Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1	3	0	0	0	0	(general comment) The Chapter is very clear, thanks also for the small introduction at the beginning of each section and for the intelligible figures. However the section needs in many parts further information and improvements; [VINCENZO ARTALE, ITALY]	Noted
3-2	3	0				There should be cohrent table headers in all tables in the chapter [Muhammad Amjad, Pakistan]	Editorial
3-3	3	0				No clear reference is made to differential changes in continental shelf waters as compared to those in open ocean waters. The significance of these changes for global ocean biogeochemistry is very important for the ocean biogeochemistry. The emphasis in global ocean (mostly deep ocean) changes can be interpreted beacuse of their large surface and volume. However, almost half of the biological productivity of the ocean depend on shelf waters, a disproportionately low fraction of the total ocean. I find a very superficial treatment of this difference in most of Chapter 3. I provide below an example on the issue of enhanced or depleted primary production in open ocean vs. shelf waters in relation to climatic changes. [Antonio Bode, Spain]	Rejected. Continental shelf waters are beyond the scope of this chapter, and will be covered in WG II
3-4	3	0				In all the chapter, it could be useful to cite authors by chronological order of the publications, when many are cited in brackets, because it indicates the progress in findings [Michel Boko, Benin]	Editorial
3-5	3	0				Chapter 3 is generally well written and comprehensive, compiling a strong case for physical and biogeochemical changes in the ocean as well as the importance of those changes for the global climate system. The report remains quite safely within the bounds of the limits of present understanding of ocean changes. While this is understandable, it neglects nuances and dimensions of ocean climate change which have been discussed in peer reviewed literature over the past 10 years which could, and I believe should be included in the IPCC report. Please see comment below for an example. [Tim Boyer, United States of America]	Noted. See resposes to subsequent comments from this reviewer.
3-6	3	0				The main comment I have regarding the manuscript is that the examination of changes in ocean heat content and thermosteric sea level are limited to 1970-present. The rationale for this limit are laid out in section 3.2.1, last paragraph, citing Lyman and Johnson, 2008 (LJ08) and Domingues et al, 2008 (D08). LJ08 do show that for yearly mean upper ocean heat content, error bars are acceptible (using SSH as a proxy) for 1970-present. But there is no reason to limit examination to yearly mean ocean heat content. Levitus et al., 2005 has used 5-year composite mean ocean heat content to examine ocean heat content back to 1955-59. Others, such as Lozier et al. 2008, have used longer compositing periods to compare time periods including periods prior to 1970. [Tim Boyer, United States of America]	Accepted. The time-series are now discussed back to the 1950s. However, in the upper ocean, 5-year composites have two problems. First, ocean advection can shift the temperature field considerably over 5 years, so that 5-year means are more subject to aliasing of shifting gyres on interannual time-scales. Second, before 1970 even over 5 years a huge fraction of the ocean remains unsampled (except the N. Atlantic, the best-sampled of all oceans, which is where Lozier et al. 2008 did their analysis). The Levitus et al. (2012, Geophys. Res. Lett.) 5-year estimates are used for 700-2000 m in the SOD. In that depth range the problem of large year-to-year advective shifts may be reduced in scope.
3-7	3	0				continuation of comment 2):The other reference justifying limitation of heat content examination to 1970-present is D08, which does show decreased error estimates after 1970. D08 uses 3-year averages. However, as I understand, each year of the 3-year average is computed as a 1-year mean before averaging the 3 1-year means together. So, this is still providing error estimates for 1-year means, not longer compositing periods, the same as LJ08. In short, the restriction to examination of ocean heat content 1970-present is not really necessary when looking at compositing periods more than 1 year in length. [Tim Boyer, United States of America]	See answer to comment 3-6.
3-8	3	0				continued from comment 3:To go further, Lyman et al., 2010 showed that time series of ocean heat content can reveal with reasonable certainty	See answer to comment 3-6.

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						decadal or longer time period changes, but cannot reliably do so for time periods shorter than 6 or 7 years. It stands to reason that examining longer averaging periods than the one year periods of LJ08 and D08 can reveal and enhance our understanding of ocean heat content changes over a longer time period than 1970-present. Salinity changes in this same ocean observations section are examined over a longer time period than temperature changes, despite more sparse data and higher difficulty in accurate measurement. [Tim Boyer, United States of America]	
3-9	3	0				references for comments 2-4:Domingues, C.M., J.A. Church, N.J. White, P.J. Gleckler, S.E. Wijffels, P.M. Barker, and J.R. Dunn, 2008: Improved estimates of upper-ocean warming and multi-decadal sea-level rise. Nature, 453, 1090-1093 Levitus, S., J. I. Antonov, T. P. Boyer, 2005: Warming of the World Ocean, 1955-2003. Geophys. Res. Lett. , 32, L02604, doi:10.1029GL021592. Lozier, M.S., S.J. Leadbetter, R.G. Williams, V. Roussenov, M.S.C. Reed and N.J. Moore, 2008. The spatial pattern and mechanisms of heat content change in the North Atlantic. Science, 319, 800-803. Lyman, J. M. and G. C. Johnson. 2008. Estimating global upper ocean heat content despite irregular sampling. Journal of Climate, 21, 5629-5641, doi:10.1175/2008JCLI2259.1 Lyman, J. M., S. A. Good, V. V. Gouretski, M. Ishii, G. C. Johnson, M. D. Palmer, D. A. Smith, and J. K. Willis. 2010. Robust warming of the global upper ocean. Nature, 465, 334-337, doi:10.1038/nature09043 [Tim Boyer, United States of America]	Noted.
3-10	3	0				Please consider to include the following article and its findings in this chapter. Friedrich T. et. al. Nature Climate Change (2012), doi:10.1038/nclimate1372 http://www.nature.com/nclimate/journal/vaop/ncurrent/full/nclimate1372.html [Øyvind Christophersen, Norway]	rejected the manuscript discusses results from models, outside the scope of this chapter
3-11	3	0				The overall chapter is well written and the text is easy to follow. At the end of the chapter, the synthesis and the FAQ provide the reader with a broad view of the main topics discussed along the chapter. The chapter is well supported with updated references, which in their majority reflect the most recent studies in the area/topic of study and therefore are suitable for such an assessment report. All the specific comments made as follows are related to “minor corrections” in the following areas: i) text clarity, ii) figure interpretation and its relation with the main text, iii) structure of the sections in the chapter and iv) references. Specifically related to the structure of the text, I found the introduction too short to provide a broad view of the whole chapter. My suggestion is either to enlarge the introduction or to suppress it and blend its current contents with the executive Summary. I also suggest the inclusion of a list of abbreviations, since the chapter uses a considerable number of acronyms/abbreviations. [Mauro Cirano, Brazil]	Noted
3-12	3	0				Although I am not an expert on that topic, I believe your chapter is well written and easy to understand because I have only those very few comments. An exemplary chapter. Congratulations. [Francois DANIS, France]	Noted
3-13	3	0				Format of reference should be arranged as uniform. Also several references are incomplete. [MASAO FUKASAWA, Japana]	Editorial
3-14	3	0				Since I have been working on the observations of carbonate system and oxygen in surface and interior of the oceans, my comments focus on the results from observations of ocean biogeochemical changes and anthropogenic acidification that are described in chapters 03. I think that this chapter provides the essence of state-of-the-art scientific knowledge on these issues. I have no major objections to what is summarized there, and have just a few comments. [Masao Ishii, Japan]	Noted

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3-15	3	0				Generally well written and balanced. Section 3.4 is especially clear, and provides a good example for the depth of coverage and tone. In this reviewer's opinion, several items need to be addressed throughout the chapter. The penchant to invoke jargon often does ore to obscure an explanation than to clarify it; specifically, the use of intensification when referring to the water cycle. I can understand the use when referring to a phenomenon, such as a cyclone, but do not see appropriate application with a principle, such as a cycle. The water cycle does not essentially change; the rate of exchange of some parts of the cycle does. Certain other phrases detract from the text. Removing the terms "consistent" and "almost certainly" would improve the chapter. The judgement of consistency should be reserved to the reader and used sparingly in the conclusion and executive summary. "Consistent" appears 55 times in the chapter! The root word "carbon" is found only 50 times in the same text. Use of the term "almost certainly" is similar to stating "almost significant" in a statistical context. The internal references to other sections for a particular topic are distracting; I know how to use a table of contents. [Christopher Kavanagh, Monaco]	Taken into account.
3-16	3	0				This chapter summarizes the observational evidence of change in the Ocean since 1970, with an emphasis on basin and global scale changes relevant to climate. The authors sufficiently documented that the parameters such as ocean temperature, upper ocean heat content, Net Ocean surface fluxes, winds and waves, sea level and anthropogenic carbon are changing (increasing trend) from 1970 using different data sets from in-situ, satellite and model analysis. Also, changes in ocean salinity (qualitatively) have been observed throughout much of the ocean, both at the sea surface and in the ocean interior. The changes in the upper ocean density have resulted in increase in the stratification in the Upper Ocean and decline in ventilation, consistent with other observations such as declining oxygen in much of the upper ocean. This proves that different ocean observation shows consistency in what is being observed. Also, this AR5 report (Observations: Oceans) is better than AR4 report since many instrumental biases in historical upper ocean temperature measurements have been identified and removed. This report is well documented by different authors. However, unlike AR4, this report is mainly focused on Global analysis / trend and little effort is given for basin scale analysis /trend, especially Indian Ocean changes is less reported (may be insufficient information is available for this region). [Ravichandran Muthalagu, India]	Accepted. More regional information on temperature trends is included in the revision of Fig. 3.1. Also note that Fig. 3.9 (former Fig. 3.10) also has zonal basin averages of temperature trends.
3-17	3	0				suggestion: May be some recommendation is required for better observations in future, so that next AR report will provide better confidence that the present or clarify some of the uncertainty, especially surface ocean flux, currents, etc. [Ravichandran Muthalagu, India]	Rejected. Not our purview to recommend observing systems - this needs to be done carefully, and in great detail, and is being done elsewhere (e.g. OceanObs09 etc.)
3-18	3	0				Recommendation: According to me, it is well written, the scientific and technical contents are accurate. [Ravichandran Muthalagu, India]	Noted
3-19	3	0				It is not clear to me why there is a section on Sea Level Change in Chapter 3. I would suggest that this section be moved to Chapter 13: Sea Level Change. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	the structure and content of the chapters have been decided on prior to the writing of the ZOD. Sea level is an observed variable, Ocean heat content change is closely related to sea level change, so it is appropriate to have a section on sea level rise in this chapter.
3-20	3	0				Where possible, it would be advantageous to follow a similar structure for the sub-sections. I am thinking particularly of section 3.2 and 3.3 where it would seem logical to progress from global-scale to regional changes for both temperature/heat content and salinity [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Noted. More regional information is included in the revision of Fig. 3.1. Also note that Fig. 3.9 (former Fig. 3.10) also has zonal basin averages of temperature trends.
3-21	3	0				This is a great chapter containing a lot of useful information. The scope of content and the depth of discussion seem just right with some limitations. There are some general and specific concerns that the authors should take into account in a revision. [Hans Poertner, Germany]	Noted
3-22	3	0				The paper uses many acronyms which makes reading very tedious for the non expert. These should not only be defined and written out on first mention but also repeatedly in text and each figure legend to remind the reader here and there of there meaning. Legends should be comprehensible for the general reader without the need to consult the text. [Hans Poertner, Germany]	Editorial - will minimise use of acronyms

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3-23	3	0				The traceability of some accounts can be improved, this should be checked systematically. Some examples are given below. [Hans Poertner, Germany]	Noted
3-24	3	0				In some sections the jargon used is very dense and tedious to read. In general, readability for the non expert should be a major concern in the revisions. [Hans Poertner, Germany]	Noted
3-25	3	0				Many statements develop a nice quantitative picture of observed trends. Others do not and one wonders whether this should be done more consistently (e.g. ice melt contribution to freshening?). [Hans Poertner, Germany]	Noted
3-26	3	0				There are some redundancies in the text which could be reduced [Hans Poertner, Germany]	Noted
3-27	3	0				Many figure legends are not comprehensible. Abbreviations should be explained. The legend text should provide access to the figure without the need to consult the text. [Hans Poertner, Germany]	Accepted, figure captions modified
3-28	3	0				If possible, observations should be explained according to present understanding. If the respective hypotheses are not available this should be said. Implications for other phenomena considered by WGI should also be mentioned. This would support building bridges between chapters. For example, the description of expanding gyres should be linked with or refer to a description of implications for nutrient inventories. Probably, reference to WGII chapters should also be included, however, discussion of the respective content should be minimized as the differential time schedule of the IPCC WGI and WGII processes would probably not allow to balance contents. [Hans Poertner, Germany]	Noted
3-29	3	0				Several figures make great contributions to AR5, some should be improved with the non expert reader in mind. [Hans Poertner, Germany]	Noted. Most figures are modified
3-30	3	0				placeholder [Reiner Steinfeldt, Germany]	Noted
3-31	3	0				Use of uncertainty language and traceable account needs to be consistently implemented throughout the chapter where assessed findings are given. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted text revised
3-32	3	0				There are some quite lengthy passages of backgound, context setting text where supporting references are missing. They need to be added. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised
3-33	3	0				Section 3.7: 'Sea Level' requires careful coordination with Chapters 4, 5, and 13 to ensure consistency and limit redundancy. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
3-34	3	0				Section 3.7.4: Reference to SREX with regards to Extremes - SREX should be the starting point for an updated assessment provided in the AR5. [Thomas Stocker/ WGI TSU, Switzerland]	Noted
3-35	3	0				I thought that Chapter 3 was very well written and agreed with nearly all the comments there. [Richard G Williams, UK]	Noted
3-36	3	0				Chapter is clearly written, well organized and complete. The only suggestion I have is that changes in ocean chemistry be included. Perhaps it is in another chapter later on. Occurences of 'dead zones' are common and seem to be increasing. These are due to increases in N and other nutrients. Not due primarily to CC/warming but perhaps exaccerabated. What changes in ocean chemistry have occurred, what have affects been and what is likely for the near future? [s. jeffress (jeff) williams, usa]	changes in ocean chemistry (carbon, oxygen, nutrients) are covered in section 3.8
3-37	3	1	1	1		Observations: Ocean [Medani Bhandari, Nepal]	Noted
3-38	3	1	1	40	44	In general for ch.3: Sparse sampling in the ocean is mentioned repeatedly. It would be good to describe two things: (1) The dilemma of large temporal sclaes in the ocean coupled with small spatial scales, and (b) to which degree this can/cannot be overcome by satellite measurements. [Christoph Heinze, Norway]	Noted
3-39	3	1	1	75	10	Overall the authors have done a very good job and the text is reasonably concise. It is a very good basis for the next step. [Christoph Heinze, Norway]	Noted
3-40	3	1	10	1	16	I just wanted to draw your attention towards the list of Contributing Authors. Nationalities are specified in	Noted - will adopt a consistent approach in AR5

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						chapters 1 and 13, but not here [Belén Martín Míguez, Spain]	
3-41	3	1	15	1	5	"Xialoan Wang" should be "Xiaolan Wang" [Zeng-Zhen HU, USA]	Accepted - text revised
3-42	3	1	15	1	15	There is a spelling error in the name of "Xiaolan Wang" (the l is after o, not before). Please correct [Xiaolan Wang, Canada]	Accepted - text revised
3-43	3	1				Overall, the chapter is comprehensive, authoritative, and informative. [Michael Meredith, UK]	Noted
3-44	3	1				There is a lot of good information in this chapter about the response of the ocean to the changes in the zonal pattern of the SAM, but less on the non-zonal part of the pattern, which are believed to impact on ocean warming in parts of western Antarctica (the Bellingshausen and Amundsen Sea regions), and freshening and probably cooling in the Ross Sea. This has been linked to an intensification of the Amundsen Sea Low, impacted by SAM intensification due to ozone depletion. [Michael Meredith, UK]	Noted
3-45	3	3	1	4	36	The executive summary reads well. A paragraph on O2, as well as on N2O and CH4 would be useful. [Christoph Heinze, Norway]	Accepted text for oxygen added. Methane and N2O are found in Ch6.
3-46	3	3	1	4	36	This Executive Summary has not been linked to the topic climate change. Are those observed changes in the ocean consistent with (anthropogenic) climate change? Are those changes extraordinary compared to earlier periods in time? Have those changes been triggered by climate change? This chapter seems to trigger more questions than to clarify the understanding of climate change. Text from the introduction could help to clarify many issues - as has been done in chapter 4 (cryosphere). It is recommended to use a similar approach in chapter 3. [Klaus Radunsky, Austria]	Noted
3-47	3	3	3	3	3	I suggest to use 'most of the upper ocean', instead of just 'the upper ocean', since we do see some coolings in the upper ocean (Fig. 3.1a), and the surface cooling in the Southern Ocean is quite broad and has been confirmed as a robust feature. These coolings reflect changes in atmospheric and oceanic circulations during this time period. [Zhaomin Wang, UK]	Accepted. The word "overall" has been added to this sentence.
3-48	3	3	3	3	5	As the first two sentences are wrong, as they now stand, pls rephrase them as follows: "It is virtually certain that parts of the upper ocean has warmed since 1970. However, when ovbservations covering more (perhaps up to ~60%) of the global ocean become available, within the coming 50 years, it is predicted that it will have shown a net warming. These predictions are consistently supported by..." [Martin Hovland, Norway]	Accepted. The word "overall" has been added to this sentence.
3-49	3	3	3	3	36	Writing makes no reference to ANY ocean observations before 1970. Given the possibility of multi-decadal natural variability in the ocean, this seems to be a perilous situation upon which to make any pronouncements about man-made climate change effects, unless these are 'averaged' out in the global sum. Perhaps models could be referenced as a test of the robustness of this global heat index as an indicator of anthropogenic warming. [Terrence Joyce, USA]	Accepted. Heat curves are now presented back to the 1950s, although sparseness of data before 1970 is still noted. A comparison of Argo to Challenger temperature data is also added to the SOD.
3-50	3	3	3	3	45	It is "virtually certain" that ocean temperatures have fluctuated since 1970. They rose from 1970 to 1980 fell between 1980 and 1984. rose from 1984 to 2005. Since 2005 they have only shown a very slight rise. .Ocean temperatures fluctuate, yet you seem obsessed with isolating regions for which you can claim they have "warmed".You are not much interested in what might have happened before 1970, or the fact that the ocean oscillations began an upwards period in that year, or the fact that the lack of temperature rise shown in the surface measurements since 2001 is reflected in the ocean heat measurements [VINCENT GRAY, NEW ZEALAND]	Rejected. Sampling is too poor prior to 1970 to form a reliable global average (Domingues et al., 2008; Lyman and Johnson 2008). The trends estimated are statistically significant despite apparent fluctuations in rates, and those fluctuations over a few years are not statistically significant (e.g. Lyman 2012). figures and text contain now data before 1970
3-51	3	3	3		16	This is a great first para. Strong declarative sentences. What we know. We are confident. [Stephen E Schwartz, USA]	Noted
3-52	3	3	4	3	4	became - typo [Roland Gehrels, United Kingdom]	Accepted - text revised
3-53	3	3	5			"including" should be "namely", since the list is complete [Michael Meredith, UK]	Agreed
3-54	3	3	8	2	10	The statement that biases have been largely removed does not reflect the discussion in the Chapter and does not reflect the current state of understanding. It needs to acknowledge that biases have been identified and	Noted - text revised to acknowledge that undetected biases may remain

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						multiple adjustments have been developed to mitigate their effects, but those adjustments differ and significant uncertainties remain, particularly regarding shorter-term variabilty. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	
3-55	3	3	8	3	10	"Instrumental biases … largely removed …" is either ambiguous or unduly positive. While it may be correct that there has been progress in reducing instrumental biases of which we are aware (i) there is no logical basis on which it can be claimed that there are not further unknown biases to be uncovered, and (ii) there are plenty of systematic effects which researchers in the field would not begin to claim have been solved. Propose making statement less seemingly definitive: "Progress since AR4 in identifying and reducing instrumental biases in historical uppoer ocean temperature measurements has reduced a spurious decadal variation ..." [Christopher Merchant, UK]	Noted - see response above
3-56	3	3	10	3	10	Executive summary: it would be a good idea to give the full reference for the IPCC AR4, at least when it appears for the first time. [Leticia Cotrim da Cunha, Germany]	Editorial
3-57	3	3	10	3	45	The first pargraph on this page nicely reports numbers but the fourth does not. Can this be reconsidered? [Hans Poertner, Germany]	Accepted added in Chapter 3.4
3-58	3	3	12	3	12	Most policy makers probably ignore how a stratification can be quantified [Michel Petit, France]	Noted
3-59	3	3	13	3	13	it would be better to give the exact time period, e.g., 1970-2009. [Rongshuo Cai, China]	Noted. The phrase "given sparse historical sampling" has been added.
3-60	3	3	14	3	15	It is stated that the trend of ocean temperatures from 2000-4000m is indistiguinshable from zero. I believe that this statement is made in a statistical sense, that is, it is not statistically different from zero or that the trend is within the uncertainty limits. Nevertheless, I think it should be rewritten because it might be confusing. Perhaps something like, "The trend in global ocean temperatures between 2000 and 4000 meters is small and difficult to quantify with measurement uncertainty". [John Abraham, USA]	Accepted text revised
3-61	3	3	15	3	16	As this last sentence in the paragraph is wrong, as they now stand, pls rephrase as follows: "Based on a limited set of observations and numerical modeling, it is likely that…" [Martin Hovland, Norway]	rejected. We believe the statement is valid based on published literature.
3-62	3	3	15		16	I would say the evidence is sufficeintly strong that the word "likely" underrepresents it. I would say "The densest water mass fed…" [Michael Meredith, UK]	Accepted.
3-63	3	3	18	78	45	Though the text is based on published references, it would be better if the considering time (i.e., 40 years for temperature, 50 years for salinity…) is standardized in Executive Summary. [Jae Hak Lee, Republic of Korea]	rejected. The text must be based on published results, which use different time periods.
3-64	3	3	18		22	there is a slight missing link in the argument here; the slope is given for upper ocean (though suggest 3.7 to 4.2 E21 J yr-1; not 37 to 42 E21 J decade-1); that is good. the stmt is that ocean = 90% of global heat content increase; also good; but need link from upper ocean to total ocean to finish the argument. Levitus 05 had another 30% below 700 m. You might also want to give this in W m-2; 4 E21J m-2 = 0.25 W m-2 globally. I see at line 36-37 you state < 0.5 W m-2. I think you can and should be more specific and precise than this. [Stephen E Schwartz, USA]	Noted. The fact that the deep ocean also takes up some heat is noted in the revision. The units for heat uptake have been standardized to TW throughout the manuscript.
3-65	3	3	19	3	53	Line 19, 53, “over the last 40 years” and line 22, 39 “over the last fifty or 50 years” and so on mean what the periods? e.g., from 19?? to 20?? [Rongshuo Cai, China]	accepted exact time periods added
3-66	3	3	21	3	22	Exeutive Summary: "increases" and "increased" in the same sentence - repetition. [Leticia Cotrim da Cunha, Germany]	Editorial
3-67	3	3	24	3	24	Delete the word "robust". The arguments are not all that convincing. [VINCENT GRAY, NEW ZEALAND]	Taken into account - we disagree with the reviewer that the changes are not convincing. We also agree that more quantitative IPCC language is necessary, and will change the statement to "It is virtually certain …"

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3-68	3	3	27		27	"rainfall-dominated" sounds odd when referring to the polar regions, where the freshwater inputs are often snow and glacial melt. [Michael Meredith, UK]	Accepted: change to "precipitation" which includes snow. Need to assess changes in S due to glacial melt to determine if significant enough to include in sentence separately.
3-69	3	3	28			"fresh Pacific" to be replaced with "fresh Pacific and Indian" [Ravichandran Muthalagu, India]	Reject: most of the Indian Ocean is highly saline, equivalent to the Atlantic; both stand in contrast to the fresher Pacific. There are fresh-saline contrasts within each ocean, as between the Arabian Sea and Bay of Bengal in the Indian ocean, and the salinity contrast between these local regions has increased as well.
3-70	3	3	29	3	30	We should also mention 'reflecting changes in atmospheric circulation', not just 'containing more moisture'. [Zhaomin Wang, UK]	Probably reject: need references to support this addition. Colleagues from other chapters who assisted with FAQ 3.3 did not raise this point.
3-71	3	3	38	3	41	It is just wasting time to look for evidence for a trend in global E-P. Both global E and global P are two large terms, and would be exactly equal if there are no moisture content changes in the atmosphere. Errors in global E and global P are very large; the real global E-P is a very small term, since the trend in global atmospheric moisture content is very small on the time-scale considered here. Instead of looking for a trend in global E-P, it is meaningful to look for a trend in global atmospheric moisture content if atmospheric humidity data are available. However, the changes in regional E-P are much larger than those in global E-P, and it is possible to detect such changes. We also need to investigate the trends in E and P (not E-P) seperately on global scale. [Zhaomin Wang, UK]	Accept: we were just stating the obvious, but we didn't make it clear that we knew this was obvious.
3-72	3	3	43	3	44	As the two last sentences in this paragraph are wrong, as they now stand, pls rephrase them as follows: "…limited and are poorly constrained, it is likely that significant wave height has increased over the North Pacific ocean since 1900, the North Atlantic ocean since 1950 and the Southern Ocean over the last two decades. Extreme wave heights have, on a similarly quasi-global scale likely increased over the past 60 years, in keeping with increases in extreme winds." [Martin Hovland, Norway]	text has been revised to make the sampling limitations clear
3-73	3	3	46			The key conclusion and its implications are missing here. The key conclusion is that the average planetary heating rate over the past few decades N is something like 0.35 ± 0.1 W m-2. And the implication is that this tells us a lot about climate change over the industrial period. In general DH/dt = N = F - lambda* DeltaT where lambda is inverse of equilibriium sensitivity. Hence lambda = (F-N)/DeltaT. Or Equilibrium Sensitivity (K/W m-2) = DeltaT/(F-N). Hence Heating rate is subtractive from forcing to give effective forcing F-N. So knowledge of N is essential to interp of obsd temp increase over industrial period. Key References: Gregory, J. M., R. J. Stouffer, S. C. B. Raper, P. A. Stott, and N. A. Rayner (2002), An Observationally based estimate of the climate sensitivity, J. Climate, 15, 3117-3121. Schwartz S. E., Charlson R. J., Kahn R. A., Ogren, J. A., and Rodhe H. Why Hasn't Earth Warmed as Much as Expected? J. Climate 23, 2453-2464 (2010); doi: 10.1175/2009JCLI3461.1. Then make the point that N is small compared to forcing by LLGHG's (2.8 W m-2) but not necessarily small relative to Total forcing that includes aerosols - refer to best estimate of this forcing from forcing chapter	Noted, the heating rate is important, but in this chapter, no discussion on attribution is included, that is in Ch 10. Also, see box 3.1 and Box 13.1

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						Adjusted Forcing = 1.95 ± 0.9 W m-2. To my thinking this sort of reasoning is essential to put determination of heating rate into context. So the key implication is that determination of the heating rate eliminates any issues associated with the planet not being at steady state (commonly called equilibrium) in response to the applied forcing. that is why it is so impt to determine heating rate. That is why the heating rate determined here is so impt. [Stephen E Schwartz, USA]	
3-74	3	3	52	3	52	(AAIW, NPIW) is a special word and what its full name is? It will be given in the glossary? It would be better to be dealt with as the ENSO, or NAO in line 54-55. [Rongshuo Cai, China]	Accepted text revised
3-75	3	3	55			add Indian Ocean Dipole (IOD) also in the climate modes [Ravichandran Muthalagu, India]	Rejected: many modes of natural climate variability have been left off this list, including the tropical Atlantic dipole, the Atlantic Nino, the Pacific Decadal Oscillation, etc. Our intent was to highlight those that are best known simply as examples, and not provide an exhaustive list.
3-76	3	3		40		Good discussion of Ocean Heat Content and Sea Level observations. 0,5 W/m2 seems well justified from 2012 publications. [Terje Wahl, Norway]	Noted.
3-77	3	4	1	4	2	As only parts of the ocean has been observed, it would be recommended to alter the two first sentences as follows: "Recent quasi-global observations have strengthened evidence for naturally caused variability in major ocean circulation systems on quasi climatic time scales (e.g., from years to decades). Much of this observed variability can be linked to natural quasi-gobal changes in wind forcing,...." [Martin Hovland, Norway]	We do not believe preceding each statement with "quasi-global" will help increase the clarity of the text. Limitations of ocean sampling are now covered more completely in the appendix.
3-78	3	4	7	4	7	In some places (e.g. page 26) this number is 1.6, not 1.7 mm/yr, which is the Church and White number. Be consistent. [Roland Gehrels, United Kingdom]	noted. Numbers have been made consistent between the chapters
3-79	3	4	7	4	7	This statement is completely misleading..Sea level varies greatly from place to place and an overall average is not much use locally. Judging the behaviour of climate time series by means of linear regression, tends to exaggerate the early, least reliable measurements, ignore important fluctuations and conceal what is happening now. Most sea level measurements are prone to positive bias because the equipment gets damaged and depressed by storms and the local land falls from weight of buildings and removal of miunersals and ground water. The variability is concealed, The recent use of accurate levelling equipment has shown a general reduction in the rate of change in many cases, and in some (such as the Pacific islands and Australia) no change at all since the equipment was installed..No mention is made of the variability in different places.You should publish a map of global variability of altimetry measrements which show that much of the rise took place in a small region north of Indonesis, and the the South Pacific is not rising at ll. [VINCENT GRAY, NEW ZEALAND]	rejected. Global averages are useful, as well as regional distributions. The contribution of land motions to observed sea level change is covered adequately in the text.
3-80	3	4	7		18	ditto [Hans Poertner, Germany]	noted
3-81	3	4	8	4	8	Changes are sometimes smaller (Pacific Islands) [VINCENT GRAY, NEW ZEALAND]	noted regional rends in sea level are different
3-82	3	4	8	4	8	In order to clarify, I suggest inserting "in mean sea level", between "Changes" and "over periods" [Martin Hovland, Norway]	accepted
3-83	3	4	9	4	10	I doubt this is the case; see this figure adapted from Holgate 2007: http://www.worldclimatereport.com/wp-images/Holgate_update2_fig1.JPG; recent trends are not that exceptional and right now, they are decelerating again. AR4 was more to the point with their remark about decadal fluctuations. [Marcel Crok, The Netherlands]	taken into account. the discussion on this topic has been revised as well as text in exec summary
3-84	3	4	9	4	10	.But there is considerable variability and there is evidence it may be falling [VINCENT GRAY, NEW ZEALAND]	Rejected. There is in fact evidence that ocean heat content continues to rise on pentadal and annual time scales (e.g. Levitus et al., 2012, Geophys. Res. Lett.; von Schuckmann and Le Traon, 2012, Ocean

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							Science).
3-85	3	4	9	4	10	According to data Church and White (2011), the statement “rate of GMSL rise since 1990 is higher than in any comparable period since 1950” is conditionally correct only. If apply moving average with 11 years window, the higher rate (up to 3 mm/year) is observed during period 1940-1950. The rate is higher than for the entire period since 1880. Qualifications must be made on reasons, such as 50-60 year cycle , as suggested by Jevrejeva et al. [Pavel Tkalich, Singapore]	taken into account. the discussion on this topic revised as well as text in exec summary
3-86	3	4	9			The Executive Summary in the AR5 FOD Observations: Oceans chapter advises “There is growing evidence that the rate of GMSL rise since 1990 is higher than in any comparable period since 1950.” This might be the case, however, such a statement could be considered misleading or “cherrypicking” time intervals to infer rates of SLR are now very high or increasing. Particularly given the broader context of the longer-term gauges in the majority of the primary ocean basins of the world which indicate rates of SLR higher during the 1920s and 1940s to around 1950 than measured during the 1990s (as advised in section 3.7.2). [Phil Watson, Australia]	taken into account. the discussion on this topic revised as well as text in exec summary
3-87	3	4	14	4	14	In order to clarify, I suggest inserting "with satellite data" after "measured" [Martin Hovland, Norway]	Editorial
3-88	3	4	17	4	17	In order to clarify, I suggest inserting "regional and local" between "in" and "extreme" [Martin Hovland, Norway]	Editorial
3-89	3	4	18		30	This is nice introductory text describing the point of departure. [Hans Poertner, Germany]	noted
3-90	3	4	20	4	25	The paragraph mainly talks CO2. Could it reflect most of biogeochemical state changes in the open ocean? [Rongshuo Cai, China]	accepted text added
3-91	3	4	27	4	27	"acidification" is misleading. Replace it with "'reduction in alkalinity" [VINCENT GRAY, NEW ZEALAND]	rejected. Acidification is a well-defined and frequently used in peer reviewed publications
3-92	3	4	27	4	31	The summary should address whether or not acifidication could be attributed to antropogenic increase of CO2 in the atmosphere. Consult also with ch 10 for consistency. [Øyvind Christophersen, Norway]	This is covered in section 3.8
3-93	3	4	28		34	Can you add numbers to the text? SAM unexplained. [Hans Poertner, Germany]	rejected The pH trend is in numbers
3-94	3	4	29	4	30	This statement can not possibly be "virtually certain". It may be so that surface ocean pH is a function of surface CO2 to this extent, but restriciting it to "anthropogenic CO2", and "solely" on top of that, appears incredible. [Tor Eldevik, Norway]	accepted. Text revised
3-95	3	4	32	4	32	It is felt that the relationship and severity of acidification should be even more clearly stated in the executive summary of this chapter. One way could be to include a sentence copied from Box 3.2: Ocean Acidification p3-32 l54-57. "Anthropogenic ocean acidification may produce far-reaching consequences of the buildup of human-induced carbon dioxide in the atmosphere. Results from laboratory, field, and modeling studies, as well as evidence from the geological record, clearly indicate that marine ecosystems are highly susceptible to the increases in oceanic CO2 and the corresponding decreases in pH and carbonate ion." [Øyvind Christophersen, Norway]	rejected. Impacts of ocean acidification are covered by WGII and are not within the scope of this chapter.
3-96	3	4	33	4	33	Improve the sentence by rephrasing to: "The observations summarized in this chapter provide virtually certain evidence…" [Martin Hovland, Norway]	Editorial
3-97	3	4	33	4	36	Is it possible to strengthen this summarizing paragraph with the words "due to anothropogenic forcing" after "has changed during the past 40 years"? If there is evidence enough to say this, I would affirm the attribution in this sentence. [Allison Crimmins, United States]	rejected. The attribution is not part of Chapter 3
3-98	3	4	33	4	36	It would be very surprising if the ocean did noit change over such a period [VINCENT GRAY, NEW ZEALAND]	noted. Text revised
3-99	3	4	33		36	The statement that the ocean has changed sounds a bit trivial and should be differentiated more by mentioned the key parameters and processes to mirror the complexity of changes. [Hans Poertner, Germany]	noted. Text revised
3-100	3	5	7	5	7	The statement : "The ocean contains 60 times more carbon than the atmosphere…" is totally senseless, and	noted text revised it refers to inorganic carbon

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						needs qualification - there must be two orders of magnitude fault here. Rephrase to: "Including micro-organisms and dissolved gases, the ocean contains ~1000 times more carbon than the atmosphere..." [Martin Hovland, Norway]	
3-101	3	5	7	5	8	These are very inaccurate estimates only. At least put "about" in front of them. [VINCENT GRAY, NEW ZEALAND]	rejected. The chapter clearly describes the accuracy and confidence of the estimates.
3-102	3	5	8			The 25% of anthropogenic emissions of CO2 that the ocean is absorbing: do you have a citation for this number? I have seen ranges from 25-40%. [Allison Crimmins, United States]	accepted citations added
3-103	3	5	9	5	10	The sentence "The ocean is also capable of relatively rapid change, with the potential for climate feedbacks." is misleading (though the sentence afterwards comments this), as feedbacks can occur on a multitude of time scales. Replace by something as: "Oceanic processes can lead to significant climate feedbacks on a large variety of timescales." [Christoph Heinze, Norway]	accepted
3-104	3	5	10		10	Does climate really evolve on the timescale of weeks? When does it become "weather"? [Michael Meredith, UK]	accepted text revised
3-105	3	5	13	5	14	Maybe add "compared to other components of the climate system." [Leticia Cotrim da Cunha, Germany]	Editorial
3-106	3	5	18	5	18	should the word "continuous" or "consistent" be inserted between "Paucity of" and "long-term"? [John Abraham, USA]	Editorial
3-107	3	5	19	5	19	Please see comment no. 1 (full reference for the AR4). [Leticia Cotrim da Cunha, Germany]	Noted
3-108	3	5	22	5	22	Improve this statement by rephrasing to read: "...Argo array of profiling floats is now providing year-round measurements (over a 6-year period) of temperature and salinity in parts of the world ocean in the upper…" [Martin Hovland, Norway]	Noted
3-109	3	5	32	5	32	The paragraph summary's sentence seems to be weak in attribution to climate change or still remains not so clear how the change in the ocean relevant to climate. [Rongshuo Cai, China]	rejected. Attribution of the cause of observed changes is covered by Chapter 10.
3-110	3	5	35			Section 3.2 "Changes in Ocean Temperature and Heat Content" includes little information on the regional and basin-scale changes. I think it is essential to document the spatial patterns of change. I would welcome the addition of ocean temperature/heat content time series for individual ocean basins and/or a spatial map of the trends in 0-700m temperature/ocean heat content. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. A spatial map of trends from 1971 to 2010 in 0-700 m temperature has been added to Fig. 3.1. Fig. 3.10 illustrates basin-average changes.
3-111	3	5	35			Following on from the comment immediately above, there is a need to highlight the particularly strong warming that has been observed in the North Atlantic ocean basin over the historical period. The work my co-authors and I have done on assessing observed subsurface temperature changes in an isotherm framework is relevant here, since it attempts to discriminate between ocean temperature changes associated with air-sea heat fluxes and ocean advection. Our analyses suggest that ocean advection have played a large role in shaping the spatial patterns of upper ocean temperature change in the mid-to-low latitudes over the historical period, and that the Atlantic may be unique in having a strong advective component to the long-term warming. These are the relevant papers: (i) Palmer M. D., K. Haines, S. F. B. Tett and T. J. Ansell (2007), "Isolating the signal of ocean global warming", Geophys. Res. Lett., 34, L23610, doi:10.1029/2007GL031712; (ii) Palmer M. D. and K. Haines (2009) "Estimating oceanic heat content change using isotherms", Journal of Climate, 22, 4953-4969. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Noted. The revision does not discuss the isothermal framework, but the particularly strong N. Atlantic warming is now discussed.
3-112	3	5	35			Section 3.2: The issue of XBT/MBT bias corrections and sampling is mentioned in both sections 3.2.2 and 3.2.3. I suggest covering this issues in section 3.2.1 and avoiding repetition in later sections. The authors might find it useful to refer to one, or both, of the following peer-reviewed publications, which review the progress in ocean heat content measurements since the AR4 and discuss in more detail many of the issues raised in this section: (i) Palmer, M.D., K. Haines and J.M. Lyman (2010), "Recent advances in our understanding of global ocean heat content", in BAMS State of the Climate, 2009; (ii) Palmer, M. & Co-Authors. 2010: "Future Observations for Monitoring Global Ocean Heat Content" in Proceedings of OceanObs'09: Sustained Ocean Observations and Information for Society (Vol. 2), Venice, Italy, 21-25	Noted. The bias corrections are more consolidated in 3.2.1. However, the references mentioned in this comment border on grey literature (being only lightly peer-reviewed), and don't add too much to the journal article references already cited.

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						September 2009, Hall, J., Harrison, D.E. & Stammer, D., Eds., ESA Publication WPP-306, doi:10.5270/OceanObs09.cwp.68. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	
3-113	3	5	35			Section 3.2: It may be useful to include a additional paragraph or two to highlight the wider importance of the ocean temperature/ocean heat content changes - such as the contribution to sea level rise, estimating the global radiation imbalance and providing observational constraints for future projections of global surface temperature rise. The suggested references above also discuss these aspects, and might be a useful resource for the chapter authors. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Noted. Box 3.1 highlights the importance of the oceans in the global energy imbalance. Section 3.6 also highlights the importance of temperature changes to sea level rise.
3-114	3	5	35			It would be useful to refer to Figure 3.10 somewhere in this section, since this figure shows long-term trends in ocean termperature for individual ocean basins. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. Figure 3.10 (now Fig. 3.9) is referenced in the revision.
3-115	3	5	35			Section 3.2: I would suggest eliminating the use of the term "anomaly", as in "upper ocean heat content anomaly" throughout this section, since I think it is potentially confusing to the general readership. I suggest instead referring to "change" or "changes" in ocean temperature or ocean heat content. This goes for Figures as well - e.g. Figure 3.2 Y-axis would read "Change in OHC (ZJ)". The caption could then include the basline period for which the change is relative to. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted.
3-116	3	5	35			Section 3.2: Define what is meant by "Upper ocean heat content". I take this to mean the 0-700m layer, but it is not obvious. It would be good to briefly mention this layer in the context of the limited depth sampling of the historical observations. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted There is already a definition in Fig. 3.2, but it has been added to the text in the first paragraph of 3.2.1
3-117	3	5	39	5	39	"Parameter" is used in different meanings (especially in discussions between modellers and observationalists). Strictly speaking is a parameter a coefficient in a process description, as in "f(x)=a times x" where a is the parameter and x the variable. I think it would be better to write here "variable" or "state variable" and not "parameter". Of course, insiders know exactly what is meant here, but for people from the outside this could confusion. [Christoph Heinze, Norway]	Accepted. The word "property" is used in the revision instead of "parameter"
3-118	3	5	39	40		I think this sentence could be mis-interpreted by the reader. The majority of ocean sub-surface temperature observations being made today (i.e. ARGO) are indeed designed to monitor and assess long-term changes. I agree that there is a challenge in quantifying historical change, since over the period from about 1970 to the mid-2000s most of the temperature observations came from XBT instruments. I suggest removing this sentence and refer to "measuring temperature" in the sentence immediately following it. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted, in part. The second phrase of this sentence has been deleted.
3-119	3	5	41	5	41	different accuracies, sampling depths, and precision' as 'accuracy' and precision' are linked, I would suggest to reorder the enumeration so that 'precision' and 'accuracy' follow directly one after anotherwith [Reiner Steinfeldt, Germany]	Accepted.
3-120	3	5	42			Reference to Boyer et al. (2009) is incomplete. Use the following reference: Boyer, T.P., J. I. Antonov , O. K. Baranova, H. E. Garcia, D. R. Johnson, R. A. Locarnini, A. V. Mishonov, D. Seidov, I. V. Smolyar, M. M. Zweng, 2009. World Ocean Database 2009. Edited by S. Levitus, NOAA Atlas NESDIS 66, U.S. Gov. Printing Office, Wash., D.C., 216 pp., DVDs. [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-121	3	5	45			Reference to Gouretski and Koltermann (2007) is incomplete, include issue number (1), article number (L01610) and doi (10.1029/2006GL027834) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-122	3	5	47	5	48	Two recent studies on XBT fall rates have been carried out using a very new technique that shows promise to improve results. The citations are: J. Stark, J. Gorman, M. Hennessey, F. Reseghetti, J. Willis, J. Lyman, J. Abraham, M. Borghini, A Computational Method for Determining XBT Depths, Ocean Sciences, Vol. 7, pp. 733-743, 2011. J. Abraham, J. Gorman, F. Reseghetti, K. Trenberth, and W. Minkowycz, A New Method of Calculating Ocean Temperatures Using Expendable Bathythermographs, Energy and Environment Research, Vol. 1, pp. 2-11, 2011.	Noted. However, these studies have not yet been used in estimating ocean heat content or temperature, only applied to profiles from a few probes, so they are not cited in the report.

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						[John Abraham, USA]	
3-123	3	5	48	5	50	The sentence starting with "One major consequence…." is utterly cryptic and needs clarification. [Martin Hovland, Norway]	Accepted. The revision now cites Domingues et al. (2008) which supports the statement.
3-124	3	5	48	50		Suggest you cite the work of Domingues et al. (2008) in reconciling model simulations and bias-corrected observational time series: Domingues, C.M., Church, J.A., White, N.J., Gleckler, P.J., Wijffels, S.E., Barker, P.M. Barker & Dunn, J.R. (2008). Improved estimates of upper-ocean warming and multi-decadal sea-level rise, Nature, 453, 1090-1093, doi:10.1038/nature07080. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted.
3-125	3	5	48			Reference to Levitus et al (2009) is incomplete, include article number (L07608) and doi (10.1029/2008GL037155) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-126	3	5	50	5	50	I am missing a reference here accounting for this reduction of the artificial decadal variation. [Belén Martín Míguez, Spain]	Accepted. Revision cites Domingues et al. (2008).
3-127	3	5	52	5	56	Maybe explicit here how long would the "longer time-scales" be (centuries?). [Leticia Cotrim da Cunha, Germany]	The sentence does specificy decadal and longer.
3-128	3	5	52	52		Suggest replacing "(hence heat content anomalies)" with "(and therefore ocean heat content)". [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted.
3-129	3	5	53	5	53	May add foloowing reference: "Xue , Y., M. A. Balmaseda, T. Boyer, N. Ferry, S. Good, I. Ishikawa, A. Kumar, M. Rienecker, T. Rosati, Y. Yin, 2012: A Comparative Analysis of Upper Ocean Heat Content Variability from an Ensemble of Operational Ocean Reanalyses.J. Climate (accepted)." [Zeng-Zhen HU, USA]	Accepted, reference is now cited in discussion of decadal variability in section 3.2.1.
3-130	3	5	55	5	55	I would insert the word "measured" before "upper ocean heat content". Really, we don't know if the heat content has such variations or if the variations are in our measurements. [John Abraham, USA]	Rejected. Sea level shows variations on long time scales. That is the infrerence made here.
3-131	3	5	55	5	55	Delete "likely" and replace with "virtually certain" [Martin Hovland, Norway]	Accepted.
3-132	3	5		8		As for Chapter 3 Section2 (Changes in Ocean Temperature and Heat Content), this section is too short or over summarized. Number of observational outputs concerning this section is likely to be the largest and contents of those outputs are very important, nevertheless, this section spends smaller portion of Chapter 3 than section 3 (Salinity). Also in Section 3(Salinity), salinity distribution is treated for each ocean separately, however, in Section 2, temperature and heat are treated only for the upper layer and the deep layer. As the results only about 20 articles are cited for temperature and heat though about 30 articles are cited for salinity. [MASAO FUKASAWA, Japana]	Noted. A spatial map of trends from 1971 to 2010 in 0-700 m temperature has been added to Fig. 3.1. Also note that Fig. 3.10 also has zonal basin averages of temperature trends, and is now referred to in section 3.2.1.
3-133	3	5				One aspect that I think is missing from the ocean temperature and heat content section is reference to the changing depth sampling with time and what is meant by the 'upper ocean'. For example what depth does the Lyman and Johnson (2008) paper say is adequately sampled back to 1967? My reading of the paper indicates that it is 300m depth but all time series shown are for the 0-700m layer. Is there a similar analysis for the 0-700m sampling, or perhaps it would be best to show curves for the upper 300 m back to 1970 and to 700m only for the most recent period when deeper XBTs became more used? [Simon Good, UK]	Noted. Lyman and Johnson (2008) actually extrapolates shallow XBT data to 700-m following Willis et al. (2004, J. Geophys. Res.). The recent published curves are almost all for 0-700 m, so that standard is adpoted here.
3-134	3	5				At the moment there is no discussion of regional (ocean-basin scale or smaller) temperature change in the ocean temperature and heat content section. Regional variations can be large - for example see Palmer et al. (2009), GRL, 36, L20709, doi:10.1029/2009GL039491. This can partly already be seen in Figure 3.10, although that figure is not referred in the ocean temperature and heat content section at the moment. I suggest showing a figure containing a spatial map of heat content change with discussion of the results. [Simon Good, UK]	Accepted. A spatial map of trends from 1971 to 2010 in 0-700 m temperature has been added to Fig. 3.1. Fig. 3.10 is also now referred to in Section 3.2.1.
3-135	3	5				Section 3.2: overall, the section seems to underplay the uncertainties in the historical ocean temperature record. The first paragraph, from lines 45-50, suggests that the problem of biases has been solved, whereas it remains a major uncertainty in estimates of ocean heat content even in the most recent period as highlighted by Lyman et al. (2010) in Nature and even further back in the record as shown in the Palmer et al. (2010)	Accepted. The number of ocean heat content anomaly time-serires in Fig. 3.2 is increased in the revision. To be included the time-series had to be long, extending back to around 1955.

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						Ocean Obs 09 white paper. In the FOD, Figure 3.2 shows only two of a larger number of estimates of upper ocean heat content (compare to Figure 2 of the Palmer et al. Ocean Obs white paper, or Figure 3.8 of the BAMS State of the Climate in 2010 report, or Figure 1 of the Lyman paper in Nature). Showing and discussing a larger number of estimates would show clearly that, without selection, the existing estimates of UOHCA all show a rise, but that the uncertainy surrounding the shorter term variability is much larger. The mitigation of biases has reduced the decadal variability (pg 5 lines 48-50), but at the expense of increased variability betweem estimates associated with uncertainty in the bias adjustments. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	
3-136	3	5				Section 3.2: I appreciate the rationale of focusing on the changes since 1970 (pg 6 lines 5-7), but temperature records go back before this period and, as shown in the Palmer et al. Ocean Obs 09 White paper, differences between analyses do not diverge by a greater degree before 1970s than they do after. One line of evidence that this section uses is that there is an agreement with SST data sets and this agreement extends back further than 1970. It would help to define the longer term context of UOHC change and the changing uncertainties related to measurement and sampling, if the records shown in the figures and discussion were extended back to the 1950s. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted, the figures are now extended back to the 1950s. However, it seems likely that the pre-1970 estimates do not diverge because they all use the same data set and mostly tend to relax back to zero in unsampled regions, which are more than half the globe. Estimating a global average when less than half the globe is sampled may not be prudent.
3-137	3	6	1	2		Suggest replacing this sentence with "The sparse historical sampling and large amplitude variations on short time and space scales (associated with mesoscale eddies, fronts, internal waves etc) raises challenges for estimating changes upper ocean temperature. However, a recent error analysis that makes use of satellite sea surface height data (and exploits it's relationship to upper ocean temperature) suggests that the sampling coverage is reasonable for estimating changes in global upper ocean temperature/heat content from about 1970." [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Noted. The suggestion has been adopted in modified form.
3-138	3	6	2	6	2	Insert "and ambiguous" between "sparse" and "historical", as this provides the reader with a more realistic impression of the status of knowledge. [Martin Hovland, Norway]	Rejected. The sentence is clear.
3-139	3	6	5	7		The results of Palmer and Brohan (2011), using a different method for estimating uncertainty, also support the conclusions of Lyman and Johnson (2008) regarding the reduction of both regional and global sampling error from about the 1970s: Palmer, M. D. and P. Brohan (2011), "Estimating sampling uncertainty in fixed-depth and fixed-isotherm estimates of ocean warming", International Journal of Climatology, 31(7), 980-986, DOI: 10.1002/joc.2224. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted.
3-140	3	6	9			Section 3.2.2: A more detailed information on regional distribution of changes of 'Upper Ocean Temperature' would be useful [Thomas Voigt, Germany]	Accepted. A spatial map of trends from 1971 to 2010 in 0-700 m temperature has been added to Fig. 3.1. The section also notes that Fig. 3.10 also has zonal basin averages of temperature trends.
3-141	3	6	11	6	40	I recommend to add a long term expansion of Indian and Pacific warm pool shown in the SST trend over the past 60 yrs (Fig 4. in Williams and Funk, 2011, Clim Dyn) as well as descriptions for changes in salinity in the tropical warm pool (Ch3.3.3.1, p 11, line 30). [Jae Hak Lee, Republic of Korea]	Rejected. SST is the purview of chapter 1 and Salinity resides in section 3.3
3-142	3	6	13	6	14	A minor point, but is it correct to describe the Ishii and Kimoto (2009) and Levitus et al. (2009) methods as being based on optimal interpolation? I understand that Ishii and Kimoto (2009) use a variational minimisation scheme and that the Levitus et al. (2009) method is based on a Cressman analysis scheme. The description as optimal interpolation analysis is also repeated in other parts of the section. [Simon Good, UK]	Accepted.
3-143	3	6	19	6	20	In this last sentence it is referred to "similar results", but without stating similar to what… This is cryptic and needs clarification. [Martin Hovland, Norway]	Accepted. Text changed to "the various analyses of temperature changes cited above yield consistent results".
3-144	3	6	22	6	34	The deep warming trends in the northern/southern midlatitudes are relatively different in Figure 3.1.a and Figure 3.10.C (last row) presumably due to the different time periods. However Figure 3.10.C (last row) is also quite different from its former version (on Figure 5.3 in AR4) with opposite trends around 60N despite a near-similar time period. I think it would be interesting to note these differences and if possible explain their causes: decadal variability or corrections in temperature profiles? [gael alory, France]	Noted. Time periods, analysis techniques, and bias corrections all differ among these figures.

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3-145	3	6	24	6	24	Southern Ocean with capital letters. [Leticia Cotrim da Cunha, Germany]	Accepted
3-146	3	6	24	6	30	Southern Ocean and Antarctic Circumpolar Current are names and always should be written starting with capital letters. [Christoph Heinze, Norway]	Accepted
3-147	3	6	25	6	25	"A maximum warming in 70-30 S is present…" I think that Gille looked south of 30 so perhaps a better statement is "A maximum warming south of 30S is present…." [John Abraham, USA]	Accepted
3-148	3	6	25	6	26	The sentence starting with 'A maximum …' needs to be revised to reflect the actual finding in Gille (2008). After the 1970s, the heat content changes in the SO are also small (see Fig. 5 and Fig. 6 of Gille 2008). A much larger warming occurred before the 1970s and since the 1930s. This time period (1930s-1970s) is not covered in Fig. 3.1a. Only when the temperature trend is calculated over the period from the 1930s (rather than from the 1970s) to the present decade , can we see a large warming in the SO. [Zhaomin Wang, UK]	Noted, However, some upper ocean warming also occured between the 1990s and 2000s.
3-149	3	6	28	6	33	Following commnet 19, I here question the explanation for the SO warming that invoked southward shifts of the ACC. Since the much larger SO warming occurred before the 1970s and the westerly winds were weaker before the 1970 than afterwards, there must be other processes involved in changing the SO temperature. [Zhaomin Wang, UK]	Noted, see also comment 3-150.
3-150	3	6	28	6	34	An important point. The mooted southward shift is just one possible factor that has led to a warming of the Southern Ocean. Another, which needs discussing here, is the possiblity that the stronger winds have led to a more intense eddy field, with a greater southward eddy heat flux. This was proposed by Meredith and Hogg (2006), and demonstrated as feasible in a modelling context by Hogg et al. (2008). How much warming has come from which process is not yet determined for the real ocean. Missing references:- (1) Hogg, A.McC., M.P. Meredith, J.R. Blundell and C. Wilson. “Eddy Heat Flux in the Southern Ocean: Response to Variable Wind Forcing”. Journal of Climate, 21, 4, 608-620, 2008. (2) Meredith, M.P. and A.McC. Hogg, “Circumpolar response of Southern Ocean eddy activity to changes in the Southern Annular Mode”. Geophysical Research Letters, 33(16), L16608, 10.1029/2006GL026499, 2006. [Michael Meredith, UK]	Noted. The possible eddy intensification is discussed in Section 3.6.5.2 entitled The Antarctic Circumpolar Current
3-151	3	6	31			Reference to Sokolov and Rintoul (2009) is incomplete, include article number (C11019) and doi (10.1029/2008JC005248) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-152	3	6	43	6	44	The statement that there a 4% increase in thermal stratification during the 40 year period should be rephrased. It appears to me that there is a clear increase until 1997 (mainly based on the 5 year running mean) but then it remains nearly constant. [Mauro Cirano, Brazil]	Noted, However, the statement is correct as written.
3-153	3	6	44	6	46	It should be noted that the increase in thermal stratification reaches a maximum at the equator as seen in Figure 3.1.a [gael alory, France]	Noted. There is a strong signal on the equator and mostly in N. Hemisphere, not so much in the S. Hemisphere (could be insufficient data in the south?). Please note that the color scale is not linear in this panel.
3-154	3	6	51	6	51	I would add "relative to the 1970-2009 average" for the sake of clarity at the end of the legend of Figure 3.1.b. Otherwise, the reader does not know how the anomalies are calculated [Belén Martín Míguez, Spain]	Noted. However, the anomalies are not relative to that average, but are as described in Levitus et al. (2009).
3-155	3	6	57	6	57	In the two cited references for the melting of Antarctic glaciers, only glaciers associated with West Antarctic Ice Sheet are concerned. There is no clear evidene for how the glaciers associated with East Antarctic Ice Sheet changed. This sentence may become misleading if people think all Antarctic glaciers melted. In fact, when Bellingshausen and Amundsen Seas became warmer, there was an overall increased sea ice extent around Antarctica, and broad cooling in the SO during the period considered here. [Zhaomin Wang, UK]	Noted. Pritchard (2012) is cited to clarify this statement.
3-156	3	6	57	7	1	Missing reference: Jenkins et al., Nature Geoscience 3, 468 - 472 (2010), doi:10.1038/ngeo890 "Observations beneath Pine Island Glacier in West Antarctica and implications for its retreat" [Michael Meredith, UK]	Noted. Here Jacobs et al. (2011) is used as an update to Jenkins (2010).
3-157	3	6				Section 3.2.2: It would useful to include some discussion of the geographical distribution of the warming trend and perhaps a figure that shows a map of trends similar to Figure 3.4 and/or time series of ocean heat content from different basins. The behaviour in each ocean basin has been different. Further to this, it would be useful	Accepted. A spatial map of trends from 1971 to 2010 in 0-700 m temperature has been added to Fig. 3.1. Also note that Fig. 3.10 also has zonal basin averages

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						to see how the geographical patterns of trends vary from one analysis to another as in Figure 2.5 of Chapter 2. Such a map would help to inform the conclusion (pg 8 Section 3.2.5) that the greatest remaining uncertainty in the upper ocean temperature evolution is in the magnitude and pattern of warming at southern high latitudes. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	of temperature trends, which are referred to in the revised section 3.2
3-158	3	6				Figure 3.1a and 3.1b, Legend color is not clear. In Fig 3.1 a, the positive value of 0.34 and negative value of -0.34 looks same color and in 3.1b, positive value of 0.4 and negative value of -0.4 looks similar. Better to change different color scheme for 3.1a and 3.1b. [Ravichandran Muthalagu, India]	Rejected. Note that surrounding colors for these temperature ranges give sufficient context for interpretation.
3-159	3	6				Figure 3.1b: The depth-time graph shows that warming is coincided with more number of observations. How can this be justified, the warming/cooling is not depend on observational density. [Ravichandran Muthalagu, India]	Rejected. Numerous studies mapping the data in different ways have shown the warming, as noted in the chapter.
3-160	3	7	1			Reference to Shepherd et al (2004) is incomplete, include issue number (23), article number (L23401) and doi (10.1029/2004GL021284) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-161	3	7	2			Reference to Dmitrenko et al. (2008) is incomplete, include issue number (C5), article number (C05023) and doi (10.1029/2007JC004158) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-162	3	7	3	7	5	With references to the Polyakov, I didn't see that this work showed warming intensifying through 2007 or that Atlantic shoaling suggests it might affect sea ice. Please check that this reference is correct. Perhaps I am mistaken but they seem to be in error to me. [John Abraham, USA]	Rejected. This is a modeling study and will not be cited in the ocean observations chapter.
3-163	3	7	5	7	5	Add cittion: Different magnitudes of projected subsurface ocean warming around Greenland and Antarctica, 2011: J. Yin, J.T. Overpeck, S.M. Griffies, A. Hu, J.L. Russell, and R.J. Stouffer, Nature Geosciences, doi:10.1038/NGEO1189. [Stephen Griffies, USA]	Rejected. This is a modeling study and will not be cited. However, Andreson et al. (2011) is now cited for Greenland, as is Jenkins et al. (2010) for Pine Island Glacier.
3-164	3	7	6			Reference to Jackson et al. (2010) is incomplete, include article number (C05021) and doi (10.1029/2009JC005265) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-165	3	7	8	7	8	Ocean Heat Content is the place to look for either global warming or global cooling. It's a much better metric than the surface temperature measurements, see Pielke Sr., R.A., 2003: Heat storage within the Earth system. Bull. Amer. Meteor. Soc., 84, 331-335 and Pielke Sr., R.A., 2008: A broader view of the role of humans in the climate system. Physics Today, 61, Vol. 11, 54-55. A statement about the importance of this metric would be welcomed. What is surprising is that in this subsection ARGO is not mentioned. The ARGO Heat Content Data are more reliable than the XBT data. The ARGO data so far show little accumulation of heat in the upper 700 meter of the ocean so far. There is still a debate whether this heat went down into deeper layers of the ocean. There is an interesting exchange between Josh Willis and Pielke about this: http://pielkeclimatesci.wordpress.com/2011/11/14/e-mail-exchange-with-josh-willis-on-the-ability-to-monitor-the-transfer-of-heat-in-the-oceans-to-levels-below-700m/ It seems unlikely to me that the heat went into the deep ocean in the period since 2003. See also http://bobtisdale.wordpress.com/2011/10/24/introduction-to-the-nodc-ocean-heat-content-anomaly-data-for-depths-of-0-2000-meters/ This topic is very important and should be discussed. [Marcel Crok, The Netherlands]	Noted. The Pielke papers are fine overviews, but their points are covered with the citations already in the chapter. The rest of the citations listed in this comment are for blogs, which are not cited in IPCC reports and will not be used. There may be some slowdown in ocean heating in the late 2000s (e.g. Hansen et al. 2011) as discussed in the revision. However, Levitus et al. (2012, Geophys. Res. Lett.), as mentioned in the revision, does show heat accumulating in the deep ocean, as do Purkey and Johnson (2012) and others.
3-166	3	7	8		42	I think essential to discuss Carson and Harrison J clim 08 p 2259 Is the Upper Ocean Warming? Comparisons of 50-Year Trends from Different Analyses who conclude: "Most of the ocean does not have significant 50-yr trends at the 90% confidence level." and earlier work by Harrison to that effect. If that work is shown wrong by other work, it is necessary to state how. [Stephen E Schwartz, USA]	Noted. Carson and Harrison (2010) is discussed in the report, showing that results are: even after XBT bias correction, basin wide interdecadal variability is still large. These authors do not attempt a global average, and they throw data away that others do not through their sampling thresholds. Many other studies cited show that it is possible to construct global averages.
3-167	3	7	16	7	17	The meaning of the phrase 'there is increasing convergence on bias correction since AR4' is unclear to me. I think that what is meant is probably that all the bias adjustments that have been proposed broadly have the same effect on global average temperature time series. However, each of the bias adjustments give slightly	Accepted.

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						different results and there are still new sets of adjustments being proposed (e.g. Gouretski 2012 in Deep Sea Research I, doi:10.1016/j.dsr.2011.12.012) so in that sense I don't think there is yet convergence on bias corrections. Perhaps this could be reworded, for example to say "After instrument bias corrections, the next largest sources of error…". I would also suggest the use of the word uncertainty rather than error when used in this context throughout the chapter. [Simon Good, UK]	
3-168	3	7	18	7	32	Linear trends for 1969-2003 and 1970-2009 periods are difficult to compare due to the use of different units. [gael alory, France]	Accepted. The revision uses TW for power (time rates of change in heat energy).
3-169	3	7	26	7	26	I would suggest that a statement like, "It is still unclear how acuracte archival information can be made in light of instrumentation accuracy." It isn't clear to me how much more improvement can be made in past archives of ocean temperatures. [John Abraham, USA]	Noted. This issue is discussed in section 3.2.1.
3-170	3	7	26	7	26	Yan et al. (2012) found that that the consistency of the HC300 among eight operational ocean reanalyses (ORAs) tends to increase with time due to an increase in observations. The consistency of mean HC300 is generally high north of 30S except near western boundary currents, in the tropical Atlantic and some regional seas. The consistency of HC300 anomalies is generally high in the tropical Pacific, tropical Indian Ocean, North Pacific and North Atlantic, but it is low in the tropical Atlantic and extra-tropical southern oceans where observations are very sparse.The consensus among ORAs suggests that the mean HC300 in 70S-70N has a brief cooling in early 1980s and 1992-1993 related to the volcanic eruptions of the El Chichon and Mt. Pinatubo, a small warming in 1985-1991, a continuous, and a more robust, warming in 1994-2003, and then a persistence or weak cooling in 2004-2009. Some ORAs also depict a warming burst in 2009. Compared with Pacific, Atlantic has much large disagreement among different ocean reanalysis (Zhu et al. 2012). [Zeng-Zhen HU, USA]	Noted. I think you mean Xue et al. (J. Climate, accepted). This is a reanalysis, not a direct data analysis.
3-171	3	7	26	7	26	Xue, Y., M. A. Balmaseda, T. Boyer, N. Ferry, S. Good, I. Ishikawa, A. Kumar, M. Rienecker, T. Rosati, Y. Yin, 2012: A Comparative Analysis of Upper Ocean Heat Content Variability from an Ensemble of Operational Ocean Reanalyses. J. Climate (accepted). [Zeng-Zhen HU, USA]	Noted.
3-172	3	7	26	7	26	Zhu, J., B. Huang, and M. A. Balmased, 2012: An ensemble estimation of the variability of upper-ocean heat content over the tropical Atlantic Ocean with multi-ocean reanalysis products. Clim. Dyn., 10.1007/s00382-011-1189-8 (publisheed online). [Zeng-Zhen HU, USA]	Noted.
3-173	3	7	28	7	35	Increases have been irregular and have fallen recently. Linear regressiion is a misleading procedure for judging time series where the earliest figures are the least accurate. [VINCENT GRAY, NEW ZEALAND]	Rejected. Linear regression is a basic, simple to explain way to estimate a trend. The uncertainties are quoted, and the results are statistically significant.H185
3-174	3	7	30			Suggest also give as W m-2 by dividing by 5.1 E 14 m2 (whole planet area). [Stephen E Schwartz, USA]	noted. TW is unambigous. The use of W m-2 can be confusing because some people use the area of the ocean, and some the area of the earth.
3-175	3	7	33	7	35	The sentence is not very clear: how do the rates of energy gain agree with their uncertainties? The (+/-) values for Levitus and Domingues are very different although the UOHCA estimate values are not very different from each other. [Leticia Cotrim da Cunha, Germany]	Noted. Within, not with. However, the sentence has been modified even more since more energy gain estimates have been added to the Second Order Draft.
3-176	3	7	37	7	42	This figure conceals the inaccuracy of the measurements by giving only one standard error, only 33%.. It should be doubled to give the conventional 95% level [VINCENT GRAY, NEW ZEALAND]	Rejected. One standard error represents roughly 68% confidence intervals for a two-sided distribution, not 33%. Also, these are the published estimates that are shown in the figure.
3-177	3	7	38	7	42	The font doesn't match the preceding font size [John Abraham, USA]	Noted. However, the figure captions have smaller font than the text body by convention.
3-178	3	7	38	8	56	What does the "Z" denote in ZJ (used here and line 56 (p. 8), and perhaps elsewhere)? [Terrence Joyce, USA]	Noted. ZJ is defined in the Figure 3.2 caption. It is now also defined the first time it appears in the text.

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3-179	3	7	46			Reference to Levitus et al (2005) is incomplete, include issue number (2), article number (L02604) and doi (10.1029/2004GL021592) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-180	3	7	46			I think essential to give best estimate of heating below 700m; not simply state studied by Levitus 05 [Stephen E Schwartz, USA]	Noted. AR5 is directed to focus on what is new since A4. However, with the publication of analyses to 2000-m in Levitus et al. (2012, Geophys. Res. Lett.) there are now new estimates that are included in the revision.
3-181	3	7	54	55		The authors refer to "greater than zero below that depth". It would be clearer to be explicit about the depth range, e.g. 3000m-6000m. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. 3000 m to the ocean floor.
3-182	3	7				It may be relevant if the authors show geographical distribution of the linear trend of 0 to 700 m upper ocean heat content for 1970 to 2009 for the world ocean. [Ravichandran Muthalagu, India]	Accepted. A spatial map of trends from 1971 to 2010 in 0-700 m temperature has been added to Fig. 3.1. Also note that Fig. 3.10 (now Fig. 3.9) also has zonal basin averages of temperature trends, which are referred to in the revised section 3.2.
3-183	3	8	8		18	This reads like there is a bias toward the Southern Ocean. The section (or elsewhere) would benefit from an introductory description of the global network of ocean currents and associated heat transport. [Hans Poertner, Germany]	Noted. The strongest warming is found in the Souther Ocean. The deep water changes in the N. Atlantic are discussed in the very next paragraph.
3-184	3	8	18	8	18	The citation to Masuda et al is puzzling, since we are most concerned with irreversible warming. Planetary waves of the sort discussed by Masuda et al (i.e., linear waves as present in an adjoint model) do not break, so do not lead to irreversible temperature change. This caveat is essential if reference to Masuda et al is made. [Stephen Griffies, USA]	Rejected. A planetary wave can create a long term change if the forcing is sustained (e.g. Kawase, 1987, J. Phys. Oceanogr.).
3-185	3	8	20	8	23	This passage is not very clear: what should one conclude then from the North Atlantic heat content? [Leticia Cotrim da Cunha, Germany]	Noted. The literature is not very clear either, without much change since AR4 in knowledge.
3-186	3	8	20			Replace "NADW" by "North Atlantic Deep Water (NADW)" since is the first time that the acronym is used. [Mauro Cirano, Brazil]	Accepted
3-187	3	8	27	8	27	The term "It is virtually certain that" makes little sense here, and should be deleted. [Martin Hovland, Norway]	Rejected. This is calibrated uncertainty language, consistent with IPCC guidance for expressing levels of confidence.
3-188	3	8	27	8	27	Here, it is stated that "It is virtually certain that the upper ocean has warmed". In Box 3.1, pg 9 line 35. it is stated that "there is unequivocal evidence that Earth has gained substantial energy from 1970-2009". The two ought to be consistent given that the Earth's energy gain is dominated by ocean heat content. Virtually certain is the phrase used in the synthesis at the end of the chapter. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. Pre 1970 estimates are now shown in Fig. 3.2, although the period 1971-2010 is chosen for trends since the upper ocean is better sampled during this 40-year period than for previous times. Also, the recent apparent slow down is not statistically signficant.
3-189	3	8	27	8	28	Why such emphasis on 1970? Is it just because that is when you made measurements. There should be some matterial on previous behaviour and you should mention that the rate of warming has fallen recently [VINCENT GRAY, NEW ZEALAND]	Noted. Pre 1970 estimates are now shown in Fig. 3.2, although the period after 1970 is chosen for trends since the upper ocean is better sampled during this period than previous years. Also, the recent apparent slow down is not statistically signficant, H214.
3-190	3	8	33		37	This information on warming trends is mentioned three times in the same format. This conclusion should just focus on the key details whereas the text could expand a little more. [Hans Poertner, Germany]	Noted. However, the conclusion does have to summarize what is found in the text.
3-191	3	8	40		40	The conclusion section lacks a statement on North Atlantic deep water formation. [Hans Poertner, Germany]	Accepted. This lack is addressed in the revised conclusion section.
3-192	3	8	40			From looking at Zenk and Morozov (reference already cited), it looks like the warming trend in AABW in the Atlantic started before the 1990s - late 1970s or early 1980s, I would say. [Michael Meredith, UK]	Rejected. From the abstract of Zenk and Morozov (2007): "Originally a long-term temperature increase in the near-bottom jet was noted from 1992

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							onward, after a period of rather constant abyssal temperatures since 1972."
3-193	3	8	45	9	43	This is all based on the theory that the only energy changes in the atmosphere are caused by radiation. It ignores convective and evaporation/precipitation exchanges which are dominant and are the main influences on the ocean. No part of the earth is ever in equiliubrium and there is overwhelming evidence that the radiant energy entering is never equal to the radiant energy leaving. The figures of a tranquil, unchanging earth and atmosphere are completely unrealistiuc. The atmosphere and the ocean are in constant motion. [VINCENT GRAY, NEW ZEALAND]	Rejected. This is an inventory of energy change in the earth climate system. Most of the energy is accumulating in the ocean. The evidence is virtually certain that heat has been accumulating in the earth system since at least 1970.
3-194	3	8	47	8	47	The statement needs clarification and can be improved by inserting: "from the sun" between "energy" and "entering" [Martin Hovland, Norway]	Accepted.
3-195	3	8	47	8	48	how many decades? [Leticia Cotrim da Cunha, Germany]	Accepted. "since at least 1970".
3-196	3	8	47	8	53	It isn't clear whose data is used in Box 3.1 figure. [John Abraham, USA]	Noted. That data used are described from page 9 lines 5-33 in the FOD. The revised figure caption directs the reader to the text.
3-197	3	8	47			I think the Murphy reference is inappropriate. Murphy conclusion is based on ocean heat content, which is really from Levitus. And the satellite data that Murphy relies on is incapable of any statement about imbalance. [Stephen E Schwartz, USA]	Accepted. The revised document references Church et al. (2011).
3-198	3	8	48	8	48	The statement needs clarification and can be improved by inserting: "solar and GHG-induced" between "excess" and "energy" [Martin Hovland, Norway]	Noted. Please see response to comment 3-194.
3-199	3	8	48			Reference to Murphy et al (2009) is incomplete, include article number (D17107) and doi (10.1029/2009JD012105) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-200	3	8	51	8	51	It is stated that "turbulence" transfers heat rapidly. Not really. Turbulence relates to randomized motions of particles and larger scale structures like turbulent eddies. This isn't really what is driving ocean heat transfer. It is more accurate to say "advection" instead of [John Abraham, USA]	Accepted. The sentence has been removed. See comment 3-201.
3-201	3	8	51	52		I would suggest removing the sentence beginning "Also, as a fluid .." - the redistribution of heat by the ocean is not of direct relevance to the global radiation imbalance (except where it moves the heat into areas which we do not observe). [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted.
3-202	3	8	52	8	52	Drop "also". [Stephen Griffies, USA]	Accepted.
3-203	3	8	52	8	53	Change sentence to "…, ICEFREE oceans have a low albedo…" [Christoph Heinze, Norway]	Accepted.
3-204	3	8	52	9	53	Again, figures which are highly inaccurate estimates. At least put "about" in front of them. [VINCENT GRAY, NEW ZEALAND]	rejected. Figures not highly inaccurate
3-205	3	8				Box 3.1, Figure 1: Purple is too dark, I cannot see the lower dashed line. [Christopher Kavanagh, Monaco]	Accepted.
3-206	3	9	5	9	13	for consistency with rest of chapter: unit of mass should be given as kg (instead of g). [Andreas Sterl, Netherlands]	Accepted.
3-207	3	9	5		13	Give numbers for atmos increase; also other components; too small to read off graph. [Stephen E Schwartz, USA]	Accepted.
3-208	3	9	9	9	10	I would recommeng using SI units i.e. kg rather than g, as done a few lines later (L22) [gael alory, France]	Accepted.
3-209	3	9	9	9	11	Use of cgs units should be avoided, to remain consistent with remainder of report. [Stephen Griffies, USA]	Accepted.
3-210	3	9	11	9	11	I see a value of 0.07 in the Held/Soden reference. I don't see 0.075… Please check [John Abraham, USA]	Noted. They round down to 0.07 everywhere but in

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							Fig 2, where they use 0.075, so 0.075 is retained.
3-211	3	9	22	9	24	There seems to be a contradiction here: if warming of the ice takes fractionally more energy than its fusion, then it is not a second order effect compared to fusion and should be accounted for [gael alory, France]	Noted. The energy going into warming the ice to just under the melting point is very small compared to the heat of fusion required to melt it. The text has been rephrased.
3-212	3	9	26	9	28	The uncertainty range is taken from the updated Domingues et al. estimate of ocean heat content (shown in Figure 3.2). Therefore it does not span the full uncertainty range. If a different estimate of upper ocean heat content was used (e.g. Levitus et al. estimate shown in Figure 3.2) then it could lie outside this uncertainty range. To get a better idea of the uncertainty, other estimates of UOHCA should also be used. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. Figure 3.2 now shows more curves. However, the updated Domingues et al. (2008) estimate is the only long-term published estimate that fills gaps and estimates uncertainties using ocean statistics, and so it is used here in the energy box."
3-213	3	9	35	9	43	What is the reason for discussing two different periods (1970-2009 versus 1993-2009)? If the idea is to draw a conclusion from the different values found, that has to be clarified in the text. Otherwise, I suggest rephrase the paragraph and discuss a single period. [Mauro Cirano, Brazil]	Noted. The shorter period is chosen to match to the altimeter period, as discussed in section 3.7 and Chapter 13.
3-214	3	9	35	9	45	Interestingly, the contribution of icemelt to the energy budget of the ocean is mentioned here but this did not happen later on for salinity of freshening trends? Can this be changed? [Hans Poertner, Germany]	Agreed. discussion of ice melt in salinity section added.
3-215	3	9	35		43	The conversions to W m-2 are useful; put ± on all numbers; specify that these are for ocean area only; suggest convert total to area of planet 5.1 e 14 m2, also. [Stephen E Schwartz, USA]	Noted. These are kept to ocean only because they are used in section 3.4
3-216	3	9	35			Delete unequivocal. I don't necessarily disagree, but it is not needed. [Christopher Kavanagh, Monaco]	Noted. Changed to "virtually certain" to be consistent with section 3.2.
3-217	3	9	48			Section 3.3: It would be useful to refer to Figure 3.10 somewhere in this section, since that figure presents long-term salinity trends for individual ocean basins. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accept. Reference to figure (now Fig. 3.9)added.
3-218	3	9	52			Section 3.3 could use a moderate re-write as it tends to emphasize a conclusive trend even where the variability contradicts a global statement. The magnitude of the salinity change is not mentioned for global scale and regional North Atlantic and Arctic Oceans. When mentioned in the other regions, units are not given. The regional descriptions vary from absolute unit change to rate of salinity change to fw volume change among regions. Depth of coverage is probably too much, e.g. sub-regional Arctic Ocean information which is only partially presented. Reference to 33ppt "fresh" Pacific waters is idiomatic. [Christopher Kavanagh, Monaco]	Accepted partially. There isn't and shouldn't be a measurable global trend, because the total amount of fresh water entering the ocean from glacial melt is not big enough to produce a discernable global trend.
3-219	3	9	52			This is the strangest sentence of the chapter. Would there be a water cycle without the ocean? [Christopher Kavanagh, Monaco]	Reject. Percentages quoted in the sentence indicate that in fact the ocean isn't all of the water cycle. H291
3-220	3	9	53			add “the horizontal salinity distribution” [VINCENZO ARTALE, ITALY]	Accept partially. Change to "horizontal salinity distribution of the upper ocean"
3-221	3	9				The box would be a good place to make the point that the determination of planetary heating rate is important to understanding the role of planetary heating in understanding the climate system response to forcing. [Stephen E Schwartz, USA]	Noted. An appropriate reference will be included.
3-222	3	10	5	10	6	The Clausius/Clapeyron relationship applies only to systems in eqauilibrium. Since no part of the climate is ever in equilibrium agreement with Clausius/ Clapeyron would be unlikely. [VINCENT GRAY, NEW ZEALAND]	Accept partially - the sentence already acknowledged that C-C would not be expected to be satisfied exactly, but is now phrased to make that stand out more clearly.
3-223	3	10	11	10	11	Atmosphere and ocean exchange freshwater rather than moisture [gael alory, France]	Accept. Corrected.
3-224	3	10	14	10	16	Change sentence to "…because salinity changes (as temperature changes) affect the density field and hence stratification and circulation, and therefore…". Add: Salinity variations are in particular important for the ocean density field at low temperatures (high latitude oceans), while in warm waters temperature changes dominate variations in seawater density. [Christoph Heinze, Norway]	Accept.

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3-225	3	10	14			Reference to Yu (2011) is incomplete, include article number (C10025) and doi (10.1029/2010JC006937) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-226	3	10	18	10	21	Would a brief comment on the new satellite-derived salinity data fit here? [Leticia Cotrim da Cunha, Germany]	Reject. Satellite mission is too new, and the expected precision too coarse to track the changes described thus far with hydrography and Argo
3-227	3	10	19			Replace "50 years" for "the period between years 1955 to 1998" [Mauro Cirano, Brazil]	Accept.
3-228	3	10	19			Reference to Boyer et al (2005) is incomplete, include issue number (1), article number (L01604) and doi (10.1029/2004GL021791) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-229	3	10	32	10	32	Boyer et al. 2007 does not describe SSS but freshwater content trends [gael alory, France]	Accept. Reference moved down to 3.3.2.2
3-230	3	10	32			Reference to Boyer et al (2007) is incomplete, include issue number (16), article number (L16603) and doi (10.1029/2007GL030126) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-231	3	10	39			“fresh Pacific” to be replaced with “fresh Pacific and Indian” [Ravichandran Muthalagu, India]	Reject. See explanation for comment 3-69.
3-232	3	10	40			“subpolar North Atlantic” is an exception to what? The sentence it relates to is so far away, that a line may help. Suggestion: “no trend has been observed” or “less rain but decrease/constant salinity”, etc. [Francois DANIS, France]	Accept. Sentence edited.
3-233	3	10	42	10	42	Significant at 99% level: This comment is valid for the whole chapter. Depending on the subject, heat-, salinity-, later on sea-level changes, the authors are using 95%, 99%, 90% confidence. Could it generate any "bad" criticism from the possible climate-skepticals reading the AR5? Maybe at least explain why different confidence levels are being used for different subjects. [Leticia Cotrim da Cunha, Germany]	Accept partially. 90% confidence interval is used throughout the chapter where possible and where calculated specifically for this chapter. In accord with TSU guidelines, when different intervals are used in the original references, they are reported here as in the references, and not altered.
3-234	3	10	55	10	56	Reference to Helm et al (2010) is incomplete, include article number (L18701) and doi (10.1029/2010GL044222) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-235	3	10				Figure 3.4 a): Purple is too dark; cannot read the contour. Use black for land masses (as with other figures). [Christopher Kavanagh, Monaco]	Editorial
3-236	3	10				Figure 3.4 b): Cannot read the contours. Remove the ocean color and leave the numbers. Use black for land masses. [Christopher Kavanagh, Monaco]	Editorial
3-237	3	10				A nice comprehensive picture emerges for the distribution of ocean salinity. [Hans Poertner, Germany]	Noted
3-238	3	11	2			Replace "abd" by "and" [Mauro Cirano, Brazil]	Accepted - text revised
3-239	3	11	18	11	20	Maybe briefly suggest here what could be done to have a quantitative assessment: models, more data, analysis of time-series data. [Leticia Cotrim da Cunha, Germany]	Reject. The purpose here is to describe what has been observed; other international groups are advising on how best to sample in the future.
3-240	3	11	22			The previous text under 3.3.2. had regional aspects which are now elaborated again. For a clearer picture authors may consider merging these two subsections or remove overlaps. [Hans Poertner, Germany]	Accept. Edited to remove overlaps.
3-241	3	11	25	11	25	correct ' regions of net evaporation have become drier' [Jae Hak Lee, Republic of Korea]	Accept. Rephrased.
3-242	3	11	33	11	34	By how much? [Christopher Kavanagh, Monaco]	Accept. Information and citations clarified.
3-243	3	11	33	11	36	“in 50 years” refers to the period of 1949-2008? [Rongshuo Cai, China]	Accept change. 1955-2003.
3-244	3	11	36	11	37	Reference to Delcroix et al (2007) is incomplete, include issue number (C3), article number (C03012) and doi (10.1029/2006JC003801) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-245	3	11	41	12	28	This part would benefit from including the more holistic perspective and observation-based analysis of Steele and Ermold (2007; J. Clim., 20, p.403-417) [Tor Eldevik, Norway]	Accept. Added reference Steele and Ermold.
3-246	3	11	43			Reference to Nakano et al (2007) is incomplete, include issue number (23), article number (L23608) and doi (10.1029/2007GL031433) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-247	3	11	44	11	44	“sutface” should be “surface”. [Rongshuo Cai, China]	Accepted - text revised
3-248	3	11	44	11	44	Spelling of surface. [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-249	3	11	44	11	44	correct ' sutface' [Jae Hak Lee, Republic of Korea]	Accepted - text revised
3-250	3	11	44			Replace "sutface" by "surface" [Mauro Cirano, Brazil]	Accepted - text revised
3-251	3	11	44			should read surface [Hans Poertner, Germany]	Accepted - text revised
3-252	3	11	50	12	8	Any comment about the South Atlantic? Looking at figure 3.4 one would think it has also become saltier. If there is a lack of data for this region, maybe also quickly comment this. It is crucial to emphasize the importance of data availability for the oceans. [Leticia Cotrim da Cunha, Germany]	Accept. Title of section 3.3.3.2 revised to" Atlantic", salinity change of S. Atlantic added.
3-253	3	11	51	11	52	between 1955-1959 and 2002-2006? [gael alory, France]	Accept (rephrased).
3-254	3	11	56	11	57	the following paper cited also in other part of AR5 could be useful for understand the connection between Mediterranean and North Atlantic at multidecadal scale : “Marullo, S, V. Artale and R. Santoleri. The SST multidecadal variability in the Atlantic-Mediterranean region and its relation to AMO, J. of Climate, 2011, Vol. 24, No. 16, pages 4384-4400, doi:10.1175/2011JCLI3884.1” that investigate SST long-time trends and multi-decadal variability in the North Atlantic and Mediterranean Sea [VINCENZO ARTALE, ITALY]	Accept partially. Marullo et al. reference deals with temperature. We have added 3 other new references (Fusco et al., Smith et al.,and Vargas-Yanez et al.)
3-255	3	11	57			Reference to Yashayaev and Loder (2009) is incomplete, include article number (L01606) and doi (10.1029/2008GL036162) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-256	3	11				(whole section 3.3.3) add value to the entire chapter a possible new section regarding the Mediterranean sea for the following reasons: first this basin is the best example where evaporation is the dominant term in hydrological cycle change, secondly because are experiencing a particular intense warming (hot spot basin) and in particular after the '90s is well evident a warming trend, but the same basin has performed also a decadal and multi decadal climate variability that unfortunately all models is not able to reproduce this kind of variability (see in particular for the warming of the Balearic Archipelago “How much is the Western Mediterranean really warming and salting?” by Manuel Vargas-Yánez et al., Journal of Geophysical Research-Oceans 115: C04001, April 2010 doi:10.1029,2009JC005816, and for multidecadal variability, Marullo et al., The SST multidecadal variability in the Atlantic-Mediterranean region and its relation to AMO, J. of Climate, 2011, doi:10.1175,2011JCLI3884.1). [VINCENZO ARTALE, ITALY]	Rejected.. Due to limited space, Chapter 3 has decided not to include sections on more regional aspects outside the major ocean basins
3-257	3	11				Albeit, the impact of the Mediterranean Outflow Water (MOW), on the strength of the Atlantic THC is considered relatively small (e.g., Rahmstorf, S., 1998: Influence of Mediterranean outflow on climate. Eos, 79 (24), 281- 282.), Artale et al. (North Atlantic THC sensitivity to Mediterranean waters, Tellus, Series A, Vol.54, Issue 2, 159-174, 2002 or Artale, V. et al., 2006: The Atlantic and Mediterranean Sea as connected systems. In: P.Lionello, P., Malanotte-Rizzoli and R. Boscolo (Eds), Mediterranean Climate Variability, Amsterdam: Elsevier, pp. 283-323) and Calmanti et al. (North Atlantic MOC variability and the Mediterranean outflow: a box-model study; Tellus, Series A , 58A, 416-423.2006) suggested that this water can contribute to the strengthening and variability of the North Atlantic THC and finally Wu et al., (Wu W., G. Danabasoglu, and W.G. Large, On the effects of parameterized Mediterranean overflow on North Atlantic ocean circulation and climate, Ocean Modelling, Volume 19, Issue 1-2, Pages 31-52, 2007) have show the role of MOW on modulating surface heat fluxes in the deep water formation region of the north Atlantic by advecting salty water of Mediterranean origin. Moreover Lozier and Stewart (On the Temporally Varying Northward Penetration of Mediterranean Overflow Water and	Rejected. Model results are outside the scope of Chapter 3 Ocean: observations. Chapter 3 has decided not to include sections on more regional aspects of marginal seas

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						Eastward Penetration of Labrador Sea Water, Journal of Physical Oceanography, Vol. 38, 2097-2103.2008) in an analysis of historical hydrographic data in the eastern North Atlantic suggest a connection between the northward penetration of the MOW and the location of the subpolar front, leaving open the discussion of the possible effect of water mass transformation in the subpolar regions. [VINCENZO ARTALE, ITALY]	
3-258	3	12	3			Reference to Holliday et al (2008) is incomplete, include volume number (350 and issue number (3) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-259	3	12	4	12	4	Fram Strait is not located at 89°N [Reiner Steinfeldt, Germany]	Text revised.
3-260	3	12	8			Reference to Reverdin et al (2002) is incomplete, include issue number (C12), article number (8010) and doi (10.1029/2001JC001010) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-261	3	12	12	12	13	Reference to Kwok et al (2009) is incomplete, include article number (C07005) and doi (10.1029/2009JC005312) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-262	3	12	12		28	There is much discussion of Arctic freshwater changes in a recent Morison et al. paper, which should be discussed and cited here: Morison et al., "Changing Arctic Ocean freshwater pathways". Nature, 481, 10.1038/nature10705. [Michael Meredith, UK]	Accept. Reference included in 3.3.3.3 Arctic Ocean.
3-263	3	12	22			Reference to Shiklomanov and Lammers (2009) is incomplete, include issue number (4), article number (045015) and doi (10.1088/1748-9326/4/4/045015) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-264	3	12	25	12	25	Canda Basin is a name (basin also starting with capital letter) [Christoph Heinze, Norway]	Accepted - text revised
3-265	3	12	25			Reference to Proshutinsky et al (2009) is incomplete, include article number (C00A10) and doi (10.1029/2008JC005104) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-266	3	12	25			Reference to Yamamoto-Kawai et al (2009) is incomplete, include article number (C00A05) and doi (10.1029/2008JC005000) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-267	3	12	26			Reference to McPhee et al (2009) is incomplete, include article number (L10602) and doi (10.1029/2009GL037525) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-268	3	12	28	12	28	A citation seems useful here to support this statement. [Stephen Griffies, USA]	Accept.
3-269	3	12	32	12	32	Please refer to comment 11. [Leticia Cotrim da Cunha, Germany]	Reject. (We think this is the comment about making recommendations about future measurements. See response to comment 3-329.)
3-270	3	12	32		39	This section includes discussion of the freshening trend in the Ross Sea, which is indeed significant. Are there salinity trends in other sectors of subpolar and shelf regions of the Southern Ocean? If so, worth mentioning briefly for completeness, especially if there are opposing trends (otherwise it looks like the authors are only interested in reporting freshening trends, which Im sure isnt the case). [Michael Meredith, UK]	Reject. We are not aware of published manuscripts since AR4 that report increasing salinity in S.O. surface water.(Reference to Ross Sea freshening has been moved to section 3.5.5.)
3-271	3	12	36			Reference to Meijers et al (2011) is incomplete, include article number (C08024) and doi (10.1029/2010JC006832) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-272	3	12	41	12	52	This is a repeat of what was discussed earlier - it seems redundant, but when I see this repeated AGAIN in section 3.3.5, it seems like overkill. Please reduce the emphasis on this 'pet rock'! And could someone comment on the possibility that the relationship between E-P and surface salinity change is entirely due to changes in the mixed layer depth and not the hydroplogical cycle? [Terrence Joyce, USA]	Accepted. Moved information into 3.3.1 and condensed.
3-273	3	12	43	12	52	This paragraph has to be expanded to include a better explanation of the freshwater content changes presented in Figure 3.5, comparing the world results with the results for the Atlantic, Pacific and Indian Oceans. It is important to point out that for some regions, the response of each ocean can be different and	Accept, include either in this material or within regional paragraphs

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						opposite (eg Northern Hemisphere). [Mauro Cirano, Brazil]	
3-274	3	12	43		52	Naveira Garabato et al. (2009) (reference already cited, but under Garabato et al, 2009) made some statements about the degree to which salinity changes in mode and intermediate waters in the Atlantic appear to be governed by modes of climate variaiblity as opposed to an acceleration of the hydrological cycle. Accepting that the latter may influence the former anyway, it is still worth discussing this here. [Michael Meredith, UK]	Accept. Reference has been corrected, but now appears in section 3.5.5 instead of 3.3.3.3. Discussion in 3.5.5 now includes modes of variability.
3-275	3	12	47	12	47	"have" instead of "has". [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-276	3	12	52			Reference to Stott et al (2008) is incomplete, include issue number (21), article number (L21702) and doi (10.1029/2008GL035874) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-277	3	13	10			“fresh Pacific” to be replaced with “fresh Pacific and Indian” [Ravichandran Muthalagu, India]	Reject. See 3-69.
3-278	3	13	26	13	26	Check if the abbreviation "SST" for sea surface salinity has been already used previously, I don't think so. [Leticia Cotrim da Cunha, Germany]	Reject. SST is indeed what they mean, and the acronym has been defined previously.
3-279	3	13	36	13	38	Please avoid using semi-colon. The sentence in lines 36 and 38 could easily be divided by full stop. [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-280	3	14	7	14	29	too long. Just mention the main differences between the three products mentioned (HOAPS, OAFlux, CORE) and motivate why in the next paragraph (lines 31-38) results from OAFLux are shown and not those from one of other sets. [Andreas Sterl, Netherlands]	Rejected. Disagree that the para is 'too long'. The reviewer advocates that we 'just mention the main differences between the three products mentioned (HOAPS, OAFlux, CORE)' and motivate use of OAFlux. This is what we have done in the existing text from the FOD and it is not possible to reduce the para length significantly without removing essential material. Nevertheless, we have reduced it as much as is reasonably possible in the SOD
3-281	3	14	10	14	10	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	Rejected. Unclear comment - what does it mean?
3-282	3	14	24	14	26	The acronym "CORE" stands for "Coordinated Ocean-ice Reference Experiments". This term was introduced in the paper Griffies et al. (2009), and this paper should be cited here on line 26. Also, the Large and Yeager (2009) dataset runs from 1948-2007. [Stephen Griffies, USA]	Accepted - text revised
3-283	3	14	47	14	47	check "tropics" with capital T. [Leticia Cotrim da Cunha, Germany]	Noted
3-284	3	14	56	15	2	which would be the ncertainty of these observations compared to the methods previously cited? [Leticia Cotrim da Cunha, Germany]	Noted Land based radiation observations are discussed in Chapter 2, we do not feel it is necessary to discuss them in further detail here.
3-285	3	14	56			Reference to Wild (2009) is incomplete, include article number (D00D16) and doi (10.1029/2008JD011470) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-286	3	15	4	15	18	The following paper gives a good review of global dimming and brightening and should be mentioned her: Wild, M. (2009) Global dimming and brightening: A review J. Geophys. Res., 114, D00D16, doi: 10.1029/2008JD011470 [Andreas Sterl, Netherlands]	Noted This paper is cited in previous paragraph (and also in the Chapter 2)- no need to repeat the citation.
3-287	3	15	6	15	7	Reference to Cermak et al (2010) is incomplete, include article number (L21704) and doi (10.1029/2010GL044632) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-288	3	15	9	15	9	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	Rejected. Unclear - what does this mean?
3-289	3	15	10			Reference to Hinkelman et al (2009) is incomplete, include article number (D00D20) and doi (10.1029/2008JD011004) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-290	3	15	11	15	11	Abbreviation of ISCCP-FD? [Leticia Cotrim da Cunha, Germany]	Accepted, text revised, as International Satellite Cloud Climatology Project - Fast delivery radiative fluxes data
3-291	3	15	11	15	11	the abbreviation ISCCP-FD is not explained [Reiner Steinfeldt, Germany]	See 3-290.
3-292	3	15	12			Reference to Romanou et al (2007) is incomplete, include issue number (5), article number (L05713) and doi (10.1029/2006GL028356) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-293	3	15	15	15	18	Very long sentence. [Leticia Cotrim da Cunha, Germany]	Accepted -sentence now split into two for clarity.
3-294	3	15	25	15	25	Again, "flux is small": small compared to what? The 2nd half of the sentence implies, "small compared to ability to detect" or possibly "small compared to natural variability", but please be explicit. [Marcus Sarofim, USA]	Accepted. "Small" put in the context of uncertainty of determination of changes
3-295	3	15	30			Reference to Kawai et al (2008) is incomplete, include issue number (C8), article number (C08021) and doi (10.1029/2007JC004525) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-296	3	15	36	15	37	Reference to Smith et al (2009) is incomplete, include article number (D12107) and doi (10.1029/2008JD011580) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-297	3	15	38	15	38	Abbreviation of SLP? [Leticia Cotrim da Cunha, Germany]	Accepted - now defined as Sea Level Pressure
3-298	3	15	38			I could not find SLP explained anywhere in the chapter. [Hans Poertner, Germany]	See 3-297.
3-299	3	15	52	15	52	Full name for MERRA should be given. [Rongshuo Cai, China]	Accepted - defined as Modern Era Retrospective Reanalysis
3-300	3	15	52	15	52	Abbreviation of MERRA and ERA? [Leticia Cotrim da Cunha, Germany]	See 3-299. For ERA - ECMWF Reanalysis
3-301	3	15	52	15	52	References and acronym explanations for the MERRA and ERA-interim reanalysis data sets would be useful. Homogenise citations of reanalyes with page 3-16, l. 13-40. [Christoph Heinze, Norway]	Accepted - see 3-299 .
3-302	3	16	4	16	4	Check the spelling of OAFLUX. All capitals or OAFlux? And maybe use here "evaporation" and "precipitation" in the sentence. [Leticia Cotrim da Cunha, Germany]	Accepted. Corrected to OAFlux.
3-303	3	16	4	16	9	According to section 3.3.2.1 there are discernible regional trends in SSS that must be related to changes in E-P. That link/reference should be made. Obviously data quality of E-P is much worse than that of SSS. [Andreas Sterl, Netherlands]	Noted A reference has now been added here to 3.3.2.1.
3-304	3	16	13	16	23	Maybe also cite: Saturation of the Southern Ocean CO2 Sink Due to Recent Climate Change Corinne Le Quéré, Christian Rödenbeck, Erik T. Buitenhuis, Thomas J. Conway, Ray Langenfelds, Antony Gomez, Casper Labuschagne, Michel Ramonet, Takakiyo Nakazawa, Nicolas Metzl, Nathan Gillett, and Martin Heimann - Science 22 June 2007: 316 (5832), 1735-1738.Published online 17 May 2007 [DOI:10.1126/science.1136188] [Leticia Cotrim da Cunha, Germany]	Rejected - this paper is not relevant to this sub-section.
3-305	3	16	13	16	34	NH wind stress discussion is pretty weak compared to previous items presented. (see Wu et al, Nature, Climate Change, 2012) [Terrence Joyce, USA]	Accepted. Wind stress discussion revised and extended along with the reference to Wu et al- 2012 in section 3.4.4.
3-306	3	16	13	16	40	Homogenise citations of atmospheric reanalysis data sets (ERA, ECMWF, NCEP) with page 3-15, l. 52, and page 3-14, l. 23. Explain respective acronyms. [Christoph Heinze, Norway]	See 3-301.
3-307	3	16	13		23	This paragraph should provide a global picture first, but focuses on the southern ocean? [Hans Poertner, Germany]	Noted. The wind stress discussion has now been revised to include other basins in more detail, specifically the North Atlantic. In section 3.4.4
3-308	3	16	15	16	15	Abbreviation of ECMWF? [Leticia Cotrim da Cunha, Germany]	See 3-301.

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3-309	3	16	17	16	18	Replace "Southern Annular Mode (SAM)" by "SAM", since the acronym was already defined before [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-310	3	16	18	16	18	change "Yan et al. (2010)" to "Yan et al. (2011)" [Zeng-Zhen HU, USA]	rejected Reference Yan et al 2010 or 2011 nor cited
3-311	3	16	19	16	19	Abbreviation of CFSR? [Leticia Cotrim da Cunha, Germany]	See 3-301.
3-312	3	16	21			NCEP CFSR is an impressive acronym and one of those unexplained in the chapter. [Hans Poertner, Germany]	See 3-301.
3-313	3	16	22	16	22	change "Yan et al. (2010)" to "Yan et al. (2011)" [Zeng-Zhen HU, USA]	rejected reference Yan et al., 2010 or 20111 not cited
3-314	3	16	28	16	28	"Xialoan Wang" should be "Xiaolan Wang" [Zeng-Zhen HU, USA]	Name corrected.
3-315	3	16	33	16	33	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	Unclear - what does this mean?
3-316	3	16	36	16	40	Please note that Vecchi et al (2006) showed a weakening trend in tropical Pacific trade winds using the same approach [gael alory, France]	accepted reference and results added
3-317	3	16	36		40	This paragraph should conclude with an assessment? [Hans Poertner, Germany]	Noted. The wind stress discussion has been significantly revised / extended
3-318	3	16	53	16	53	Maybe use here "more reliable than the other" instead of superior. [Leticia Cotrim da Cunha, Germany]	Editorial
3-319	3	17	4	17	4	What is definition or method of the “20C Reanalyses”? [Rongshuo Cai, China]	accepted explained in text
3-320	3	17	4	17	4	What are 20C reanalyses? Maybe explicit here "20th Century". [Leticia Cotrim da Cunha, Germany]	See 3-320
3-321	3	17	4	17	4	Explain "20C Reanalyses". Do you mean the "20C Reanalysis" or various reanalyses for the 20th century (I think the latter would be correct). [Christoph Heinze, Norway]	see 3-320
3-322	3	17	4	17	6	the wave hindcasts in Wang et al. (2009) is not based on 20C Reanalyses, it is is based on ERA40 wave reanalyses and on statistical hindcasts using HadSLP2 data. The reference for this sentence should be Wang et al. (2012), which is a manuscript yet to be published (should be submitted before 31 July 2012). [Xiaolan Wang, Canada]	accepted
3-323	3	17	9	17	9	Abbreviation of WAM? [Leticia Cotrim da Cunha, Germany]	noted definition included
3-324	3	17	15	17	15	It is unclear whether the "Trends" referred to in this sentence are increasing or decreasing. [Stephen Griffies, USA]	accepted quantitative estimates added from e.g. Young et al. (2011)
3-325	3	17	15	17	18	What are the trends for extreme waves? [Christopher Kavanagh, Monaco]	taken into account Indeed, there is a problem with the accurate quantification of the trends in extreme waves, instead changes from decade to decade were added
3-326	3	17	15			This text is unnecessarily vague. One wonders what the trends in extreme waves are and whether they could be quantified? [Hans Poertner, Germany]	Accepted text revised and numbers added
3-327	3	17	16	17	18	Important reference that could be added: Ruggiero, Peter; Komar, Paul D.; Allan, Jonathan C. 2010. Increasing wave heights and extreme value projections: The wave climate of the US Pacific Northwest. COASTAL ENGINEERING: 57(5): 539-552. 10.1016/j.coastaleng.2009.12.005 [Eduardo Siegle, Brazil]	Accepted. Reference added.
3-328	3	17	17			Reference to Menendez et al (2008) is incomplete, include issue number (22), article number (L22607) and doi (10.1029/2008GL035394) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-329	3	17	17			Reference to Sasaki et al (2005) is incomplete, include issue number (15), article number (L15607) and doi (10.1029/2005GL023722) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-330	3	17	20	17	22	Figure 3.9 has to be included [Mauro Cirano, Brazil]	FOD Fig. 3.9 was removed,
3-331	3	17	25	17	25	Spelling of inhomogeneity. [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-332	3	17	28			Reference to Hemer (2010) is incomplete, include article number (L18601) and doi (10.1029/2010GL044595) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-333	3	17	33	17	42	The Young et al. paper has been heavily criticized in a comment by Wentz and Ricciardulli (Science 334, 18 Nov 2011, 905-b, doi: 10.1126/science.1210317). Reply by Young et al in same issue, p 905-c (doi: 1026/science.1210548). [Andreas Sterl, Netherlands]	accepted. Wentz' comment and the corresponding reference added, also the text refering to Young et al. (2011) have been adjusted accordingly
3-334	3	17	44	17	46	Is this finding related to less wave measurements (time series) in the southern oceans? Since there is a limitation of long time series measurements in the southern oceans (mainly South Atlantic), is this conclusion based on the global model results only? It should be stated clearly, since it is still difficult to confirm this tendency in the South Atlantic. Due to its importance and potential impacts on coastal communities, the description of wind-generated waves and changes in extreme events could be further explored. [Eduardo Siegle, Brazil]	taken into account a regional specification where SWH has increased was added
3-335	3	17	44	17	46	These lines are redundant, as they reappear directly in the next paragraph. [Reiner Steinfeldt, Germany]	See comment 3-334
3-336	3	17	46			ditto [Hans Poertner, Germany]	see comment 3-334
3-337	3	17	47	17	47	Considering the comment below about the sea level change, I suggest to include in this section (3.4) the text about storm surges, which are directly related to the waves. [Eduardo Siegle, Brazil]	taken into account. This issue is discussed, surges are related to waves, however, the major factor is on-shore wind current (going along with waves)
3-338	3	17	50	17	52	"extremely small": small compared to what? The second half of the sentence is more precise and I'd suggest rewording to combine the two halves - ie, "small compared to the detection ability of…". For example, one could state that ~0.5 W/m2 could actually be considered a large imbalance compared to the imbalance likely experienced over the last thousand years... [Marcus Sarofim, USA]	See 3-294
3-339	3	17	50		55	The results of this clarifying discussion should become evident earlier, in the respective text sections? [Hans Poertner, Germany]	Noted.
3-340	3	17	51	17	53	It is concluded that over the last fifty years, the regional pattern of sea surface salinity has been enhanced (Ch 3.3.5 p. 13, lines 7-9). Also, interbasin constrast between saline Atlantic and fresh Pacific surface waters has increased (p.13, line 10). But, why there is no significant trend in E-P? [Jae Hak Lee, Republic of Korea]	Noted. E-P datasets are not sufficiently accurate to identify E-P trend over past 50 years, see 3.4.3 last para.
3-341	3	17	53	17	53	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	Unclear comment - what does it mean?
3-342	3	17	54	17	54	For a trend in global mean E-P, see comment 18. [Zhaomin Wang, UK]	See 3-340.
3-343	3	17	55			Replace "Southern Annular Mode (SAM)" by "SAM", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - text revised
3-344	3	17	56	18	2	same as previous comment. [Eduardo Siegle, Brazil]	See 3-343
3-345	3	17	56			Replace "Significant Wave Height (SWH)" by "SWH", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - text revised
3-346	3	18	9	18	13	The following sentence is difficult to understand: "The water characteristics resulting from these interactions (e.g., temperature, salinity, dissolved gas concentrations, and dissolved nutrients concentrations) are transferred into the ocean interior in a process known as subduction or ventilation, and slowly modified by mixing and for some substances biogeochemical cycling as the water masses are advected by the large scale flow." Better split into two sentences: "The water characteristics resulting from these interactions (e.g., temperature, salinity, dissolved gas concentrations, and dissolved nutrients concentrations) are transferred into the ocean interior in a process known as subduction or ventilation, and slowly modified by mixing as the water masses are advected by the large scale flow. In addition, some substances are	Accepted text changed

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						influenced by biogeochemically induced sources and sinks." [Christoph Heinze, Norway]	
3-347	3	18	9	18	13	Run on sentence. [Christopher Kavanagh, Monaco]	editorial - text changed
3-348	3	18	11	18	11	Deep convection as process for water mass formation is missing here. [Reiner Steinfeldt, Germany]	accepted text changed
3-349	3	18	14	18	15	The meaning of the following sentence is unclear: "Water masses therefore provide a useful perspective on ocean change." Suggest to rpelace by: "Water mass analysis, therefore, provides a useful tool to asses modes and rates of ocean climate change." [Christoph Heinze, Norway]	Accepted. Text changed
3-350	3	18	18	18	24	when discuss the Figure 3.10, could be very relevant to comment the strong anomaly in temperature (but also in salinity) observed between 10S-20N in Pacific and in the Indian Ocean and between 100 an 200 meters, moreover part of this anomaly persist in density and temperature at global scale, but it seems a spike, an error; [VINCENZO ARTALE, ITALY]	accepted Fig. 3.10 modified
3-351	3	18	23	18	23	Replace "Stronger stratification inhibits ventilation…" by "Increasing stratification reduces ventilation…". (Full inhibition of ventilation may not necessarily result from an increase on stratification.) [Christoph Heinze, Norway]	Accepted - text modified
3-352	3	18	24	18	26	Four not two limitation are identified. The two refers to limitations of record length versus characteristics of the proxies; however, distribution, reliability and signal are properties of the proxies represent quite different limitations. They have different causes and are overcome in quite difference ways. [David Sauchyn, Canada]	Comments by David Sauchyn relate to another chapter
3-353	3	18	28	18	28	Suggest to insert 'e.g.,' before 'NAO', since other modes may also have impacts. [Zhaomin Wang, UK]	text removed, not applicable
3-354	3	18	30	18	34	where are these water masses ventilated? [Leticia Cotrim da Cunha, Germany]	text removed, not applicable
3-355	3	18	30	18	34	Awkward run on sentence. [Christopher Kavanagh, Monaco]	text removed, not applicable
3-356	3	18	31	18	31	Capital W for water. [Leticia Cotrim da Cunha, Germany]	text removed, not applicable
3-357	3	18	31			regarding the MOW role on the THC variability I found very interesting and new from physical point of view the Lozier and Stewart (2008) paper that in an analysis of historical hydrographic data in the eastern North Atlantic suggest a connection between the northward penetration of the MOW and the location of the subpolar front, leaving open the discussion of the possible effect of water mass transformation in the subpolar regions. [VINCENZO ARTALE, ITALY]	Noted. Topic beyond the scope of the assessment
3-358	3	18	34			Reference to Sarafanov et al (2008) is incomplete, include volume number (113) and issue number (C12) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-359	3	18	36	18	45	The discussion concerns "strong decadal variability" that is specifically related to water mass density changes restricted to the second decimal (at the 10 grams pr m3). This level of detail appears irrelevant in the present context. [Tor Eldevik, Norway]	Accepted. Paragraph is simplified and shortened.
3-360	3	18	36		45	This text would benefit from further explanations of symbols and contents. [Hans Poertner, Germany]	Accepted. However not relevant after change of text (see #359)
3-361	3	18	36			Replace "North Atlantic Deep Water (NADW)" by "NADW", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - text revised
3-362	3	18	38	18	38	Use "became warmer and saltier" instead of "warmer and got saltier". [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-363	3	18	39	18	39	in winter 2007/2008, Labrador Sea Water with a maximum density around sigma-theta=27.75 kg/m^3 has been formed (see, e.g. Väge et al., Nature Geosci., doi:10.1038/NGEO382 (2009), Yashayaev and Loder, Geophys. Res. Lett., 36, L01606 (2009)) so the formulation Since 1997, only lighter modes (27.68 < σΘ < 27.74 kg m-3 vs. 27.74 < σΘ < 27.80 kg m-3) have been ventilated' should be modified (e.g. 'mainly' instead of 'only') [Reiner Steinfeldt, Germany]	Accepted. The reference is added and the text changed.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-364	3	18	40	18	40	check citation [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-365	3	18	40	18	41	bizarre comment about invalid cittion. Also, a redundant reference is made to the deeper ventilation of LSW: Lebel et al. Also, reference could be made to the appearance of the changed mode of LSW in mid-latitudes in the DWBC (Pen˜a-Molino et al DSR I, 2011). [Terrence Joyce, USA]	partly Accepted. Pena et al. is added. LeBel et al. is added because the paper reports formation rates of uLSW and LSW in time periods where mostly LSW was formed.
3-366	3	18	40			Insert the missing reference [Mauro Cirano, Brazil]	Accepted - text revised
3-367	3	18	40			Invalid citation. [Christopher Kavanagh, Monaco]	Accepted - text revised
3-368	3	18	42	18	43	The decline in LSW formation from 7.7 Sv in 1997-99 to 0.5 Sv in 2003-05 cites Rhein et al. (2011). In this paper, these two different formation rates are apparent in Fig. 3C, part of a decadal decline, and are estimated from CFC inventories. There is presumably some uncertainty in such estimates. Can this be specified in the form of error bars? Other estimates for part of this period are not in agreement - e.g., model hindcast estimates (Marsh et al. 2005, see Fig. 11d). Reference: Marsh. R., de Cuevas, B. A., Coward, A. C., Nurser, A. J. G., and S. A. Josey (2005). Water mass transformation in the North Atlantic over 1985-2002 simulated in an eddy-permitting model. Ocean Science, 1, 127-144. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Partly accepted, error bars given.The model results are outside the scope of Ch3
3-369	3	18	44	18	45	The last sentence seems hastily written, could be improved. [Leticia Cotrim da Cunha, Germany]	Accepted sentence removed
3-370	3	18	45	18	45	the inventory increase was much smaller than expected' What does 'expected' refer to? [Reiner Steinfeldt, Germany]	accepted text removed
3-371	3	18	45			Reference to Steinfeldt et al (2009) is incomplete, include doi (10.1029/2008GB003311) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-372	3	18	45			The deeper form of LSW was renewed in 2008 - Våge et al, Nature Geosciences 2008… This should be mentioned as well. [Terrence Joyce, USA]	Accepted. Text changed. Reference added.
3-373	3	18	47	18	52	A much more holisitic and up-to-date description that covers the observed interannual-to-decadal water mass variability of inflow and overflows, including their potential sources, is provided by Eldevik et al. (2009; Nature Geoscience, doi: 10.1038/NGEO518). [Tor Eldevik, Norway]	Accepted - the sources of dense waters are beyond the scope, but the reference to the variability in the overflows is well worth referencing. Text changed.
3-374	3	18	47	18	52	It has recently been established that there are 2 distinct pathways for NADW entering trough Denmark Strait (Våge et al, Nature Geosciences 2011). So it is not just a matter of assessing changes in circulation, but also which pathways are being altered. This is a recent paper and was obviously missed in the first draft. Please cite and briefly discuss. [Terrence Joyce, USA]	Noted However, the pathways of the water masses outside the main ocean basins is beyond the scope of this chapter.
3-375	3	18	49	18	52	Any comment about the Baltic Sea and the water exchange dynamics through the Danish Straits? Jilbert, T., Slomp, C. P., Gustafsson, B. G., and Boer, W.: Beyond the Fe-P-redox connection: preferential regeneration of phosphorus from organic matter as a key control on Baltic Sea nutrient cycles, Biogeosciences, 8, 1699-1720, doi:10.5194/bg-8-1699-2011, 2011. [Leticia Cotrim da Cunha, Germany]	Noted. However, we only deal here with the main water masses. Denmark Strait is between Greenland and Iceland, not to be confused with the danish straits.
3-376	3	18	51		52	This statement reads fine but the reader's curiosity would be satisfied by providing an explanation. [Hans Poertner, Germany]	Accepted. Text rewritten
3-377	3	19	4	19	21	These two paragraphs could be re-written. Line 4 paragraph discusses O2 concentration changes but doesn't say much about what happens to the water masses themselves. It seems also a bit out of place because in the Atlantic section (page 18) this tracer (O2) is not cited. [Leticia Cotrim da Cunha, Germany]	partly accepted.text rewritten and shortened.
3-378	3	19	5	19	5	Check capital letters for eastern, western subarctic gyres [Leticia Cotrim da Cunha, Germany]	Accepted text rewritten and shortened
3-379	3	19	8	19	8	“CFC” should be given its terminology word: Chloro-fluoro-carbon? [Rongshuo Cai, China]	Accepted
3-380	3	19	8			Reference to Mecking et al (2008) is incomplete, include issue number (3), article number (GB3015) and doi (10.1029/2007GB003101) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-381	3	19	10			Reference to Deutsch et al (2006) is incomplete, include issue number (C9), article number (C09S90) and doi (10.1029/2005JC003179) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-382	3	19	15			Reference to Nakanowatari et al (2007) is incomplete, include volume number (34) and issue number (4) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-383	3	19	16			Reference to Kouketsu et al (2009) is incomplete, include article number (C01008) and doi (10.1029/2008JC004778) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-384	3	19	25	19	27	abbreviations for WOCE, CLIVAR? Capitals for Southern and Northern Hemispheres? [Leticia Cotrim da Cunha, Germany]	Accepted
3-385	3	19	31	19	31	Replace "intermediate surfaces" by "intermediate water density surfaces". [Christoph Heinze, Norway]	Accepted - text revised
3-386	3	19	33			Reference to Ridgway and Dunn (2007) is incomplete, include issue number (13), article number (L13612) and doi (10.1029/2007GL030392) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-387	3	19	34			Reference to Speich et al (2007) is incomplete, include issue number (23), article number (L23614) and doi (10.1029/2007GL031583) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-388	3	19	40		55	heavy uses of acronyms and jargon [Hans Poertner, Germany]	Accepted. Text modified
3-389	3	19	42			Update reference details for Schmidtko and Johnson (2011) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-390	3	19	44	19	50	Very long sentence. [Leticia Cotrim da Cunha, Germany]	Accepted. See #388
3-391	3	19	45			Update reference details for Kobayashi et al (2011) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-392	3	19	51	19	51	Sentence starting with "WW". [Leticia Cotrim da Cunha, Germany]	Rejected - text is correct, WW stands for Winter Water introduced in the sentece before, text modified
3-393	3	19	51		54	As well as precipitation and sea ice, there remains the possiblitiy (indeed, likelihood) that glacial melt inputs had some impact on WW freshening in the sector of Antarctica where AAIW precursors are formed. Should be mentioned. [Michael Meredith, UK]	accepted text revised
3-394	3	19	53		54	As well as the SAM, I recall ENSO and the IPO being cited as causal factors in the changes seen - check Naveira Garabato et al. (2009) [Michael Meredith, UK]	text removed, not applicable
3-395	3	20	1	20	2	Reference to Aoki et al (2005) is incomplete, include issue number (23), article number (L23601) and doi (10.1029/2005GL024246) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-396	3	20	1		14	There is discussion here of AABW warming and freshening, but a some important Fahrbach et al. references about temperature change of AABW and WDW in the Weddell Sea are omitted, most pertinently "Warming of deep and abyssal water masses along the Greenwich meridian...." - Deep-Sea Research II, 58, 25-28, 2509-2523. [Michael Meredith, UK]	Noted citation added and results of Fahrbach et al discussed
3-397	3	20	1		14	This text should be checked for redundancies and the possibility to quantify statements (lines 1 to 5). [Hans Poertner, Germany]	Accepted. text shortened, and warming and freshening quantified
3-398	3	20	2			Reference to Johnson et al (2008b) is incomplete, include issue number (22), article number (L22601) and doi (10.1029/2008GL035619) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-399	3	20	2			Reference to Kouketsu et al (2011) is incomplete, include article number (C03012) and doi (10.1029/2010JC006464) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-400	3	20	2			Reference to Rintoul (2007) is incomplete, include issue number (6), article number (L06606) and doi (10.1029/2006GL028550) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-401	3	20	7			Reference to Jacobs (2004) is incomplete, include volume number (16) and issue number (4) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication

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3-402	3	20	8	20	9	Reference to Fukasawa et al (2004) is incomplete, include publication name (Nature), volume number (427), issue number (6977) and doi (10.1038/nature02337) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-403	3	20	9			Reference to Kawano et al (2006) is incomplete, include issue number (23) and doi (10.1029/2006GL027933) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-404	3	20	11			Reference to Johnson and Doney (2006) is incomplete, include issue number (14) and doi (10.1029/2006GL026769) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-405	3	20	12	20	13	Reference to Zenk and Morozov (2007) is incomplete, include publication name (Geophysical Research Letters), volume number (34), issue number (14), article number (L14607) and doi (10.1029/2007GL030340) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-406	3	20	13			Reference to Andrie et al (2007) is incomplete, include issue number (5) and doi (10.1029/2002GL015766) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-407	3	20	14			See above point for timing of AABW warming in the Atlantic. [Michael Meredith, UK]	accepted results from Fahrbach et al 2011 are included
3-408	3	20	16	20	22	Figure 3.10 is only referred in the text at section 3.5.1. The authors should consider referring tothis figure at other parts of section 3.5 in order to exemplify the text (eg at section 3.5.6) [Mauro Cirano, Brazil]	Accepted. Former Fig. 3.10 is Fig. 3.9 in SOD
3-409	3	20	24	20	33	It would be useful to emphasize the wide range of timescales (years to centuries) on which changes of temperature and salinity (particularly trends) spread into the ocean interior, distinguishing between the shortest (subtropical thermoclines) and longest (abyssal northern oceans). [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	noted
3-410	3	20	30	20	30	The sentence is incomplete? For example, the intermediate depth salinity minimum waters in both hemispheres? [Rongshuo Cai, China]	accepted, text revised
3-411	3	20	30	20	30	The sentence on this line has no verb. [Stephen Griffies, USA]	accepted, tet revised
3-412	3	20	30	20	30	For example, the intermediate depth salinity minimum waters in both hemispheres.'This is not a complete sentence. Besides, what is the change regarding the salinity minimum waters? Their presence alone is not a consequence of global change. [Reiner Steinfeldt, Germany]	accepted
3-413	3	20	34	20	34	Any comments about the Equatorial Oceans (Atlantic, Pacific, Indian)? Here it would be good to say if there is a lack of data. [Leticia Cotrim da Cunha, Germany]	Noted
3-414	3	20	49	20	50	... as well as mid-depth velocity' normally LADCP measurements go from top to bottom, so I wonder why the velocity measurements should be restricted to mid-depth [Reiner Steinfeldt, Germany]	Accepted - text revised
3-415	3	21	6			SSH is another acronym not explained on first mention and not explained in figure legends [Hans Poertner, Germany]	Accepted - note to replace "record of sea level rise" in Exec summary 3-3 line 7, with "record of rising sea surface height (SSH)"
3-416	3	21	8	21	8	Which observations? Satellite? ARGO? [Leticia Cotrim da Cunha, Germany]	Taken into account - the present sentence clearly refers to SSH
3-417	3	21	31			Reference to Douglass et al (2006) is incomplete, include issue number (C4), article number (C04001) and doi (10.1029/2005JC003015) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-418	3	21	34	21	35	Reference to Carton et al (2005) is incomplete, include issue number (C9), article number (C09006) and doi (10.1029/2004JC002817) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-419	3	21	39			Update reference details for Qiu and Chen (2011) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-420	3	21	40	21	40	Should "Figure 3.10" be "Figure 3.11" ?? [Bogi Hansen, Faroe Islands]	Accepted - text revised
3-421	3	21	40	21	40	Figure 3.10' it should presumably be Figure 3.11 [Reiner Steinfeldt, Germany]	Accepted - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-422	3	21	40	21	45	Figure 3.11 in place of Figure 3.10 [VINCENZO ARTALE, ITALY]	Accepted - text revised
3-423	3	21	40			Replace "Figure 3.10" by "Figure 3.11" [Mauro Cirano, Brazil]	Accepted - text revised
3-424	3	21	40			Reference to Figure 3.10 should be to Figure 3.11 [Christopher Kavanagh, Monaco]	Accepted - text revised
3-425	3	21	40			Wrong figure number; should be 3.11 [Andreas Sterl, Netherlands]	Accepted - text revised
3-426	3	21	42			Again, reasons explaining these observations should be given. [Hans Poertner, Germany]	Taken into account - the text notes the circulation changes are due to change in wind stress curl (i.e. sverdrup dynamics). A more detailed explanation cannot be provided in the space available.
3-427	3	21	43			Reference to Mitas and Clement (2005) is incomplete, include issue number (3), article number (L030809) and doi (10.1029/2004GL021765) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-428	3	21	45	21	45	Reference to Figure 3.10 is incorrect? Should it be Fig. 3.11? [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
3-429	3	21	45			Replace "Figure 3.10" by "Figure 3.11" [Mauro Cirano, Brazil]	Accepted - text revised
3-430	3	21	45			Wrong figure number; should be 3.11 [Andreas Sterl, Netherlands]	Accepted - text revised
3-431	3	21	49	21	58	The link with SAM changes (sec. 3.5.5) should be made [Andreas Sterl, Netherlands]	Accepted - SAM, 3.5.4 referenced
3-432	3	21	50			Sounds strange/contradictory: "and a lesser increase or decrease" [Christopher Kavanagh, Monaco]	Accepted - "or decrease" is deleted
3-433	3	21	53			Reference to Cai (2006) is incomplete, include issue number (3), article number (L03712) and doi (10.1029/2005GL024911) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-434	3	21	55	21	56	Extended to which depth? [Leticia Cotrim da Cunha, Germany]	Taken into account - text added "> 1800 m"
3-435	3	21	55			Reference to Hill et al (2008) is incomplete, include issue number (8), article number (L08602) and doi (10.1029/2007GL032912) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-436	3	21	55			Reference to Ridgway (2007) is incomplete, include issue number (13), article number (L13613) and doi (10.1029/2007GL030393) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-437	3	21	56		58	Implications for relevant biogeochemical processes should be mentioned or referred to. [Hans Poertner, Germany]	Rejected - although an important issue, no clear biogeochemical implication is drawn. The topic is beyond the scope of the sub-chapter.
3-438	3	21				Figure 3.11: Cannot read the contour numbers. [Christopher Kavanagh, Monaco]	Taken into account - text added to caption "10 cm CI"
3-439	3	22	1		10	Can you quantify some of these changes? [Hans Poertner, Germany]	Accepted - text added "1° of latitude per 40 years"
3-440	3	22	6	22	7	Reference to Alory et al (2007) is incomplete, include issue number (2), article number (L02606) and doi (10.1029/2006GL028044) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-441	3	22	9			Reference to Lumpkin and Garzoli (2011) is incomplete, include article number (C01014) and doi (10.1029/2010JC006285) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-442	3	22	10			Reference to Goni et al (2011) is incomplete, include article number (C10037) and doi (10.1029/2011JC007198) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-443	3	22	14	22	42	Paragraph and figure text rearrangement: Part of figure 3.12 Caption could be used as an "introduction" to the following paragraphs. [Leticia Cotrim da Cunha, Germany]	Accepted - rearranged as suggested
3-444	3	22	15	22	18	Remove these lines from the figure caption and include them in the main text, at the beginning of section	Accepted - same as comment 3-443

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						3.6.3. [Mauro Cirano, Brazil]	
3-445	3	22	19	22	19	Abbreviation of RAPID/MOCHA? [Leticia Cotrim da Cunha, Germany]	Accepted - abbreviation text inserted
3-446	3	22	32			Replace "Antarctic Bottom Water" by "AABW", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - replaced with AABW
3-447	3	22	38			Reference to Willis (2010) is incomplete, include article number (L06602) and doi (10.1029/2010GL042372) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-448	3	22	40	22	42	include in this list should be latitude of seperqation of the Gulf Stream at the western boundary (Joyce and Zhang, J. Climate 2010) [Terrence Joyce, USA]	Accepted - text and reference added "latitude of the Gulf Stream after separation"
3-449	3	22	40			Reference to Josey et al (2009) is incomplete, include article number (C09022) and doi (10.1029/2008JC005230) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-450	3	22	42			Reference to Bingham and Hughes (2009) is incomplete, include article number (L02603) and doi (10.1029/2008GL036215) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-451	3	22	42			Reference to Fischer et al (2010) is incomplete, include article number (L24610) and doi (10.1029/2010GL045321) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-452	3	22	44	22	44	How long? [Leticia Cotrim da Cunha, Germany]	Accepted. Text reworded to: "Continuous time series of components of the AMOC, longer than those of the complete system, have been obtained using moored…"
3-453	3	22	44	22	50	The second inflow to the Arctic, through the Barents Opening, is actually better constrained from observations (including longer time series) than that through the Fram Strait (the former carries more heat and a similar amount of mass). See, e.g., the referred paper by Mauritzen et al. (2011). [Tor Eldevik, Norway]	Taken into account - add text "and through the Barents Sea" and citation "(since 1997, Ingvaldsen et al., 2004, Mauritzen et al, 2011)" Reference to be added is: Ingvaldsen, R. B., Asplin, L., & Loeng, H. (2004). Velocity field of the western entrance to the Barents Sea. Journal of Geophysical Research, 109, C03021, doi:10.1029/2003JC001811.
3-454	3	22	55	23	15	The references for Figure 3.12 should be removed from these paragraphs, since the numbers presented in the text are different from those presented in the figure and can confuse the reader. [Mauro Cirano, Brazil]	Taken into account - Text added in caption to Figure 3.12 for clarification is "The means and standard deviations are of the low-pass timeseries.for the common period of 2nd April 2004 to 1st April 2010 are 17.5±3.8 Sv, 13.7±3.3 Sv and -16.8±4.1 Sv for 26.5°N, 41°N and 16°N respectively. Means over this period are indicated by the horizontal line on each timeseries."
3-455	3	22				along the section the are missed references and discussion on Bryden's papers, Vol 438,1 December 2005|doi:10.1038/nature04385, where a transatlantic section along latitude 25N has been used as a baseline for estimating the overturning circulation and associated heat transport, where they analyze a new 25N transatlantic section and compare it with four previous sections taken over the past five decades or updated paper, " Monitoring the Atlantic meridional overturning circulation" by Darren Rayner et al , Deep-Sea Research II 58 (2011) 1744–1753 [VINCENZO ARTALE, ITALY]	Accepted - Sentence added at original page 3-22 line 57 "Earlier estimates of AMOC strength from 5 shipboard expeditions over 47 years (Bryden et al, 2005) were found to fall with the range of variability seen by the RAPID/MOCHA array (Rayner et al., 2011)." Added references are : * Bryden, H., H. Longworth and S. Cunningham, 2005. Slowing of the Atlantic meridional overturning circulation at 25oN, Nature, 438, 655-657 and Rayner, D. and 11 co-authors (2011) Monitoring the Atlantic meridional overturning circulation. Deep-Sea Research II, 58, 1744-1753.
3-456	3	23	5			Reference to Willis (2010) is incomplete, include article number (L06602) and doi (10.1029/2010GL042372)	Editorial - copyedit to be completed prior to publication

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						[Mauro Cirano, Brazil]	
3-457	3	23	8			2.4 Sv should read 2.6 Sv, and the confidence is low from the regression coefficients of the study! [Christopher Kavanagh, Monaco]	Accepted - 2.4 changed to 2.6; uncertain changed to low
3-458	3	23	13			Why would it be representative? [Christopher Kavanagh, Monaco]	Accepted - text added: "assuming a constant reference level for the geostrophic calculations"
3-459	3	23	15	23	16	Given the large variability, why call something a possible decline when it is not statistically significant? [Christopher Kavanagh, Monaco]	Taken into account - sentence updated and revised as "For the period 2000 to mid-2009, a downward trend was found (at 85% confidence) by Send et al. (2011); for the updated transport timeseries shown in Figure 3.12b, including mid-2009 through 2010, there is no apparent trend."
3-460	3	23	16			Update reference details for Send et al (2011) including volume number (38), article number (L24606) and doi (10.1029/2011GL049801) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-461	3	23	28	23	28	Use "but no trend was detected/seen". [Leticia Cotrim da Cunha, Germany]	Accepted - text revised "but no trend."
3-462	3	23	32	23	35	Re-write this passage. It seems that bits of text are missing here. [Leticia Cotrim da Cunha, Germany]	Accepted - sentence broken into 2 sentences and rearranged
3-463	3	23	32	23	35	Do not split none from exhibit. Run on sentence. [Christopher Kavanagh, Monaco]	Accepted - see 3-462
3-464	3	23	40			"for or against" ~strange wording here and elsewhere. [Christopher Kavanagh, Monaco]	Accepted - wording changed to "there is insufficient evidence to support a finding of meridionally coherent change"
3-465	3	23	43	23	43	suggest to use 'The Antarctic Bottom Water Formation', in order to be very specific here. [Zhaomin Wang, UK]	Accepted
3-466	3	23	43	24	2	I think that Section 3.6.4 should be expanded to include the upwelling of Circumpolar Deep Water (CDW) and the subduction of Antarctic Intermediate Water (AAIW) and Subantarctic Mode Water (SAMW). Alternatively, a new section could address the wind- and buoyancy-driven upper overturning cell of the Southern Ocean. This is presently hinted at in Section 3.6.5.2, but the 3-D circulation of the Southern Ocean is a broader phenomenon than can be addressed in "Water Exchange Between Ocean Basins". I recommend that recent key papers are cited. Hogg (2010) shows that buoyancy forcing is a primary control on the overturning (rather than wind). Meanwhile, Meredith et al. (2012) show that eddy compensation is invariant at timescales shorter than centennial, indicating that the wind-driven overturning cell of the Southern Ocean is sensitive to changes in wind forcing on decadal timescales. There is much uncertainty in the recent and ongoing response of the Southern Ocean to climate change, and this should be emphasized in Chapter 3, especially given the significance for ocean uptake of carbon (see comment 7). References: (1) Hogg, A. McC. (2010). An Antarctic Circumpolar Current driven by surface buoyancy forcing. Geophys. Res. Lett., 37.; (2) Meredith, M. P., Naveira-Garabato, A. C., Hogg, A. McC., and R. Farneti (2012). Sensitivity of the overturning circulation in the Southern Ocean to decadal changes in wind forcing. J. Climate, 25, 99-110. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Rejected: Agreed that all of the Antarctic MOC is of interest, but the observational basis is lacking. The work cited in this comment is largely theoretical/modelling evidence. An enlarged discussion of the Antarctic MOC would require more space, and is not justified without observations.
3-467	3	23	45			Replace "Antarctic Bottom Water" by "AABW", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - text revised
3-468	3	23	46	23	47	Replace "Antarctic Bottom Water" by "AABW", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - text revised
3-469	3	23	55			Update reference details for Purkey and Johnson (2011) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-470	3	24	2	24	8	A recent paper by Wu et al (Nature Climate Change 2012) should be cited; it does a better job of presenting shifts in ocean gyres and mid-latitude winds than is found in this chapter. [Terrence Joyce, USA]	Accepted - reference added at the end of section 3.6.2: Wu, L. and 11 co-authors, 2012. Enhanced warming over the global subtropical western boundary

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							currents. Nature Climate Change, DOI: 10.1038/NCLIMATE1353
3-471	3	24	4	24	19	Please add a reference: Gordon et al., 2012 South China Sea Throughflow Impact on the Indonesian Throughflow, submitted to Nature Geoscience. I includes results form an extension of the INSTANT [Jae Hak Lee, Republic of Korea]	Taken into account. A paper with the same title and lead author is now under review by GRL. It does not consider the full Indonesian Throughflow and does not include an extension of full INSTANT results, and is therefore not cited.
3-472	3	24	4	24	57	I think that Section 3.6.5 is missing a sub-section addressing changes in Agulhas leakage between the Indian and Atlantic Oceans. I suggest a new section 3.6.5.4 to cover this important inter-basin connection. A key review paper is Beal et al. (2011), citing two further papers on the evidence for recent changes in Agulhas leakage (Alory et al., 2007; Roualt et al., 2009). All three papers could be cited in Chapter 3, if Agulhas Leakage is properly addressed. References: (1) Alory, G., Wijffels, S., and Meyers, G. (2007). Observed temperature trends in the Indian Ocean over 1960–1999 and associated mechanisms. Geophys. Res. Lett. 34.; (2) Beal, L. M., et al. (2011). On the role of the Agulhas system in ocean circulation and climate. Nature, 472, 429–436 ; (3) Rouault, M., Penven, P., and Pohl, B. (2009). Warming in the Agulhas Current system since the 1980's. Geophys. Res. Lett. 36. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Rejected - outside the scope of the chapter. The suggested topic is of importance, but the cited papers provide only indirect evidence (i.e. warming) of transport change in the Agulhas leakage.
3-473	3	24	6	24	19	Please add a reference: Wunsch, C, 2010 Variaility of the Indo-Pacific Ocean exchanges, Dyn. Atmos. Oceans., 50, 157-173. It shows circulation in the region of ITF for 1992-2007 based on modeling output. [Jae Hak Lee, Republic of Korea]	Accepted - the suggested reference, a result from ocean data assimilation models, is useful for interpreting and extending the Indonesian Throughflow observations. Reference to be added: Wunsch, C, 2010 Variability of the Indo-Pacific Ocean exchanges, Dyn. Atmos. Oceans., 50, 157-173.
3-474	3	24	13	24	14	Reference to Wainwright et al (2008) is incomplete, include issue number (3), article number (L03604) and doi (10.1029/2007GL031911) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-475	3	24	13			Reference to Sprintall et al (2009) is incomplete, include article number (C07001) and doi (10.1029/2008JC005257) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-476	3	24	17			Reference to Potemra and Schneider (2007) is incomplete, include issue number (C5), article number (C05035) and doi (10.1029/2006JC003808) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-477	3	24	21	24	40	Section 3.6.5.2 addresses changes in ACC transport that should be considered in a 3-D context. I recommend some restructuring to address the Southern Ocean in a more holistic fashion (see comment 4). [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Taken into account - see response to comment 3-466. The observational basis is not sufficient for the suggested additions.
3-478	3	24	21			Section 3.6.5.2 should refer to the review paper of Meredith et al. Maybe the easiest thing is to put a sentence at the end of this section saying 'Meredith et al. (2011) provides a detailed review of historical monitoring of flows through Drake Passage and recommendation for further work'. Reference is Meredith, M.P., Woodworth, P.L., Chereskin, T.K., Marshall, D.P., Allison, L.C., Bigg, G.R., Donohue, K., Heywood, K.J., Hughes, C.W., Hibbert, A., Hogg, A. McC. Johnson, H.L., King, B.A., Leach, H., Lenn, Y-D., Morales Maqueda, M.A., Munday, D.R., Naveira Garabato1, A.C., Provost, C. and Sprintall, J. 2011. Sustained monitoring of the Southern Ocean at Drake Passage: past achievements and future priorities. Reviews of Geophysics, 49, RG4005, doi:10.1029/2010RG000348. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Accepted - text added as suggested, and reference to be added (Meredith et al. 2011, Reviews of Geophysics)
3-479	3	24	23		39	Some caution in the statements are needed here. The text is correct that no trends in ACC transport have been detected on decadal timescales - but the observational techniques that have been used could only detect trends with a relatively large magnitude, given the aliassing of sparse sections, and the issues surrounding the use of tide gauge data on decadal periods. It is possible that a trend of up to around 10Sv has occurred, but has remained undetected (and this would be consistent with the year-to-year changes in transport taht have been observed in response to wind changes, and the decadal increase in winds). There is a review of all these issues in: Meredith, M.P., P.L. Woodworth, T.K. Chereskin, D.P. Marshall, L.C. Allison, G.R. Bigg, K. Donohue, K.J. Heywood, C.W. Hughes, A. Hibbert, A. McC. Hogg, H.L. Johnson, L. Jullion, B.A. King, H. Leach, Y.-D. Lenn, M.A. Morales Maqueda, D.R. Munday, A.C. Naveira Garabato, C.	Accepted - Reference added (same as 3-478) and changes to wording: "no evidence of trends" to "no significant trends" in 24-31, and "no evidence for multi-decadal changes" to "no strong evidence for (or against) multi-decadal changes"

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						Provost, J.-B. Sallee and J. Sprintall. "Sustained monitoring of the Southern Ocean at Drake Passage: past achievements and future priorities". Reviews of Geophysics, 49, RG4005, doi:10.1029/2010RG000348, 2011. [Michael Meredith, UK]	
3-480	3	24	24	24	25	Reference to Fyfe and Saenko (2006) is incomplete, include issue number (6), article number (L06701) and doi (10.1029/2005GL025332) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-481	3	24	24	24	25	There are two mistakes here. Firstly, results from coarse resolution climate models do not produce consistent changes in the ACC transport under increasing wind stress forcing, with the ACC transport being reduced in about half IPCC AR4 models and being increased in the other half IPCC AR4 models (Wang, Z., T. Kuhlbrodt, and M. P. Meredith (2011), On the response of the Antarctic Circumpolar Current transport to climate change in coupled climate models, J. Geophys. Res., 116, C08011, doi:10.1029/2010JC006757). Secondly, Fyfe and Saenko (2006) examined the changes in maximum velocity, rather than the ACC transport. Detailed analysis for the diversified ACC transport changes in these climate models are given in Wang et al. (2011). [Zhaomin Wang, UK]	Accepted - sentence beginning "Coarse resolution climate models …" deleted as outside the scope of the present sub-chapter.
3-482	3	24	24			Replace "Southern Annular Mode (SAM)" by "SAM", since the acronym was already defined before [Mauro Cirano, Brazil]	Accepted - text revised
3-483	3	24	27			Reference to Hughes et al (2003) is incomplete, include issue number (9), article number (1464) and doi (10.1029/2003GL017240) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-484	3	24	27			Reference to Meredith et al (2004) is incomplete, include issue number (21), article number (L21305) and doi (10.1029/2004GL021169) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-485	3	24	29			Reference to Cunningham et al (2003) is incomplete, include issue number (C5), article number (8084) and doi (10.1029/2001JC001147) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-486	3	24	30	24	31	Reference to Rintoul et al (2002) is incomplete, include issue number (C10), article number (3155) and doi (10.1029/2001JC000787) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-487	3	24	30			Reference to Swart et al (2008) is incomplete, include issue number (C9), article number (C09014) and doi (10.1029/2007JC004223) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-488	3	24	34	24	36	It was found that narrowing of the ACC region also contributes to causing the insensitive ACC transport to changes in wind forcing on decadal and longer time scales. The narrowing is caused by the poleward shift in westerly winds when the westerlies become intensified. See Wang et al. (2011), the reference given in comment 25. Considering the effects of narrowing on the ACC transport is important for us to understand the role of eddies. [Zhaomin Wang, UK]	Taken into account - deletion of sentence as described in response to comment 3-481 makes the present comment moot.
3-489	3	24	36	24	36	Add citation to: The role of mesoscale eddies in the rectification of the Southern Ocean response to climate change, 2010: R. Farneti, T.D. Delworth, A.J. Rosati, S.M. Griffies, and F\ Zeng, Journal of Physical Oceanography, vol. 40, 1539-1557. [Stephen Griffies, USA]	Accepted, Citation to be added - Farneti et al, 2010.
3-490	3	24	44	24	51	To me, this paragraph implies a controversy, but I find that misleading. The two drifter studies that are cited, address the internal circulations in the North Atlantic and Nordic Seas, respectively, rather than the flow across the Greenland-Scotland Ridge. Also, there are updated publications on all the Atlantic inflow branches. I suggest to replace this paragraph by the following text: There is no observational evidence of changes during the past two decades in the flow across the Greenland-Scotland Ridge, which connects the North Atlantic with the Norwegian and Greenland Seas. Hakkinen and Rhines (2009), analyzing surface drifter tracks in the North Atlantic, found a greater tendency after 2000 for drifters in the North Atlantic Current to continue northward across 50°N rather than recirculate toward the southeast. This may affect the temperature and salinity of the Atlantic inflow water crossing the ridge (Hatun et al., 2005), but direct current measurements since the mid-1990s have not shown any significant trends in volume transport for any of the three inflow branches (Jonsson and Valdimarsson, 2012; Hansen et al., 2010; Mauritzen et al., 2011).	Accepted - text changed similarly to reviewer version, except both drifter references are removed. New version: "There is no observational evidence of changes during the past two decades in the flow across the Greenland-Scotland Ridge, which connects the North Atlantic with the Norwegian and Greenland Seas. Direct current measurements since the mid-1990s have not shown any significant trends in volume transport for any of the three inflow branches (Østerhus et al (2005); Jonsson and Valdimarsson, 2012; Hansen et al., 2010; Mauritzen et al., 2011)." References to be added are: Hansen, B., H. Hatun, R. Kristiansen, S. M. Olsen, and S. Osterhus, 2010: Stability and forcing of the Iceland-Faroe inflow of

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						Hansen, B., H. Hatun, R. Kristiansen, S. M. Olsen, and S. Osterhus, 2010: Stability and forcing of the Iceland-Faroe inflow of water, heat, and salt to the Arctic. Ocean Science, 6, 1013–1026. Jonsson, S., and H. Valdimarsson, 2012: Water mass transport variability to the north Icelandic shelf, 1994–2010. ICES Journal of Marine Science, in press. [Bogi Hansen, Faroe Islands]	water, heat, and salt to the Arctic. Ocean Science, 6, 1013–1026. Jonsson, S., and H. Valdimarsson, 2012: Water mass transport variability to the north Icelandic shelf, 1994–2010. ICES Journal of Marine Science, in press. Østerhus, S., Turrell, W. R., Jonsson, S., & Hansen, B. (2005). Measured volume, heat, and salt fluxes from the Atlantic to the Arctic Mediterranean. Geophysical Research Letters, 32, L07603, doi:10.1029/2004GL022188.
3-491	3	24	45	24	46	Reference to Hakkinen and Rhines (2009) is incomplete, include article number (C04005) and doi (10.1029/2008JC004883) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-492	3	24	48	24	48	As argued in my comment No. 2, above, I do not find the reference to Andersson et al. (2011) to be relevant in this context, but if you decide to retain it, please note that the only Andersson reference in the reference list must be to some other publication. [Bogi Hansen, Faroe Islands]	Accepted - Andersson et al (2011) reference is deleted
3-493	3	24	49	24	51	The cited ref (Mauritzen et al. 2011) does not document the (lack of) trend, nor variability in general, of the "northward … flow across the Faroe-Shetland Channel". A more appropriate ref would be, e.g., Østerhus et al. (2005, GRL, doi:10.1029/2004GL022188), or Hansen et al. (2010, Ocean Sci., doi:10.5194/os-6-1013-2010) [Tor Eldevik, Norway]	Taken into account - revised text in comment 3-490 cites Østerhus et al. (2005) and Hansen et al. (2010).
3-494	3	24	55			Reference to Macrander et al (2005) is incomplete, include issue number (6), article number (L06606) and doi (10.1029/2004GL021463) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-495	3	25	1			Fig 3-13; suggest extend records of all stations to present. suggest put all on single graph so slopes can be compared; they can be displaced vertically so as not to overlap; suggest draw slopes on graphs to guide the eye. Right now this figure is eye candy with graphs on map of world; you could simply show a small map of world with station locations just in case some one doesnt know where Sydney is. [Stephen E Schwartz, USA]	Accepted - Will update in SOD, but data often lags by 6 months to a year. Most data in PSMSL now through December 2011. Figure 3-13 was not meant to focus on slopes, but decadal variations and to show that sites on opposite sides of basins often have anti-correlated behavior. We believe this figure shows this best.
3-496	3	25	1			Sec 3.7. Suggest cite and refute Morner (global planet change 2004, 2008); Nerem (ibid 2007); Suggest better to cite and refute than simply ignore. [Stephen E Schwartz, USA]	rejected. The Morner study was wrong in too many ways to address in the space we have, and the 2008 paper was not published in the peer reviewed literature.
3-497	3	25	4			What scale of variability? [Christopher Kavanagh, Monaco]	Taken into account - The time-scale is stated on the next line (years to decades).
3-498	3	25	10	28	49	The section about sea level seems to be repeated when compared to Chapter 13. In Chapter 13 sea level is discussed in detail, therefore I would suggest to cut this section in Chapter 3 and leave it for Chapter 13 only. [Eduardo Siegle, Brazil]	Rejected - Sea level is included in Chapter 3 as an Ocean Observation at the Direction of the IPCC. Chapter 13 is meant to be a synthesis of the observation chapters and also deal with projections. Chapter 13 and Chapter 3 have resolved duplications, and changes will be made for SOD
3-499	3	25	10			Section 3.7 I have a general comment here. Despite finding it easy to read and pertinent, I find this section is not well linked to the other sections in the same chapter nor to chapter 13 (devoted to sea level). I think a sentence should be included so that the reader knows he can find more information in chapter 13. I include more specific comments in rows below. [Belén Martín Míguez, Spain]	Accepted - We will add a sentence pointing to Chapter 13. We already do link to other portions of Chapter 3 that are relevant to sea level - Section 3.2 (e.g., 3-27) line 31 and Section 3.6 (e.g., 3-25, Line 29).
3-500	3	25	12	26	42	Please consider including a table that shows the observed long term trends for various properties of the ocean (such as temperature, salinity) in AR4 and AR5 for easy comparison. [Tsz-cheung Lee, Hong Kong]	Noted - We will add such comments in the text instead of as a table, as this pertains to the entire chapter, not just sea level.

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3-501	3	25	15	18	20	Satellite altimetry has severe limitations in relatively small (compared to the global ocean) semi-enclosed seas like the Baltic Sea primarily due to the limited accuracy of altimeter data near the coast and their low spatial resolution. The two dataset differ at a level of 0.5-4 cm. The errors in altimetry data may obscure the true interannual variabilities. Therefore,iIn the coastal regions, numerical models are usually validated against observed sea level from tide gauge stations. [Weiwei Fu, Denmark]	Noted - While true, this was not the point of this paragraph. The point was to show that the two independent observations (altimetry and tide gauges) agree quite well for global averages. We will clarify this: "verified that they agree at the level of 0.5 mm yr-1 or better over periods of a decade and longer for global mean sea level rise"
3-502	3	25	15	25	15	"much smaller number of records" - can you specify the exact number? [Roland Gehrels, United Kingdom]	Accepted - will add, although numbers vary from study to study
3-503	3	25	16	25	16	"nearly global coverage" - can you give a percentage? [Roland Gehrels, United Kingdom]	Accepted - will add
3-504	3	25	22	25	22	This is misleading for a number of reasons. Linear "trends" are an unsatisfactory and even irrespoonsible technique for studying climate time series as it exaggerates the importance of the earliest, least accurate measurements. Tide gauge measurements often have an upwards bias as the equoment is damaged by storms and may be displaced downwards.The neighbouring land may fall from weight of buildings and removal of groundwater and minerasls. Recent use of accurate levelling equipnebnt has seen a recent decline in rate of increase and in some cases its disappearance. The altimetry measurements have recently levelled and their overall distribution (which you should show) has a high region above Indonesia and a region with no change around the South Pacific., South Pacific islands are not sinking and you should say so. [VINCENT GRAY, NEW ZEALAND]	Rejected - Trends are the primary method used for reporting long-term changes in sea level and are discussed here to be consistent with the literature. We do discuss (3-25, lines 22-32) and show regional time-series (Figure 3.13).The discussion states "sea level change can vary significantly from one area to another on periods from a decade to several decades." Land motion effects are also described in the following paragraph.
3-505	3	25	22	25	36	You should include a world map showing the variability everywhere, not just a few examples [VINCENT GRAY, NEW ZEALAND]	Rejected - Showing sea level "variability" with a single map is problematic. Does one use a trend over a certain period? Show the standard deviation of the data, which may reflect short-term and long-term changes? How does one fill in gaps between the very sparse tide gauge data, or do we limit ourselves to the post-1993 era? We believe that showing single tide gauge records better reflects the true level of regional variability that we can identify. We have selected the tide gauges with longest records to show the variability before 1950. Many tide gauges are in the same regions, so we have chosen a representative tide gauge from each region.
3-506	3	25	29	25	29	delete "in" - typo [Roland Gehrels, United Kingdom]	Accepted - text revised
3-507	3	25	30			Reference to Sturges and Douglas (2011) is incomplete, include article number (C06008) and doi (10.1029/2010JC006492) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-508	3	25	31	25	40	Poor grammar, making the comprehension of this material difficult. What does ‘it’ refer to in line 39? [David Sauchyn, Canada]	Rejected - Comments from David Sauchyn apparently refer to a different chapter
3-509	3	25	34	25	36	Enhance the resolution for the 3 sea level plots in Figure 3.13. The graphics are small and need a better resolution, so that the reader can identify the legend properly as well as the curves. [Mauro Cirano, Brazil]	Accepted - Figure will be changes so that plots are larger
3-510	3	25	38	25	51	Nobody seems to have attempted corrections for storm damge or for land changes caused by weight of buildings and removal of minerals or groundwater. Or for changes in the neighbouring harbour [VINCENT GRAY, NEW ZEALAND]	Taken into account - The combined effect of these should be seen by colocated GPS receivers, if they exist. However, the two studies by Merrifield et al. and Woepellmann et al. find that when the difference between applying and not applying GPS corrections is much less than the uncertainty. This is discussed on 3-25, lines 45-49 of the FOD.
3-511	3	25	43			Reference to Holgate (2007) is incomplete, include issue number (1), article number (L01602) and doi	Editorial - copyedit to be completed prior to publication

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						(10.1029/2006GL028492) [Mauro Cirano, Brazil]	
3-512	3	25	47	25	47	A typo here: It should say Woppelmann (not Woeppelmann) [Belén Martín Míguez, Spain]	Accepted - text revised, please note that oe is the official replacement for German o with an Umlaut
3-513	3	25	47			Reference to Woeppelmann et al (2009) is incomplete, include article number (L12607) and doi (10.1029/2009GL038720) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-514	3	25	47			Woeppelmann should be o with an umlaut instead of the oe [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised, please note that oe is the official replacement for German o with an Umlaut
3-515	3	25	50	25	50	change "correcting for" to "to account for" [Roland Gehrels, United Kingdom]	Accepted - text revised.
3-516	3	25	53	26	17	All these methods tend to exaggerate the impotanace of the least reliable earlier data [VINCENT GRAY, NEW ZEALAND]	Noted - trends of sea level rise are the common method to quantify long-term change. Older data at a single site are not that much more inaccurate than modern data. Uncertainty generally arises from interpretation of averaging sparser data, and this is included in uncertainty estimates to the best of our capability, using various methods desribed in the source papers. Uncertainty of trends in global mean sea level rise in the source papers account for higher uncertainty in earlier years.
3-517	3	25	55			Reference to Jevrejeva et al (2006) is incomplete, include issue number (C9), article number (C09012) and doi (10.1029/2005JC003229) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-518	3	25	56			Reference to Jevrejeva et al (2008) is incomplete, include issue number (8), article number (L08715) and doi (10.1029/2008GL033611) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-519	3	26	4	26	4	the Executive Summary gives 1.7, not 1.6. See comment 2. [Roland Gehrels, United Kingdom]	Accepted - Executive summary will be changed to be consistent
3-520	3	26	19	26	19	There is a reduction in rate of increase for many records in the last 5-10 years which may be related to the levelling down of global temperature anomalies [VINCENT GRAY, NEW ZEALAND]	Accepted - This interannual fluctuation will be discussed in the SOD
3-521	3	26	19	26	24	Inaccurate statement "…change in observed rate of sea level rise is real and not an artefact of the different sampling…" . Surely rate of sea level rise is real, but vary depending on different sampling processing techniques, see my comment above. [Pavel Tkalich, Singapore]	Rejected - the statement is accurate. Rates of sea level rise measured since 1993 by altimetry and tide gauges do agree, so the change in sampling from tide gauges to global observations from altimetry does not explain the the change from 1.8 mm/year to 3.3 mm/year. Different processing techniques do change results slightly, but only within stated uncertainties.
3-522	3	26	21	26	21	Please refer to comment 11. [Leticia Cotrim da Cunha, Germany]	Accepted - uncertainty computed for Chapter 3 will be at evaluated consistently at 90% confidence
3-523	3	26	22	26	22	Figure 3.12 seems to be a wrong figure. Probably note Figure 3.14 or others. [Sok Kuh Kang, South Korea]	Accepted - text revised
3-524	3	26	22	26	22	Figure 3.12' it should presumably be Figure 3.14 [Reiner Steinfeldt, Germany]	Accepted - text revised
3-525	3	26	22			Replace "Figure 3.12" by "Figure 3.14" [Mauro Cirano, Brazil]	Accepted - text revised
3-526	3	26	22			Wrong figure number; should be 3.13 [Andreas Sterl, Netherlands]	Accepted - text revised
3-527	3	26	24			In section 3.7, at the end of this para a reference could be: Holgate, S.J. and Woodworth, P.L. 2004. Evidence for enhanced coastal sea level rise during the 1990s. Geophysical Research Letters, 31, L07305, doi:10.1029/2004GL019626. which remarked on the similarities of tide gauge and altimeter data. [Philip	Accepted - text revised

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Woodworth, United Kingdom of Great Britain & Northern Ireland]	
3-528	3	26	36			Replace "Figure 3.12" by "Figure 3.14" [Mauro Cirano, Brazil]	Accepted - text revised
3-529	3	26	36			Wrong figure number; should be 3.13 [Andreas Sterl, Netherlands]	Accepted - text revised
3-530	3	26	39	26	42	Cause for the phase shift is unexplained here. [Christopher Kavanagh, Monaco]	Noted - However, no cause for the observed phase shift has been offered in the literature that we know of.
3-531	3	26	44	27	20	The issue of acceleration in SLR records has become extremely contentious during 2011 particularly with published works relating to regional based long tide gauge records in America (Houston and Dean, 2011) and Australasia (Watson, 2011). Although these published works are quite different, they point to a general trend of weak average deceleration in the longer-term records around America and throughout Australasia over the period post 1930. Trends of acceleration are particularly sensitive to the data period available and the curve fitting chosen to consider the acceleration component of non-linear time series. The quadratic curve fit chosen by Houston and Dean (2011), Watson (2011), Woodworth et al (2011) and the researchers considering the GMSL reconstructions inherently apply the curve fit to the length of the data resulting in an average rate of acceleration over the whole time period. The fitting of a quadratic does not facilitate breaking the record down to distinguish periods of high or low (even negative) acceleration. This has been facilitated by looking at alternative analyses such as decadal rates of rise. Importantly, more work remains to be done at the international level on the use of innovative non linear time series analyses that provide a more authoritative position on “real-time” SLR and associated accelerations. Notwithstanding the acceleration analysis, Watson (2011) also looked at decadal rates of rise from the 4 longest tide gauge records available for the southern hemisphere (Australasia). This is the first time this has been undertaken by any researcher. Watson (2011) concluded that short period trends of acceleration in mean sea level post 1990 were evident at each site, although they were not yet abnormal or higher than other short term rates measured throughout the historical record. Watson’s conclusions for the southern hemisphere records are significant in that they accord with the findings of other researchers looking at international (northern hemisphere) records, notably Haigh et al (2009), Hannah (1990, 2004), Holgate and Woodworth (2004), Holgate (2007) and Wahl et al (2011). These findings are highly relevant for inclusion within section 3.7.2. In particular, these various findings provide some robustness to the caution exercised in the AR4 findings relevant to the rates of SLR measured during the altimetry era. It is relevant that the next 10-20 years will provide the keenest evidence yet of whether the post altimetry era data start to indicate rates of rise in mean sea level and associated accelerations that are indeed higher or abnormal in the context of the historical records of key long-term tide gauge records. When these historical thresholds have been surpassed (on a consistent basis at key long-term gauge sites representative of the major ocean basins of the world), then it might be definitively concluded that we are moving into an era signalling the onset of high (anthropogenically forced) projected sea level rise for the 21st century. Work is urgently needed beyond AR5 to investigate analytical techniques to distinguish the “tipping points” in records where the anthropogenic forcing is clearly differentiated from the inter-decadal variability of long term sea level records. The AR5 FOD Observations: Oceans chapter might benefit from some broader discussion around the above-mentioned issues integrated with the Sea Level Change chapter (13). References Haigh, I.D., Nicholls, R.J., Wells, N.C (2009). Mean sea level trends around the English Channel over the 20th century and their wider context. Continental Shelf Research, 29 (17), 2083-2098. Hannah, J. (1990). Analysis of mean sea level data from New Zealand for the period 1899-1988. Journal of Geophysical Research, Vol 95(B6), pp 12,399-12,405. Hannah, J. (2004). An updated analysis of long-term sea level change in New Zealand. Geophysical Research	Accepted - reference by Watson is added and discussion on "acceleration" vs. "deceleration" is revised to make clear that quadratic terms can shift sign depending on starting point. However, for longest recrods extending before 1920, quadratic terms are positive.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Letters, 31:L03307, DOI:10.1029/2003GL019166. Holgate, S.J., and Woodworth, P.L. (2004). Evidence for enhanced coastal sea level rise during the 1990s. Geophysical Research Letters, 31:L07305, DOI:10.1029/2004GL019926. Holgate, S.J. (2007). On the decadal rates of sea level change during the twentieth century. Geophysical Research Letters, 34:L01602, DOI:10.1029/2006GL028492. Houston, J.R., and Dean, R.G. (2011). Sea-Level Acceleration Based on U.S. Tide Gauges and Extensions of Previous Global Gauge Analyses. Journal of Coastal Research, Vol 27 (3), pp. 409-417, DOI: 10.2112/JCOASTRES-D-10-00157.1, May. Wahl, T., Jensen, J., Frank, T., Haigh, I.D. (2011). Improved estimates of mean sea level changes in the German Bight over the last 166 years. Ocean Dynamics, 61 (5), 701-705. Watson, P.J. (2011). Is There Evidence Yet of Acceleration in Mean Sea Level Rise Around Mainland Australia. Journal of Coastal Research, Vol 27 (2), pp. 368-377, DOI: 10.2112/JCOASTRES-D-10-00141.1, February. Woodworth, P.L.; White, N.J.; Jevrejeva, S.; Holgate, S.J.; Church, J.A., and Gehrels, W.R. (2009). Review – Evidence for the Accelerations of Sea Level on Multi-Decade and Century Timescales. International Journal of Climatology, Vol 29, pp 777-789, doi:10.1002/joc.1771. Woodworth, P. L., M. Menendez, and W. R. Gehrels, (2011). Evidence for Century-Timescale Acceleration in Mean Sea Levels and for Recent Changes in Extreme Sea Levels. Surveys in Geophysics, 32, 603-618. [Phil Watson, Australia]	
3-532	3	26	46	27	3	Ablain et al (Ablain, M., Cazenave, A., Valladeau, G., and Guinehut, S.: A new assessment of the error budget of global mean sea level rate estimated by satellite altimetry over 1993–2008, Ocean Sci., 5, 193-201, doi:10.5194/os-5-193-2009, 2009) find a reduction in the rate of sea level rise after 2005 by about 2mm/yr. [Andreas Sterl, Netherlands]	Accepted - reference added.
3-533	3	26	46	27	20	"Decadal" values can conceal fluctuations and periodic changes [VINCENT GRAY, NEW ZEALAND]	Noted - We already state: " uncertainty caused by sparse observations with large, uncorrelated decadal variations makes it impossible to determine conclusively whether similar large trends in GMSL occurred in the period between 1700 and 1950" and later :"In light of the large decadal variability in regional sea level and still uncertain decadal variations in GMSL, it is difficult to quantify accelerations in GMSL"
3-534	3	26	53			Reference to Church and White (2006) is incomplete, include issue number (1), article number (L01602) and doi (10.1029/2005GL024826) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-535	3	26	56	26	56	Please refer to comment 11. [Leticia Cotrim da Cunha, Germany]	Accepted - uncertainty computed for Chapter 3 will be at evaluated consistently at 90% confidence
3-536	3	27	5			This acceleration section is not too bad but it omits mention of saltmarsh records. It does refer to Woodworth et al. (2011) (Surveys in Geophysics) and it could refer to: Woodworth, P.L., Gehrels, W.R and Nerem, R.S. 2011. Nineteenth and twentieth century changes in sea level. Oceanography, 24(2), 80â€“93, doi:10.5670/oceanog.2011.29. which has some similarities. They discuss accelerations in both tide gauges and saltmarsh data. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Noted - salt marsh records are discussed in Chapter 5 (paleoclimate) (Figure 5.17), and are synthesized in Chapter 13. Will not discuss here in detail, but will reference Chapter 5.
3-537	3	27	7	27	7	quadratic function? [Leticia Cotrim da Cunha, Germany]	Accepted - text revised.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-538	3	27	10	27	10	yr-2 should be yr-1? [Rongshuo Cai, China]	Rejected - we do mean yr**-2 as we are discussing accelerations
3-539	3	27	13			This result of Houston and Dean is not new at all. I suggest after 'negative acceleration' put '(cf. Holgate, 2007 and other authors previously)'. Reference is: Holgate SJ (2007) On the decadal rates of sea level change during the twentieth century. Geophys Res Lett 34: L01602, doi:10.1029/2006GL028492. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Accepted - text has been revised to comment on "deceleration" in 1960s noted by Holgate and others.
3-540	3	27	21			Section 3.7 seems to me to be missing mention of observed sea level rise and acceleration being due to wind-driven redistribution of water as well as global warming. References could be the following: Miller L, Douglas BC (2007) Gyre-scale atmospheric pressure variations and their relation to 19th and 20th century sea level rise. Geophys Res Lett 34 : L16602, doi:10.1029/GL030862. Woodworth, P.L., Pouvreau, N. and Woppelmann, G. 2010. The gyre-scale circulation of the North Atlantic and sea level at Brest. Ocean Science, 6, 185-190, doi:www.ocean-sci.net/6/185/2010/. Merrifield, M.A., 2011. A shift in western tropical pacific sea-level trends during the 1990s. Journal of Climate, 24, 4126-4138, doi:10.1175/2011JCLI3932.1. Merrifield, M.A. and Maltrud, M.E. 2011. Regional sea level trends due to a Pacific trade wind intensification. Geophys. Res. Lett., 38, L21605, doi:10.1029/2011GL049576. There are also papers by Han et al. (J Climate?), Boenning et al. (??). [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Accepted - discussion added to Section 3.7.1 (Patterns in Sea Level). Sturges and Douglas (2011) was referenced, but a discussion of other papers on topic will be added as well.
3-541	3	27	24	27	25	Forgot to mention isostatic adjustment in the list of sea level change components , unless we are talking on global sea level [Pavel Tkalich, Singapore]	Noted - GIA was mentioned previously and (3-25, lines 38-40), when we state we are correcting for GIA to observe volume changes.
3-542	3	27	24	27	25	This sentence needs to be revised. I think global water cycle includes E, P, runoff and mass balance of ice sheets and glaciers. Global E-P change has a very tiny contribution to global sea level change, since the atmospheric moisture content change is a tiny amount. If runoff here has nothing to do with mass balance of terristrial ice, runoff is just an amount of water transported away from ocean to land and returned to ocean from land and hence has zero contribution to global sea level changes. So here, we can increase the clarity to say that "water is added to it by melting of ice sheets and glaciers", if we neglect the changes in atmospheric moisture content and changes in land liquid water storage. [Zhaomin Wang, UK]	Rejected - The seasonal and interannual imbalance in P-E+R over the ocean has measurable changes in sea level and ocean mass. See Chambers et al. (2004), Willis et al.,2008.
3-543	3	27	28			Reference to Antonov et al (2002) is incomplete, include issue number (C12), article number (8013) and doi (10.1029/2001JC000964) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-544	3	27	28			Dhomps et al (Dhomps, A. -L., Guinehut, S., Le Traon, P.-Y., and Larnicol, G.: A global comparison of Argo and satellite altimetry observations, Ocean Sci., 7, 175-183, doi:10.5194/os-7-175-2011, 2011) show that the use of measured S-profiles (from Argo) yields to an improvement in SSH calculations form in-situ observations and a better agreement with altimeter measurements. So there is a discernible local effect of salinity on SSH. [Andreas Sterl, Netherlands]	Rejected - we already state salinity will have local effects on sea level; globally averaged, the contribution is small.
3-545	3	27	29	27	29	You mean Section 3.2.2 instead of Section 3.1? [Belén Martín Míguez, Spain]	Accepted - yes, Section 3.2 is meant. Text changed.
3-546	3	27	29	27	52	I think this whole section should be more consistent which section 13.4.1 in terms of rates presented and references. I will address this in my comments included in rows 27 and 28 of this excel sheet [Belén Martín Míguez, Spain]	Accepted - differences with Chapter 13 have been resolved and will be corrected for SOD.
3-547	3	27	31	27	31	The reference to Section 3.1 does not make sense here [Belén Martín Míguez, Spain]	Accepted - Section 3.2 is meant. Text changed.
3-548	3	27	32	27	33	North America (NA) and Asia are huge landmasses; please be more specific (as in NW Europe and NW Africa in the next sentence). The ENSO zonal (N-S) dipole in NA is well documented. Thus it is not possible to generalize about ENSO teleconnections across NA. [David Sauchyn, Canada]	Rejected - David Sauchyn's comments refer to a different chapter
3-549	3	27	34	27	34	Please refer to comment 11. [Leticia Cotrim da Cunha, Germany]	Accepted - uncertainty computed for Chapter 3 will be at evaluated consistently at 90% confidence
3-550	3	27	36	27	37	Reference to Leuliette and Miller (2009) is incomplete, include article number (L04608) and doi (10.1029/2008GL036010) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication

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3-551	3	27	37			Reference to Llovel et al (2011) is incomplete, include article number (L15608) and doi (10.1029/2011GL047411) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-552	3	27	37			Reference to Willis et al (2008) is incomplete, include issue number (C6), article number (C06015) and doi (10.1029/2007JC004517) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-553	3	27	42	27	44	A very important reference here is Villalba, R. et al. 2011. Dendroclimatology from Regional to Continental Scales: Understanding Regional Processes to Reconstruct Large-Scale Climatic Variations Across the Western Americas. Chapter 7 in M.K. Hughes et al. (eds.), Dendroclimatology, Developments in Paleoenvironmental, Springer. [David Sauchyn, Canada]	Rejected - David Sauchyn's comments refer to a different chapter
3-554	3	27	54	27	54	I think it wouldn't hurt to include a reference to GRACE here just as in the legend of Figure 3.14. I suggest "The mass component of mean sea level rise has only recently been measured by using satellite observations (GRACE, see section 13.4.5) of time-variable gravity at monthly time scales (Chambers et al., 2004). [Belén Martín Míguez, Spain]	Accepted - text revised.
3-555	3	27	55			Reference to Chambers et al (2004) is incomplete, include issue number (13), article number (L13310) and doi (10.1029/2004GL020461) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-556	3	28	2			Reference to Chambers et al (2010) is incomplete, include article number (B11415) and doi (10.1029/2010JB007530) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-557	3	28	13	28	14	It states that “Which gives increased confidence that the current ocean observing system is capable of resolving the long-term rate of sea level rise and its components, assuming continued measurements”. This proves undoubtedly, the ocean is warming during different periods. Why we can not provide such a confidence for other ocean properties such as ocean temperature, upper ocean heat content, winds, salinity, etc.,. [Ravichandran Muthalagu, India]	Noted - The statement refers to a very short period (< 10 years) where we have multiple observations of sea level and its major components. We address the confidence of upper ocean warming in Section 3.2 & Box 3.1, and in surface salinity in Section 3.3. Surface temperature and winds are assessed in Chapter 2.
3-558	3	28	14	28	15	This is useful information; why is there no similar statement for droughts? [David Sauchyn, Canada]	Rejected - this comment must be misplaced, as this discussion has no relationship to droughts.
3-559	3	28	16	28	34	The text about extreme sea level and storm surges could be included in section 3.4, since these extremes are well related to the wave climate. [Eduardo Siegle, Brazil]	Rejected - since changes in extreme sea level and storm surge appears to be most related to GMSL change, it is more appropriate in Section 3.7
3-560	3	28	16	28	34	Scales of extreme sea levels due to strom surges differ from the scales assessed in the discussion about sea level, therefore I suggest to discuss this in two separate topics. Additionally, since the knowledge of storm surges is of prime importance for the low-lying coastal zones and coastal communities, this topic could be further explored. [Eduardo Siegle, Brazil]	Noted - However, Chapter 3 is limited in pages, and there is no room to explore the details of the studies referenced, other to summarize the main point that extremes are increasing and much of this has been linked to mean sea level change.
3-561	3	28	18	28	18	“a certain threshold” is ambiguous. The threshold will change in the different regions? [Rongshuo Cai, China]	Accepted - sentence will be revised to be clear that the threshold can differ from region to region, or from study to study
3-562	3	28	19	28	20	Storm surges (plural)? [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-563	3	28	25			Reference to Marcos et al (2009) is incomplete, include article number (C01007) and doi (10.1029/2008JC004912) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-564	3	28	27	28	27	Reference out of brackets (Woodworth) [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-565	3	28	27	28	27	delete "Woodworth" - typo [Roland Gehrels, United Kingdom]	Accepted - text revised
3-566	3	28	27	28	27	Dangling "Woodworth". [Stephen Griffies, USA]	Accepted - text revised
3-567	3	28	27	28	27	"copious Woodworth" does not make sense [Belén Martín Míguez, Spain]	Accepted - text revised

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3-568	3	28	27			Replace "copious Woodworth" by "copious" [Mauro Cirano, Brazil]	Accepted - text revised
3-569	3	28	27			Reference to Menendez and Woodworth (2010) is incomplete, include article number (C10011) and doi (10.1029/2009JC005997) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-570	3	28	30	28	34	There are some contradictions in conclusions within the series of publication united by common author Philip L. Woodworth. For instance, Earlier work by WOODWORTH and BLACKMAN (2004) state “there is indeed evidence for an increase in extreme highwater levels worldwide in the period since 1975, and that the variations in extremes in this period are related to changes in regional climate”. Later work by Mene´ndez and Woodworth (2010) state different “that the MSL rise is the major reason for the rise in extreme high water at most stations”. While latest work by Woodworth, Mene´ndez and Gehrels (2011) seams not that certain in their statement “It appears that rates of change in extreme sea levels are similar to those in mean levels, although this general finding does not apply at every location”. AR5 seams given priority to conclusions by Mene´ndez and Woodworth (2010), without proper qualifications. Probably WGI could try to explain mechanisms by which MSL rise lead to increase of sea level extremes. [Pavel Tkalich, Singapore]	Noted - this section will be modified slightly to make clear that extremes can vary interannually (connected to regional climate variations, like ENSO, NAO, etc), but there are also long-term statistically significant increases at most stations that is related to mean sea level rise.
3-571	3	28	30			Can you quantify some of these changes? [Hans Poertner, Germany]	Accepted - a brief summary of "extremes" are added (highest daily sea level, etc)".Most analyses have focused on specific regions and find that extremes have been increasing, using various statistical measures such as annual maximum surge, annual maximum surge-at-high-water, or changes in 99th percentile events"
3-572	3	28	38	28	38	1900 - on page 26 it says 1901 and 1.6, not 1900 and 1.7. Be consistent. [Roland Gehrels, United Kingdom]	Accepted - text changed
3-573	3	28	38	28	49	Because of issues raised after AR4, the authors should also summarize the case (or not) for accelration of GMSL increase. [Terrence Joyce, USA]	accepted - we will revise the section on acceleration and discuss the evidence for a change over the 20th century in the observational tide gauge records
3-574	3	28	38			This number on sea level rise is not quite the same as given elsewhere? [Hans Poertner, Germany]	Accepted - will be made consistent with Table 3.1 and elsewhere in text.
3-575	3	28	39			can be larger than this => can be larger or smaller than this [Andreas Sterl, Netherlands]	Accepted - text will be revised that local changes can be severla times smaller as well.
3-576	3	28	40	28	41	Re-write this sentence (since used 2x). [Leticia Cotrim da Cunha, Germany]	Accepted - text will be revised
3-577	3	28	41	28	41	The conclusion is misleading. GMSL rise rate during 1940th has been higher than anytime after 1950 if 11-year running average is applied. [Pavel Tkalich, Singapore]	Accepted - text will be revised in SOD based on recent studies that have been submitted finding a 60-year oscillation in GMSL
3-578	3	28	43		44	Can you add information on the contribution of glacier melts to sea level rise here? [Hans Poertner, Germany]	Rejected - our chapter deals exclusively with ocean observations. Chapter 4 will address glacier contributions.
3-579	3	28	46		48	Unclear message in sentence "These recent observations indicate that sea level budget can close,….". The same sentence can be found under the 'Executive Summary' p.4; line 14-16 [Thomas Voigt, Germany]	Accepted - will revise sentence to clarify what is meant by sea level budget (i.e., 2 independent

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							measurements of components and the total)
3-580	3	28	48	28	49	WGI could try to explain mechanisms by which MSL rise lead to increase of storm surges [Pavel Tkalich, Singapore]	Accepted - will add a sentence to explain basic mechanism, i.e., same variable state around a growing mean state will led to apparent increase in extremes relative to a fixed mean state.
3-581	3	28	52	29	6	All these figures exaggerate the importance of thb earlier least reliable fdata and fail to point out the falling rates which can be seen in recent figures, particularly after the installation of accurate levelling equipment [VINCENT GRAY, NEW ZEALAND]	Noted - rates are simply the values calculated from accepted data over periods that are commensurate with observing system. "Falling rates" (due to interannual fluctuations) are included in the 2005-2010 periods.
3-582	3	28	52	29	6	Last two lines of Table 3.1 show that Thermosteric+mass 2005-2010 1.6+-0.5c. The paper (Leuliette and Willis,2011) describes "steric" instead of "thermosteric" here. [Sok Kuh Kang, South Korea]	Rejected - the global halosteric change is too small to make a meaningful difference between steric and thermosteric
3-583	3	28	52	29	6	This rate(1.6+-0.5 mm/yr) is some smaller than rate(2.1+-0.4mm/yr)(in same table by Nerem et al.(2010)) from satellites, probably out of error range. This thermosteric sea level over the 2005-2010 is also smaller than 0.75+-0.15mm/yr by Schukmann and LeTranon (sumitted, 3-4 lines in 13-18page). From this sense should it be mentioned that closure problem is still open or some limit exists. SInce it is described in chapter 3 and main table 3.1 such a mass balance seems to imply a general conclusion. [Sok Kuh Kang, South Korea]	Rejected - the rates clearly have overlapping uncertainty bars. The thermosteric trend from Leuliette and Willis is not statistically different than the rate from Schukmann and LeTranon, and the uncertainty estimate is more relaisitc. The budget does close within uncertainty.
3-584	3	28	52			I am not sure how useful Table 3.1 is. It takes a lot of space and doesn't say much. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Noted - Table 3.1 is a nice summary of the rates of GMSL over the different observing periods and for the different components. It was requested by Chapter 13 so they could reference it.
3-585	3	28				Table 3.1 may be shifted on page 29 [Muhammad Amjad, Pakistan]	Accepted - will be fixed in next draft.
3-586	3	29	8			There is a conspicuous absence of the PDO. It is considered in other chapters; 2 and 14 in particular. Reference should be made to Table 1 in Box 2.4 [David Sauchyn, Canada]	Rejected - David Sauchyn's comments refer to a different chapter
3-587	3	29	9	29	9	Since all ocean is alkaline a better term is "alkalinity reduction" [VINCENT GRAY, NEW ZEALAND]	rejected Chapter 3 retains the terminology used in published manuscripts in peer reviewed journals
3-588	3	29	9	34	25	This is a very clear, updated account on the issue of ocean acidification. [Hans Poertner, Germany]	noted.
3-589	3	29	9	34	25	Any reference to biological impact should be carefully balanced with the WGII assessment or be reduced to a minimum and the statement that such assessment can be found there (e.g. WGII, chapter 6). [Hans Poertner, Germany]	noted.
3-590	3	29	9	34	25	The balance of the citations chosen should be checked. There seems to be a citation bias for US American authors (see also p. 39)? [Hans Poertner, Germany]	Accepted - text revised
3-591	3	29	12	29	24	There is a discrepancy between this chapter and chapter 6 on how the oceanic inventory for 2010 is computed. In chapter 6, it was decided to start with the Sabine et al. 2004 estimate and then add the integrated uptake flux since 1994 to that number. Here, it was decided to base the oceanic inventory on Khatiwala et al.. I recommend to go with the approach used in chapter 6. The main reason is that Khatiwala's estimate is not really an observation, but an observationally based model (see my detailed comments above (comment 51) [Nicolas Gruber, Switzerland]	the discrepancy between Ch3 and Ch6 is resolved. The inventories based on the different aspproaches agree within their uncertianties.
3-592	3	29	13	29	13	Inorganic ocean carbon content is roughly 50 times the carbon content (CO2) of the current atmosphere. [Christoph Heinze, Norway]	Accepted - text revised
3-593	3	29	13		15	This initial statement should be complemented by one which mentions that the carrying capacity of the oceans per unit volume for CO2 gas is similar to that in air, due to high physical solubility. This text does not	The text has remained the same because of space constraints.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						emphasize that DIC has different species and may thus be misleading for the non expert. The relative volumes of atmosphere and oceans would also be relevant. [Hans Poertner, Germany]	
3-594	3	29	17	29	18	Add the following two references to support that ca. 25% of the annual anthropogenic CO2 emissions enter the ocean: (1) Mikaloff Fletcher, S. E., N. Gruber, A. R. Jacobson, S. C. Doney, S. Dutkiewicz, M. Gerber, M. Follows, F. Joos, K. Lindsay, D. Menemenlis, A. Mouchet, S. A. Müller, and J. L. Sarmiento, 2006, Inverse estimates of anthropogenic CO2 uptake, transport, and storage by the ocean, Global Biogeochemical Cycles, 20, GB2002, doi:10.1029/2005GB002530, 16 p.; (2) Le Quéré, C., T. Takahashi, E. T. Buitenhuis, C. Rödenbeck, and S. C. Sutherland, 2010, Impact of climate change and variability on the global oceanic sink of CO2, Global Biogeochemical Cycles, 24, GB4007, doi:10.1029/2009GB003599, 10 p.. [Christoph Heinze, Norway]	Accepted - text revised
3-595	3	29	17	29	18	"Currently, approximately 25% of the CO2 released to the atmosphere by burning of fossil fuels and land-use change enters the ocean across the air-sea interface.": I feel like the language of the carbon cycle can be imprecise: does this mean that 25% of the carbon atoms in fossil fuels end up in the ocean, or that oceanic carbon has increased by an amount equal to 25% of the total fossil fuel carbon atoms? These are subtly different, like the difference between "residence lifetime" and "perturbation/adjustment lifetime"... [Marcus Sarofim, USA]	Accepted - text revised
3-596	3	29	19	30	5	Model estimations of antropogenic carbon uptake are mainly based on the physical pump fuctioning, with a constant biological pump. However, recent reviews indicate significant multiyear changes in net oceanic primary production that can be related to global warming. Analysis of satellite data (McClain et al., 2004; Behrenfeld et al., 2006; Polovina et al., 2008) claimed a decrease in most of open ocean productivity in subtropical gyres due to increased ocean stratification and reduced flux of nutrients. However, in these global studies, data from the productive continental shelves are generally outweighed by those from the larger oligotrophic areas of the ocean, where most of the production and chlorophyll is well below the surface layer, and does not include cyanobacteria and other small phytoplankton. The specific analysis of continental shelf ecosystems, including field data in the most productive upwelling areas (e.g. Chavez et al., 2011), revealed a large variety of trends at scales of several decades but a general increase of phytoplankton carbon fixation by phytoplankton in most shelves (Sherman and Hempel, 2009; Chavez et al., 2011; Bode et al., 2011). In the coastal studies, however, it is generally acknowledged that the increases may have been caused both by climate changes (both natural and anthropogenic) and by direct anthropogenic effects (e.g. artificial fertilizers release). At least a mention to this issue should be incorporated in this section. [Antonio Bode, Spain]	Accepted
3-597	3	29	19	30	5	References from comment above: Behrenfeld, M. J., R. T. O'Malley, D. A. Siegel, C. L. McClain, J. L. Sarmiento, G. C. Feldman, A. J. Milligan, P. G. Falkowski, R. M. Letelier, and E. S. Boss, 2006: Climate-driven trends in contemporary ocean productivity. Nature, 444, 752-755. Bode, A., J. Hare, W. K. W. Li, X. A. G. Morán, and L. Valdés, 2011: Chlorophyll and primary production in the North Atlantic. ICES Status Report on Climate Change in the North Atlantic., P. C. Reid and L. Valdés, Eds., International Council for the Exploration of the Sea, 77-102. Chavez, F. P., M. Messié, and J. T. Pennington, 2011: Marine primary production in relation to climate variability and change. Annual Review of Marine Science, 3, 227-260. McClain, C. R., S. R. Signorini, and J. R. Christian, 2004: Subtropical gyre variability observed by ocean-color satellites. Deep Sea Res. II, 51, 281-301. Polovina, J. J., E. A. Howell, and M. Abecassis, 2008: Ocean's least productive waters are expanding. . Geophysical Research Letters, 35, doi:10.1029/2007GL031745. Sherman, K. and G. Hempel, 2009: The UNEP Large Marine Ecosystem Report: A perspective on changing conditions in LMEs of the world's Regional Seas. Vol. 182, UNEP Regional Seas Reports and Studies, United Nations Environmental Programme, 872 pp. [Antonio Bode, Spain]	Accepted - text revised

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3-598	3	29	19			What is a Green function approach? [Allison Crimmins, United States]	The Green's function approach is discussed briefly in section 3.8.1.2 and references therein.
3-599	3	29	21	29	24	Anthropogenic CO2 uptake fluxes: I suggest to be careful here to use the expression "consistent" since the estimates pertain to different periods. In addition. Is there any reason not to include the ocean inversion based numbers here? [Nicolas Gruber, Switzerland]	Accepted - text revised
3-600	3	29	23	30	2	Increase or no increase in uptake rate over what time scale? The 2.2/2.5/2.0 PgC/yr from earlier in the paragraph refers to recent uptake rates, but presumably ocean uptake rates were lower a couple decades ago… Or does this refer not to net uptake, but rather gross uptake, such that maybe gross uptake hasn't changed, and it is just gross release of carbon back to the atmosphere that has dropped resulting in net uptake? This "anthropogenic plus natural" uptake is not clear, at least to me. [Marcus Sarofim, USA]	Accepted - text revised
3-601	3	29	37			C anthropogetic should be expalained here [Muhammad Amjad, Pakistan]	The method is described in the reference (see Sabine et al 2004).
3-602	3	29		40		There are a lot of very strong statements which lack references - I'm sure references will be added at a later stage, but their absence makes section 3.8 (the only section I've looked at in this chapter) very hard to review. [Paul Halloran, UK]	Accepted - text revised
3-603	3	29				Table 3.1 seems incomplete. For example, why is the ocean mass component not given for longer time periods? [Roland Gehrels, United Kingdom]	This is covered in Chapter 13
3-604	3	30	1	30	2	This is not the way I read Sarmiento et al. In fact, they show that the ocean models began to deviate from the expected uptake evolution. So there is a change in the rate of uptake. I suggest to rephrase this. [Nicolas Gruber, Switzerland]	Accepted - text revised
3-605	3	30	5			Reference to Bates (2007) is incomplete, include issue number (C9), article number (C09013) and doi (10.1029/2006JC003759) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-606	3	30	5			Reference to Feely et al (2006) is incomplete, include issue number (C8), article number (C08S90) and doi (10.1029/2005JC003129) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-607	3	30	7		32	The text should be clearer about the direction of change of delta PCO2 and whether this is a direct correlate of flux. [Hans Poertner, Germany]	Accepted - text revised
3-608	3	30	15	30	15	I think that (Feely et al., 2002), which is the synthesis of observations for seasonal and interannual variability of CO2 in the equatorial Pacific, is more appropriate than (Feely et al. 2006) here. [Feely R. A., et al., 2002: Seasonal and interannual variability of CO2 in the equatorial Pacific. Deep-Sea Research II, 49, 2443-2469.] [Masao Ishii, Japan]	Accepted - text revised
3-609	3	30	20	30	20	Some word are missing? For example, “surface ocean waters” refers to "uptake of CO2" in surface ocean waters? [Rongshuo Cai, China]	Accepted - text revised
3-610	3	30	20	30	21	“Globally, the DpCO2 remains unchanged.” should be rather “It appears that, globally, the DpCO2 remains unchanged”. I would also suggest that authors mention the decadal variation of pCO2 that was began to be well recognized after AR4. Seawater pCO2 is still severely under-sampled in space and time in global oceans and, as mentioned in the last sentence of the previous paragraph, quantitative information on its trend over the last two decades are available only for some selected locations such as in the subtropical North Atlantic , subtropical North Pacific, western equatorial Pacific, and in the Southern Ocean (Keeling et al., 2004; Inoue and Ishii, 2005; Midorikawa et al., 2005, Takahashi et al., 2006; Dore et al., 2009; Ishii et al., 2009; McKinley et al., 2011; and others that are quoted in this section). In several of these locations, authors argued that DpCO2 is changing (e.g. the Kuroshio Extension region in the western North Pacific). However, with the extension of the pCO2 records over some decades, it began to look like DpCO2 remain unchanged, i.e., the mean rate of increase in pCO2 converges with the trend of atmospheric CO2 over the time scale of a few decades (Ishii et al., 2009 for the western equatorial Pacific; McKinley et al., 2011 for the North Atlantic).	Accepted - text revised

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						The decadal pCO2 variation and its controlling processes will have implications for the response of pCO2 to the longer-term climate change, but are yet to be understood. [Masao Ishii, Japan]	
3-611	3	30	20	30	21	References for the above comments that have not been quoted in this chapter: Keeling, C. D., H. Brix, and N. Gruber, 2004: Seasonal and long-term dynamics of the upper ocean carbon cycle at Station ALOHA near Hawaii, Global Biogeochemical Cycles, 18, GB4006, doi:10.1029/2004GB002227. Inoue, H. Y., and M. Ishii, 2005: Variations and trends of CO2 in the surface seawater in the Southern Ocean south of Australia between 1969 and 2002. Tellus 57B. 58-69. Midorikawa,T., K. Nemoto, H. Kamiya, M. Ishii, H. Y. Inoue, 2005: Persistently strong oceanic CO2 sink in the western subtropical North Pacific. Geophysical Research Letters, 32, L05612, doi:10.1029/2004GL021952. Takahashi, T., S. C. Sutherland, R. A. Feely, 2006: Decadal change of the surface water pCO2 in the North Pacific: A synthesis of 35 years of observations, Journal of Geophysical Research-Oceans, 111, C07S05, doi:10.1029/2005JC003074. Ishii, M., Inoue, H.Y., Midorikawa, T., Saito, S., Tokieda, T., Sasano, D., Nakadate, A., Nemoto, K., Metzl, N., Wong, C. S., Feely, R. A., 2009: Spatial variability and decadal trend of the oceanic CO2 in the western equatorial Pacific warm/fresh water. Deep-Sea Research II, 56, 591-606. McKinley, G. A., A. R. Fay, T. Takahashi, and N. Metzl, 2011: Nature Geoscience, 4, 606-610, doi: 10.1038/ngeo1193. [Masao Ishii, Japan]	Accepted - text revised
3-612	3	30	20			I would have thought that we don't enough data to say this. We know that regionally it certainly is not the case, and we know theoretically that the change in revelle factor and change in temperature over the historical period mean that this is unlikely to be the case. [Paul Halloran, UK]	Accepted - text revised
3-613	3	30	23		25	The rationale for cause and effect could be elaborated more clearly. It is not clear how a change in circulation drives a higher efflux as not all factors are mentioned in the explanation. Does this include shoaling of OMZ waters? [Hans Poertner, Germany]	Accepted - text revised
3-614	3	30	27	30	28	Reference to Schuster and Watson (2007) is incomplete, include issue number (C11), article number (C11006) and doi (10.1029/2006JC003941) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-615	3	30	30	30	32	In addition to changes in the air-sea flux of CO2 attributed to changes in wind speed, it is likely that the carbon inventory is also affected by changes in the meridional overturning of the Southern Ocean (le Quere et al., 2007). This should be linked to earlier consideration (in chapter 3) of changes in the overturning (see comment 4). Reference: Le Quere, C., et al. (2007). Saturation of the Southern Ocean CO2 sink due to recent climate change. Science, 316, 1735-1738. [Robert Marsh, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
3-616	3	30	36	30	45	I suggest to add that the ΔC* method is the only based on carbon observations, while the other two are based solely on CFC observations. [Nicolas Gruber, Switzerland]	We are required to report results based on CFC and C-14 measurements as well.: they are observations
3-617	3	30	37			* is not explained as footnote [Muhammad Amjad, Pakistan]	ÂC* is described in Sabine et al 2004
3-618	3	30	41		43	This text would benefit from some clarifications for the non expert. [Hans Poertner, Germany]	Accepted - additional references provided
3-619	3	30	44	30	44	Why is particularly at high latitudes? References are missing? Or assessment should be given here. [Rongshuo Cai, China]	Accepted - additional references provided
3-620	3	30	45	30	46	Rather than “hitherto unknown” [note correct spelling], the reasons for this divergence seem to be contained in the preceding sentence: “both marine and terrestrial proxy archives from different locations have been used to	Editorial - copyedit to be completed prior to publication

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						reconstruct the AMO". The nature and strength of the AMO signals will differ between proxies and sites. [David Sauchyn, Canada]	
3-621	3	30	49			Reference to Tanhua et al (2009) is incomplete, include issue number (C1), article number (C01002) and doi (10.1029/2008JC004868) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-622	3	30	50			Reference to Olsen et al (2010) is incomplete, include doi (10.1029/2009JC005488) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-623	3	30	50			Reference to Schneider et al (2010) is incomplete, include article number (RG3001) and doi (10.1029/2009RG000302) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-624	3	30	51			Reference to Park et al (2010) is incomplete, include issue number (4), article number (GB4013) and doi (10.1029/2005GB002676) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-625	3	30	53	31	10	The recent paper by Jeansson et al. (2011, Global Biogeochem. Cycles, 25, doi:10.1029/2010GB003961) could be a very useful addition here. It documents the sources, sinks, and uncertainties for the Nordic Seas carbon budget, including contrasting the present CO2-uptake with the preindustrial, and change in total versus anthropogenic uptake. [Tor Eldevik, Norway]	rejected due to limited space, Ch3 does not discuss marginal seas
3-626	3	30	54			The word although should probably be deleted. [Hans Poertner, Germany]	noted
3-627	3	31	2	31	2	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	reference to comment 18 is not clear
3-628	3	31	9			Reference to Wanninkhof et al (2010) is incomplete, include doi (10.1029/2010JC006251) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-629	3	31	10	31	10	The large uncertainties about the anthropogenic carbon inventory in the Southern Ocean as derived from observations using a broad variety of different reconstruction methods have been nicely been documented by: Vázquez-Rodríguez, M., F. Touratier, C. Lo Monaco, D. W. Waugh, X. A. Padin, R. G. J. Bellerby, C. Goyet, N. Metzl, A. F. Ríos, and F. F. Pérez, 2009, Anthropogenic carbon distributions in the Atlantic Ocean: data-based estimates from the Arctic to the Antarctic, Biogeosciences, 6, 439–451, www.biogeosciences.net/6/439/2009/. Especially their Figure 6 is revealing. I suggest to add this figure to the chapter as it documents the state of the art. [Christoph Heinze, Norway]	rejected. The regional differences inferred from different methods are not discussed here owing to lack of space.
3-630	3	31	13	31	22	Specify in the legend of Figure 3.16 (bottom panels) what is the meaning of the width of the horizontal bars that represent the measurements. Are they associated with the period where the measurements took place? [Mauro Cirano, Brazil]	noted. Figure and caption revised
3-631	3	31	20			Reference to Murata et al (2008) is incomplete, include issue number (C6), article number (C06007) and doi (10.1029/2007JC004424) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-632	3	31	20			Reference to Olsen et al (2006) is incomplete, include issue number (3), article number (GB3027) and doi (10.1029/2005GB002669) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-633	3	31	21			Reference to Murata et al (2007) is incomplete, include issue number (C5), article number (C05033) and doi (10.1029/2005JC003405) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-634	3	31	21			Reference to Murata et al (2009) is incomplete, include doi (10.1029/2008JC004920) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-635	3	31	21			Reference to Sabine et al (2008) is incomplete, include issue number (C7) and doi (10.1029/2007JC004577) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-636	3	31	22			Reference to Matear and McNeil (2003) is incomplete, include issue number (4), article number (1113) and doi (10.1029/2003GB002089) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-637	3	31	22			Reference to Murata et al (2010) is incomplete, include doi (10.1029/2010JC006250) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-638	3	31	24	31	33	Since all ocean is alkaline a better term is "alkalinity reduction" [VINCENT GRAY, NEW ZEALAND]	we acknowledge the reviewer's point, but "acidification" is now a well-established term

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-639	3	31	24	34	25	The attribution of anthropogenic ocean acidification should be delt with in the same format as for other variables like in Ch. 10 either in Ch. 3 or 10. It will be important to know if acifidication could be attributed to antropogenic increase of CO2 in the atmosphere. [Øyvind Christophersen, Norway]	rejcected. The attribution is beyond the scope of Chapter 3.
3-640	3	31	24			Section 3.8.2 discusses pH changes but does not develop the parallel issue of carbonate ion deficiency and its biological effects, other than incidental mention of the need for calcium carbonate production by some organisms. Physiological effects on invertebrates as well as finfish due to carbonate chemistry change is not addressed. The saturation state of ocean waters is not developed in the text. The fact that all discussion of ocean acidification appears to be about surface waters, also ignores the deep water change in saturation state and expected shoaling of omega for aragonite in some ocean areas. [Christopher Kavanagh, Monaco]	partly accepted. the biological impact of pH changes is beyond the scope of Chapter 3. chnges in deep water are addressed
3-641	3	31	26		28	Adding the principle equations and concentrations would be nice to illustrate the composition of DIC. [Hans Poertner, Germany]	rejected this is beyond the scope of Ch3
3-642	3	31	30	31	30	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	rejected the comment is unclear
3-643	3	31	30	31	31	The definition of ocean acidification sounds awkward and is a partial tautology:"Ocean uptake of CO2 results in gradual acidification of seawater; this process is termed ocean acidification (Caldeira and Wickett, 2003)." A recommendation for an updated definition of ocean acidification has been made during the IPCC WGII&I brainstorming workshop on ocean acidification at Okinawa, Japan, January 2011: "Ocean acidification refers to a reduction in the pH of the ocean over an extended period, typically decades or longer, which is caused primarily by uptake of carbon dioxide from the atmosphere, but can also be caused by other chemical additions or subtractions from the ocean. Anthropogenic ocean acidification refers to the component of pH reduction that is caused by human activity." Reference: IPCC, 2011: Workshop Report of the Intergovernmental Panel on Climate Change Workshop on Impacts of Ocean Acidification on Marine Biology and Ecosystems [Field, C.B., V. Barros, T.F. Stocker, D. Qin, K.J. Mach, G.-K. Plattner, M.D. Mastrandrea, M. Tignor and K.L. Ebi (eds.)]. IPCC Working Group II Technical Support Unit, Carnegie Institution, Stanford, California, United States of America, pp. 164. The recommendation is found on page 5 of this report. Perhaps this definition could be used, also for the sake of consistency between the reports. [Christoph Heinze, Norway]	rejected attribution is beyond the scope of Ch3, and change in pH can be caused by different processes
3-644	3	31	30			Reference to Orr et al (2005) is incomplete, include issue number (C9), article number (C09S01) and doi (10.1029/2005JC003086) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-645	3	31	31			Note that ocean acidification was first introduced before 2003. See: Broecker W. & Clark E., 2001. A dramatic Atlantic dissolution event at the onset of the last glaciation. Geochemistry Geophysics Geosystems 2, 2001GC000185. [Jean-Pierre Gattuso, France]	Noted.
3-646	3	31	32		33	The conclusion of the sentence starting with ...The consequences... (poorly understood) should probably be deleted as this global statement does and cannot appreciate the differential knowledge that exists. The text could say that this is dealt with in WG II assessments (e.g. chapter 6). [Hans Poertner, Germany]	Noted.
3-647	3	31	33			One could be more positive because some effects are well-known, for example CaCO3 dissolution. See also Gattuso et al. (2011). [Jean-Pierre Gattuso, France]	Accepted - text revised
3-648	3	31	33			Sarutation state should be defined [Jean-Pierre Gattuso, France]	Accepted - text revised
3-649	3	31	35	31	46	It would be well worth adding from recent literature the rate of current acidification exceeds glacial-interglacial variability (Friedrich, T., ,2012, Detecting regional anthropogenic trends in ocean acidification against natural variability, Nature Clim. Change, advance online publication, doi:10.1038/nclimate1372.) [William Howard, Australia]	rejected Paleo time series are discussed in Chapter 5
3-650	3	31	35			Chemists seem to prefer CT (see Dickson et al., 2007). [Jean-Pierre Gattuso, France]	Accepted - text revised
3-651	3	31	38	31	39	I believe that the citations should be listed chronologically [Jean-Pierre Gattuso, France]	Editorial - copyedit to be completed prior to publication
3-652	3	31	38			The decline in pH should be in units per year, right? [Jean-Pierre Gattuso, France]	yes, corrected

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-653	3	31	39			Reference to Santana-Casiano et al (2007) is incomplete, include issue number (1), article number (GB1015) and doi (10.1029/2006GB002788) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-654	3	31	43	31	44	Replace "Figure 3.16, Table 3.1" by "Figure 3.17b, Table 3.2" [Mauro Cirano, Brazil]	Accepted - text revised
3-655	3	31	44			Reference to Byrne et al (2010) is incomplete, include article number (L02601) and doi (10.1029/2009GL040999) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-656	3	31	49	31	58	Again "long-term trends" exaggerate the importance of the earlier least reliable data [VINCENT GRAY, NEW ZEALAND]	The trends are clearly documented in this chapter
3-657	3	31				Section 3.8.2: I appreciate describing both the fundamental and state-of-the-art knowledge of anthropogenic ocean acidification in this section and also in Box.3.2 and FAQ 3.2 quite intensively. As mentioned in the FAQ 3.2, it is a direct consequence of CO2 rise that is physico-chemically indisputable. I strongly hope that the actual state of ocean acidification, its consequence on marine ecosystem, and its risks on our society and economy are further understood and they help international and domestic arguments about the mitigation of CO2 rise move forward. [Masao Ishii, Japan]	Noted
3-658	3	32	1	32	1	Include here North Atlantic? [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-659	3	32	6	32	7	Would it be clearer to say that Figure 3.18 shows "the portion" of pH changes that are attributable to anthropogenic causes? [Marcus Sarofim, USA]	Accepted - text revised
3-660	3	32	8	32	8	Should "Table 3.1" be "Table 3.2" ?? [Bogi Hansen, Faroe Islands]	Accepted - text revised
3-661	3	32	8			Replace "Table 3.1" by "Table 3.2" [Mauro Cirano, Brazil]	Accepted - text revised
3-662	3	32	8			Reference to Table 3.1 should be Table 3.2 Rewrite the sentence. [Christopher Kavanagh, Monaco]	Accepted - text revised
3-663	3	32	12	32	14	It is also noteworthy that reduction of CO32- concentration, thereby carbonate saturation level, is larger in tropical and subtropical zones where CO32- concentration and carbonate saturation level are higher than in subpolar and polar regions. [Masao Ishii, Japan]	Noted
3-664	3	32	15		16	The term buffer capacity should be specified here. Which of the seawater buffer components has a lower capacity in the cold? Would the key difference not be in the solubility of CO2 in relation to temperature supporting a stronger acidification at higher latitudes and an apparent lowering of buffering by DIC? [Hans Poertner, Germany]	This detail is clearly explained in the reference cited.
3-665	3	32	16			Reference to Egleston et al (2010) is incomplete, include article number (GB1002) and doi (10.1029/2008GB003407) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-666	3	32	21	32	28	Since all ocean is alkaline a better term is "alkalinity reduction" [VINCENT GRAY, NEW ZEALAND]	rejected Ch3 retains the terminology used in published peer reviewed manuscripts
3-667	3	32	21	33	7	The box 3.2. on ocean acidification currently describes mainly the link between rising CO2/sinking pH and its impact on ecosystems. The relation to climate change is addressed later on in the FAQ section. The link to climate change, however, should be given already here, as the entire report deals with the (physical) basis of climate change and not necessarily with chemically forced ecosystem impacts of high CO2/ocean acidification (strictly speaking ocean acidification is more a pollution problem than a climate problem, though the forcings of acidification and climate change are both linked to fossil fuel burning and respective requests on emission reductions). The link with climate would be twofold: (1) Through climate feedbacks induced by pH dependent marine processes. These are: Slight negative feedback due to reduced calcification in the ocean surface (Heinze, C., 2004, Simulating oceanic CaCO3export production in the greenhouse, Geophysical Research Letters 31, L16308; Gehlen, M., R. Gangstø, B. Schneider, L. Bopp, O. Aumont, and C. Ethe, 2007, The fate of pelagic CaCO3 production in a high CO2 ocean: a model study, Biogeosciences, 4, 505–519; Ridgwell, A., I. Zondervan, J.C. Hargreaves, J. Bijma, and T.M. Lenton, 2007, Assessing the potential long-term increase of oceanic fossil fuel CO2 uptake	Accepted - text revised and new references provided.

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						due to CO2-calcification feedback, Biogeosciences, 4, 481–492), potential positive feedback due to reduced ballasting (Armstrong, R.A., C. Lee, and J.I. Hedges, 2002, A new, mechanistic model for organic carbon fluxes in the ocean based on the quantitative association of POC with ballast minerals, Deep-Sea Research Part II, 49(1-3), 219-236; Klaas, C., and D. Archer, 2002, Association of sinking organic matter with various types of mineral ballast in the deep sea: Implications for the rain ratio, Global Biogeochemical Cycles, 16, 1116, 14p.; Heinze, C., 2004, Simulating oceanic CaCO3export production in the greenhouse, Geophysical Research Letters 31, L16308), significant potential feedback possibly due to carbon overconsumption (controversially discussed) (Riebesell, U., K.G. Schulz, R.G.J. Bellerby, M. Botros, P. Fritsche, M. Meyerhöfer, C. Neill, G. Nondal, A. Oschlies, J. Wohlers, and E. Zöllner, 2007, Enhanced biological carbon consumption in a high CO2 ocean, Nature, 450(7169), 545-548), and substantial long-term through dissolution of sedimentary CaCO3 (Archer, D., 2005, Fate of fossil fuel CO2 in geologic time, Journal of Geophysical Res. - Oceans, 110(C9), C09S05). (2) Through acting in concert with other stressors (warming, increased stratification, de-oxygenation) to modify the biological carbon pumps and hence changing global carbon cycling with not yet identified overall feedback strenghth to climate. [Christoph Heinze, Norway]	
3-668	3	32	21	34	25	It might be interesting to add a sentence or two on the impacts of acidifcation in the context of daily & seasonal variability of local acidity (perhaps this might be better explored in the biogeochemistry chapter, though): but basically, whether organisms care about the maximum acidity that they experience, or an integrated acidity over time - maybe a discussion of aragonite saturation would also be useful here. [Marcus Sarofim, USA]	This is a good idea but for reasons of space limitations we have not addressed this issue.
3-669	3	32	21			Discussion of Calcium carbonate saturation state in deep waters, and shoaling in the Pacific could be discussed here. [Christopher Kavanagh, Monaco]	This is a good idea but for reasons of space limitations we have not addressed this issue.
3-670	3	32	23	32	24	Disagree with the definition, "over an extended period" and we can not use the word "typically" at this point. I suggest something like [The global decrease in ocean pH due to the shift in chemical equilibrium of the carbonate system]. [Christopher Kavanagh, Monaco]	Rejected. We used the definition provided in Caldeira K., 2011. Ad hoc break out group: glossary entry for “ocean acidification”. In: Field C. B., Barros V., Stocker T. F., Qin D., Mach K. J., Plattner G.-K., Mastrandrea M. D., Tignor M. & Ebi K. L. (Eds.
3-671	3	32	23	32	42	these two paragraphs are mainly a repetition; they should be incorporated into the preceding paragraphs [Reiner Steinfeldt, Germany]	rejected. The second paragraph builds on the ideas of the first.
3-672	3	32	23	33	7	A time series of surface ocean pH versus time from 1751-2004, calculated consistently with observed CO2 changes in the atmosphere, is given in Figure 2 of Jacobson, M.Z., Studying ocean acidification with conservative, stable numerical schemes for nonequilibrium air-ocean exchange and ocean equilibrium chemistry, J. Geophys. Res., 110, D07302, doi:10.1029/2004JD005220, 2005. pH changes and DIC changes between 1750 and 2004 with depth are given in Figure 3. [Mark Z. Jacobson, U.S.A.]	Accepted - text revised and new references provided.
3-673	3	32	23			Credit should be given to: Caldeira K., 2011. Ad hoc break out group: glossary entry for “ocean acidification”. In: Field C. B., Barros V., Stocker T. F., Qin D., Mach K. J., Plattner G.-K., Mastrandrea M. D., Tignor M. & Ebi K. L. (Eds.), Workshop report of the Intergovernmental Panel on Climate Change workshop on impacts of ocean acidification on marine biology and ecosystems, pp. 37-38. Stanford, California: IPCC Working Group II Technical Support Unit, Carnegie Institution. [Jean-Pierre Gattuso, France]	Accepted - text revised and new references provided.
3-674	3	32	23			It looks odd to have the opening sentence as "What is ocean acidification?", since the rest of the chapter is not written in this way. I would suggest removing this sentence, since it reads equally well withouit it. [Michael Meredith, UK]	rejected The question is the title of the FAQ
3-675	3	32	30	32	42	Carbon dioxide is not measureds from a representive sample, either now or in the past, and measurements made netween 1815 and 1958 have been suppressed or forgotten, and show much variability The details have been recorded by Beck at Beck, E-G, 2007. 150 Years of Atmospheric Gas Analysis by Chemical Methods, Energy and Environment 18 259-281 [VINCENT GRAY, NEW ZEALAND]	rejected. The oceanic increase in CO2 is supported by the data sets decribed here and in the references cited.
3-676	3	32	32	32	32	The CO2 concentration of 392 ppm is based on what? [AKIHIKO MURATA, Japan]	acccepted text revised

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3-677	3	32	33			replace “now” by the year (2012?) [Jean-Pierre Gattuso, France]	acccepted text revised
3-678	3	32	33			This statement needs more or a better reference and appears rather conservative? The reference cited concentrates on the period 650000 to 800000 years ago. Literature considering wider time spans should be included. [Hans Poertner, Germany]	acccepted text revised - aaded new reference to Siegenthaler et al., 2005.
3-679	3	32	34	32	34	Box 3.2: units billion tons of carbon. The previous text uses Pg C. [Leticia Cotrim da Cunha, Germany]	acccepted text revised
3-680	3	32	35			Misspelling: Le Quéré [Jean-Pierre Gattuso, France]	Accepted - text revised
3-681	3	32	39			Always mention the pH scale [Jean-Pierre Gattuso, France]	acccepted text revised
3-682	3	32	39			I believe that the citations should be listed chronologically [Jean-Pierre Gattuso, France]	Editorial - copyedit to be completed prior to publication
3-683	3	32	44		45	It may be worth mentioning temperature and stratification in this context? [Hans Poertner, Germany]	noted.
3-684	3	32	48	32	5	This section suggests suscepibility of biota to ocean acidification, and could bolster this point by noting changes in the biological systems have already been observed consistent with laboratory-inferred sensitivities. See for example: De'ath et al 2009, Declining Coral Calcification on the Great Barrier Reef, Science, 323(5910), 116-119, doi:10.1126/science.1165283; de Moel et al., 2009, Planktic foraminiferal shell thinning in the Arabian Sea due to anthropogenic ocean acidification?, Biogeosciences, 6(9), 1917-1925; Moy, A. D., 2009, Reduced calcification in modern Southern Ocean planktonic foraminifera, Nat. Geosci., 2, 276-280, doi:10.1038/ngeo460. Though this point is perhaps more appropriate for WGII it may be worth making here as, given it's been alluded to. [William Howard, Australia]	Because of space limitations a reference to WGII Chapter 6 is given.
3-685	3	32	48			We have little monitoring data to make a statement like this. [Christopher Kavanagh, Monaco]	The analysis in the Byrne et al (2010) paper is consistent with the HOT time-series results of Dore et al.(2009).
3-686	3	32	49			This explanation comes after the first mention that CO3 decreases (l47). Hence duplication. [Jean-Pierre Gattuso, France]	noted.
3-687	3	32	50			Water carbonate ion content is a good correlate of effects of ocean acidification on calcifiers but does not directly influence calcification as it is not effective at the site of calcification. The carbonate concentration at the site of calcification results from different processes and dissociation equilibria in a compartment different from water. The authors should just say that ocean acidification can affect ocean biology and refer to the WG II assessment. [Hans Poertner, Germany]	Accepted - text revised
3-688	3	32	52	32	55	Surely an exaggerstoin. Reductions in alkalinity are confined only to some regions. After all, parts of the ocean emit carbon diodie and are presumably saturated. Also any changes will favour some organisms over others and provide incentives for evolution. [VINCENT GRAY, NEW ZEALAND]	rejected. Increases in pCO2 and decreases in pH have ben observed throught most of the ocean.
3-689	3	32	52			This seems to be an american-centric selection. Other , non-US, reviews could be mentioned (see below). I know that space is a premium but the chapter would benefit from a more balanced approach to citations. [Jean-Pierre Gattuso, France]	Accepted - text revised and new references provided.
3-690	3	32	52			I believe that the citations should be listed chronologically [Jean-Pierre Gattuso, France]	Editorial - copyedit to be completed prior to publication
3-691	3	32	55			Why? This is not explained in this paragraph. If you are referring to feedbacks, then a few additional words are needed. [Jean-Pierre Gattuso, France]	Accepted - text revised and new references provided.
3-692	3	32	56		57	ditto [Hans Poertner, Germany]	Accepted - text revised and new references provided.
3-693	3	33	1	33	1	Why do seagrasses appear to benefit from ocean acidification? The related issues seems should be discussed in WGII. [Rongshuo Cai, China]	Accepted. We have cited working Group II for more information.

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3-694	3	33	1	33	4	Responses can not be identified by class of organism, but may be at the genus or even species level. [Christopher Kavanagh, Monaco]	Accepted. We have cited working Group II for more information.
3-695	3	33	3	3	4	I feel that Hendriks et al. is a poor reference, but since it is a summary I can see why it is cited. I do not like how this paper arbitrarily decides if an impact is positive or negative, then averaged these completely different impacts together - Given that a lot of people will use this box as a starting point to learning about ocean acidification, and would therefore turn to whatever references are cited here, I think that individual papers that have looked at these things would be better - or failing that Andy Ridgwell's compilation in (http://www.biogeosciences.net/6/2611/2009/bg-6-2611-2009.html), although this is now somewhat out of data. [Paul Halloran, UK]	Accepted. We have cited the Kroeker et al., 2010 paper.
3-696	3	33	7			Another possible, non-US, citation could be Turley and Boot, 2011. An additional benefit is that this reference is general while Cooley's addresses the US only. [Jean-Pierre Gattuso, France]	Accepted - text revised and new references provided.
3-697	3	33	9	33	14	Is there any actual observational evidence to support the models? [VINCENT GRAY, NEW ZEALAND]	The observations are reported throughout this chapter.
3-698	3	33	9			Box 3.2, Figure 1: A reduction in color bar range (eg from 7.9 to 8.4) would highly facilitate the visualization of the decreases in pH levels from one figure to the other. [Mauro Cirano, Brazil]	Accepted. Figure revised.
3-699	3	33	10			See comments made on the figure legend. [Jean-Pierre Gattuso, France]	Accepted - text revised
3-700	3	33	16			Box 3.2, Figure 2: An explanation of this figure should be inserted in the Box 3.2 main text. [Mauro Cirano, Brazil]	Accepted - text revised
3-701	3	33	17			See comments made on the figure legend. [Jean-Pierre Gattuso, France]	Accepted - text revised
3-702	3	33	20			Meters are not units of volume. [David Sauchyn, Canada]	David Sauchyn's comments refer to a different chapter
3-703	3	33	22			It seems the IPCC want the AR5 to be accessible to a “wider community”....those boxes may be read by lay readers... explaining why the saturation lines are important could be useful. [Francois DANIS, France]	Accepted - text revised
3-704	3	33	27	33	28	Include the definition of the red isolines (sigma-t probably) in the caption of Figure 3.18. [Mauro Cirano, Brazil]	Accepted - text revised
3-705	3	33	27		28	This figure seems rather specialized and one wonders what the added value is for this chapter. [Hans Poertner, Germany]	noted.
3-706	3	33	31	34	25	It is impossible to beleive the claimed accuracy of these figures [VINCENT GRAY, NEW ZEALAND]	Both the accuray and the precision of these measurements have been verified.
3-707	3	33	33	33	33	caption of tabl3e 3.2: '... Hydrostation S(32°10',...)' the letter for latitude (N or S) is missing [Reiner Steinfeldt, Germany]	Accepted - text revised
3-708	3	33				Table 3.2: (Typo) Site: coast of western N. Pacific: the rate of pH change is not a positive but a negative value. [Masao Ishii, Japan]	Accepted - text revised
3-709	3	33				Box 3.2, Figure 1: Cannot discern the reefs. [Christopher Kavanagh, Monaco]	Accepted - The figure will be revised
3-710	3	33				Box 3.2, Figure 2: Saturation states in the graphics not discussed in text. [Christopher Kavanagh, Monaco]	Accepted - text revised
3-711	3	33				Figure 3.18 Needs greater explanation. e.g., red isobars. [Christopher Kavanagh, Monaco]	Accepted - text revised
3-712	3	33				Table 3.2 Move to 3-32 line 18. Also, Table 3.2 has saturation state data not described by text. [Christopher Kavanagh, Monaco]	Accepted - text revised
3-713	3	34	3	34	3	legend to table 3.2: 'a Bates' appears twice (in l.2 and l.3, in l.3 it should probably be 'b Bates') [Reiner Steinfeldt, Germany]	Accepted - text revised

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3-714	3	34	19			Reference to Ishii et al (2011) is incomplete, include article number (C06022) and doi (10.1029/2010JC006831) [Mauro Cirano, Brazil]	Accepted - text revised and new references provided.
3-715	3	34	30	36	3	Suggestion: use throughout the manuscript text either “dissolved oxygen” or simply “O2” when referring to the dissolved oxygen concentrations and/or sea-air fluxes. I know that currently we all say simply “oxygen”, but the above notations are more appropriate to a scientific text (“oxygen” could also refer to the element O, and not the gas O2). [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-716	3	34	40	34	43	“Models suggest ……” The references are missing? [Rongshuo Cai, China]	Accepted - text revised and new references provided.
3-717	3	34	40	34	43	This sentence should be confirmed through citation of respective references. [Christoph Heinze, Norway]	References added
3-718	3	34	42			It would be good to include the findings from the recent Helm, Bindoff and Church paper in GRL (2011) here [Paul Halloran, UK]	Accepted - text revised and new references provided.
3-719	3	34	43	34	45	Most oceans show a decreasing trend in dissolved O2, but not all (e.g. parts of the subtropical gyres), see: Stramma, L., S. Schmidtko, L. A.Levin, and G. C. Johnson, 2010, Ocean oxygen minima expansions and their biological impacts, Deep-Sea Research I, 57(2010)587–595. [Christoph Heinze, Norway]	Accepted - text revised and new references provided.
3-720	3	35	1	35	7	I would suggest to be more balanced here. There are regions and water masses that have experienced an increase in O2 (e.g. Labrador Sea Water over the last 50 years, (see e.g. Stendardo and Gruber, submitted, but also regions in the Southern hemisphere) This should be mentioned here as well. [Nicolas Gruber, Switzerland]	Accepted - text revised and newStendardo and Gruber reference provided.
3-721	3	35	1		2	The numbers derived from the work by either Stramma or Keeling (p.34., l. 35) should match or differences be explained [Hans Poertner, Germany]	The numbers are from different locations as explained in the text.
3-722	3	35	2	35	5	A persistent declining trend of dissolved oxygen superimposed on oscillations has also been observed in the subsurface of western subarctic Pacific for the last 40 years (Ono et al., 2001). Ono, T., T. Midorikawa, Y. W. Watanabe, K. Tadokoro, and T. Saino, 2001: Temporal increases of phosphate and apparent oxygen utilization in the subsurface waters of western subarctic Pacific from 1968 to 1998. Geophysical Research Letters, 28, 3285-3288. [Masao Ishii, Japan]	Accepted - text revised and new references provided.
3-723	3	35	12	35	12	spelling of oxygen. [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-724	3	35	12	35	12	"dissolved" and not "dissoved" [Belén Martín Míguez, Spain]	Accepted - text revised
3-725	3	35	16	35	28	Define the term "hypoxic". [Christoph Heinze, Norway]	Accepted - text revised
3-726	3	35	16			Reference to Bograd et al (2008) is incomplete, include issue number (12), article number (L12607) and doi (10.1029/2008GL034185) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-727	3	35	22	35	23	It is a bit vague. Which evidences, for instance? [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-728	3	35	22		23	Can the greater oxygen decline rates in coastal oceans be quantified? [Hans Poertner, Germany]	Accepted - text revised
3-729	3	35	27	35	28	'the increase of primary production and consequent worldwide coastal eutrophication fueled by riverine runoff of fertilizers': the increase of primary production is a consequence of worldwide coastal eutrophication and note vice versa [Reiner Steinfeldt, Germany]	Accepted - text revised
3-730	3	35	28	35	28	Also atmospheric anthropogenic nitrogen deposition has added to the deoxygenation problem (Duce, R.A., J. LaRoche, K. Altieri, K.R. Arrigo, A.R. Baker, D.G. Capone, S. Cornell, F. Dentener, J. Galloway, R.S. Ganeshram, R.J. Geider, T. Jickells, M.M. Kuypers, R. Langlois, P.S. Liss, S.M. Liu, J.J. Middelburg, C.M. Moore, S. Nickovic, A. Oschlies, T. Pedersen, J. Prospero, R. Schlitzer, S. Seitzinger, L.L. Sorensen, M. Uematsu, O. Ulloa, M. Voss, B. Ward, and L. Zamora, 2008, Impacts of Atmospheric Anthropogenic Nitrogen	Accepted - text revised

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						on the Open Ocean, 320, 893-897). [Christoph Heinze, Norway]	
3-731	3	35	37	35	37	Comma before "consistent" [Stephen Griffies, USA]	Accepted - text revised
3-732	3	35	38	35	38	"given the current rate of deoxygenation and Redfield stoichiometry" This argument is unclear. Only the stratification part of the deoxygenation is associated with a change in the nutrient concentration. Was this taken into account here? And where do these numbers come from? [Nicolas Gruber, Switzerland]	accepted sentence removed
3-733	3	35	42			Reference to Rykaczewski and Dunne (2010) is incomplete, include article number (L21606) and doi (10.1029/2010GL045019) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-734	3	35	44	35	45	Use comma instead of semi-colon. [Leticia Cotrim da Cunha, Germany]	Accepted - text revised
3-735	3	35	44			Reference to Cianca et al (2007) is incomplete, include issue number (C7), article number (C07025) and doi (10.1029/2006JC003788) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-736	3	35	45			Reference to Di Lorenzo et al (2009) is incomplete, include article number (L14601) and doi (10.1029/2009GL038261) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-737	3	35	46	35	48	Despite the close coupling between "ocean ecology and climate", the observed changes in the most productive waters in continental shelves cannot be attributed uniquely to climate as other direct factors (e.g. artificial fertilizer release and sewage inputs) are implied (see global and regional reviews as Sherman and Hempel, 2009; Bode et al., 2011). (see complete citations in the comment above) [Antonio Bode, Spain]	Accepted - text revised
3-738	3	35	46	35	48	This section 3.8.4 is a section for nutrients. Thus this description provides me a feeling of strangeness. [AKIHIKO MURATA, Japan]	noted
3-739	3	35	46		48	This text is vague, authors might consider to refer to WGII, chapter 6. [Hans Poertner, Germany]	Reference made to WGII
3-740	3	35	46			Reference to Alvarez et al (2011) is incomplete, include article number (C09016) and doi (10.1029/2010JC006475) [Mauro Cirano, Brazil]	Editorial - copyedit to be completed prior to publication
3-741	3	35	50			Section 3.8.5 does not mention saturation state or carbonate deficiency effects. [Christopher Kavanagh, Monaco]	Accepted - text revised
3-742	3	35	52	35	53	These numbers should be made consistent with chapter 6. [Nicolas Gruber, Switzerland]	Accepted - text revised
3-743	3	35				Figure 3.19 DO and deltaDO appear to have the same definition. The difference in color scheme for the CI is indiscernable, so no need to have it. [Christopher Kavanagh, Monaco]	The Figure will be revised.
3-744	3	36	3	36	3	Though N2O (nitrous oxide) and CH4 (methane) as shorter lived greenhouse gases are discussed in chapter 6 of the WGI report, recent progress in observations of N2O and CH4 in the ocean should be mentioned. Respective climate feedbacks potentially could be larger than those indiced by ocean acidification. Among recommended papers to be cited are: Naqvi, S.W. A, H. W. Bange, L. Farías, P. M. S. Monteiro, M. I. Scranton, and J. Zhang, 2010, Marine hypoxia/anoxia as a source of CH4 and N2O, Biogeosciences, 7, 2159–2190; Wittke, F., A. Kock, and H. W. Bange, 2010, Nitrous oxide emissions from the upwelling area off Mauritania (NW Africa), Geophysical Research Letters, 37, L12601, doi:10.1029/2010GL042442; Bange, H., 2006, Nitrous oxide and methane in European coastal waters, Estuarine, Coastal and Shelf Science 70 (2006) 361-374; Shakhova, N., I. Semiletov, A. Salyuk, V. Yusupov, D. Kosmach, and Ö. Gustafsson3, 2010, Extensive Methane Venting to the Atmosphere from Sediments of the East Siberian Arctic Shelf, Science 327, 1246-1250, DOI: 10.1126/science.1182221 (see also comment by Peternko et al., in Science, 3 Sept. 2010, vol 329, p. 1146 about the natural/human induced controversy) - this paper should be seen in the context of: Dlugokencky, E. J., L. Bruhwiler, J. W. C. White, L. K. Emmons, P. C. Novelli, S. A. Montzka, K. A. Masarie, P. M. Lang, A. M. Crotwell, J. B. Miller, and L. V. Gatti, 2009, Observational constraints on recent increases in the atmospheric CH4 burden, Geophysical Research Letters, 36, L18803, doi:10.1029/2009GL039780. I suggest that CLAs/LAs get into contact to coordinate the description of N2O and CH4 in both chapters.	This is addressed in WGI Chapter 6 as noted by the reviewer.

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						[Christoph Heinze, Norway]	
3-745	3	36	22	36	29	The very large inaccuracies of these lines is not given [VINCENT GRAY, NEW ZEALAND]	This is addressed in Section 3.8.3 (see Keeling et al., 1010 for more details).
3-746	3	36	31	36	48	The "cofidence" of the "experts" who are paid for the work is unsurprising. [VINCENT GRAY, NEW ZEALAND]	noted.
3-747	3	36	36	36	39	Some assessment results of salinity change in the tropic or in the polar are missing? For example, page 15, line 45-48 talk about the precipitation has a stronger increase over the tropical ocean of 0.06 mm day-1 per decade (Gu et al., 2007), how about the related salinity change in the tropical ocean or in the polar regions? [Rongshuo Cai, China]	low latitude salinity change is covered in section 3.3
3-748	3	36	40	36	42	Is this statement really "virtually certain"? It may be so that surface ocean pH and CO3- are functions of surface CO2 to this extent, but restriciting it to "anthropogenic CO2", it appears incredible to consider the relation virtually certain. [Tor Eldevik, Norway]	The estimates of the amount of anthropogenic CO2 in the oceans are in agreement from both observations and models.
3-749	3	36	44		48	This text does not quite fit the argument that the change in solubility plays a major role (p. 34) [Hans Poertner, Germany]	accepted text revised
3-750	3	36	51			This figure nicely integrates the findings of the chapter. [Hans Poertner, Germany]	Noted
3-751	3	37	7	37	12	"The largest changes … are observed along known ventilation pathways", is this correct and/or reflected by the preceeding documentation? For the specific case of the Nordic Seas, the source region of about 2/3 of NADW, I am not convinced, neither from what is presented in the present chapter nor from the literature in general (cf., e.g., the synthesis of available observations, fig 2, in Eldevik et al. 2009, Nature Geoscience, doi: 10.1038/NGEO518). [Tor Eldevik, Norway]	As noted in the text, the North Atlantic is dominated by decadal variability
3-752	3	37	14	37	20	The paragraph or the chapter remained unclear if the major modes of natural climate variability such as NAO, ENSO, SAM and the PDO are greatly influenced by the anthropogenic climate change or not, i.e., the impacts of climate warming due to GHGs. It possible, the paragraph or chapter should give an assessment of impacts of climate warming on the above mentioned natural variability modes. [Rongshuo Cai, China]	A discussion of the response of modes of variability to climate change is beyond the scope of this chapter, but addressed in several other chapters of AR5.
3-753	3	37	20			add Indian Ocean Dipole (IOD) also in the climate modes [Ravichandran Muthalagu, India]	rejected - we mention some modes here, but do not attempt a comprehensive list for which there is not space.
3-754	3	37	22	37	25	Regional variability not mentioned. [Christopher Kavanagh, Monaco]	Rejected - we believe this extra level of detail is not needed in the summary paragraphs.
3-755	3	37	38	38	56	FAQ3.1: This FAQ starts with a well-expressed and easily understandable overall answer. The language for the whole FAQ is generally clear, and appears understandable to a non-expert. [David Wratt, New Zealand]	Noted.
3-756	3	37	40	38	58	It is extremely unclear what period is claimed to be "warming". Signs that this"warming" is coming to an end are ignored [VINCENT GRAY, NEW ZEALAND]	Rejected. Decadal variability is mentioned in the second sentence of the chapeau. The warming periods are clearly stated early in the second paragraph. Also, there is no evidence in the most recent published estimates of ocean heat content (e.g. Levitus et al. 2012, Geophys. Res. Lett.) that this warming is coming to an end.
3-757	3	37	42	37	43	I suggest this "initial summary answer" paragraph be italicised, in line with the standard WG1 FAQ style. [David Wratt, New Zealand]	Accepted - text revised
3-758	3	37				FAQ 3.1: Italicize, and consider expanding the chapeau, eg, to include the patterns of warming. [Thomas Stocker/ WGI TSU, Switzerland]	Italics accepted but chapeau ended with the second sentence to keep it concise.
3-759	3	37				FAQ 3.1: Brief description of 'Argo' needed. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. A parenthetical description of Argo has been addded.

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3-760	3	37				FAQ 3.1: Final statement in (...) should be removed. This seems unnecessary and could be misinterpreted as suggesting land ice is not expected to contribute. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
3-761	3	37				FAQ 3.1: Avoid reference to 'since AR4' which assumes the reader has some familiarity with this report. Better to simply state 'over the past 10 years' for example. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. "over the past several years" and "prior to 2008" are now used.
3-762	3	37				FAQ 3.1, Fig 1: It should be made clear that the water masses in the top left panel are also relevant in the Atlantic. This could be ensured by adding the red arrows in both the lower panels. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Figure revised as suggested.
3-763	3	38	35		39	This text could be moved up to the beginning of FAQ3.1. [Hans Poertner, Germany]	Noted. However, the text in question is left it its place to avoid interrupting the flow of the FAQ from general to more detailed information.
3-764	3	38	38	38	38	Again, when using words like "small" or "large", there needs to be a clear comparison: in this case, deep ocean temperature warming rates are small compared to surface temperature change (but are large when considering total energy heat content change). I suggest being explicit about the comparison, rather than making it implicitly. [Marcus Sarofim, USA]	Accepted. The sentence in equation now reads "Deep warming rates are generally less pronounced than ocean surface rates."
3-765	3	38	48		49	"It takes decades for near surface ocean temperatures to adjust in response to climate forcing such as changes in greenhouse gas concentrations." Reference needed. Seems at variance with numerous calcs with coupled GCM that suggest time constant of about 4-8 years. A rapid initial response to a step-function forcing that exhibits most of the climate system response followed by a further response of lower magnitude and long duration is widely exhibited in coupled climate model runs (Brasseur and Roeckner, 2005; Matthews and Caldeira, 2007; Knutti et al., 2008; Held et al., 2010; Hansen et al., 2011; Knutti and Plattner, 2012). Brasseur GP, Roeckner E (2005) Impact of improved air quality on the future evolution of climate. Geophys Res Lett 32:L23704. doi:10.1029/2005GL023902 Held IM, Winton M, Takahashi K, Delworth T, Zeng F, Vallis GK (2010) Probing the Fast and Slow Components of Global Warming by Returning Abruptly to Preindustrial Forcing. J Climate 23:2418-2427. doi:10.1175/2009JCLI3466.1 Hansen J, Sato M, Kharecha P, von Schuckmann K (2011) Earth's energy imbalance and implications. Atmos. Chem. Phys. 11:13421-13449. doi:10.5194/acp-11-13421-2011 Knutti R, Krähenmann S, Frame DJ, Allen MR (2008) Comment on ''Heat capacity, time constant, and sensitivity of Earth's climate system'' by S. E. Schwartz. J Geophys Res 113:D15103. doi:10.1029/2007JD009473 Knutti R., and G.-K. Plattner, 2012: Comment on "Why Hasn't Earth Warmed as Much as Expected?" by Schwartz et al. 2010. J. Climate. In press, http://dx.doi.org/10.1175/2011JCLI4038.1 Matthews HD, Caldeira K (2007) Transient climate-carbon simulations of planetary geoengineering. Proc Natl Acad Sci USA 104:9949-9954 [Stephen E Schwartz, USA]	Noted. Text changed to "about a decade" which is appropriate in an FAQ for an exponential time constant of 4-8 years. However, there is a longer time-scale surface warming as well in the references quoted here, which is characterized in revision by the text "and slightly more in subsequent decades". References are not permitted in FAQs.
3-766	3	38	48		49	In addition to model studies, there is also empirical determination of the time constant from autocorrelation of temperature anomaly (Schwartz 2007, 2008; Scafetta 2008) that shows the time constant to be about 8-9 years: Scafetta, N. (2008), Comment on ''Heat capacity, time constant, and sensitivity of Earth's climate system'' by S. E.Schwartz, J. Geophys. Res., 113, D15104, doi:10.1029/2007JD009586. Schwartz SE (2007) Heat capacity, time constant, and sensitivity of Earth's climate system. J Geophys Res 112 (D24):D24S05. doi:10.1029/2007JD008746	Noted. Again, this is an FAQ, so a detailed quantitive discussion is not expected and references are not permitted.

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Schwartz SE (2008a) Reply to comments by G. Foster et al., R. Knutti et al., and N. Scafetta on "Heat capacity, time constant, and sensitivity of Earth's climate system". J Geophys Res 113:D15105. doi:10.1029/2008JD009872 Under condition of ramped forcing, expect near surface ocean to be ca 1 time constant lag. If forcing is increasing at say 0.02 W m-2 yr-1, and eq sensivitity is 0.75 K/ (W m-2) (equiv to 3 K for CO2 doubling) then for 6 year time constant, that corresp to lag of 0.09 K. No sense propagating misinformation. " Earth's surface would continue to warm for decades" Needs quant discussion. Given the rapid response of upper ocean, most of committed warming to a given forcing to present, held constant, is already realized. So yes, would continue to rise, but the total additional increase likely much less (ca 10-20%) than already experienced. Further Reference: Schwartz S. E. (2012) Determination of Earth's transient and equilibrium climate sensitivities from observations over the twentieth century: Strong dependence on assumed forcing. Surveys Geophys. In press. http://www.ecd.bnl.gov/steve/pubs/ObsDetClimSensy.pdf [Stephen E Schwartz, USA]	
3-767	3	38	52		54	The contribution by melting ice should be integrated or referred to. [Hans Poertner, Germany]	Noted. The parenthetic statement to ice has been ommited in revision in accord with review comment 3-760
3-768	3	39	1	39	40	Throughout "Reduced alkalinity": should replace :"acidification". You seem to imply that ocean pH is uniform and constant whereas it is highly variabilie and this variability does not seem to have any disastrous effects. The changes mentioned could merely change the size of the upper and lower extreme regions, cause a slight change in numbers of diufferent species and encourage evolution to change to a new mixture. [VINCENT GRAY, NEW ZEALAND]	The biological impacts of changes in pH are a topic for WGII
3-769	3	39	1	39	40	FAQ 3.2: Overall, this is a concise and clearly written FAQ. I've made a few suggestions regarding modifications to particular lines in the text. [David Wratt, New Zealand]	Noted
3-770	3	39	3	39	37	The answer to FAQ "3.2: How Does Anthropogenic Ocean Acidification Relate to Climate Change?" is not clear. I suggest to describe the twofold links with climate (see comments above for Box 3.2 starting on p. 32): (1) Through climate feedbacks induced by pH dependent marine processes. These are: Slight negative feedback due to reduced calcification in the ocean surface (Heinze, C., 2004, Simulating oceanic CaCO3export production in the greenhouse, Geophysical Research Letters 31, L16308; Gehlen, M., R. Gangstø, B. Schneider, L. Bopp, O. Aumont, and C. Ethe, 2007, The fate of pelagic CaCO3 production in a high CO2 ocean: a model study, Biogeosciences, 4, 505–519; Ridgwell, A., I. Zondervan, J.C. Hargreaves, J. Bijma, and T.M. Lenton, 2007, Assessing the potential long-term increase of oceanic fossil fuel CO2 uptake due to CO2-calcification feedback, Biogeosciences, 4, 481–492), potential positive feedback due to reduced ballasting (Armstrong, R.A., C. Lee, and J.I. Hedges, 2002, A new, mechanistic model for organic carbon fluxes in the ocean based on the quantitative association of POC with ballast minerals, Deep-Sea Research Part II, 49(1-3), 219-236; Klaas, C., and D. Archer, 2002, Association of sinking organic matter with various types of mineral ballast in the deep sea: Implications for the rain ratio, Global Biogeochemical Cycles, 16, 1116, 14p.; Heinze, C., 2004, Simulating oceanic CaCO3export production in the greenhouse, Geophysical Research Letters 31, L16308), significant potential feedback possibly due to carbon overconsumption (controversially discussed) (Riebesell, U., K.G. Schulz, R.G.J. Bellerby, M. Botros, P. Fritsche, M. Meyerhöfer, C. Neill, G. Nondal, A. Oschlies, J. Wohlers, and E. Zöllner, 2007, Enhanced biological carbon consumption in a high CO2 ocean, Nature, 450(7169), 545-548), and substantial long-term through dissolution of sedimentary CaCO3 (Archer, D., 2005, Fate of fossil fuel CO2 in geologic time, Journal of Geophysical Res. - Oceans, 110(C9), C09S05). (2) Through acting in concert with other stressors (warming, increased stratification, de-oxygenation) to modify the biological carbon pumps and hence changing global carbon cycling with not yet identified overall feedback strenghth to climate. [Christoph Heinze, Norway]	Rejected. This point was addressed on page 33 lines 3 to 5 as recommended by the reviewer earlier.

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3-771	3	39	15	39	17	I suggest that the susceptibility of marine organisms to ocean acidification is a matter for assessment by WG2, so that this sentence should probably be removed from this WG1 FAQ. [David Wratt, New Zealand]	WGII will address this issue in more detail.
3-772	3	39	15			I suggest that names be added to the chemical formulae in this line, for the benefiit of non-specialist readers. [David Wratt, New Zealand]	The names have been previously described.
3-773	3	39	17			This contradicts the main body of the text which says that the effects are unknown. Also, other review papers could be cited. [Jean-Pierre Gattuso, France]	References are not to be cited in this FAQ. New references, however,have been added to the text as suggested by the reviewer.
3-774	3	39	19			I do not think this statement can be made. pH values near 7.0 can already be measured. It can be mentioned that negative effects can be seen without reaching 7.0, as most marine organisms are currently acclimated to 8.2 Perhaps, this is where the expected rate of change can be mentioned. [Christopher Kavanagh, Monaco]	The statement is correct as it stands here. The reader is directed to the details of the impacts on marine organisms in section 3.8.2 and in WGII Chapter 6.
3-775	3	39	21			FAQ 3.2, Figure 1: An explanation of this figure should be inserted in the FAQ 3.2 main text. [Mauro Cirano, Brazil]	Accepted - text revised
3-776	3	39	22	39	24	I suggest this "initial summary answer" paragraph be italicised, in line with the standard WG1 FAQ style. [David Wratt, New Zealand]	Accepted - text revised
3-777	3	39	26	39	33	An additional impact of ocean acidification is the nontrivial transfer of ammonia from the atmosphere to the ocieans and a smaller transfer of hydrochloric acid, nitric acid, and sulfurous acid from the ocean to the atmosphere (Jacobson, M.Z., Studying ocean acidification with conservative, stable numerical schemes for nonequilibrium air-ocean exchange and ocean equilibrium chemistry, J. Geophys. Res., 110, D07302, doi:10.1029/2004JD005220, 2005) [Mark Z. Jacobson, U.S.A.]	Accepted - text revised
3-778	3	39	26	39	33	Ocean pH between 1751 and 2100 (under the SRES A1B emission scenario after 2004) is estimated to increase H+ by a factor of 2.5 (decreaswe the pH by 0.4 pH units (Jacobson, M.Z., Studying ocean acidification with conservative, stable numerical schemes for nonequilibrium air-ocean exchange and ocean equilibrium chemistry, J. Geophys. Res., 110, D07302, doi:10.1029/2004JD005220, 2005) [Mark Z. Jacobson, U.S.A.]	Accepted - text revised
3-779	3	39	26	39	37	The temperature effect on solubility (K0) is discussed, but not the effect on the dissociation constants of carbonic acid (K1,K2), which have an antagonistic temperature effect to K0. Carbonic acid dissociates better in warm waters, hence warm waters have a lower Revelle factor or buffer factor than cold waters and a better buffer capacity in the theoretical equilibrium limit. The combined effect of changes in solubility and dissociation has to be looked at (resulting in a strongest pH drawdown in cold high latitude waters). I rather suggest to report the difference in ocean acidification and ocean carbon uptake for an addition of CO2 (emission) to the coupled ocean atmosphere system rather than for a consideration of fixed atmospheric pCO2: Define a preindustrial start equilibrium of the carbon system in a coupled 2 box model (atmosphere, ocean; given values for ocean temperature, salinity, and alkalinity; realistic reservoir sizes), then add an amount of CO2 to the system, then compute new equilibria for different temperatures, and finally report values of pCO2, pH, DIC etc. This would normalise the results to a certain amount of emissions. [Christoph Heinze, Norway]	FAQ 3.2 Table 1 takes both of these effects into account.
3-780	3	39	36			FAQ 3.2, Table 1: Maybe add data for current levels? [Christopher Kavanagh, Monaco]	FAQ 3.2, Table 1 is intended to be a model output for a doubling of CO_2. We wanted to keep it simple here.
3-781	3	39	43	40	40	FAQ 3.3: Overall, this is a concise and clearly written FAQ. I suggest consideration of a few editorial matters. In particular, WG1 FAQs are expected to be able to be read stand-alone, so I suggets removal of the references to other FAQs or chapter sections in lines 6,8,11,15,and 19. Also the style is to not include paper references in the body of the text, so I suggest removal of the reference to Durack and Wijffels in line 31 (hopefully this topic, including the reference is covered in the main text of the chapter). [David Wratt, New Zealand]	Editorial
3-782	3	39				FAQ 3.2, Table 1: (Typo) Unit of H+ and so on is “umol kg-1” (“-1” is missing). [Masao Ishii, Japan]	Accepted - text revised
3-783	3	39				FAQ 3.2: Consider adding components from Box 3.2 to strengthen this FAQ. Start the FAQ with briefly	We are trying to keep this as simple as possible to be

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						outlining what ocean acidification is, what has been observed, before moving on to how it is related to climate change, including future projections. [Thomas Stocker/ WGI TSU, Switzerland]	consistent with Dave Hansfords's suggestions.
3-784	3	39				FAQ 3.2; Table 1: Too technical for an FAQ so we suggest removal. Some relevant numbers from this table could be incorporated directly into the text. [Thomas Stocker/ WGI TSU, Switzerland]	We are trying to keep this as simple as possible to be consistent with Dave Hansfords's suggestions.
3-785	3	39				FAQ 3.2, Fig 1: would be strengthened by including observed/inferred changes in carbonate iron concentrations if available, including the saturation concentrations for different carbonate types. [Thomas Stocker/ WGI TSU, Switzerland]	We are trying to keep this as simple as possible to be consistent with Dave Hansfords's suggestions.
3-786	3	39				FAQ 3.3: This FAQ would benefit from including a compelling, supporting figure. Consider a schematic figure based on observations that compliments FAQ 12.2 Figure 1. [Thomas Stocker/ WGI TSU, Switzerland]	accepted a figure is added
3-787	3	40	1	40	3	I suggest this "initial summary answer" paragraph be italicised, in line with the standard WG1 FAQ style. [David Wratt, New Zealand]	Accepted - text revised
3-788	3	40	6			I suggest removal of the reference here to FAQ 12.2, since the standard WG1 FAQ style is for each FAQ to be "stand-alone" without references to other FAQS or chapter text. [David Wratt, New Zealand]	Editorial - fixed.
3-789	3	40	8			I suggest removal of the reference here to Section 2.3 since the standard WG1 FAQ style is for each FAQ to be "stand-alone" without references to other FAQS or chapter text. [David Wratt, New Zealand]	Editorial - fixed.
3-790	3	40	11			I suggest removal of the reference here to Section 12.4.3 since the standard WG1 FAQ style is for each FAQ to be "stand-alone" without references to other FAQS or chapter text. [David Wratt, New Zealand]	Editorial - fixed.
3-791	3	40	15			I suggest removal of the reference here to Section 2.3 since the standard WG1 FAQ style is for each FAQ to be "stand-alone" without references to other FAQS or chapter text. [David Wratt, New Zealand]	Editorial - fixed.
3-792	3	40	17	40	17	Add "been" between "have more" [Stephen Griffies, USA]	Accepted - text revised
3-793	3	40	17			... have been more ... [Andreas Sterl, Netherlands]	Accepted - text revised
3-794	3	40	18	40	18	Please refer to comment 18. [Leticia Cotrim da Cunha, Germany]	Can't find the comment (18 is about the Indian Ocean)
3-795	3	40	19			I suggest removal of the reference here to Section 2.7 since the standard WG1 FAQ style is for each FAQ to be "stand-alone" without references to other FAQS or chapter text. [David Wratt, New Zealand]	Editorial - fixed.
3-796	3	40	21	40	26	Do you need parenthesis in this paragraph? [Leticia Cotrim da Cunha, Germany]	Editorial - delete parentheses.
3-797	3	40	26	40	26	"from in" - here may be a word (or more) missing or one may have to be deleted, depending on the meaning intended [Christoph Heinze, Norway]	Editorial - fixed.
3-798	3	40	26			.... from in ocean .... => .... from in-situ ocean ..... ?? [Andreas Sterl, Netherlands]	Editorial - fixed.
3-799	3	40	41			It looks odd having a reference at the end of this sentence, since this section has been non-referenced up to this point. I would suggest removing it. [Michael Meredith, UK]	Editorial - deleted (see also comment 3-781 and 3-800)
3-800	3	40	41			I suggest removal of the reference here to Durack and Wijffels 2010, since the standard WG1 FAQ style is to not include paper references in the body of the text. [David Wratt, New Zealand]	Editorial - deleted (see also comment 3-781)
3-801	3	41				References: there are cited five "Proceedings of OceanObs'09"-papers. These white papers should be avoided if possible as such communications often have more the appearance of promoting a certain research field/activity/priority, than presenting actual scientific research. [Tor Eldevik, Norway]	Noted - primary sources are cited where possible.
3-802	3	48	24	48	24	Check authors name [Jae Hak Lee, Republic of Korea]	Accepted - text revised
3-803	3	51	14	51	14	Again. A typo here: Woppelmann or, better Wöppelman (not Woeppelmann) [Belén Martín Míguez, Spain]	Accepted - text revised, please note that oe is the official replacement for German o with an Umlaut

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3-804	3	51	28	51	30	Update the reference: "Xue, Y., B. Huang, Z.-Z. Hu, A. Kumar, C. Wen, D. Behringer, and S. Nadiga, 2011: An assessment of oceanic variability in the NCEP Climate Forecast System Reanalysis. Clim. Dyn., 37 (11-12), 2511-2539, DOI: 10.1007/s00382-010-0954-4." [Zeng-Zhen HU, USA]	Editorial - copyedit to be completed prior to publication
3-805	3	51	51	51	51	suggest to add following reference: "Zhu, J., B.Huang, and M. A. Balmased, 2012: An ensemble estimation of the variability of upper-ocean heat content over the tropical Atlantic Ocean with multi-ocean reanalysis products. Clim. Dyn., 10.1007/s00382-011-1189-8." [Zeng-Zhen HU, USA]	Noted. However, this reference is on regional reanalysis products, whereas the assessment focuses on global data-based analyses.
3-806	3	53	1			why is figure restricted to these two studies. Other studies integrating this increase in heat content are Ishii, 2009, Lyman, 2008, Palmer, 2007, Gouretski, 2010, Willis, 2004 The data are tablulated at http://www.ncdc.noaa.gov/bams-state-of-the-climate/2009-time-series/ohc [Stephen E Schwartz, USA]	Accepted. The number of ocean heat content anomaly time-serires in Fig. 3.2 is increased in the revision. All those with long duration are included.
3-807	3	53				Figure 3.1: I would suggest repeating in the text describing figure 3.1.b the year the anomaly data is based on. [Allison Crimmins, United States]	Accepted.
3-808	3	53				Figures general: An indication of uncertainty in many figures is lacking. [Thomas Stocker/ WGI TSU, Switzerland]	accepted uncertainties added
3-809	3	54	1	54	4	Units for UOHCA: maybe use 10e21 J? Maybe add "annual global mean upper ocean…" [Leticia Cotrim da Cunha, Germany]	noted
3-810	3	54				There is potential to much improve Figure 3.2 and the discussion of it in the text by including more time series, which are available from other authors. For example see the BAMS State the Climate reports, which show three time series. Time series could also be displayed based on the work of Gouretski and Resghetti 2010 and Ishii and Kimoto (2009). This would give the reader a better understanding of the uncertainty in our knowledge of upper ocean heat content. [Simon Good, UK]	Accepted. The number of ocean heat content anomaly time-serires in Fig. 3.2 is increased in the revision. All those with long duration are included.
3-811	3	54				Figure 3.2: Showing all current estimates of UOHCA on the figure would give a better idea of the current state of uncertainty regarding the historical record. Pushing the start date back to the 1950s would give a better appreciation of the long-term changes and how they relate to other variables, for example sea-surface temperatures. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. The number of ocean heat content anomaly time-serires in Fig. 3.2 is increased in the revision. The start date has been pushed back to 1950. All the time-series with long duration are included.
3-812	3	54				Figure 3.2: I am puzzled as to why only two estimates of ocean heat content (OHC) change are shown in this figure. Since the AR4 there have been great efforts to bring together a number of observation-based OHC estimates, as documented in Lyman et al (2010) and Palmer et al (2010). It is important that the IPCC recognizes the international effort in this area, and this should be reflected by including as many such time series (with error estimates, if possible) as is practical. There are an additional two OHC estimates that are regularly reported on in the BAMS State of the Climate report - referred to as "PMEL/JPL/JMAR" and "Hadley" (Johnson et al., 2011) that could be included. The Ishii group have also updated analyses (Ishii et al., 2006; Ishii and Kimoto, 2009) that could be included - their work was included in the AR4 Figure 5.1 but is absent in the current chapter draft. References: (i) G. C. Johnson, J. M. Lyman, J. K. Willis, S. Levitus, T. Boyer, J. Antonov, and S. A. Good (2011): Ocean heat content [in .State of the Climate in 2010.]. Bull. Amer. Meteor. Soc., 92 (6), S81-S84.; (ii) Ishii, M. M. Kimoto, K. Sakamoto and S.-I. Iwasaki (2006): Steric sea level changes estimated from historical ocean subsurface temperature and salinity analyses, J. Oceanogr., 62, 155-170. ;(iii) Ishii, M. and M. Kimoto, 2009: Reevaluation of Historical Ocean Heat Content Variations With Time-Varying XBT and MBT Depth Bias Corrections, J. Oceaongr., 65, 287-299. ;(iv) Lyman, J.M., S.A. Good, V.V. Gouretski, M. Ishii, G.C. Johnson, M.D. Palmer, D.M. Smith and J.K. Willis (2010): Robust Warming of the Global Upper Ocean, Nature, 465, 334-337 ; (v) Palmer, M. & Co-Authors. (2010): Future Observations for Monitoring Global Ocean Heat Content in Proceedings of OceanObs'09: Sustained Ocean Observations and Information for Society (Vol. 2), Venice, Italy, 21-25 September 2009, Hall, J., Harrison, D.E. & Stammer, D., Eds., ESA Publication WPP-306, doi:10.5270/OceanObs09.cwp.68. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. The number of ocean heat content anomaly time-serires in Fig. 3.2 is increased in the revision. The start date has been pushed back to 1950. All the time-series with long duration are included.
3-813	3	54				Figure 3.2: I would suggest extending the time period of the figure back to 1950. This would illustrate the	Accepted.

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						growth of error for the Domingues et al. (2008) estimate, that is referred to in the main text (i.e. poor sampling prior to 1970). I think it better to show the estimated changes back to 1950 with error estimates, rather than exclude them. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	
3-814	3	54				As an example for similar problems with several figures, Figure 3.2. is not comprehensible with respect to axes legend and caption, unexplained abbreviations etc. [Hans Poertner, Germany]	Noted. The axis labels are explained in the figure caption.
3-815	3	55				I saw blue when it is said purple in the caption. I do not think I am color-blinded. [Zhaomin Wang, UK]	Noted.
3-816	3	56	1	56	9	Y-axis: Energy change (10e21 J) [Leticia Cotrim da Cunha, Germany]	Noted. ZJ is an SI abbreviation, and is explained in the figure caption.
3-817	3	56				Box 3.1 Figure 1: The uncertainty range looks to be very close to that of the updated Domingues et al. estimate of ocean heat content shown in Figure 3.2. Therefore it does not span the full uncertainty range. If a different estimate of upper ocean heat content was used (e.g. Levitus et al. estimate shown in Figure 3.2) then it could lie outside this uncertainty range. To get a better idea of the uncertainty other estimates of UOHC should also be used. [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Noted. The Domingues et al. (2008) error bars are adopted for Box3.1, as is their estimate of UOHC. This estimate is adopted because its error bars and methodology for infill is assessed to be the best. However, more estimates of UOHC are now included in Figure 3.2, where it can be seen that the Domingues et al. (2008) one standard error envelope overlaps with about 2/3 of the spread of the curves, as would be expected statistically for correctly sized errors.
3-818	3	56				Box 3.1, Figure 1: A reference or references should be provided for this figure. Where do the various estimates of energy storage terms come from? [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. The text of Box 3.1 details from whence the energy storage estimates are obtained. The revised figure caption points the reader to the text.
3-819	3	57	1	57	7	Maybe add a colour bar to the figures? Can't see much of the 0.2 white contours either. [Leticia Cotrim da Cunha, Germany]	Editorial - fixed. Contours are actually black; caption was wrong.
3-820	3	57				Figures, e.g., 3.4, 3.5, 3.10, 3.11, do in general not include the (sub)polar regions, but the regions are very much discussed in the main text of the chapter. This appers contradictory. [Tor Eldevik, Norway]	Accept - subpolar regions are clearly included. Durack and Wijffels maps go to 70S-70N, so we have extended the maps in 3.4, 3.5, 3.10 to that full region.
3-821	3	57				Fig 3.4: "Contours plotted in Black". Not clear. Need colour bar. [Thomas Stocker/ WGI TSU, Switzerland]	Editorial - fixed
3-822	3	58	1	58	8	Y-axis units? [Leticia Cotrim da Cunha, Germany]	Editorial - to be fixed on figure
3-823	3	58	8	58	8	(thin lines)' needs to be inserted after 'confidence intervals'. [Zhaomin Wang, UK]	Editorial - fixed
3-824	3	58				Fig. 3.5 The units at the x-and y-axis are missing (the units appear in the legend, but I would suggest to add them at the axes) [Reiner Steinfeldt, Germany]	Editorial - to be fixed on figure
3-825	3	58				This figure needs improvement: the dotted lines are hardly visible, and in the second panel the lines for the different basins are hard to distinguish. Make one panel per basin, or leave out error bounds. [Andreas Sterl, Netherlands]	Accept. Figure has been edited for clarity.
3-826	3	59	1	59	5	Maybe organise figure differently. For example, explain that 1960-2010 data comes from NCEP + ERA40, and that data from mid-eighties includes satellite data too. Is this data combination valid for the three panels? Or only for the left-hand one? [Leticia Cotrim da Cunha, Germany]	taken into account figure and figure caption revised
3-827	3	59	4	59	5	Fig. 3.6 caption. Not sure about citing Yu (2007) for a plot that includes data up to 2010. Perhaps some details about the updating are needed or perhaps there is a more up to date reference. [Terrence Joyce, USA]	taken into account. Adedd to caption: Time series has been updated to 2010 by Yu following the method descibed in Yu (2007).
3-828	3	59				range in rightmost figure much too large - (-15 ... -5) would suffice [Andreas Sterl, Netherlands]	Rejected: Range in rightmost figure has been set at 30 Wm-2 to allow direct comparison with the central

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							latent heat figure.
3-829	3	59				I suggest to plot global averaged SST together with those variables, in order to have a comparison and an evaluation. [Zhaomin Wang, UK]	Rejected. The global average fluxes depend on a number of variables: SST, air temp, air humidity, wind speed. Further, the global mean fluxes may not scale directly with the global means of these variables. Thus, a comparison and evaluation agaisnt these variables would require significant research effort and such a study is not available in the literature. Hence, it is not appropriate to extend the figure as suggested.
3-830	3	60				(figures 3.7) pag. 60 in the figure the reference of Smith et al. is 2009 and not 2010; [VINCENZO ARTALE, ITALY]	Editorial
3-831	3	61	1	6	61	Graphic resolution could improve for the final document. [Leticia Cotrim da Cunha, Germany]	Editorial - copyedit to be completed prior to publication
3-832	3	61	6	61	6	change "Yan et al. (2010)" to "Yan et al. (2011)" [Zeng-Zhen HU, USA]	Editorial - copyedit to be completed prior to publication
3-833	3	61				A line for ERA-interim should be added! [Andreas Sterl, Netherlands]	Noted. There are a range of other reanalysis datasets that could be added to this figure including ERA-interim. However, our preference at this stage is to retain the figure as it appears in the paper of Xue et al. It may be revised further depending on publications that appear before the March 2013 final deadline.
3-834	3	61				Fig 3.8: There is an apparent offset of NCEP-DOE which requires some explanation. [Thomas Stocker/ WGI TSU, Switzerland]	Noted. The offset of NCEP-DOE is not discussed in the Xue et al paper but probably reflects a difference in drag coefficient scheme betwen reanalyses.
3-835	3	63	1	63	12	Improve figure: put the names of the ocean basins outside the panels, add the units and parametre names to the right-hand colour bar. The same applies to the global plots. [Leticia Cotrim da Cunha, Germany]	noted figure and figure caption revised
3-836	3	64	1	64	8	Units and parametre name should appear close tot the colour bar. Maybe use a blue-red pallete? [Leticia Cotrim da Cunha, Germany]	noted
3-837	3	65	1	65	16	Please number the red, black, and blue lines (1,2,3). [Leticia Cotrim da Cunha, Germany]	rejceted. There is no need to number them
3-838	3	65	5	65	5	Here in the caption, AMOC time series are presented, so I suggest to use 'across the Atlantic basin'. [Zhaomin Wang, UK]	rejected. AMOC is defind in the text
3-839	3	65		65		Figure 3.12: The references for each observational dataset should be added to figure caption. [Chris Roberts, Uk]	accepted, references included in figure caption
3-840	3	66	1	66	5	Re-organise the figure: map separeted from the plots, improve graphic resolution for the final document. [Leticia Cotrim da Cunha, Germany]	Editorial - copyedit to be completed prior to publication
3-841	3	66				Fig 3.13: Why are these seven sites selected? Are these the only long term records? Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	accepted text and figure caption revised
3-842	3	67	1	67	13	Improve graphic resolution for the final document. [Leticia Cotrim da Cunha, Germany]	Editorial - copyedit to be completed prior to publication
3-843	3	67	1	67	14	It might be useful to have a "rate of change" graph - see, for example, some of the chapter 2 concentration graphs for CO2/CH4: it enables visual detection of changes in rates of sea level rise in a way that a graph of mean sea level over time does not… [Marcus Sarofim, USA]	Taken into account. A new figure showing 18-year rates of change will be added and discussed in the text.
3-844	3	67				Fig 3.14: X and Y axis changes from panel to panel. It would therefore be useful to graphically depict the shifting time horizon considered in each plot. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account figure and figure caption revised
3-845	3	68	1	68	7	Units and parametre name close to the colour bar. [Leticia Cotrim da Cunha, Germany]	Editorial - copyedit to be completed prior to publication

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3-846	3	68				Figure 3.15: I would appreciate more explanation of this figure, and details as to why the Gulf Stream area (and Mediterranean) are so much higher than surrounding seas. [Allison Crimmins, United States]	noted. Discussion see text Chapter 3.8
3-847	3	69	1	69	13	Please refer to comment 83. [Leticia Cotrim da Cunha, Germany]	not clear what this comment is referring to
3-848	3	69				Figure 3.16, Bottom: Using the same scale for ocean Cant storage rate (vertical axis) would be better to help readers quickly recognize the difference among oceans. [Masao Ishii, Japan]	noted. Figure redrawn
3-849	3	69				Fig. 3.16 top figure: the scale of the colorbar is different for each of the three figures; this difference should be mentioned in the caption; besides, I would sugest to use the same color scale at least for the Indic and Pacific [Reiner Steinfeldt, Germany]	Accepted Figure redrawn
3-850	3	70	1	70	12	Take out the grid lines from the 3 panels, maybe use a smaller symbol to improve figure readability. [Leticia Cotrim da Cunha, Germany]	Editorial - copyedit to be completed prior to publication
3-851	3	71	1	71	8	the difference between the dark and light grey areas are not very easy to see. Maybe improving the graphic resolution would help. Please add name of parametre (pH) to the colour bar. Would it be useful to mention if you are using the pH total scale, like in the next figure? [Leticia Cotrim da Cunha, Germany]	accepted figure redrawn, color scale changed
3-852	3	71	1			The pH scale should be mentioned. [Jean-Pierre Gattuso, France]	accepted
3-853	3	71	4			Note that the locations of coral reefs and deep coral communities are not legible. [Jean-Pierre Gattuso, France]	taken into account figure redrawn
3-854	3	71	4			The lowest pH values of the pH scale are not used in the figure. The pH scale could therefore be restricted in order to better illustrate the pH differences. [Jean-Pierre Gattuso, France]	taken into account figure redrawn
3-855	3	71	6			Coral reefs are, by definition, navigational hazards. There are no deep-water coral reefs. These should be called bioherms or deep-coral communities. [Jean-Pierre Gattuso, France]	rejected. Coral reefs are habitats where corals live.
3-856	3	71				Box 3.2, Fig 1: Colour bar should be shortened to the necessary range. Coral reefs are not visible. [Thomas Stocker/ WGI TSU, Switzerland]	taken into account figure redrawn
3-857	3	72	1			revise µmol/kg in order to use negative exponent. [Jean-Pierre Gattuso, France]	rejected
3-858	3	72	1			What do the envelopes show? [Jean-Pierre Gattuso, France]	accepted. Figure caption revised
3-859	3	72	4			A and B are used in the figure while a and b are used in the legend. [Jean-Pierre Gattuso, France]	accepted figure caption revised
3-860	3	72	6	72	6	Should "to panel" be "top panel" ?? [Bogi Hansen, Faroe Islands]	Accepted - text revised
3-861	3	72	7			“to” should read "top" [Jean-Pierre Gattuso, France]	Accepted - text revised
3-862	3	73	1	73	5	Please add parametre name to the colour bar. What are the red lines? What is the dashed green line? [Leticia Cotrim da Cunha, Germany]	taken into account figure redrawn
3-863	3	73	4			The legend should explain what the red lines and the dotted green line are. [Jean-Pierre Gattuso, France]	accepted figure caption revised
3-864	3	73	6			what are those red lines and the green line? [Francois DANIS, France]	accepted figure caption revised
3-865	3	73				Fig. 3.18 The color bar encompasses a much broader range of delta-ph-values than is shown in the figure [Reiner Steinfeldt, Germany]	taken into account figure redrawn
3-866	3	73				Fig 3.18: Caption to be expanded, eg, what does the green dashed line indicate? [Thomas Stocker/ WGI TSU, Switzerland]	accepted figure caption revised
3-867	3	74	1	74	8	Please refer to comment 83. [Leticia Cotrim da Cunha, Germany]	not clear what this refers to

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3-868	3	74	6			apparent O2 utilization. For me, utilization means some organisms or some chemistry is using the O2... and in the text you mention mainly temperature/solubility and stratification... It will be good to change the word “utilization”; “deficit”? May be also you could lead the lay reader further: O2 deficit = decrease in absolute O2 measured and decrease in O2 solubility (because of T and salinity)? [Francois DANIS, France]	oxygen utilisation is the term commonly used in the literature, but text has been revised to improve clarity
3-869	3	74				Fig 3.19: AOU is not referred to in the caption so further explanation is needed as to the significance of including AOU. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted - text revised
3-870	3	75				Fig 3.20: We do not find this version of the figure to be particularly compelling or quantitative. Could this information simply be put into a table or given directly in the text. [Thomas Stocker/ WGI TSU, Switzerland]	Accept partially. Figure 3.20 has been improved.
3-871	3	76	1			This is a great figure that summarizes a lot of information. Note that the legend does not indicate what - and -- mean. I assume leels of confidence as + and ++. [Jean-Pierre Gattuso, France]	Accept. Figure 3.21 has been edited for clarity.
3-872	3	77	11	77	11	Should "only Southern Hemisphere shown, but Northern Hemisphere similar" be "only Pacific shown, but Atlantic similar" ?? [Bogi Hansen, Faroe Islands]	noted figure redrawn
3-873	3	77				FAQ 3.1, Figure 1. Make it explicitly clear to the uninitiated that "warming with time" relates to climatic trends and not to the temperature changes that would befall these water masses on their journeys even if the climate were unchanging. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - text revised
3-874	3	78	1			Chemists seem to recommend the use of CT and AT (see Dickson et al., 2007). [Jean-Pierre Gattuso, France]	noted
3-875	3	78	1			Is this figure useful? The bottom part duplicates what is already shown on fig. 3.17. [Jean-Pierre Gattuso, France]	We believe the figure is useful, and it has been revised to make it more effective
3-876	3	78	1			It seems that there are two pH data at some time points: one obtained from calculation based on CT and AT and one corresponding to direct measurements. If that is correct, the regression line is biased as the direct measurements are not distributed evenly as a function of time. [Jean-Pierre Gattuso, France]	noted