Pacific-wide SST pattern of Fig 3.6.3?) [Chris Folland (Reviewer's comment ID #: 71-180)] | Noted. Yes, it looks consistent with the pattern. However, we asked Roemmich directly about connections with the PDO. He cites Schneider and Cornuelle (J. Clim, 2005) in saying that the PDO itself is not a robust mode, but rather a combination of several other modes. Therefore, he is unwilling to make the connection to the PDO. Since his paper does not make the connection, we will not make it here. |
| 5-281 | A | 14:17 | 14:18 | The long-term heat content trend most likely includes the positive PDO state, rather than saying it is related to the PDO. [Franklin SCHWING (Reviewer's comment ID #: 230-9)] | Reject. Language seems clear enough and we don't want to overstate without a reference. |
| 5-282 | A | 14:17 | :18 | The long-term heat content trend includes the positive PDO state, rather than saying it is related to the PDO. [Govt. of United States of America (Reviewer's comment ID #: 2023-337)] | Reject. See 5-281. |
| 5-283 | A | 14:24 | 14:25 | "increased more than 20% over the 1990s" - increased with respect to what time period? [Govt. of Australia (Reviewer's comment ID #: 2001-280)] | Accept: from 1993 to 2003 stated now. |
| 5-284 | A | 14:45 | 14:46 | There is a reference here to the work of Yasuda et al and the state of the N. Pacific Ocean after the 1976 regime shift. This concerns me quite a lot. The statistical evidence to support the hypothesis that such a regime shift occurred in the PHYSICAL data extremely weak. Recently a paper was published by Hsieh et al "Distinguishing random environmental fluctuations from ecological catastrophes for the N. Pacific Ocean", Nature 435/19 May 2005, pp 336-340. This in my opinion shows very clearly that "regime shifts" cannot be supported by the evidence in the physical time series. The biological evidence is different, but that isn't the topic of this chapter. He is also extremely critical about the statistical methods used. My concern is that there is even any mention in this chapter of something that many credible scientists consider to be very weak statistics. Everything else in the chapter is strong, so why put even a passing reference to something this weak that can be attacked easily? I can hear the messages in the newspapers next year reading "in Chapter 5 they talk about regime changes and the majority of ocean scientists think this is garbage science, so what can we make of the rest of the report? Is it equally bunk?" So, I don't really understand why we set up a flag that can be shot at so easily. I think that the sentence starting on 5-14 line 45 and finishing on the next line should be removed and this would significantly improve the whole chapter. | Accept. Remove "regime" in all instances and "shift" in most. |

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| | | | | [Howard Freeland (Reviewer's comment ID #: 75-2)] | |
| 5-285 | A | 15:4 | 5: | what is the age of this water mass? My guess is that is several hundred years old and the changes may have nothing to do with present changes, but rather with something that happened hundreds of years ago, say between 1400 and 1430 [Thomas Crowley (Reviewer's comment ID #: 51-3)] | Accept. Johnson and Orsi's explanation for a shallower water mass change had crept in here. This is fixed now. |
| 5-286 | A | 15:4 | 15:7 | does the warming at the leading to cooling in the sw Pacific arise because the source has weakened? [David Rind (Reviewer's comment ID #: 214-53)] | No change necessary because the explanation was not the correct one and has been replaced (see 5-285). |
| 5-287 | A | 15:4 | :7 | Does the warming at the leading to cooling in the sw Pacific arise because the source has weakened? [Govt. of United States of America (Reviewer's comment ID #: 2023-338)] | No change. Same comment as 5-286. |
| 5-288 | A | 15:12 | | Arnold Gordon in 1971 Geol Soc. Am. Mem 126, pp 23-39 Geolog invest. Of the north Pacific that heat flow changes from ocean floor could conceivably cause the type of changes you are seeing - someone would have to check this with updated heat flow information but in principle it could be easily done - may provide the most straightforward explanation, esp since Gordon mentioned the effect as penetrating through the lower 1000 m (as does text in SOD) - note date is 1970 not 1971 [Thomas Crowley (Reviewer's comment ID #: 51-4)] | Taken into account. Fukasawa et al. found that geothermal heat flux is too weak to account for the warming. Text modified to specifically state this since the reviewer's suggestion is an obvious concern. |
| 5-289 | A | 15:20 | 15:37 | Section 5.3.3.3 Japan (East) Sea should be delete as a whole, in order to keep the balance of content in Chapter 5. [Govt. of China (Reviewer's comment ID #: 2006-49)] | Taken into account. We have removed the separate section, but retain the information in reduced form, with justification added. |
| 5-290 | A | 15:28 | 15:28 | "slowed for many decades" - which decades? [Govt. of Australia (Reviewer's comment ID #: 2001-281)] | Accept. Specific dates included now. |
| 5-291 | A | 15:28 | 15:32 | Need indications of the time periods over which changes have been reported throughout this paragraph. [Govt. of Australia (Reviewer's comment ID #: 2001-282)] | Accept. |
| 5-292 | A | 15:29 | 15:29 | L.D. Talley" should be "Talley [Melissa Bowen (Reviewer's comment ID #: 28-13)] | Accept |
| 5-293 | A | 15:41 | 15:46 | In the Indian Ocean, decadal variability of the shallow cross-equatorial cell has recently been reported (GRL, Apr. 2006), and I suggest adding the following sentence at the end of that section: The cross-equatorial heat exchange is accomplished by a shallow cross-equatorial cell and assimilation model analysis has shown a significant decadal reduction of its transport (Schoenefledt and Schott, 2006). Ref, Schoenefeldt, R., and F. A. Schott, 2006: Decadal variability of the Indian Ocean | Accept. |

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| | | | | cross-equatorial exchange in SODA, Geophys. Res. Lett., 33, L08602, doi:10.1029/2006GL025891. [Friedrich Schott (Reviewer's comment ID #: 228-16)] | |
| 5-294 | A | 15:44 | 15:46 | The sentence that spans these lines is about attribution, so perhaps would be more appropriate in Chapter 9. [Adrian Simmons (Reviewer's comment ID #: 242-89)] | Accept. Attribution statements have been deleted |
| 5-295 | A | 15:49 | 15:49 | typo error: "consistent the significant" --> "consistent with the significant" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-14)] | Accept. |
| 5-296 | A | 15:52 | 15:51 | (Qian et al., 2003)" should be added before the end of this line, for they have relative study with similar result. The reference should be added is "Qian H.?Yin Y. and Ni Y. Tropical Indian Ocean subsurface dipole mode and diagnostic analysis of dipole event in 1997-1998.(in Chinese). JOURNAL OF APPLIED METEOROLOGICAL SCIENCE, 2003, Vol. 14, No.2 129-139. [Govt. of China (Reviewer's comment ID #: 2006-50)] | Accept: reference included |
| 5-297 | A | 16:4 | 16:13 | This paragraph can be shortened and a sentence on more recent developments can be added: "Recently it has been noted that a "super gyre" connects the southern subtropical Indian Ocean with the subtropical South Pacific south of Australia and that the strengthening of the Southern Annular Mode in recent decades has resulted in an enhanced circulation of the "super gyre" (Cai et al., GRL 2005). Coupled model simulations predict this strengthening to continue into the 21st century with increasing GH gas concentrations (Cai et al., GRL 2006) 425 5-425 13 [Friedrich Schott (Reviewer's comment ID #: 228-50)] | Partially taken into account. We don't mention the supergyre, because it is more related to climate rather than climate change. We do discuss the strengthening of the SAM, and connect this strengthening to warming signal found in Cai et al 2006 paper. No suggestion here of what content should be deleted, but the paragraph has been shortened. |
| 5-298 | A | 16:4 | 16:13 | It's good to see the more recent observations discussed here. Because of the undersampling of the observations, I do think a caveat should be added that we could be just looking at aliased interannual variability (as discussed in Ch 9 page 41 17 and in Stark et al. 2006) [Richard Wood (Reviewer's comment ID #: 294-10)] | Accept. We have added two references to support the fact that the observed cooling on pressure surfaces in the recent times can be related to an earlier observed cooling in the surface temperature. Murray et al. 2006. We have included the Stark et al paper recognizing that the changes in coupled models include a larger range of variability. |
| 5-299 | A | 16:19 | 16:19 | why "A"nthropogenic "C"arbon "D"ioxide ? | Accept. Also removed "dioxide" to be |

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| | | | | [Pascale DELECLUSE (Reviewer's comment ID #: 58-11)] | consistent with section 5.4. |
| 5-300 | A | 16:38 | 16:38 | "to the increase in surface air temperatures" - where, locally, globally? [Govt. of Australia (Reviewer's comment ID #: 2001-283)] | Accept. "local" although by this is meant the ocean and atmosphere temperatures in the whole southern ocean region, since the warming at mid-depth was widespread. |
| 5-301 | A | 16:38 | 16:41 | Figure 5.2.3d appears to show highest zonally-averaged warming at about 15N, not 40S as the previous sentence states. [Melissa Bowen (Reviewer's comment ID #: 28-14)] | Accept. This was greatly overstated. We also note that Fig. 5.2.2 doesn't show the 40S heating at all, whereas Fig. 5.2.3 does, and Willis et al. (2004) do. |
| 5-302 | A | 16:51 | 16:51 | mix" should be "mixed [Melissa Bowen (Reviewer's comment ID #: 28-15)] | Accept |
| 5-303 | A | 16:51 | 16:51 | replace "mix layer" by "mixed layer" [Pascale DELECLUSE (Reviewer's comment ID #: 58-12)] | Accept |
| 5-304 | A | 16:51 | 16:51 | typo error: "the mix layer" --> "the mixed layer" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-15)] | Accept |
| 5-305 | A | 16:54 | | add "including Antarctic Circumpolar Current" [Walter Zenk (Reviewer's comment ID #: 301-6)] | Accept. Deleting separate heading for ACC. |
| 5-306 | A | 17:0 | :18 | Skip this header by combining the text with 5.3.5.2. The text is so short that it does not justify its own header. [Walter Zenk (Reviewer's comment ID #: 301-7)] | Accept. |
| 5-307 | A | 17:1 | :4 | My recollection of the Meredith and King paper is that they ascribe the increased salinity mainly to reduced sea ice production (did they really mention iceberg calving?). They highlighted the facts that salinity increase is greatest at the surface as is ocean warming to deduce that the reduced ice production in the region is primarily due (this needs emphasising) to atmospheric rather than ocean circulation changes here. For the benefit of the unfamiliar reader it would be useful to just state here that the Antarctic Peninsula is the region of greatest surface warming in the southern hemisphere. It would also be helpful to have a cross-reference to the main discussion of climate change in the Antarctic Peninsula in chapter 4. [Steve Harangozo (Reviewer's comment ID #: 98-16)] | Accept. They did not mention ice bergs; comment deleted. Additional information from Meredith and King incorporated and reference to great warming in the region. |
| 5-308 | A | 17:7 | | It might be better to replace "from" by "measured by" or something similar, as a reader not familiar with WOCE might at first sight think that the word "Experiment" refers to something other than a measurement campaign. | Accept, but he must mean line 27. Replace WOCE with measurements in 1950s and 1990s since WOCE didn't |

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| | | | | [Adrian Simmons (Reviewer's comment ID #: 242-90)] | exist back in the 1950s! |
| 5-309 | A | 17:18 | 17:21 | Section 5.3.5.3 is rather brief. A further point could be made regarding evidence for a link between the Southern Annular Mode and transport through Drake Passage, by adding after reference to Cunningham et al. (2003): "... , although continuous subsurface pressure measurements suggest that trends in seasonality of transport are highly correlated with similar trends in the Southern Annular Mode index (Meredith et al. 2004)." Reference: Meredith, M. P., Woodworth, P. L., Hughes, C. W., and V. Stepanov (2004). Changes in the ocean transport through Drake Passage during the 1980s and 1990s, forced by changes in the Southern Annular Mode. Geophys. Res. Lett., 31, L21305, doi:10.1029/2004GL021169. [Robert Marsh (Reviewer's comment ID #: 163-9)] | Accepted. The section has been incorporated in the previous section. Cunningham reference added. |
| 5-310 | A | 17:23 | 17:23 | Assesment" should be "Assessment [Melissa Bowen (Reviewer's comment ID #: 28-16)] | Accept |
| 5-311 | A | 17:30 | 31: | although this topic has been discussed by many, Manabe and Bryan showed in 1985 with an idealized coupled oc-atm gcm that enhanced subtropical salinities are a consequence of strenghtening of the hydrological cycle. This appears to bea very robust result although I don't know if anyone has done formal detection and attribution on it (chap 9?) - the salinity result is very consistent with predictions of global warming driven by ghg changes. paper is in JGR v 90, 11689-11708. v nice figure illustrating the concept [Thomas Crowley (Reviewer's comment ID #: 51-5)] | Reject. This is material for chapter 9. |
| 5-312 | A | 17:34 | 17:34 | "is closer to zero" instead "is consistent with zero". [Roxana Bojariu (Reviewer's comment ID #: 24-18)] | Reject. Can't find the phrase. |
| 5-313 | A | 17:51 | 17:51 | Remind the reader of the meaning of "mode water". [Chris Folland (Reviewer's comment ID #: 71-181)] | Accept. |
| 5-314 | A | 18:1 | 18:1 | 5.2.6b" should be "5.2.6d [Melissa Bowen (Reviewer's comment ID #: 28-17)] | Accept. |
| 5-315 | A | 18:5 | 18:5 | "North Pacific subtropical gyres" either change to "gyre" or, if the Japan Sea is included, specifically mention it. [Melissa Bowen (Reviewer's comment ID #: 28-18)] | Accept. The sentence didn't make sense. Fixed. |
| 5-316 | A | 18:6 | 18:6 | There is no section 5.3.3.4 [Melissa Bowen (Reviewer's comment ID #: 28-19)] | Accept. |
| 5-317 | A | 18:6 | 18:7 | It is important to note that changes in ocean fresh water transport may also be a significant contributor. I believe there are a few papers 'in the works' which suggest that this is an important term. I think "are consistent with" would be more accurate than "suggest". [Richard Wood (Reviewer's comment ID #: 294-11)] | Accept. |
| 5-318 | A | 18:9 | 18:18 | This is Chapter 9 territory, although I think the points here are useful. Suggest add | Accept. Also made some corrections to |

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| | | | | sentence at end of this para: "The few detection/attribution studies of ocean changes are discussed in Section 9.5.1." [Richard Wood (Reviewer's comment ID #: 294-12)] | this section based on van Aken and Lavin comments above. |
| 5-319 | A | 18:14 | 18:14 | "could well" is too strong, "might be" is better, as there is no definite evidence either way. [Chris Folland (Reviewer's comment ID #: 71-182)] | Accept. |
| 5-320 | A | 18:27 | 18:37 | ?It is misleading to state that the deep waters show no significant change. Please see Fig. 5.2.3, in which temperature has changed at 2000 m in the Atlantic, and also Fig. 5.2.6 for salinity. The distributions below 200m should be shown here. [Motoyoshi Ikeda (Reviewer's comment ID #: 113-4)] | Accept. Phrase added. |
| 5-321 | A | 18:31 | | That statement about combined warming and freshening in the deep North Atlantic is wrong. Below 2500m, the decadal density decrease due to freshening is almost completely compensated by cooling, it has not been warming. Besides, "Penetrative warming" is the wrong expression: Deep convection has been found to be non-penetrative. [Friedrich Schott (Reviewer's comment ID #: 228-14)] | Accept. Also made additions to section 5.3.2.2 since temperature was not actually mentioned there. |
| 5-322 | A | 18:32 | 18:32 | fresher waters Antarctic shelf waters is there an extra word here? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-24)] | Accept. |
| 5-323 | A | 18:32 | :32 | Too many 'waters' in the sentence. [Steve Harangozo (Reviewer's comment ID #: 98-17)] | Accept. |
| 5-647 | B | 18:39 | 23:38 | Comment for entire section 5.4: This section strongly emphasizes changes in ocean circulation as the cause for most of the observed changes, particularly oxygen. I agree that most of the evidence in the ocean's interior that we have collected so far tends to support this conclusion. On the other hand, this is not really the case for near surface changes, where biology generally plays a more important role. The problem is that we have very few timeseries that would permit us to say anything quantitatively, but nevertheless, there are a couple of biological changes that are quite dramatic. For example, the large shift in zooplankton observed in the California Current System (Roemmich & McGowan, Science, 1995); the large shifts between anchovies and sardines documented by Chavez et al (Science, 2003). I know that these are not purely biogeochemical, but ultimately, if we think about the state of the ocean, we need to consider the higher trophic levels as well, don't we? I therefore wish this section could be pepped up a bit with more biology. [Nicolas Gruber (Reviewer's comment ID #: 307-11)] | Rejected – we cannot speculate what is the impact of changes in biology on biogeochemistry if there is not information. |
| 5-648 | B | 18:45 | 18:45 | "in response to the atmospheric CO2 increase, CO2 dissolved into the ocean" . Maybe I am picky here, but this gives the impression that CO2 dissolves only into the ocean when atmospheric CO2 is increasing. Please reformulate. [Nicolas Gruber (Reviewer's comment ID #: 307-12)] | Accepted – text modified (“additional CO2...”) |
| 5-324 | A | 18:48 | 18:48 | insert "of" between "penetration" and "anthropogenic" | Accepted |

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| | | | | [Melissa Bowen (Reviewer's comment ID #: 28-20)] | |
| 5-325 | A | 18:48 | 18:48 | penetration "of" anthropogenic... [Pascale DELECLUSE (Reviewer's comment ID #: 58-13)] | Accepted |
| 5-649 | B | 18:48 | 18:48 | add "biogeochemical" to the "combined physical changes" to read the "combined physical and biogeochemical changes" [Nicolas Gruber (Reviewer's comment ID #: 307-13)] | Accepted |
| 5-650 | B | 18:51 | 18:54 | paragraph unclear. What is meant with "consequences for chemical equilibration"? When I read this statement, I relate this to the observation that as a result of the acidification, the equilibration timescale for the gas exchange across the air-sea interface decreases. But I don't think that this is what is meant here. Perhaps "equilibrium" was meant here instead of "equilibration". [Nicolas Gruber (Reviewer's comment ID #: 307-14)] | Accepted – equilibrium was meant |
| 5-326 | A | 18:54 | 18:54 | "unknown response of marine ecosystems" - there is some evidence of how marine organisms might respond to changing ocean chemistry; probably need some reference to WGII assessments of effects of changing ocean chemistry. [Govt. of Australia (Reviewer's comment ID #: 2001-284)] | Accepted |
| 5-327 | A | 18:54 | 18:54 | Section 7.3.2.2 -> 7.3.4.2 ? [Michio Kawamiya (Reviewer's comment ID #: 124-27)] | Accepted |
| 5-651 | B | 18:54 | 18:54 | consequences for...: I think this statement needs to be made more carefully. On the one hand, we understand the consequences of the acidification on the CO2 chemistry extremely well, e.g. we know how the carbonate concentration is going to change with great certitude. What we don't know is how marine organisms respond to these changes in chemistry. I therefore would prefer if this sentence made this distinction somewhat more explicit. [Nicolas Gruber (Reviewer's comment ID #: 307-15)] | Accepted |
| 5-328 | A | 18:56 | 18:56 | Comment on sentence in section 5.4.1: Start of sentence should be Oxygen (not O2). [Hernan Garcia (Reviewer's comment ID #: 81-1)] | Accepted |
| 5-329 | A | 18:56 | 18:56 | Consider adding "dissolved in the ocean" after "O2" and before "is affected by..." [Melinda Marquis (Reviewer's comment ID #: 162-14)] | Accepted O2→Dissolved oxygen in the ocean |
| 5-330 | A | 19:1 | 19:1 | Consider changing "Changes in O2" to "Changes in oceanic O2 concentration." [Melinda Marquis (Reviewer's comment ID #: 162-15)] | Accepted |
| 5-652 | B | 19:3 | 19:5 | Changes this sentence to "Furthermore, changes in the oceanic O2 content are needed to estimate the anthropogenic CO2 budget from the combined measurements of changes in atmospheric CO2 and O2/N2 ratio. At the moment, this method estimates the net air-sea flux of O2 on the basis of heat fluxes, which is an imperfect proxy). 16 5-16 16 | Accepted – sentence revised |

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| | | | | [Nicolas Gruber (Reviewer's comment ID #: 307-15)] | |
| 5-331 | A | 19:5 | 19:5 | Section 7.3.3 -> 7.3.2 ? [Michio Kawamiya (Reviewer's comment ID #: 124-28)] | Accepted |
| 5-653 | B | 19:10 | 10:10 | Section 5.4.2: Honestly, I found the organization of this section a bit confusing, particularly the separation into Sections 5.4.2.1 and 5.4.2.2. In my understanding, what was tried here is to discuss separately the (inorganic) carbon changes that are driven by the uptake of anthropogenic CO2 and those that are driven by other processes (I call this changes in the natural carbon cycle). However, the discussion of the air-sea CO2 fluxes in 5.4.2.1 break this separation again, as the discussion of changes in this flux is all done in the context of the oceanic uptake of anthropogenic CO2. I recommend a reorganization of these two sections: I would start with a brief intro that lays out the main processes that could alter inorganic carbon, i.e. the uptake of anthropogenic CO2 from the atmosphere and changes in the "natural" carbon cycle. Then one subsection talks about the anthropogenic CO2 (which should include more detail, using e.g. Mikaloff-Fletcher's work) and the other (short) subsection discusses the few studies that have addressed "natural" DIC changes. [Nicolas Gruber (Reviewer's comment ID #: 307-17)] | Accepted (partly). Added a few sentences of clarification but no changes in re-organisation. |
| 5-332 | A | 19:12 | 21:32 | This section deals with carbon changes in the ocean. The time period used to examine the change is 1750 to 1994. Data to produce this time series uses both proxy measurements and direct measurements, which means the error bars in the earlier part of the record (relying on proxies) are much larger than in the later part, and this alters the quality of the inferences that can be drawn. This difference in error bars and its impact is not made clear in the text. [Govt. of Australia (Reviewer's comment ID #: 2001-285)] | Accepted – uncertainty clarified |
| 5-333 | A | 19:17 | 19:17 | "deep" water formation? [Chris Folland (Reviewer's comment ID #: 71-183)] | Accepted, text clarified |
| 5-334 | A | 19:17 | 19:17 | Reduced water formation - Specify what water and explain why it will lead to increased carbon storage. [Donald L. Forbes (Reviewer's comment ID #: 72-3)] | Accepted, text clarified |
| 5-335 | A | 19:17 | 19:18 | Is it correct that reduced water formation results in increased storage in intermediate ocean? This seems counterintuitive...Please check. [Christopher Sabine (Reviewer's comment ID #: 224-1)] | Accepted, text clarified |
| 5-336 | A | 19:20 | 19:20 | What do you mean by "relatively short time scales"? ENSO related changes can occur over a year or two and NAO or PDO type changes can occur over a decade. Are these short time scales...not if you are used to thinking in terms of weeks to seasons? It might be useful if you put a time frame in brackets at the end of this sentence e.g. (years to decades). | Accepted, text clarified |

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| | | | | [Christopher Sabine (Reviewer's comment ID #: 224-2)] | |
| 5-337 | A | 19:30 | 19:31 | I do not agree with the statement that the CO2 flux is constant as long as the ocean and atmospheric CO2 increase at the same rate. This ignores the contribution of winds to the flux. Changing the wind speed or the location of the winds can have a dramatic impact on the net flux. [Christopher Sabine (Reviewer's comment ID #: 224-3)] | Accepted, sentence deleted Accepted, sentence deleted from line 30-32. |
| 5-338 | A | 19:32 | | not a sentence... [David Rind (Reviewer's comment ID #: 214-54)] | Accepted, sentence deleted |
| 5-339 | A | 19:32 | | Not a sentence... [Govt. of United States of America (Reviewer's comment ID #: 2023-339)] | see previous comment |
| 5-340 | A | 19:35 | 19:35 | global CO2 sink; should it be global oceanic CO2 sink? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-25)] | Accepted |
| 5-341 | A | 19:43 | 19:43 | Replace "2.2±0.4" with "2.2±0.8", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-587)] | Rejected – error calculation clarified |
| 5-342 | A | 19:53 | 20:43 | The comparison of ocean carbon uptake 1750-1994 with ocean carbon uptake 1980-2005 is important enough to have been included in the Technical Summary, but the methodology for estimating past ocean carbon uptake is poorly described. The discussion of this methodology (Pg. 5-19, lines 53-55) should be amplified to better describe how observed DIC concentration is corrected and to give some idea of the magnitude of the corrections. The results will be more believable if the corrections are small than if they are large. [Lenny Bernstein (Reviewer's comment ID #: 20-58)] | Accepted – error calculation and methodology clarified |
| 5-343 | A | 19:53 | 20:43 | The discussion of the methodology for estimating past rates of ocean carbon uptake needs to be expanded to give some indication of the magnitude of corrections to observed dissolved inorganic carbon, and how these corrections are made. Changes in the rate of ocean carbon uptake are a critical factor in projecting climate change, and readers should be able to judge for themselves the validity of these estimates. [Jeff Kueter (Reviewer's comment ID #: 137-55)] | Accepted – error calculation and methodology clarified |
| 5-344 | A | 19:53 | 20:43 | The discussion of the methodology for estimating past rates of ocean carbon uptake needs to be expanded to give some indication of the magnitude of corrections to observed dissolved inorganic carbon, and how these corrections are made. Changes in the rate of ocean carbon uptake are a critical factor in projecting climate change, and readers should be able to judge for themselves the validity of these estimates. [Govt. of United States of America (Reviewer's comment ID #: 2023-340)] | Accepted – error calculation and methodology clarified |
| 5-345 | A | 20:3 | | If mapping errors do not include uneven data distribution then the latter should be included as a source of uncertainty (particularly data voids). Please describe what is | Noted. The error resulting from data coverage has been tested and was small |

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| | | | | included in mapping errors. [Govt. of United States of America (Reviewer's comment ID #: 2023-341)] | |
| 5-654 | B | 20:10 | 20:10 | "mixing": add "transport" here [Nicolas Gruber (Reviewer's comment ID #: 307-18)] | Accepted |
| 5-655 | B | 20:11 | 20:11 | "undetectable": add "in most of the deep ocean". [Nicolas Gruber (Reviewer's comment ID #: 307-19)] | Accepted |
| 5-346 | A | 20:14 | 20:16 | Also mention the direct effect of low temperature to lower surface pCO ₂ . [Michio Kawamiya (Reviewer's comment ID #: 124-29)] | Rejected – the method does not detect changes caused by temperature |
| 5-347 | A | 20:21 | 20:21 | perhaps Figure 5.2.3 instead of Figure 5.2.2? [Melissa Bowen (Reviewer's comment ID #: 28-21)] | Accepted |
| 5-656 | B | 20:31 | 20:31 | "water at high concentrations" Unclear as written. I suggest to write it as "that reduces the CO ₂ uptake capacity of water as CO ₂ increases". [Nicolas Gruber (Reviewer's comment ID #: 307-20)] | Accepted, text clarified |
| 5-348 | A | 20:33 | 20:33 | rate of atmospheric CO ₂ increases is this correct? Or should it be "atmospheric CO ₂ concentration increases"? Or "rate of increasing atmospheric CO ₂ concentration accelerates"? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-26)] | Accepted, text clarified |
| 5-657 | B | 20:33 | 20:34 | "as the rate of atmospheric CO ₂ increases": I don't think we have evidence for this. The primary factor is the total perturbation in the atmosphere, and not so much how fast it increases. What is the basis for this statement? [Nicolas Gruber (Reviewer's comment ID #: 307-21)] | accept, sentence deleted |
| 5-658 | B | 20:39 | 20:40 | "land use change, and the terrestrial biosphere response". Unclear to me. I understand that this is simply the net biosphere exchange, which consists of emissions from land use change and a CO ₂ sink whose cause we don't fully know. In any event, the uncertainty seems too small too me. In Sabine et al. (2004) we listed an uncertainty of +/- 20 Pg C for the emissions alone. [Nicolas Gruber (Reviewer's comment ID #: 307-22)] | Noted, the information is correct but the text is clarified |
| 5-659 | B | 20:41 | 20:41 | delete "large". [Nicolas Gruber (Reviewer's comment ID #: 307-23)] | Accepted |
| 5-349 | A | 20:48 | 20:48 | "acidity increases" - maybe mention that the oceans are still alkaline. [Govt. of Australia (Reviewer's comment ID #: 2001-286)] | Accepted, text clarified |
| 5-660 | B | 20:51 | 20:51 | I suggest to add "consistent with the lower buffer capacity of the high latitudes compared to the low latitudes" [Nicolas Gruber (Reviewer's comment ID #: 307-24)] | Accepted |
| 5-350 | A | 21:2 | 21:2 | I did not understand line 2. [Chris Folland (Reviewer's comment ID #: 71-184)] | Accepted, text clarified ("are" -> "is") |

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| 5-661 | B | 21:6 | 21:6 | which observed a decrease": write "where a decrease was observed [Nicolas Gruber (Reviewer's comment ID #: 307-25)] | Accepted |
| 5-662 | B | 21:8 | 21:8 | add "on marine organisms" [Nicolas Gruber (Reviewer's comment ID #: 307-26)] | Accepted (add after "changes in pH") |
| 5-351 | A | 21:9 | 21:9 | Section 7.3.2.2 -> 7.3.4.2 ? [Michio Kawamiya (Reviewer's comment ID #: 124-30)] | Accepted |
| 5-663 | B | 21:11 | 21:32 | I suggest to start this section with a brief statement about the current distribution of super- and undersaturation. That way, the changes discussed later become more clear. [Nicolas Gruber (Reviewer's comment ID #: 307-27)] | Accepted |
| 5-352 | A | 21:17 | 21:17 | Useful to indicate a typical depth of undersaturation here. [Chris Folland (Reviewer's comment ID #: 71-185)] | Accepted |
| 5-353 | A | 21:20 | 21:25 | Sentence is too long. [Chris Folland (Reviewer's comment ID #: 71-186)] | Accepted |
| 5-354 | A | 21:25 | | Can the shoaling of the saturation horizons be related to vertical movement of density surfaces? If so, this cause of changes in horizon depths should be given. [Govt. of United States of America (Reviewer's comment ID #: 2023-342)] | Accept. Most of the observed changes can be explained by the uptake of anthropogenic CO ₂ . However changes in biological or physical processes can also contribute locally, although even the local effect is thought to be dominated by anthropogenic CO ₂ . Text has been changed accordingly |
| 5-355 | A | 21:26 | 21:26 | Explain these causes of shoaling of the saturation horizon in more detail. [Chris Folland (Reviewer's comment ID #: 71-187)] | Accepted |
| 5-664 | B | 21:26 | 21:26 | "respiration processes in the intermediate waters": I am not aware that someone has demonstrated a shoaling of the saturation horizon that is driven by the anomalous addition of respiration derived DIC. [Nicolas Gruber (Reviewer's comment ID #: 307-28)] | Accepted – sentence removed |
| 5-356 | A | 21:36 | 21:36 | Comment on sentence in section 5.4.3: The term "large" in this sentence is vague (large compared to what?). Since at present there are no global- or basin-wide studies (full-depth or studies at comparable depth or density layers) or set of accepted values that can serve as reference baselines, the term "large" is a relative term. Suggest deleting "large" from this sentence. [Hernan Garcia (Reviewer's comment ID #: 81-2)] | Accepted |
| 5-357 | A | 21:36 | 21:40 | Comment on paragraph in section 5.4.3: The studies cited in the text (and references in Emerson et al., 2004) are all insightful papers. However, at least some of these studies focus on specific oceanic regions and caution should be taken to interpret (extrapolate?) | Accepted, text clarified. |

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| | | | | these results as representative of basin or global scale inter-annual or longer time-scale variability in O2 or AOU concentrations. The comparison of individual long hydrographic sections collected several years apart is useful and informative but it has its limitations. Although at risk of pointing out the obvious, long hydrographic repeat sections used to compute differences in O2 concentration as a function of depth (density) are snap shots in time of oceanic conditions at the time of sampling (i.e., a comparison of a reasonable number of long repeat basin-wide sections at different are needed to get an estimate of basin-scale decadal-scale variability). I suggest including a sentence at the beginning of this paragraph such as for example, "Our understanding of O2 decadal-scale variability is based on the comparison of O2 measurements collected on a few hydrographic sections". [Hernan Garcia (Reviewer's comment ID #: 81-3)] | |
| 5-665 | B | 21:36 | 21:36 | I suggest to delete "ventilated". [Nicolas Gruber (Reviewer's comment ID #: 307-29)] | Accepted, word changed. |
| 5-358 | A | 21:42 | 21:43 | Comment on sentence in section 5.4.3: The Deutsch et al., (2005; thereafter D05) description is a fine and insightful paper. However, this paper illustrates a comparison between model results and a selection of hydrographic O2 sections. First, the observation based results of D05 are discussed by Emerson et al., 2004 (redundancy in content of section 5.4.3?). Second, I found it confusing to read in this paragraph a mixture of observation and model based results. I was under the impression that Chapter 5 concentrates on "observations" rather than "model results". I suggest that if the authors want to include model results in section 5.4.3, then perhaps a new paragraph could be added at the end of this section illustrating the D05 and other peer-reviewed model-based results on O2 or AOU long-term variability. [Hernan Garcia (Reviewer's comment ID #: 81-4)] | Noted. Indeed the chapter deals with observed results, not with model result. The data presented in Deutsch et al. has however been included (in figure 5.12). |
| 5-359 | A | 21:42 | 21:42 | Comment on sentence in section 5.4.3: The term "everywhere" in this sentence appears to be true in the context of the Deutsch et al. (2005) model results in the N. Pacific. I suggest deleting the word "everywhere" or using "everywhere in the model results" if the latter is what the authors mean to say by in this sentence (See comment 4 for other details regarding mixing observations and model results in this section). [Hernan Garcia (Reviewer's comment ID #: 81-5)] | Accepted, text clarified to say this is data along transect only |
| 5-360 | A | 21:42 | 21:51 | Comment on paragraph in section 5.4.3: Some of the authors listed in this paragraph describe O2 and AOU concentration changes in the North Pacific along density layers. This depth (density) and geographic limitation should be noted in the paragraph because it gives the reader the (wrong) impression that the reported changes are representative of the changes expected in the major ocean basins. To my knowledge, such an inter-annual to decadal full-depth basin or global analysis on O2 or AOU concentration changes does not yet exist. | Accepted. We have included a figure (fig. 5.12) which shows at which depth interval the oxygen change signal exists. |

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| | | | | [Hernan Garcia (Reviewer's comment ID #: 81-7)] | |
| 5-666 | B | 21:44 | 21:46 | <p>explanation of AOU: I am not so happy with this definition. First, one also removes the effect of salinity. But more importantly, the concept of AOU is to estimate the change in oxygen that has occurred in the water parcel as a result of the remineralization of organic matter since the water parcel was last in contact with the atmosphere, i.e. $O_2 = O_{2_init} - \Delta O_{2_remin}$. Now, we just conveniently use O_{2_sat} as an estimator for O_{2_init}, but it doesn't have to be that way. The reason why I am emphasizing this is because the same separation is applied to nutrients, where a statement such as "removing the effect of temperature" would make little sense. I suggest to reformulate this.</p> <p>[Nicolas Gruber (Reviewer's comment ID #: 307-30)]</p> | Accepted – will try to re-write the text in O2 only (not AOU) |
| 5-361 | A | 21:45 | 21:45 | <p>Comment on sentence in section 5.4.3: "All studies indicate that the O2 decrease and AOU increase are consistent with reduction in ocean ventilation from physical processes". It is not clear what "all studies" refers to in this sentence (which ones?). Are the authors here referring to the Deutsch et al. (2005) paper? The term "ventilation" should be defined. I do not necessarily disagree with the author's statement of reduction in ventilation but is there factual evidence for this reduction?, and if so, cite references. I think that it is important to keep only factual information.</p> <p>[Hernan Garcia (Reviewer's comment ID #: 81-6)]</p> | Accepted, text aclarified |
| 5-667 | B | 21:46 | 21:46 | <p>"consistent with reduction in ocean ventilation" See remark 2 above. First, I think the expression "ventilation" should be handled more carefully. Second, this expression doesn't really mean anything. A reduction in the rate of ocean interior circulation will always lead to a reduction of the ocean's interior oxygen field. I therefore suggest to write this more straightforwardly, i.e. that the changes appear to be driven primarily by changes in ocean circulation, and less by changes in the rate of oxygen demand from downward settling organic matter and DOM.</p> <p>[Nicolas Gruber (Reviewer's comment ID #: 307-31)]</p> | Accepted, text clarified |
| 5-362 | A | 21:49 | 21:49 | <p>reduced apparent CFC age -> increased apparent CFC age ?</p> <p>[Michio Kawamiya (Reviewer's comment ID #: 124-31)]</p> | Accepted partly,, changed to "changed" CFC ages |
| 5-363 | A | 21:54 | 21:54 | <p>Comment on sentence in section 5.4.3: Where it says "Recent data in the Indian Ocean have shown a reversal of the O2 decrease between 1987 and 2002" What is the magnitude of this reversal? (see comment 6)</p> <p>[Hernan Garcia (Reviewer's comment ID #: 81-8)]</p> | Accepted, magnitude is enough to cancel out the earlier decrease and stated in the text. |
| 5-364 | A | 21:55 | 21:55 | <p>Comment on sentence in section 5.4.3: where it says "as well as large variability"...The term "large" is relative (vague). What is the magnitude of these "large" O2 concentration changes ("large" when compared to what)? Is the change in O2 concentration representative of the N. Atlantic? What years?</p> <p>[Hernan Garcia (Reviewer's comment ID #: 81-9)]</p> | Accepted, information added relative to long term trends. |

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| 5-365 | A | 22:1 | 22:4 | <p>Comment on paragraph in section 5.4.3: I had difficulties understanding this paragraph. I suggest deleting this entire paragraph (i.e., lines 1 through 4 in page 22) for several reasons. First, the O2 and AOU changes outlined elsewhere in section 5.4.3 (cited references) refer to O2 and AOU local changes in concentration (i.e., differences in concentration between one time period and another time period from individual hydrographic sections) whereas the Garcia et al., (2005; thereafter G05) describes depth-integrated anomaly changes (inventories) of O2, AOU, and Heat as a function of time based on 5-year composite global climatologies where the seasonal climatological mean has been removed. To my mind, local changes in O2 concentration based on differences between measurements from hydrographic sections collected several years apart or measurements collected at time series stations at specific geographic locations cannot be easily compared to O2 concentration changes in basin or global inventories. Second, the quoted decadal variation in O2 concentration of "0.5 mol/kg" in line 1 of page 22 is clearly incorrect. To be sure, the G05 paper outlines that linear trends in O2, AOU, and Heat are all time dependent and vary as a function of depth as well as latitude. The global linear trends (1955-1998) in O2 concentration changes for individual depth layers in the 0-100 m depth range is about ±0.05 to 0.1 µmol/kg per year (Figure 2 of G05). The trends for the 1983-1998 and 1955-1998 composite periods are different in magnitude and sense (Figure 3 of G05). These linear trends are superimposed on peak-to-peak variations equivalent to up to ±4 µmol/kg as is the case in the Northern Hemisphere for this depth layer (Figure 3 of G05). The statement that "Surface AOU changes are damped by the equilibration with the atmosphere and thus are smaller than the changes in the intermediate waters" is relative. All oceanic layers are "damped" by vertical and horizontal mixing. Any measurable change in O2 (or AOU) concentration at intermediate depths must have been originated at the surface (preformed values). My point here is that the content described in this paragraph in section 5.4.3 is not useful or correct. Thus, I suggest deleting this paragraph entirely (See also comment 11). In fact, I do not find it useful to include the G05 paper in this section.</p> <p>[Hernan Garcia (Reviewer's comment ID #: 81-10)]</p> | Accepted partly. We reduced paragraph but did not remove it entirely because the fact that we cannot fully explain the surface O2 variations is because the processes are opposing one another, and it is still relevant information. |
| 5-366 | A | 22:6 | 22:14 | <p>Comment on paragraph in section 5.4.3: I had difficulties understanding the content of this paragraph. This entire paragraph (lines 6-14 on page 22) should be deleted or re-written including factual information helpful in assessing the magnitude of the O2 and AOU changes in concentration described elsewhere in section 5.4.3. For example, this paragraph could describe (1) the lack of O2 or use of certified reference materials (CRM) that would allow the comparison of historical or modern O2 measurements as is presently the case for salinity and CO2 measurements and (2) that the local changes in sub-surface O2 and AOU concentration described in this section 5.4.3 at specific geographic locations are difficult to extrapolate to basin or global scales, and (3) that a basin- to global-</p> | Accepted partly. We reduced paragraph but did not remove it entirely because the fact that we cannot fully explain the surface O2 variations is because the processes are opposing one another, and it is still relevant information. |

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| | | | | assessment study of O2 changes is clearly needed. The lack of O2 CRM is particularly relevant because reproducibility (intralaboratory), precision, and accuracy are terms not discussed in section 5.4.3. Since there are no O2 CRM presently in use now or in the past, it is difficult to quantify potentially important calibration problems in the O2 measurements of the pre-, during-, or post-WOCE field sampling eras. For example, line 10 of page 22 of section 5.4.3 states that "the O2 accuracy in the early decades is difficult to determine". I think that the authors meant "reproducibility" rather than "accuracy" in this sentence. Nevertheless, lacking quantitative knowledge of an accepted set of oceanic O2 reference measurements (\pm errors) as function of depth (density) for a given time period and geographic locations (a difficult task given the size and coverage of the available high-quality O2 data) and the unavailability of O2 CRM, I would argue that there is no meaningful way at present to quantify the accuracy of older or modern oceanic O2 measurements. As stated in line 12 of this paragraph that "we have little confidence in the early measurements" is not useful or correct (the authors should cite peer-reviewed references that support this statement). A number of authors have described estimates of reproducibility and systematic adjustments in the modern (i.e., post-1980s) O2 instrumental record (i.e., Saunders, 1986; Broecker et al., 1985; Johnson et al., 2001; Gouretski and Jancke, 1996; Gouretski and Jancke, 1999; Gouretski and Jancke, 2001; Garcia et al., 1998; Johnson and Gruber, 2006; and others). If the authors do not want to re-write this paragraph, then I suggest deleting this paragraph entirely (See also comment 10). [Hernan Garcia (Reviewer's comment ID #: 81-11)] | |
| 5-668 | B | 22:6 | 22:14 | Changes in upper ocean AOU are in my opinion virtually uninterpretable. I therefore think that a paragraph is wasted here. I would simply ignore this and focus instead on a more detailed discussion, for example, of the results of Deutsch et al. [Nicolas Gruber (Reviewer's comment ID #: 307-32)] | Accepted partly. We reduced paragraph but did not remove it entirely because the fact that we cannot fully explain the surface O2 variations is because the processes are opposing one another, and it is still relevant information. |
| 5-367 | A | 22:16 | 22:16 | Comment on title of section 5.4.4: I suggest that the title of this section should be "dissolved inorganic nutrients" or "inorganic nutrients" to avoid confusion with dissolved organic nutrients (i.e., DON, DOP which are not discussed in this section). [Hernan Garcia (Reviewer's comment ID #: 81-12)] | Rejected – we included clarification in main text but not in title. |
| 5-368 | A | 22:16 | | Section 5.4.4.: The purpose of this chapter is not evident for us. In our opinion eutrophication is the main reason for changes in nutrient concentration especially in coastal regions. It is unclear whether this chapter addresses these trends for the whole oceans or parts thereof. [Govt. of Germany (Reviewer's comment ID #: 2011-12)] | Noted. Our focus is on global ocean changes, not on coastal issues. Nutrient changes are of interest because they can be related to both carbon cycle and ocean circulation changes, this has bee |

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| | | | | | clarified. We do not discuss eutrophication changes because this is a WGII issue.. |
| 5-369 | A | 22:18 | 22:29 | Comment on paragraph in section 5.4.4: There is a geographic bias in this section on changes in "nutrient" concentrations in the Pacific Basin. What about nutrient concentration changes in the Atlantic or Indian Oceans? There are a number of potentially useful references available. [Hernan Garcia (Reviewer's comment ID #: 81-14)] | Rejected – information exists primarily for the Pacific ocean |
| 5-370 | A | 22:19 | 22:19 | Comment on sentence in section 5.4.4: What is meant by "the concentration of N and P" in this sentence? Do N and P refer only to nitrate+nitrite and phosphate? See also comment 12 [Hernan Garcia (Reviewer's comment ID #: 81-13)] | Accepted, text clarified |
| 5-371 | A | 22:31 | 22:32 | Comment on sentence in section 5.4.4: Where it says "In some cases" should that be "In some cases"? At the end of this sentence where it says "(Pahlow and Riebesell, 2000: Emerson 2001 #167),," there are some obvious typos (i.e., delete #167) and extra commas (" ,,"). 179 5-179 15 [Hernan Garcia (Reviewer's comment ID #: 81-13)] | Accepted, text clarified |
| 5-372 | A | 22:32 | 22:32 | suppress reference after Emerson, 2001 [Pascale DELECLUSE (Reviewer's comment ID #: 58-14)] | Accepted |
| 5-373 | A | 22:34 | 22:34 | remove extra "." [Melissa Bowen (Reviewer's comment ID #: 28-22)] | Accepted |
| 5-374 | A | 22:34 | 22:34 | suppress ".." [Pascale DELECLUSE (Reviewer's comment ID #: 58-15)] | Accepted |
| 5-375 | A | 22:34 | 22:34 | Comment on sentence in section 5.4.4: Remove extra period after "...Watanabe et al, 2005) and before "Thus all trends..." 180 5-180 16 [Hernan Garcia (Reviewer's comment ID #: 81-15)] | Accepted |
| 5-669 | B | 22:37 | 23:2 | Section 5.4.5: Biological changes: I think this section should be expanded a bit. I mentioned some higher trophic level studies above. But even the timeseries stations HOT and BATS have revealed quite important shifts in ecosystem structure (the "changing seas" story of Dave Karl from HOT, for example). I can also think of the recent occurrence of large coccolithophorid blooms in the Bering Sea. There are likely many additional studies if one searches a bit deeper. In my opinion, this is something that people care much about it, and IPCC should try to cover this as well as it can. [Nicolas Gruber (Reviewer's comment ID #: 307-33)] | Rejected – this is the work of WGII. We clarified that we focus on biological changes relevant to biogeochemistry |
| 5-376 | A | 22:44 | 44:52 | It seems that you are suggesting that a real decrease in biological productivity of 6% occurred between early 1980s and late 1990s, and a real increase of 10% between 1997 | Accepted – text clarified |

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| | | | | and 2000. Or are you suggesting that different sensors (which happen not to be completely overlapping in time) give different trends? Clarify what your point is, and whether you think that there is real short-term variability that could initially makes any longterm trends. [Danny Harvey (Reviewer's comment ID #: 101-33)] | |
| 5-377 | A | 22:47 | 22:47 | North" and "South" should be "northern" and "southern" [Melissa Bowen (Reviewer's comment ID #: 28-23)] | Accepted, wording has been changed |
| 5-378 | A | 22:47 | | should be "Southern" [Danny Harvey (Reviewer's comment ID #: 101-32)] | Accepted |
| 5-379 | A | 23:0 | | In Section 5.5 "Changes in Sea Level", it is worth the effort to put together estimations from different researchers based on data from tide gauges and satellite altimetry, in order to explain the large amount of results found on literature. However, it is still uncertain whether sea rise indicates an accelerated trend or whether it is associated with variability in decadal sea level trends. [Govt. of Chile (Reviewer's comment ID #: 2005-8)] | Noted. We have made clear that the 20th century has higher sea level rise than previous centuries. |
| 5-380 | A | 23:5 | 23:27 | This paragraph bothers me, because there seems to be an assumption that trends over a long period should be qualitatively similar to trends over a shorter period. But the trends over different (say) 10-year periods will be quite different, depending on what decadal events are included in the period. For example, if there was a large El Nino at the start of the 10-year period we would have large negative trends in the eastern tropical pacific, and large positive trends in the western tropical pacific. And vice versa if the El Nino event was at the end of the 10-year period, and the trends would all be close to zero if there was one El Nino event close to the centre of the period. Trends over a longer period will contain some (probably non-integral) number of decadal events and we should not expect the trends over this longer period to bear any particular relationship to trends over a shorter period. [Neil White (Reviewer's comment ID #: 286-3)] | Noted. The focus of the biogeochemistry section is on long-term trends. Obviously, all parameters are subject to variability on all time scales, but this is not different from the physical parameters. Nevertheless, we are confident about our conclusions on long-term changes. |
| 5-670 | B | 23:6 | 23:7 | As in my comment #17 above, I think it is important to stress here again the difference between anthropogenic and natural changes in DIC and air-sea CO2 fluxes. [Nicolas Gruber (Reviewer's comment ID #: 307-34)] | Accepted |
| 5-381 | A | 23:22 | | if ocean circulation 'what' persists into the future? [David Rind (Reviewer's comment ID #: 214-55)] | Accepted text clarified |
| 5-382 | A | 23:22 | | If ocean circulation, 'what' persists into the future? [Govt. of United States of America (Reviewer's comment ID #: 2023-343)] | Accepted, text clarified |
| 5-671 | B | 23:25 | 23:34 | I suggest to delete this paragraph. As mentioned above, the surface ocean O2 changes are extremely difficult to interpret. What is the benefit of wasting precious space to essentially argue that we haven't learned anything? | Accepted, text reduced |

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| | | | | [Nicolas Gruber (Reviewer's comment ID #: 307-35)] | |
| 5-383 | A | 23:25 | :27 | Why do circulation changes affect oxygen more than temperature? Changes in advection due to circulation variability could affect oxygen more but because of geostrophy it is not clear that temperature is changed more by gyre changes for example. Fact needs to be verified. [Govt. of United States of America (Reviewer's comment ID #: 2023-344)] | Accepted, text clarified |
| 5-384 | A | 23:29 | 23:29 | Section 7.3.2.2 -> 7.3.4.4 ? [Michio Kawamiya (Reviewer's comment ID #: 124-32)] | Accepted |
| 5-385 | A | 23:32 | 23:32 | East" should be "east [Melissa Bowen (Reviewer's comment ID #: 28-24)] | Accepted |
| 5-386 | A | 23:39 | | As stated above, a box indicating the significance of the small sea level changes to features that can be comprehended such as the rate of flooding of specific coastal areas or islands is needed to put these numbers in perspective. [Govt. of United States of America (Reviewer's comment ID #: 2023-345)] | Reject: This is the area of the WG2 Note there is a box in the Technical Summary. |
| 5-387 | A | 23:44 | 23:44 | Delete "in response to global warming" [VINCENT GRAY (Reviewer's comment ID #: 88-588)] | done |
| 5-388 | A | 23:47 | 23:47 | "Section 6.3.3" should read "Section 6.4.3". [John Hunter (Reviewer's comment ID #: 112-3)] | done |
| 5-389 | A | 23:52 | 23:52 | "thermal expansion which is the largest effect": this is not true for 1961-2003, when thermal expansion was small compared with ice melt (see table 5.5.2 and Figure 5.5.9). [John Hunter (Reviewer's comment ID #: 112-13)] | corrected |
| 5-390 | A | 24:7 | 24:7 | Insert after "signal". "Also the measurements are biased by their predominant presence near large ports and in the Northern Hemisphere. The measurements are influenced by removal of ground water, oil and minerals, and by the effects of severe storms on measuring equipment>" [VINCENT GRAY (Reviewer's comment ID #: 88-589)] | Rejected: the basis for the insertion of this sentence is not supported by the comment or by appropriate references, we already discuss vertical land motions and rejection of tide gauges for known or suspected vertical land motions in making global estimates of sea-level. |
| 5-391 | A | 24:7 | 24:10 | I do not understand why the GIA adjustment for satellite altimetry is described as "small". Figure 5.5.2 indicates that the GIA adjustment for the global average sea level derived from the altimeters is 0.3 mm/year, while inspection of Douglas (1997; Surveys in Geophysics, 18, 279-292) shows that the average GIA he applied to 24 tide gauge records was 0.35 mm/year. So the GIA adjustments for altimeters and tide gauges are of similar magnitude (although resulting from different processes). I therefore also disagree with the statement "Sea level change based on satellite altimetry is not distorted by land | (1) 0.3mm/yr is small compared to 3.1 mm/yr (2) the sentence has beenn rephrased |

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| | | | | motions". [John Hunter (Reviewer's comment ID #: 112-18)] | |
| 5-392 | A | 24:9 | 24:10 | GIA doesn't need to be spelled out here again, because you spelled it out above (page 23, line 56). [Melinda Marquis (Reviewer's comment ID #: 162-16)] | done |
| 5-393 | A | 24:9 | 24:10 | Where altimetry is corrected on the basis of T/G data there must be some GIA contamination at least to the extent that the T/G data are also affected. [Michael Tsimplis (Reviewer's comment ID #: 268-7)] | The altimetry curve is not corrected on the basis of TG data |
| 5-394 | A | 24:14 | 24:14 | I do not believe that the "0.67 mm/year" was strictly a "median value". In the TAR, it is called a "central value". I think it is actually a MEAN value. [John Hunter (Reviewer's comment ID #: 112-14)] | corrected |
| 5-395 | A | 24:16 | 24:16 | Replace "1.5±0.5" with "1.5±1.0", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-590)] | Rejected: already a confidence interval. |
| 5-396 | A | 24:17 | 24:17 | add "...as large as the TAR's estimate of climate-related contributions." [Vivien Gornitz (Reviewer's comment ID #: 87-1)] | Done |
| 5-397 | A | 24:17 | 24:19 | The caveat on this sentence is that the uncertainties in the two estimates (predominantly the uncertainty in the "budget" estimate) was so large that the "budget" estimate and the "observational" were still consistent. Therefore there is no strict statistical reason why we can say that the values disagreed -- we may well believe that they do and, if so, this must be due to some other reason -- perhaps this reason (or reasons) should be given here. [John Hunter (Reviewer's comment ID #: 112-15)] | Noted. Sentence has been changed. We have not discussed the TAR in detail. |
| 5-398 | A | 24:17 | 24:17 | the TAR's estimate. Should it say "the TAR's estimate of the various contributions"? [Govt. of United Kingdom (Reviewer's comment ID #: 2022-27)] | Done |
| 5-399 | A | 24:18 | 24:18 | Insert after "rise" "or the influence of groundwater and mineral removal and weight of buildings" [VINCENT GRAY (Reviewer's comment ID #: 88-592)] | The comment is unclear and no change has been made |
| 5-400 | A | 24:19 | 24:19 | Replace "higer" with "higher" [VINCENT GRAY (Reviewer's comment ID #: 88-591)] | Done |
| 5-401 | A | 24:33 | 29:7 | 5.5.2 Observations in Sea level Change. There is an inconsistent message in this section about sea level rise acceleration. Sec 5.5.2.4 (p26 ln 50-52 states that interannual variability (and inadequate network density to a lesser extent) are major reasons why acceleration cannot be identified using 20th C data alone. The most marked acceleration prior to 1993 -2003 was in the period 1870 to 1900. Since then there has not been a steady rise - in fact there has been one or two decadal periods (figure 5.5.6) when no rise has been observed. Thus we have an issue with the terminology used when comparing rates of sea level change between the 19th and 20th centuries. Similar to our comment in sec | Noted. We have now increased the discussion of variability in sea level change for the last 50 years. Also we have clearly stated that there has been an increase in the rates between mid-19 th and mid-20 th centuries. |

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| | | | | 5.1 and 5.2, rather than an acceleration in sea level rise (which implies a consistent change) what has been observed is a 'change in the rate of change in sea level', when comparing the records for the two centuries. [Govt. of Australia (Reviewer's comment ID #: 2001-287)] | |
| 5-402 | A | 24:45 | 24:45 | Replace "1.8±0.3" with "1.8±0.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-593)] | Reject. The number 0.3 is quoted from the publication and is meant to be a 95% confidence limit. In our assessment, we have used 90% confidence limits but kept this number to be on the safe side. |
| 5-403 | A | 24:46 | 24:47 | Church and White (2006) should be briefly described, prior to it being introduced in Figure 5.5.1. [John Hunter (Reviewer's comment ID #: 112-17)] | Accept, the description is given in section 5.5.2.3 |
| 5-404 | A | 24:49 | 24:49 | Replace "1.8±0.5" with "1.8±1.0", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-594)] | Reject: see comment 5-46 |
| 5-405 | A | 24:54 | 24:54 | Replace "consensus" with "opinion" [VINCENT GRAY (Reviewer's comment ID #: 88-595)] | Reject- no reason given for suggested change |
| 5-406 | A | 24:56 | 24:56 | Delete "significantly" This word has a statistical connotation not presebnt here [VINCENT GRAY (Reviewer's comment ID #: 88-596)] | accept |
| 5-407 | A | 25:1 | 25:6 | The models require further validation in many areas and this needs to be acknowledged. [Donald L. Forbes (Reviewer's comment ID #: 72-4)] | accept, this is now done at the end od 5.5.2.1 |
| 5-408 | A | 25:3 | 25:5 | I noted a problem with a similar sentence in the first order draft and it does not appear to have been fixed. As far as I understand Peltier (2001), I believe that the discrepancy of "several tenths of mm/year" is not related to a difference of method (i.e. models versus geological inference), but rather to a difference of averaging time scale (geological estimates are generally over a longer time scale). [John Hunter (Reviewer's comment ID #: 112-4)] | Sentence removed |
| 5-409 | A | 25:4 | 25:5 | The underestimate in adjusted rates cited by Peltier (2001) arises from applying a linear extrapolation to the geological data, rather than GIA being the only geological process involved. [Vivien Gornitz (Reviewer's comment ID #: 87-2)] | Sentence removed |
| 5-410 | A | 25:8 | 25:9 | Suggest change in wording to read: Although some model validation has been achieved, systematic problems with such techniques, including short data spans, have yet to be fully resolved. [Donald L. Forbes (Reviewer's comment ID #: 72-5)] | accept see comment 5-407 |
| 5-411 | A | 25:9 | 25:9 | Add at end "There have been, so far, very few corrections for land subsidance due to | Not done: tide gauge analyses take into |

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| | | | | removal of groundwater and minerals, and by the weight of buildings" [VINCENT GRAY (Reviewer's comment ID #: 88-597)] | account the tectonic location for global sea-level rise. |
| 5-412 | A | 25:12 | 25:12 | Delete "precision" There have been many arguments about the accuracy of these measurements [VINCENT GRAY (Reviewer's comment ID #: 88-598)] | Done |
| 5-413 | A | 25:12 | 25:13 | Delete "although the road to success was paved by" We don't need this purple prose. [VINCENT GRAY (Reviewer's comment ID #: 88-599)] | Done |
| 5-414 | A | 25:13 | 25:13 | Replace "and" by "gave way to" [VINCENT GRAY (Reviewer's comment ID #: 88-600)] | Accept, sentence deleted |
| 5-415 | A | 25:14 | 25:14 | Delete "ushered in a new paradigm in satellite altimetry, largely" More purple prose.. [VINCENT GRAY (Reviewer's comment ID #: 88-601)] | Done |
| 5-416 | A | 25:15 | 25:15 | Delete "launch of the" sounds like a battleship? [VINCENT GRAY (Reviewer's comment ID #: 88-602)] | Done |
| 5-417 | A | 25:15 | 25:15 | Replace "mission" by "series" Who are you trying to convert? [VINCENT GRAY (Reviewer's comment ID #: 88-603)] | Done |
| 5-418 | A | 25:15 | | "Jason" should be "Jason-1" This is (a) the usual designation and (b) Jason-2 is planned [Neil White (Reviewer's comment ID #: 286-2)] | Done |
| 5-419 | A | 25:16 | 25:16 | Delete "precision" But tell us how much. [VINCENT GRAY (Reviewer's comment ID #: 88-604)] | Done |
| 5-420 | A | 25:16 | 25:16 | Delete "seamlessly" It is not an embroidery [VINCENT GRAY (Reviewer's comment ID #: 88-605)] | Done |
| 5-421 | A | 25:17 | 25:17 | Replace "is a reasonably straightforward exercise if" by "requires" [VINCENT GRAY (Reviewer's comment ID #: 88-606)] | done |
| 5-422 | A | 25:18 | 25:18 | Delete "is" [VINCENT GRAY (Reviewer's comment ID #: 88-607)] | Done |
| 5-423 | A | 25:20 | 25:20 | "10 day" should be "10-day". [Chiu-Ying LAM (Reviewer's comment ID #: 139-6)] | Done |
| 5-424 | A | 25:22 | 25:22 | "10 day" should be "10-day". [Chiu-Ying LAM (Reviewer's comment ID #: 139-7)] | Done |
| 5-425 | A | 25:24 | 25:24 | Replace "+3.1±0.8" with "+3.1±1.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-608)] | No : the 0.8 mm/yr uncertainty is already 2 sigmas |
| 5-426 | A | 25:25 | 25:25 | The overall rise and fall of global sea level seems appreciably less than 15mm in Fig 5.5.2, at least from the smoothed curve. "Global" should be added in front of "mean sea level". [Chris Folland (Reviewer's comment ID #: 71-188)] | accept, sentence deleted |

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| 5-427 | A | 25:25 | 25:27 | Make clear that these are steric effects (or otherwise). [Donald L. Forbes (Reviewer's comment ID #: 72-6)] | sentence deleted |
| 5-428 | A | 25:31 | 25:32 | Delete from "The accuracy needed" on line 31 to "and thus" on line 32. Redundant comment [VINCENT GRAY (Reviewer's comment ID #: 88-609)] | reject – we believe the comment is not redundant |
| 5-429 | A | 25:39 | 25:39 | Delete "considered to be extremely robust" to "sea level rise of" An unnecessary statement [VINCENT GRAY (Reviewer's comment ID #: 88-610)] | Done |
| 5-430 | A | 25:39 | 25:39 | Replace "+3.1±0.8" with "+3.1±1.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-611)] | No : the 0.8 mm/yr uncertainty is already 2 sigmas |
| 5-431 | A | 25:40 | 25:40 | Delete "is reliable within these error bars" I should hope so! [VINCENT GRAY (Reviewer's comment ID #: 88-612)] | Done |
| 5-432 | A | 25:42 | 25:42 | Delete "Note that" [VINCENT GRAY (Reviewer's comment ID #: 88-613)] | Done |
| 5-433 | A | 25:43 | 25:46 | The sense of the adjustment is unclear. Please be explicit. [Donald L. Forbes (Reviewer's comment ID #: 72-7)] | Done |
| 5-434 | A | 25:44 | 25:44 | Replace "plausible" by "possible" [VINCENT GRAY (Reviewer's comment ID #: 88-614)] | Done |
| 5-435 | A | 25:45 | 25:45 | "This number". Which number is that, "-0.3" or "+0.15"??? [VINCENT GRAY (Reviewer's comment ID #: 88-615)] | Clarified |
| 5-436 | A | 25:48 | 25:48 | Delete "An important result of" [VINCENT GRAY (Reviewer's comment ID #: 88-616)] | Done |
| 5-437 | A | 25:48 | 25:48 | Replace "is" by "allows" [VINCENT GRAY (Reviewer's comment ID #: 88-617)] | Done |
| 5-438 | A | 25:48 | 25:49 | It seems to me important in this paragraph to make the point that these are only decadal long trends--not an early sign of what might be long-term local trends. My guess is that these have been heavily influenced by the 1997-98 El Nino effects and so great care should be taken in interpreting them--and indeed in comparing them to long-term trends. For example, there is no reason to be expecting sea level off the west coast of North America to continue to drop through the 21st century, and this point needs to be made clearly so analysts do not just go off and multiple the local rate in Figure 5.5.3a by 10 to get the century long trend. [Michael MacCracken (Reviewer's comment ID #: 152-260)] | Clarified |
| 5-439 | A | 26:0 | | Section 5.5.2.4: it is strange that a section entitled "Interannual/decadal variability and recent accelerations in sea level" should omit a reference to Church and White (2006), even though the paper is referenced in Figure 5.5.1. Perhaps this is just a matter of | Title changed |

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| | | | | definition of "recent", as the Church and White paper is noted in Section 5.5.2.5 ("Long term sea level change"). [John Hunter (Reviewer's comment ID #: 112-20)] | |
| 5-440 | A | 26:1 | 26:1 | Replace "signature" by ":influence" [VINCENT GRAY (Reviewer's comment ID #: 88-619)] | Done |
| 5-441 | A | 26:11 | 26:39 | Present long term information before the information on shorter time-scales. [Govt. of Australia (Reviewer's comment ID #: 2001-288)] | Not done: no clear argument to do this |
| 5-442 | A | 26:19 | 26:19 | Delete "however" [VINCENT GRAY (Reviewer's comment ID #: 88-620)] | reject-no reason given for suggested change |
| 5-443 | A | 26:23 | 26:23 | Replace "trends" by "changes" [VINCENT GRAY (Reviewer's comment ID #: 88-621)] | reject-no reason given for suggested change |
| 5-444 | A | 26:23 | 26:23 | I didn't understand the phrase "(and the implied global correlations)" in the first-order draft. I still don't understand it in the second-order draft. [John Hunter (Reviewer's comment ID #: 112-5)] | accept – text modified |
| 5-445 | A | 26:25 | 26:35 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-622)] | No change made since a linear trend has been indeed computed at each grid mesh |
| 5-446 | A | 26:33 | 26:33 | "The results help reconcile...". I think the reader needs more help to do this reconciliation. [Pascale DELECLUSE (Reviewer's comment ID #: 58-16)] | Clarified |
| 5-447 | A | 26:41 | 27:25 | 3 different paragraphs to give details of 3 different time scales may be joined in a single one. The reconstruction on 2000 years is not crucial. [Pascale DELECLUSE (Reviewer's comment ID #: 58-19)] | Done |
| 5-448 | A | 26:41 | 27:5 | 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. [Neil White (Reviewer's comment ID #: 286-4)] | Noted- however paper not referred to for space reasons. |
| 5-449 | A | 26:41 | 27:5 | The recent acceleration in sea level has been talked about for some time (see, e.g. 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.). [Neil White (Reviewer's comment ID #: 286-5)] | see previous comment. |
| 5-450 | A | 26:41 | 27:25 | Sections 5.5.2.4 and 5.5.2.5 seem to be a bit mixed up. 5.5.2.5. has acceleration in the title and talks a bit about acceleration, then there is more talk about acceleration in 5.5.2.5. Perhaps these sections could be reorganised or merged? [Neil White (Reviewer's comment ID #: 286-6)] | Accept. We have modified the text, and changed the section headings |
| 5-451 | A | 26:41 | 27:5 | Please delete comment 4 - the one containing only the Maul and Martin reference - I pasted it in, but the cell now seems to be write-protected??? | Noted and taken into account, see above |

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| | | | | [Neil White (Reviewer's comment ID #: 286-7)] | |
| 5-452 | A | 26:44 | 26:44 | "good" correlation - significant? [Govt. of Australia (Reviewer's comment ID #: 2001-289)] | Done |
| 5-453 | A | 26:46 | 26:48 | I would recommend to suppress the sentence on large volcanic eruptions because it is a model result suggestion, in a part dedicated to observatons [Pascale DELECLUSE (Reviewer's comment ID #: 58-17)] | Not done : it is not the only place where processes are mentioned |
| 5-454 | A | 26:46 | 26:46 | typo error: "in the in altimetric" --> "in the altimetric" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-16)] | Done |
| 5-455 | A | 26:50 | 26:50 | I am not sure that I entirely agree with this sentence. Holgate and Woodworth (2004) used data from 1993 to 2002 and Church et al. (2004) used data from 1950-2000: neither detected an acceleration. However, Church and White used data from 1870 to 2004 and did detect a significant acceleration: 0.013 +/- .006 mm/year squared. Using only data after 1900, they found a weaker acceleration: 0.008 +/- 0.008, but one that is still significant at the 95% level. Perhaps I am being over-critical, but I think this sentence needs a little rewording in light of the most recent results. [John Hunter (Reviewer's comment ID #: 112-21)] | Rejected. We have explicitly stated that there is an acceleration when data from the 19 th century are included. The results by Church and White for the 20 th century data alone is barely significant and not included |
| 5-456 | A | 26:54 | 27:5 | I really think that for these suggestions that the present rate might just be a natural variation, especially as they are being carried forward to the summary, more needs to be said about their likelihood and basis, how good the coverage was, etc. After all, the satellite rate is fully global and its causes identified, whereas, given that there is not enough explanation for even the average century rate of 1.5 mm/yr, how would one support a tide gage estimate of 3 mm/yr when the terms add up to much less? [Michael MacCracken (Reviewer's comment ID #: 152-261)] | Taken into account: natural variability on ten year timescales is presented much more strongly and reflected into the Executive Summary. |
| 5-457 | A | 27:7 | 27:25 | Although I am probably not the one who should be promoting this argument, I feel I must comment on the exclusion of the historic sea-level observations from Port Arthur (Hunter, J., Coleman, R. and Pugh, D., 2003. GRL, 30, 7, 1401, doi:10.1029/2002GL016813) from the second-order draft, after it had been included in the first-order draft. I feel strongly that reference to this work should be included, for a number of reasons. Firstly, I believe the Port Arthur observations represent the earliest tide-gauge measurements related to a STILL-EXISTING benchmark anywhere in the world, and certainly the earliest surviving tide-gauge measurements in the Southern Hemisphere. Secondly, the estimation of mean sea level at Port Artur in 1841, when compared with estimates of 20th sea level rise from the long Australian records (Fremantle and Fort Denison), gives a clear indication of an acceleration in sea-level rise prior to the 20th century. [John Hunter (Reviewer's comment ID #: 112-6)] | Noted. The Port Arthur time series is indeed an excellent example of a long-term record. However, we have focussed on the global mean sea level. Lack of space has prevented us to deal with individual stations, |
| 5-458 | A | 27:7 | 27:25 | (Long comment continued) Thirdly, I believe that the results from Port Arthur were misused by those sceptical of sea-level rise (and of the whole issue of anthropogenic | see previous comment |

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| | | | | global warming) to "show" that global sea level has not indeed risen -- this case was promoted widely in the media and on the Internet; to omit reference to this work would, I feel, suggest that their claims were correct. [John Hunter (Reviewer's comment ID #: 112-7)] | |
| 5-459 | A | 27:8 | 27:8 | The use of the word "accelerations" is confusing. Perhaps consider rewording. [Govt. of Australia (Reviewer's comment ID #: 2001-290)] | text reformulated |
| 5-460 | A | 27:8 | 27:8 | correct "American" [Pascale DELECLUSE (Reviewer's comment ID #: 58-18)] | done |
| 5-461 | A | 27:12 | 27:12 | Replace "a significant" with "an" [VINCENT GRAY (Reviewer's comment ID #: 88-623)] | Accept, text changed significantly |
| 5-462 | A | 27:12 | 27:12 | Replace "1.3±0.6" with "1.3±1.2", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-624)] | reject-see comment 5-46 |
| 5-463 | A | 27:24 | 27:24 | Delete "significantly" [VINCENT GRAY (Reviewer's comment ID #: 88-625)] | Done |
| 5-464 | A | 27:27 | 27:32 | Again I think that removing the Mediterranean Sea is making the report poorer, Arguably more people live I the Mediterranean coasts than the Arctic and a better understanding and linkage with the oceanography has been achieved in the Mediterranean Sea. Laving it as it is it creates the impression that either not significant progress has been made or the behaviou of sea level in the Mediterranean is not understood. None of these is correct. It is also misleading in respect of projections for the Mediterranean Sea as these do not include a number of very important (for sea level) physical processses thus making the projections unreliable, [Michael Tsimplis (Reviewer's comment ID #: 268-8)] | Noted. The material on the Mediterranean sea level has been deleted. Because of length restrictions we had to focus on global sea level. |
| 5-465 | A | 27:27 | 28:26 | In the Japan (East) Sea 9 year-long TOPEX/Poseidon analyses revealed average trends of 5.4+ 0.3 mm/yr for all of Japan (East) Sea and 6.6 + 0.4 mm/yr for the southern Japan (East) Sea which is much larger than the global rate reported by Cabanes et al.(2001). T/P rate was compared with 26 year-long sea level anomaly from 13 tidal stations and 40 year-long thermosteric sea level. (Kang et al., 2005, Jour. Geophy. Res., Vol. 110, No. C7, July 8) [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-17)] | Noted. Our focus in on global sea level, and it has not been possible to include interesting regional details. |
| 5-466 | A | 27:31 | 27:32 | Remove "potential" - their low elevation makes them vulnerable to sea-level rise, regardless of the cause. [Govt. of Australia (Reviewer's comment ID #: 2001-291)] | Done |
| 5-467 | A | 27:31 | | Delete "potential". Small Pacific Islands are vulnerable to future sea-level rise, if it occurs. It is the sea-level rise, rather than the vulnerability, that is a potential. [Adrian Simmons (Reviewer's comment ID #: 242-91)] | Done |

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| 5-468 | A | 27:35 | 27:44 | Indicate length of records & also magnitude of NE Atlantic sea-level change. [Govt. of Australia (Reviewer's comment ID #: 2001-292)] | Noted. The NE-Atlantic section has been shortened to focus on variability only. |
| 5-469 | A | 27:36 | 27:37 | Not strictly GIA. [Donald L. Forbes (Reviewer's comment ID #: 72-8)] | Accept, GIA reference removed and text shortened |
| 5-470 | A | 27:37 | 27:39 | Reference needed. This would be a good figure. [Donald L. Forbes (Reviewer's comment ID #: 72-9)] | Reference given, text shortened. |
| 5-471 | A | 27:54 | 27:56 | Should this be nearly half of the variability? [Donald L. Forbes (Reviewer's comment ID #: 72-10)] | Sentence deleted |
| 5-472 | A | 28:0 | 29:7 | Not just Cartonl et al. reproduced the observed trend in sea level satisfactorily. The same holds for ECCO and other results. However, it is limited to thermosteric contributions. All estimates appear to have problems with the freshwater component. Estimates of absolute SSH increase are therefore difficult to obtain currently until the freshwater cycle was improved. [Detlef Stammer (Reviewer's comment ID #: 251-6)] | Accept, a reference to recent ECCO results has been added. |
| 5-473 | A | 28:2 | 28:26 | It may be too late, but a better reference to cover much of this material is a paper which has now been accepted by Global and Planetary Change: Church, J.A., White, N.J. and Hunter, J.R., Sea-level rise at tropical Pacific and Indian Ocean islands. This would also save referring to Hunter (2004) which was in Energy & Environment (a social science journal -- the Hunter reference is only a comment) and which I know for a fact was not peer-reviewed. [John Hunter (Reviewer's comment ID #: 112-8)] | added |
| 5-474 | A | 28:9 | 28:9 | Add at end "And the absence of allowances for removal of ground water, weight of buildings (such as air bases or luxury hotels) and the effects of cyclones" [VINCENT GRAY (Reviewer's comment ID #: 88-626)] | Vertical land motions include anthropogenic effects; no change made |
| 5-475 | A | 28:18 | 28:18 | Reference to Chapter 3, as well as Folland regarding recent changes in ENSO events? [Govt. of Australia (Reviewer's comment ID #: 2001-293)] | Reference to chapter 3 included |
| 5-476 | A | 28:20 | 28:20 | Replace "1.9±0.8" with "1.9±1.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-627)] | Rejected: are 95% confidence intervals |
| 5-477 | A | 28:20 | 28:20 | There is no reference to support the rate of sea level rise given ("1.9 +/- 0.8 mm/year"). [John Hunter (Reviewer's comment ID #: 112-22)] | Accept, reference to Church et al 2006 has been included |
| 5-478 | A | 28:22 | 28:22 | Sea-level change on the atoll islands of Tuvalu ... [Donald L. Forbes (Reviewer's comment ID #: 72-11)] | Done |
| 5-479 | A | 28:26 | 28:26 | Replace "1.2±0.8" with "1.2±1.6", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-628)] | Rejected: are 95% confidence intervals |
| 5-480 | A | 28:26 | 28:26 | The uncertainty shown for the sea-level rise at Tuvalu (+/- 0.8 mm/year) is +/- 1 standard | Errors are now given as 90% errors |

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| | | | | deviation; I believe all uncertainties should be expressed as +/- 2 standard deviations, unless otherwise stated. If the latest reference (Church, White and Hunter; see comment to Ch. 8, p.28, line 2) is used, then this estimate should be revised to 2 +/- 2 mm/year. [John Hunter (Reviewer's comment ID #: 112-9)] | |
| 5-481 | A | 28:28 | | Section 5.5.2.7: As I noted in my comments on the first-order draft, this section requires a few definitions -- for example "annual maximum surge at high water" (is this the annual maximum of the difference between observed high water minus predicted high water, or is it the annual maximum observed high water? -- I presume it is the first, but this should be clarified). [John Hunter (Reviewer's comment ID #: 112-10)] | Accepted, the definition of annual maximum surge-at-high water is included Accepted |
| 5-482 | A | 28:29 | 28:29 | Useful to give an introductory outline of what causes "extreme" sea levels; presumably storm activity and tropical cyclones. [Govt. of Australia (Reviewer's comment ID #: 2001-294)] | Introductory sentence has been added |
| 5-483 | A | 28:29 | 28:29 | "impacts of sea level change..." : ambiguous start [Pascale DELECLUSE (Reviewer's comment ID #: 58-20)] | Text modified |
| 5-484 | A | 28:29 | 28:29 | This sentence has lost a word or two during editing -- it needs something like an "occur". [John Hunter (Reviewer's comment ID #: 112-11)] | Accepted (Regret the typing error) |
| 5-485 | A | 28:31 | 28:31 | I am not sure that I agree entirely with the reason: "mutidecadal records are few". Is it immediately obvious that analysis of change of extremes needs longer records than analysis of change of mean sea level? Perhaps this "reason" should be expanded. [John Hunter (Reviewer's comment ID #: 112-23)] | The text is modified. (True that "multi decadal records are few" is true for mean as well as extremes. In fact, the study of extremes is more complex than the study of mean. That is the reason, probably, people have started looking at it only recently) |
| 5-486 | A | 28:43 | 28:46 | This conclusion seems to be contradicted by the new paper by Alexander et al (2005), referenced in CH3. [Chris Folland (Reviewer's comment ID #: 71-189)] | Partly accept, the reference to the causes of change in extremes has been deleted. The Alexander paper is not referred to because it does not refer to sea level extremes. |
| 5-487 | A | 28:51 | 28:51 | change "extreme dailies" by "daily extremes" ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-21)] | Accepted (correction is incorporated) |
| 5-488 | A | 28:51 | 28:51 | What are extreme dailies? [Chris Folland (Reviewer's comment ID #: 71-190)] | Accepted (same as above) |
| 5-489 | A | 28:52 | 28:52 | typo error: "due on" --> "due to an" | Accepted (Regret typing error) |

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| | | | | [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-18)] | |
| 5-490 | A | 29:9 | | It should be pointed out that atmospheric pressure fluctuations causing SSH to change have to be regional deviations from an over-the-ocean mean. The end of the paragraph is not clear. I guess what is meant is that the estimates of global SSH are affected by the regional nature of altimetric data. The IB effect seems to have a contribution resulting in - 0.06 mm/yr. In the same paper, however, Ponte describes the fact that regional atmospheric sea level pressure pattern change on time scales of decades in NCEP and ERA 40. I have results now available that suggest that those pattern under climate scenarios change by about ¼ of global SSH increase. These are not yet submitted so not very useful for this report. In any case, long-period changes of sea level pressure can cause regional SSH to change and those changes could have some impact on coastal security. Without any specific reference this should be indicated. [Detlef Stammer (Reviewer's comment ID #: 251-7)] | While we agree with the reviewer, the role of pressure fluctuations for sea level is discussed in section 5.5.4.3, and we believe our text is clear. |
| 5-491 | A | 29:17 | 29:18 | All basins - including the Arctic? Not shown in Figure 5.2.2 and unclear in Figure 5.2.4. [Donald L. Forbes (Reviewer's comment ID #: 72-12)] | Yes; clear in Fig. 55.3 |
| 5-492 | A | 29:19 | 29:19 | Replace "0.4±01" with "0.4±0.2", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-629)] | Rejected: are 95% confidence intervals |
| 5-493 | A | 29:25 | 29:25 | Replace "0.33±0.08" with "0.33±0.16", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-630)] | Rejected: are 95% confidence intervals |
| 5-494 | A | 29:26 | 29:26 | Replace "0.36±0.14" with "0.36±0.28", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-631)] | Rejected: are 95% confidence intervals |
| 5-495 | A | 29:32 | 29:32 | Replace "0.32±0.10" with "0.32±0.20", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-632)] | Rejected: are 95% confidence intervals |
| 5-496 | A | 29:37 | 29:37 | Replace "0.12±0.06" with "0.12±0.12", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-633)] | Rejected: are 95% confidence intervals |
| 5-497 | A | 29:41 | 29:41 | Replace "0.42±0.14" with "0.42±0.28", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-634)] | Rejected: are 95% confidence intervals |
| 5-498 | A | 29:43 | 29:43 | Replace 1.6±0.6" with "1.6±1.2", to give 95% confidence limits 265 5-265 635 [VINCENT GRAY (Reviewer's comment ID #: 88-634)] | Rejected: are 95% confidence intervals |
| 5-499 | A | 29:50 | 29:50 | It "is" presently... [Pascale DELECLUSE (Reviewer's comment ID #: 58-22)] | Done |
| 5-500 | A | 29:50 | | Sentence needs editing, add verb. [Franklin SCHWING (Reviewer's comment ID #: 230-10)] | Done |
| 5-501 | A | 29:50 | | Sentence needs editing, add verb. | Done |

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| | | | | [Govt. of United States of America (Reviewer's comment ID #: 2023-346)] | |
| 5-502 | A | 29:51 | | is it Levitus or Antonov (see line 42) who made the estimate for 1993-2003? [David Rind (Reviewer's comment ID #: 214-56)] | Clarified |
| 5-503 | A | 29:51 | | Is it Levitus or Antonov (see line 42) who made the estimate for 1993-2003? [Govt. of United States of America (Reviewer's comment ID #: 2023-347)] | Clarified |
| 5-504 | A | 30:1 | 30:1 | Replace 1.5±0.6" with "1.5±1.2", to give 95% confidence limits 266 5-266 636 [VINCENT GRAY (Reviewer's comment ID #: 88-347)] | Rejected: are 95% confidence intervals |
| 5-505 | A | 30:1 | 30:1 | Replace 1.6±0.6" with "1.6±1.2", to give 95% confidence limits 267 5-267 637 [VINCENT GRAY (Reviewer's comment ID #: 88-347)] | Rejected: are 95% confidence intervals |
| 5-506 | A | 30:8 | 30:8 | Replace 0.04±0.02" with "0.04±0.04", to give 95% confidence limits 268 5-268 638 [VINCENT GRAY (Reviewer's comment ID #: 88-347)] | Rejected: are 95% confidence intervals |
| 5-507 | A | 30:17 | 30:17 | All the confidence intervals in this Table should be doubled , for 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-639)] | Rejected: are 95% confidence intervals |
| 5-508 | A | 30:23 | 30:23 | suppress "than" [Pascale DELECLUSE (Reviewer's comment ID #: 58-23)] | Done |
| 5-509 | A | 30:23 | 30:23 | typo error: "processes than" --> "processes then" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-19)] | Done |
| 5-510 | A | 30:26 | 30:30 | An example of a tide-gauge closely resembled by the steric variations during the past 30 is that of Lagos (Portugal) that appears in Tsimplis and Rixen 2002. This directly contradicts Miller and Douglas as their stations Cascais and Tenerife are north an south of Lagos but this plot appeared long before Miller and Douglas and to me it indicates that the spatial averaging they did in their hydrographic data eliminated all relevant variability. [Michael Tsimplis (Reviewer's comment ID #: 268-5)] | We agree with the Reviewer's comment. However, the comment is not relevant for this subsection where the Miller and Douglas paper is not referenced. No changes made. |
| 5-511 | A | 30:36 | 30:38 | This paragraph compares the EOF of thermosteric sea level to the spatial distribution of thermosteric sea level trends. Is it a comparison between 5,5,8 and 5,5,4 ? The figures should be specified. And the agreement is arguable : : equatorial Pacific, eastern Indian ocean. [Pascale DELECLUSE (Reviewer's comment ID #: 58-24)] | Text clarified |
| 5-512 | A | 30:36 | 30:41 | If the spatial patterns change in time shouldn't this be linked as a comment to the results of Church and White and White and Church refered to earlier? [Michael Tsimplis (Reviewer's comment ID #: 268-6)] | Accept, we have added a cautionary remark to this effect in section 5.5.2.3, first para |
| 5-513 | A | 30:38 | 30:38 | First principal component? [Chris Folland (Reviewer's comment ID #: 71-191)] | Yes; corrected |

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| 5-514 | A | 30:45 | 30:45 | Delete "remarkable" [VINCENT GRAY (Reviewer's comment ID #: 88-640)] | Done |
| 5-515 | A | 30:48 | 31:1 | Changes in the ocean's thermal structure are driven by surface heating effects not only ocean circulation as is implied in this sentence. [Govt. of United States of America (Reviewer's comment ID #: 2023-348)] | Corrected |
| 5-516 | A | 31:4 | 31:4 | Replace "a significant" with "an" [VINCENT GRAY (Reviewer's comment ID #: 88-641)] | Done |
| 5-517 | A | 31:5 | 31:5 | Change "E.g.," to "For example" or something similar. [Melinda Marquis (Reviewer's comment ID #: 162-17)] | Done |
| 5-518 | A | 31:9 | | could explain (in a few words) how the wind contributes to density changes via some process other than fluxes - or is the point that it does so by influencing fluxes in ways other than buoyancy? [David Rind (Reviewer's comment ID #: 214-57)] | A sentence has been added to section 5.5.4.2 to better explain the role of the wind for steric changes |
| 5-519 | A | 31:16 | 31:25 | The Eastern Mediterranean Transient is a prime example of deep water formation changes and associated circulation changes affecting sea level variability in a region [Michael Tsimplis (Reviewer's comment ID #: 268-9)] | Noted, see however comment 5-464 |
| 5-520 | A | 31:27 | 31:35 | Again the Mediterranean and the East Atlantic coast are regions where atmospheric pressure changes associated with the NAO have changed the trends (in the Mediterranean reversing them and in the Atlantic coasts reducing them after 1950). [Michael Tsimplis (Reviewer's comment ID #: 268-10)] | Noted. For the East Atlantic this has been noted in section 5.5.2.5.1. For the Mediterranean see comment 5-464 |
| 5-521 | A | 31:38 | 31:51 | The GIA is introduced and explained here, yet the acronym is used quite often earlier in the chapter. This paragraph could be moved to somewhere earlier in the chapter. [Adrian Simmons (Reviewer's comment ID #: 242-92)] | (1) corrected (2) no change made for clarity |
| 5-522 | A | 31:40 | 31:40 | Add Peltier, W.R. 2004. Global glacial isostasy and the surface of the ice-age earth: the ICE-5G (VM2) model and GRACE. Annual Reviews Earth & Planet. Sciences, 32, 111-149. This is the latest version of Peltier's GIA model. [Vivien Gornitz (Reviewer's comment ID #: 87-3)] | Done |
| 5-523 | A | 31:57 | 32:1 | Isn't this also part of the GIA discussed above? [Vivien Gornitz (Reviewer's comment ID #: 87-4)] | GIA discussion deleted from 2nd para in subsection |
| 5-524 | A | 32:0 | 32: | Section on ocean mass change should mention GRACE measurements of global ocean mass [Chambers et al., 2004]. [Robert Steven Nerem (Reviewer's comment ID #: 189-4)] | While I agree with the Reviewer's comment, no reference to GRACE is added because of the still preliminary results |
| 5-525 | A | 32:8 | | Here and in section 5.5.3, it is stated that only 25% of global sea level rise is due to thermal expansion. This does not seem to agree with Table 5.5.2. [Franklin SCHWING (Reviewer's comment ID #: 230-11)] | No change made because the result is correct |

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| 5-526 | A | 32:8 | | Here and in section 5.5.3, it is stated that only 25% of global sea level rise is due to thermal expansion. This does not seem to agree with Table 5.5.2. [Govt. of United States of America (Reviewer's comment ID #: 2023-349)] | No change made because the result is correct |
| 5-527 | A | 32:19 | 32:22 | It is stated here that the ocean's salinity changes due to addition of fresh water, either from melting sea ice or from changes in land ice and terrestrial water storage. Why no mention of changes in precipitation and evaporation over the ocean? Especially as they feature in the Synthesis section of the chapter (page 5-36, lines 5 and 6). [Adrian Simmons (Reviewer's comment ID #: 242-93)] | For the global salinity changes, as distinct from regional changes, in precipitation are not relevant as the amount of water in the atmosphere is very small. |
| 5-528 | A | 32:19 | :21 | Why isn't increased precipitation given as a potential cause of decreases in ocean salinity? [Govt. of United States of America (Reviewer's comment ID #: 2023-350)] | see previous comment |
| 5-529 | A | 32:20 | 32:27 | melting sea-ice does not affect sea level (20) or it does (26-27) ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-25)] | text has been reformulated |
| 5-530 | A | 32:21 | 32:21 | typo error: "which do" --> "which does" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-20)] | text reformulated |
| 5-531 | A | 32:33 | 32:42 | Section 5.5.5.2 Add references for the sea level contributions. [Vivien Gornitz (Reviewer's comment ID #: 87-5)] | Rejected: references are given in chapter 4 |
| 5-532 | A | 32:35 | 32:35 | Replace "0.51±0.32" with "0.51±0.64", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-642)] | Reject: see comment 5-46 |
| 5-533 | A | 32:35 | 32:35 | Replace "0.81±0.43" with "0.81±0.43", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-643)] | Rejected: are 95% confidence intervals |
| 5-534 | A | 32:37 | 32:37 | Replace "0.05±0.12" with "0.05±0.24", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-644)] | Rejected: are 95% confidence intervals |
| 5-535 | A | 32:38 | 32:38 | Replace "0.21±0.07" with "0.21±0.14", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-645)] | Rejected: are 95% confidence intervals |
| 5-536 | A | 32:39 | 32:40 | Replace "0.14±0.13" with "0.14±0.82", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-646)] | Rejected: are 95% confidence intervals |
| 5-537 | A | 32:40 | 32:40 | Replace "0.21±0.35" with "0.21±0.70", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-647)] | Rejected: are 95% confidence intervals |
| 5-538 | A | 33:1 | | This section on land-water storage is interesting and useful to fully understand the sea level change. However this chapter is on oceanic observations. Is it consistent with the chapter on land surfaces ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-27)] | No change made since land water contribution is not discussed elsewhere in the IPCC report |
| 5-539 | A | 33:14 | 33:14 | remove "tion" [Melissa Bowen (Reviewer's comment ID #: 28-25)] | Done |
| 5-540 | A | 33:14 | 33:14 | suppress "tion" [Pascale DELECLUSE (Reviewer's comment ID #: 58-26)] | Done |

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| 5-541 | A | 33:17 | 33:36 | This section (5.5.5.3.2) is clearer than before. You may wish to consider adding two additional anthropogenic causes (i.e., human activities) of SL rise: 1.) urbanization because vegetated areas are replaced by pavement, hence leading to increased runoff to oceans and 2.) combustion of fossil fuels and decomposition of biomass release water, which lead to increased runoff to oceans. These factors are mentioned in Cazenave and Nerem, 2004, to which you already refer. [Melinda Marquis (Reviewer's comment ID #: 162-18)] | Noted. We have decided to shorten this section because the results were unconvincing quantitatively, and have not included the suggested causes. |
| 5-542 | A | 33:42 | 33:43 | Gornitz (2001) estimates -0.33 to -0.27 mm/yr sea level rise equivalent held by dams, not counting additional potential storage due to subsurface infiltration. (see Gornitz, V., 2001. Impoundment, Groundwater Mining and other Hydrologic Transformations. In: Sea Level Rise: History and Consequences, B.C. Douglas, M.S. Kearney, and S.P. Leatherman, eds., Academic Press, San Diego, pp. 97-119). [Vivien Gornitz (Reviewer's comment ID #: 87-6)] | Added |
| 5-543 | A | 33:46 | 33:46 | typo error: "known that" --> "known than" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-21)] | Done |
| 5-544 | A | 33:47 | 33:47 | On the other hand, Gornitz (2001) estimates that the net effect of these anthropogenic contributions is to withhold around 0.9 +/- 0.55mm/yr from sea level rise. This figure likely represents an upper bound. [Vivien Gornitz (Reviewer's comment ID #: 87-7)] | Noted. Because there are controversial conclusions on the land water budget, we have not given a number for this. |
| 5-545 | A | 33:56 | 34:8 | land Water storage estimates should also be included as in TAR. Figure 5.5.9 makes the point that the last decade was exceptional. However one may ask whether such a change has happened before during the last century and I guess one can find such periods Thus I would argue that a comment to this effect must be made. [Michael Tsimplis (Reviewer's comment ID #: 268-11)] | We have decided not to give land water estimates because we have no new observations since the TAR. |
| 5-546 | A | 34:0 | 34: | Question 5.1, Figure 1 - The projections for the amount of sea level rise by year 2100 seem very low to me. I have not reviewed Chapter 10, but even if the altimeter rate remains constant (3.2 mm/year), we will reach 320 mm of sea level rise by 2100. Even a modest amount of acceleration in the contributions from thermal expansion, Greenland, Antarctica, or mountain glaciers will cause the sea level rise in 2100 to be larger than depicted in 2100. Therefore, I believe this figure misrepresents the sea level rise that we can expect in the future. [Robert Steven Nerem (Reviewer's comment ID #: 189-1)] | Reject: the figure will be updated from the Chapter 10 results and so will reflect the projections from IPCC models. The climate rate of sea-level rise is ~1.8 mm (not 3.2mm), and so we would expect 200mm over the next century and with acceleration should get something like ~300mm shown in the graph. |
| 5-547 | A | 34:0 | 34: | Figure 5.6.1 - The average sea level rise versus latitude from altimetry (black curve) seems different from the results I have computed myself. These numbers should be checked. [Robert Steven Nerem (Reviewer's comment ID #: 189-2)] | Our numbers reflect a zonal average. We have high confidence that they are correct. Here no cos (lat) weighting applied |

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| 5-548 | A | 34:0 | 34: | Figure 1 - I believe the errors shown on the sea level reconstruction of the instrumental record are overly optimistic, especially before 1920. [Robert Steven Nerem (Reviewer's comment ID #: 189-3)] | Accept: we are updating our estimates of noise to include the uncertainty associated with natural variability. |
| 5-549 | A | 34:9 | 34:12 | 'More discussion should be given here to the terrestrial storage term and its uncertainty. It seems inexplicable to ignore it.' [Govt. of Australia (Reviewer's comment ID #: 2001-295)] | We have given bounds for the terrestrial storage term which we believe are more accurate than those obtained from direct estimates. |
| 5-550 | A | 34:14 | 34:19 | Back to my comment on bullet 12 in the ES. I have reservations about the apparent satisfactory closure for a single 10-year interval, albeit an interesting and provocative one and the most recent and most likely to show effects of warming. I would suggest downplaying the appearance of satisfactory closure. My reservations are primarily related to the meaning or attribution of the observed value. [Donald L. Forbes (Reviewer's comment ID #: 72-13)] | Taken into account: in revised bullet for TOD executive summary. |
| 5-551 | A | 34:21 | 34:21 | All the figures in this Table need to double the confidence intervals to give 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-648)] | Reject: they are already 95% confidence intervals |
| 5-552 | A | 34:21 | 34:26 | Add "Source" or "Source of sea level rise" to the heading of column 1. [Govt. of Hungary (Reviewer's comment ID #: 2012-30)] | Done |
| 5-553 | A | 34:30 | | [From David Fahey - since David Wratt is Review Editor for this chapter]. Suggest that the question be answered more directly. Suggest that the current text be retained and that the answer begin with a new headline paragraph (in italics) as: 'Yes, there is strong evidence that global sea level has risen through the 20th Century, after a period of little change between 0 and 1900 AD. Rates of sea level change vary between regions.' [David Wratt & David Fahey (Reviewer's comment ID #: 67-43)] | Done |
| 5-554 | A | 34:32 | | [From David Fahey. David Wratt Review Editor for this Chapter] The use of 'did not change significantly' is a bit confusing since it can be interpreted as commenting on the present for which a value of 2 mm/yr has been given. So, by implication, 2 mm/yr is being inadvertently labeled as insignificant. [David Wratt & David Fahey (Reviewer's comment ID #: 67-44)] | Comment not understood |
| 5-555 | A | 34:40 | 34:40 | Replace "very precise" by "better" [VINCENT GRAY (Reviewer's comment ID #: 88-649)] | Done |
| 5-556 | A | 34:40 | 34:42 | are the higher rates obtained by satellites also seen by local measurements? If not, the higher recent rates would be due to the change in observational technique. (This may have been answered earlier in the chapter.) [David Rind (Reviewer's comment ID #: 214-58)] | A reference has been added; tG and altimetry give almost similar results over the last decade |
| 5-557 | A | 34:40 | :42 | Are the higher rates obtained by satellites also seen by local measurements? If not, the | A comment has been added; tide gauge |

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| | | | | higher recent rates would be due to the change in observational technique. Omit phrase in parentheses. [Govt. of United States of America (Reviewer's comment ID #: 2023-351)] | and altimetry give almost similar results over the last decade |
| 5-558 | A | 34:42 | 34:42 | Insert after "of "approximately" 280 5-280 650 [VINCENT GRAY (Reviewer's comment ID #: 88-351)] | Reject: no reason given for suggested change |
| 5-559 | A | 34:43 | 34:45 | Delete from "However" on line 34 to "effects" on line 34. Don't parade your ignorance! [VINCENT GRAY (Reviewer's comment ID #: 88-651)] | Noted, text changed |
| 5-560 | A | 35:1 | 35:1 | Insert the word "rise" between "sea level" and "rates." The current wording is unclear. [Lenny Bernstein (Reviewer's comment ID #: 20-59)] | Taken into account during revisions. |
| 5-561 | A | 35:1 | | Insert the word "change" between "sea level" and "rates." The current wording is unclear. [Govt. of United States of America (Reviewer's comment ID #: 2023-352)] | Taken into account during revisions. |
| 5-562 | A | 35:16 | | [QUESTION 5.1, FIGURE1] In the final version, make sure that the "Projections of the future" shading corresponds to the results of Chapter 10. [Gerrit Burgers (Reviewer's comment ID #: 34-2)] | Accepted: the updated figure will include projections from Chapter 10. |
| 5-563 | A | 35:19 | 36:31 | Comment on the sea level rise Enigma. Does it still exist or not? Cross refer to modelling chapters if these still show much less sea level rise since c. 1870. This is an important point to summarise. [Chris Folland (Reviewer's comment ID #: 71-192)] | We have deleted the word "enigma". However, the sea level budget for the last 50 years or so is still not closed which is now explicitly mentioned. |
| 5-564 | A | 35:25 | 35:25 | Delete "compelling" [VINCENT GRAY (Reviewer's comment ID #: 88-652)] | reject- no reason given for suggested change |
| 5-565 | A | 35:25 | 35:25 | Replace "increased" by "fluctuated considerably" [VINCENT GRAY (Reviewer's comment ID #: 88-653)] | reject- no reason given for suggested change |
| 5-566 | A | 35:26 | 35:26 | Replace "(Figure 5.6.2.)" with ",see Figure 5.6.2" [VINCENT GRAY (Reviewer's comment ID #: 88-654)] | Done, text has been changed |
| 5-567 | A | 35:46 | 35:46 | replace "is" by "brings" [Pascale DELECLUSE (Reviewer's comment ID #: 58-28)] | Done, text has been changed |
| 5-568 | A | 35:51 | 25:51 | Delete "unambiguous" [VINCENT GRAY (Reviewer's comment ID #: 88-618)] | Comment for page 25:51: Reject, no reason given for suggested change |
| 5-569 | A | 35:52 | 35:52 | replace "acidic" by "acid" 100 5-100 29 [Pascale DELECLUSE (Reviewer's comment ID #: 58-618)] | Rejected, acidity was defined as H+ concentration in the footnote (page 20). Here acidic expresses acidity increase. |
| 5-570 | A | 36:15 | 36:17 | In chapter 3, it is recognized that El Nino varies a lot and that there was a shift in the 70's. It is not stated that there was an increase in frequency and duration. [Pascale DELECLUSE (Reviewer's comment ID #: 58-30)] | Reject: the wording will change, but warm phase is more common now as stated in Ch3 (see comment below). |
| 5-571 | A | 36:15 | | except the western tropical Pacific has shown quite strong positive temperature anomalies | Reject: this is more appropriate |

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| | | | | since 2000, so much so that it has been blamed for the pattern of droughts that have been seen in NH mid-latitudes. Perhaps this perspective should be mentioned to bring the point up to date. [David Rind (Reviewer's comment ID #: 214-59)] | comment for Chapter 3. |
| 5-572 | A | 36:15 | | Except the western tropical Pacific has shown quite strong positive temperature anomalies since 2000, so much so that it has been blamed for the pattern of droughts that have been seen in NH mid-latitudes. Perhaps this perspective should be mentioned to bring the point up to date. [Govt. of United States of America (Reviewer's comment ID #: 2023-353)] | Reject: this is more appropriate comment for Chapter 3. |
| 5-573 | A | 36:16 | 36:17 | Replace "the increased frequency and duration of El Nino" by "an on average more El Nino like state". While the shift to higher SSTs in the eastern and central Pacific has resulted in an average state that is more El Nino like, this does not imply a higher ENSO frequency. Actually the list of NCEP/CPC of El Nino events (Google "Cold Warm Episodes by Season" to find the webpage) gives the same number of El Nino's for the period 1950-1975 as for the period 1978-2003; other methods of defining El Nino's may give slightly different numbers, but I doubt that they will be significantly different. Note: I have a similar comment to Chapter 3, page 48, line 44. [Gerrit Burgers (Reviewer's comment ID #: 34-4)] | Accept, we have referred to chapter 3 and use their exact wording |
| 5-574 | A | 36:17 | 36:17 | remove "mean" [Melissa Bowen (Reviewer's comment ID #: 28-26)] | Accept: |
| 5-575 | A | 36:29 | 36:29 | typo error: "All of the these" --> "All of these" [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-22)] | Accept: |
| 5-576 | A | 41:45 | | ICES. 2005. The Annual ICES Ocean Climate Status Summary 2004/2005. ICES Cooperative Research Report, No. 275. 37 pp [Alicia M. Lavín (Reviewer's comment ID #: 141-2)] | Accept ICES reference added. |
| 5-577 | A | 45:43 | | Correct style- no caps [Walter Zenk (Reviewer's comment ID #: 301-8)] | Accept: |
| 5-578 | A | 50:1 | 51:7 | Comment on ARGO float data. Clarify if any analyses used in the chapter include these. If not, give brief forward look to what improvements these may bring to CH5 work. [Chris Folland (Reviewer's comment ID #: 71-193)] | Accept: ARGO data are used in these analyses. |
| 5-579 | A | 50:2 | 50:3 | Start with a statement that all confidence intervals in this Chapter are for 95% confidence [VINCENT GRAY (Reviewer's comment ID #: 88-655)] | Accept: this statement will be included in the introduction. |
| 5-580 | A | 50:11 | 50:11 | correct "homogeneous" [Pascale DELECLUSE (Reviewer's comment ID #: 58-31)] | Accept: will re-phrase sentence to clarify. |
| 5-581 | A | 50:29 | 50:29 | insert "shows" between "figure" and "the" [Melissa Bowen (Reviewer's comment ID #: 28-27)] | Accepted. |

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| 5-582 | A | 50:29 | 50:29 | incorrect sentence "this figure the insitu..." [Pascale DELECLUSE (Reviewer's comment ID #: 58-32)] | Accepted. |
| 5-583 | A | 50:43 | 50:43 | remove comma [Melissa Bowen (Reviewer's comment ID #: 28-28)] | Accepted. |
| 5-584 | A | 50:43 | 50:43 | correct ",," [Pascale DELECLUSE (Reviewer's comment ID #: 58-33)] | Accepted. |
| 5-585 | A | 51:11 | 51:13 | The satellite component of the meteorological observing system is not as inhomogeneously distributed in space as implied by the text on these lines. Whether data from it can be assimilated sufficiently well to provide reliable heat fluxes, from reanalyses for example, is another matter. [Adrian Simmons (Reviewer's comment ID #: 242-94)] | Accept. Para deleted because surface fluxes are discussed in chapter 3. |
| 5-586 | A | 52:5 | 52:5 | suppress the in "the correct the sea surface height" [Pascale DELECLUSE (Reviewer's comment ID #: 58-34)] | Accept: |
| 5-587 | A | 54:0 | | It needs to be noted in both the caption and on the y-axis label that this is change in heat content (not heat content), presumably yearly change in heat content. Is the zero level arbitrary? [Melissa Bowen (Reviewer's comment ID #: 28-29)] | Rejected- Ocean heat content is by definition an "anomaly" since it is the difference between a synoptic measurement at a location and the climatological mean at that location. |
| 5-588 | A | 54:0 | | it is not the yearly heat content but its anomaly ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-35)] | see response to comment 5-99 |
| 5-589 | A | 54:0 | | Figure 5.2.1. The caption is misleading, in terms of the quantify plotted, which is surely the annual anomaly of ocean heat content. According to Levitus et al. (2005), the anomaly is relative to the period mean for 1957-1990. I suggest a more explicit caption: "Figure 5.2.1. Time series of annual anomaly (relative to the mean over 1957-1990) in ocean heat content (10^{22} J), for the 0-700 m layer." This is one of the most important figures in WG1, so the caption should be absolutely unambiguous. [Robert Marsh (Reviewer's comment ID #: 163-10)] | Partly accepted. We have now included an explicit reference period for the heat content but see no reason to use the word "anomaly". |
| 5-590 | A | 54:5 | 54:5 | Fig 5.2.1. caption. Add "global" in front of "ocean". [Chris Folland (Reviewer's comment ID #: 71-194)] | Accepted. |
| 5-591 | A | 54:8 | 54:8 | Add at end "Note that such an apparently periodic graph as this cannot be considered to represent a linear trend" [VINCENT GRAY (Reviewer's comment ID #: 88-656)] | Rejected- Obviously the time series has interannual and interdecadal variability as stated in the text. However there is a linear trend. |
| 5-592 | A | 55:5 | 55:5 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-657)] | Rejected- We believe there is a linear trend and have fit a linear trend to the data. |

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| 5-593 | A | 55:6 | 55:6 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-658)] | Rejected- We believe there is a linear trend and have fit a linear trend to the data. |
| 5-594 | A | 56:6 | 56:6 | Replace "linear" by "pseudo-linear" [VINCENT GRAY (Reviewer's comment ID #: 88-659)] | Rejected- We believe there is a linear trend and have fit a linear trend to the data. |
| 5-595 | A | 58:0 | | caption notes there are no confidence estimates for continental heat gain yet the figure shows some sort of error bar [Melissa Bowen (Reviewer's comment ID #: 28-5)] | Taken into account, but an unfortunate artifact of the software is to plot a zero sized error bar with a finite size. Caption is still correct. |
| 5-596 | A | 58:0 | | Figure 5.2.5: I recognize that part of this graphs point is to show that ocen heat content is very significatn when addressing a total change in energy content; however, I do think that the minor components are also important points. Consequently, I might suggest showing a second bar graph that is a callout of the "minor" components. I would also suggest enlarging the vertical component of the figure and/or highlighting every other row so that the numerical data are more readable. [WG1 TSU (Reviewer's comment ID #: 285-5)] | Callouts lead to an overly complex figure and the suggestion is rejected. Time didn't permit the refinement of highlighting every other row. |
| 5-597 | A | 58:1 | 58:3 | Fig. 5.2.5. Label the top bar Upper Ocean Heat Content [Chris Folland (Reviewer's comment ID #: 71-195)] | Taken into account, but all components are now labelled simply as "oceans", "glaciers and ice caps"... etc. The top most bar is 0-3000m heat content, so not upper ocean. Burgundy is upper ocean. |
| 5-598 | A | 58:1 | 58:13 | On the graph: - the caption above the graph should be deleted (to avoid double captioning) - consistency: "energy" or "heat" content? - it is not clear if the first three and the last items are for heat content, or for "energy content", or for the change of any of them? It seems that "change" needs to be added everywhere. - although the text says that no estimate of confidence is available for the continental heat gain, error bars seem to be present in the graph - the colour legend should be not in the text, but as a colour legend. [Govt. of Hungary (Reviewer's comment ID #: 2012-29)] | Reject: energy is preferred for atmosphere where moisture also provide energy (but not heat). The Energy Content Change is the equivalent of a "y-axis" label, and so is not double captioning.. Accept: but an unfortunate artifact of the software is to plot a zero sized error bar with a finite size. Caption is still correct. Reject colour legen should be on graph, and would be excessive with the numbers, and labels of each component. |

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| 5-599 | A | 60:0 | | It would also be nice to point out in the caption the period of deep water formation in the 1990s. [Melissa Bowen (Reviewer's comment ID #: 28-31)] | The caption does not mention this, however it is stated in the text |
| 5-600 | A | 60:5 | 60:5 | "56 year" should be "56-year". [Chiu-Ying LAM (Reviewer's comment ID #: 139-8)] | Accept, but text modified |
| 5-601 | A | 60:6 | 60:7 | It is hard to see how the Labrador Sea has been freshening since 1965: 1970 looks even saltier. From the figure it looks like it has been steadily freshening since 1977. [Melissa Bowen (Reviewer's comment ID #: 28-30)] | Taken into account. In new version, text is modified . See reply to comments 5-180 and 5-182. |
| 5-602 | A | 61:0 | | I am confused: Do we have negative temperatures in the western Med? [Walter Zenk (Reviewer's comment ID #: 301-10)] | Reject. In new version of this chapter, this figure is removed. |
| 5-603 | A | 62:0 | | it is not "the changes in temperature below 4000 metres " ? [Pascale DELECLUSE (Reviewer's comment ID #: 58-36)] | Accept. Figure has been removed |
| 5-604 | A | 62:4 | 62:4 | Either remove "below 4000 metres" or add more text to explain that there is basin-wide warming below 4000 metres. [Melissa Bowen (Reviewer's comment ID #: 28-32)] | see previous comment |
| 5-605 | A | 62:5 | 62:5 | Insert after "accuracy" "(95% confidence)" [VINCENT GRAY (Reviewer's comment ID #: 88-660)] | see previous comment |
| 5-606 | A | 63:1 | 63:4 | Fig 5.3.4. Add an approximate depth scale to the diagrams. [Chris Folland (Reviewer's comment ID #: 71-196)] | Taken into account, depth axis is in metres.. |
| 5-607 | A | 64:1 | 64:11 | The thin linear fit is nearly invisible. The thick lines pass a lot of variance of a time scale <5 years. [Chris Folland (Reviewer's comment ID #: 71-197)] | Accepted, trend lines removed |
| 5-608 | A | 65:5 | 65:5 | Fig 5.4.2. The word Inventory is ambiguous. Be more explicit. [Chris Folland (Reviewer's comment ID #: 71-198)] | Accepted |
| 5-672 | B | 65:6 | 65:6 | Self-serving I know. Nevertheless, I think it would be appropriate to cite the method as the "DC* method of Gruber et al. (1996)". [Nicolas Gruber (Reviewer's comment ID #: 307-36)] | Rejected – already in main text. |
| 5-609 | A | 67:0 | 68: | Suggest adding the diagram TS21 into the figures here as it captures all 3 sea level reconstructions on the one graph [Govt. of Australia (Reviewer's comment ID #: 2001-297)] | Accepted: TS 21 and this figure will be the same. |
| 5-610 | A | 67:0 | | Add a ZERO line as reference in Fig 5.5.1 [Walter Zenk (Reviewer's comment ID #: 301-11)] | We have no zero line because the interpretation of the curve is clear. |
| 5-611 | A | 67:1 | 67:9 | Figure 5.5.1 Check caption with caption in the Church and White paper (2006) [Govt. of Australia (Reviewer's comment ID #: 2001-298)] | Accept: |
| 5-612 | A | 67:1 | 67:1 | It would be better if (mm) was shown on the ordinate of the figure instead of in the figure | Taken into account with new figure |

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| | | | | caption. Also saying 'the blue curve has been shifted' implies that the red one is 'correct' somehow and the blue one has been offset from that, whereas both have arbitrary datum. I suggest word it 'The red and blue curves are offset by 20 mm for clarity.' [Philip Woodworth (Reviewer's comment ID #: 295-3)] | 5.5.1 with curves overlying each other. |
| 5-613 | A | 67:6 | 67:6 | Caption to Figure 5.5.1: I do not understand the meaning of "updated from Church and White, 2006" -- does this mean simply "from Church and White, 2006" or even just "Church and White, 2006" ? I can't see how this data can have been "updated". [John Hunter (Reviewer's comment ID #: 112-16)] | We have now referred to Church and White 2006. |
| 5-614 | A | 67:7 | 67:7 | Fig 5.5.1 caption. Add "down" after "shifted". [Chris Folland (Reviewer's comment ID #: 71-199)] | Taken into account with new figure where all data are plotted without shifts. |
| 5-615 | A | 68:8 | 68:8 | Replace "2.9±0.40" with "2.9±0.80", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-661)] | Rejected: already 95% confidence intervals |
| 5-616 | A | 68:8 | 68:8 | Replace "3.2±0.4" with "3.2±0.8", to give 95% confidence limits [VINCENT GRAY (Reviewer's comment ID #: 88-662)] | Rejected: already 95% confidence intervals |
| 5-617 | A | 69:7 | | The third panel in this figure (5.5.3) should be discussed in the text. It looks like it shows the answer to part of Munk's enigma on sea level rise, with significant non-thermotic sea level rise throughout the Atlantic Basin. If not discussed in the text of the document, why show the figure at all? [Terrence Joyce (Reviewer's comment ID #: 122-5)] | Accepted: Third panel now removed |
| 5-618 | A | 70:6 | 70:6 | Caption to Figure 5.5.4 (a): the dates for the upper plot should be "1955-2001". [John Hunter (Reviewer's comment ID #: 112-19)] | Accept, has been changed |
| 5-619 | A | 74:0 | | Figure 5.5.8 Ishii et al.(2005) --> Ishii et al. (2006) [Govt. of Republic of Korea (Reviewer's comment ID #: 2015-23)] | Accepted: |
| 5-620 | A | 74:5 | 74:7 | Suggest to add "Upper figure" and "Lower figure" to differentiate the captions of the two figures. [Chiu-Ying LAM (Reviewer's comment ID #: 139-9)] | Taken into account: a and b used. |
| 5-621 | A | 75:0 | | Figure 5.5.9: I have never understood why the "budgets" figure in the TAR, in the first-order draft and in the second-order draft do not show a vertical line indicating the zero "rate of sea level rise". Surely if these are budgets, we need to be able to visually "add" of "subtract" the contributions -- which is virtually impossible if the zero line is not clearly shown. I noted this in my first-order review: please draw a vertical line indicating the zero of "rate of sea level rise". [John Hunter (Reviewer's comment ID #: 112-12)] | Accepted: We have added a zero line. |
| 5-622 | A | 75:1 | 75:7 | Figure 5.5.9. TAs this diagram is comparing the sum of the individual components contributing to changes in sea level with the observed sea level rise it should include the | We have decided not to add the terrestrial bar because we do not have |

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| | | | | best estimate of terrestrial storage (and its recognised uncertainty). It seems misleading to exclude it. Also the difference in the rate of sea level rise when comparing the time periods 1961 - 2003 and 1993 - 2003 (an important finding that this figure shows) could be further highlighted if the time-series graph of sea level rise from 1865 to 2003 included in the Technical Summary (TS21) was placed side by side with this figure [Govt. of Australia (Reviewer's comment ID #: 2001-299)] | direct reliable information on it. |
| 5-623 | A | 76:1 | 76:16 | Question 5.1, Figure 1 It is of concern that current rates of observed SLR (1993-2004) are actually higher than the projected maximum future rate over the next few decades. Whilst this is to some extent explained within the text in terms decadal variability and the possibility of an unexplained term, both explanations suggest the possibility that sea level at a given future time may actually be higher than the maximum projected. This possibility should be addressed on the figure through either error bands or some other mechanism. This point is particularly important as policy makers are likely to use this figure as representative of the full range of potential future SLR. (and most view the upper scenarios as highly unlikely). The uncertainty of the blue curve should include model uncertainty. [Govt. of Australia (Reviewer's comment ID #: 2001-296)] | Taken into account: this figure is being revised with new error bars, and with each of the model scenarios and their individual errors. |
| 5-624 | A | 76:5 | 76:5 | remove "Question 5.1," [Melissa Bowen (Reviewer's comment ID #: 28-34)] | Reject: published in a different part of the Chapter, and also in SPM. |
| 5-625 | A | 76:5 | 76:15 | Excellent figure. This is the figure I was looking for in Chapter 10. However, for the part showing projected SLR to 2100, it is clear what is shown, as the range is described as being due to different choices of emission scenario only. What about uncertainty of projections for a given scenario? Is this included or is the spread shown only for central values under different scenarios? [Donald L. Forbes (Reviewer's comment ID #: 72-14)] | Taken into account in revised version of this figure, with errors around each selected scenario. |
| 5-626 | A | 76:13 | 76:14 | The projected sea level values shown on this graph do not seem to bear any close resemblance to those given in Chapter 10 for AOGCM runs – see Figure 10.6.1 – which is given as the source of the information. Not sure what has happened here but it is necessary to be totally consistent. I suggest that you use exactly the envelope of model results shown in panel (a) of Figure 10.6.1 – i.e. the A1B range – and in the caption to this figure indicate that the projection shown is for a “mid-range” emission scenario. [Martin Manning (Reviewer's comment ID #: 155-6)] | Noted: our figure IS consistent with Chapter 10. |
| 5-627 | A | 77:0 | | My printout from a 1st class printer hardly distinguishes between blue and black in Fig. 5.6.1 . Here is room for improvements. [Walter Zenk (Reviewer's comment ID #: 301-12)] | Noted, however colors are clear on all other printer we have used. Unchanged. |
| 5-628 | A | 77:5 | 77:5 | "Anthropogenic" misspelled [Melissa Bowen (Reviewer's comment ID #: 28-33)] | Accepted |

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| 5-629 | A | 78:2 | 78:16 | Should you skip the future part of the figure in THIS chapter dealing with observations. On the other hand, the figure as such would WELL be fitting in the SPM and/or Ts. [Govt. of Finland (Reviewer's comment ID #: 2009-57)] | Reject: the figure will be published with the SPM as part of an FAQ, and also in this chapter. |
| 5-630 | A | 79:10 | 79:10 | Add at end "Note the comparatively poor coverage of the Southern oceans" [VINCENT GRAY (Reviewer's comment ID #: 88-663)] | Rejected- This is noted in the text. |
| 5-631 | A | 80:0 | | Explain dashed lines in fig 5.3.1. Skip THETA sign in lower figure or add "S" in upper figure. [Walter Zenk (Reviewer's comment ID #: 301-9)] | Accept, dashed lines will be explained in figure caption. |
| 5-632 | A | 80:0 | | NH and SH from (a) should be repeated in the legend: "northern hemisphere (NH)..." [Walter Zenk (Reviewer's comment ID #: 301-13)] | Accept: |
| 5-633 | A | 80:0 | | Figure 5.A.2: I would suggest expanding the horizontal aspect of images b-d; I would also suggest using color to mark the tide guage locations-- Perhaps these graphs can be placed next to one another or in a stack to better illustrate the time sequence? [WG1 TSU (Reviewer's comment ID #: 285-6)] | accept, we have changed the figure to color and stacked them vertically. |
| 5-634 | A | 80:5 | 80:5 | Delete "in Northern Hemisphere and Southern Hemisphere" It is redundant [VINCENT GRAY (Reviewer's comment ID #: 88-664)] | Reject: see comment 5-632. |
| 5-635 | A | 80:6 | 80:6 | This should say ..used to derive the red and blue global sea level curves in ... [Philip Woodworth (Reviewer's comment ID #: 295-4)] | Accept, change as been made |