

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1	3	0	0	0	0	Overall the chapter has captured the main areas of progress since AR4 and has used accurate likelihood estimates. However the authors use words like 'no detection of a trend' for several very noisy variables where we have no observational capability (and are never likely to) to detect the small expected trend. Clarity could be improved for those cases. [Government of Australia]	noted. Ch3t has been edited, clarified, and figures have been revised and added
3-2	3	0	0	0	0	Statements on the rate of sea level rise in the Executive Summary of this Chapter differ from those in the Executive Summary of Chapter 13 (Sea level Change). While this is due to the level of likelihood given to the two different sets of numbers, most non-technical readers will not understand this and it will cause much confusion. Unless absolutely necessary, consistency of such important policy statements across Chapters would be preferable (noting that the SPM uses the numbers of this Chapter 3). [Government of Australia]	Accepted - text has been changed to use 90% confidence in line with Chapter 13.
3-3	3	0	0	0	0	Some clarity is needed as to what the 'Southern Ocean' refers to in this chapter. At least 3 definitions are in use: south of 60 S, south of the Antarctic Convergence, and extending northwards to the southern coast of Australia (http://www.hydro.gov.au/factsheets/WFS_Names_and_Limits_of_Oceans_and_Seas_Around_Australia.pdf). Some parts of the chapter appear to use the term rather loosely. [Government of Australia]	accepted. In Ch3, the definition used in oceanography is used (south of the subtropical front)
3-4	3	0	0	0	0	Sea level research is extensively reviewed in this chapter, however, it is not clear to what extent the material will be repeated in chapter 13. Some space could be saved to cover broader oceanographic topics. [Government of Australia]	Noted - Chapter 3 discusses ocean observations of sea level. Chapter 13 discusses processes at combines observations from the cryosphere and paleo-records, and Ch5 summarizes sea level changes in geological time scales.
3-5	3	0	0	0	0	On temperature change, it is worth stressing that in studies that do not use XBTs (e.g. Roemmich et al, 2012; Durack and Wijffels 2010, Levitus et al, if you compare OHC for pre1970 vs Argo era), ocean warming is evident and the 50 year change pattern is very similar to that in Figure 3.1b. Thus we have evidence and estimates of the subsurface warming rate and pattern independent of XBT bias issues. While XBTs are needed to track the detailed temporal pathway from the 1950s/60s to the present, the key patterns and warming rates are robust to their use. Reference to Figure 3.9c could be made. [Government of Australia]	Accepted. This fact is now noted in Section 3.2
3-6	3	0	0	0	0	<p>According to recent analysis, the global averaged surface temperature and global upper ocean heat content show little increase or even negative trend [Easterling and Wehner, 2009; Katsman and Oldenborgh, 2011; Meehl, et al. 2011]. Although the phenomena were only observed in last decade (2000-2012), it is one of the main observed results since the implementation of Argo program, which uses most advanced technology to observe the in-situ temperature of upper ocean from surface to 2000m depth.</p> <p>We have also found that globally-averaged temperature difference between the ocean surface and 200m depth in Figure 3.1d and global upper ocean heat content in Figure 3.2 in the report also shows little increase or even negative trend after 2000.</p> <p>Therefore, it is necessary to mention these phenomena from observations in the report. We suggest to make the following modification:</p> <p>Line 15 of page 3 in Chapter 3, after "from 1971–2010", please add the following sentence: It is likely that upper ocean heat content shows little increase or even negative trend since 2000, which is partly because of the El Nino variability on decadal time scales, and partly due to a decrease in the strength of Atlantic meridional overturning circulation.</p> <p>Line 6 of Page 9, Chapter 3: After "as expected", add the following sentences" Some analysis revealed the upper ocean heat content during 2000-2009 shows little increase or even negative trend [Easterling and Wehner, 2009; Katsman and Oldenborgh, 2011], which is partly because of the El Nino variability on decadal time scales, and partly due to a decrease in the strength of Atlantic meridional overturning circulation [Katsman and Oldenborgh, 2011]".</p> <p>Line 1 of Page 64, Chapter 3: The color of lines used in Figure 3.2 should be modified in order to show the results of hiatus period of upper ocean heat content since 2000.</p> <p>Line 1 of Page 76, Chapter 3 : The different vertical scales of Figure b) and d) should be highlighted. A sentence could be added in the Figure caption in line 11. Notice the different vertical scales used for each plot. Also, on Line 31, Page 5, TS, after {Box 3.1}., add the following sentence: It is likely that upper ocean heat content shows little increase or even negative trend since 2000, which is partly because of the El Nino</p>	Partly accepted. Section 3.2.4 now notes that while globally averaged ocean heat content in some of the 0–700 m estimates appears to be increasing more slowly from 2003–2010 than over the previous decade (Figure 3.2a), ocean heat uptake from 700–2000 m continues unabated during the more recent period (Figure 3.2b).

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						<p>variability on decadal time scales, and partly due to a decrease in the strength of Atlantic meridional overturning circulation.</p> <p>Reference: Easterling, D. R., and M. F. Wehner (2009), Is the climate warming or cooling? <i>Geophys. Res. Lett.</i> 36, L08706. Meehl, G. A., J. M. Arblaster, J. T. Fasullo, A. X. Hu, and K. E. Trenberth (2011), Model-based evidence of deep-ocean heat uptake during surface-temperature hiatus periods, <i>Nature Clim.</i> 1, doi: 10.1038/NCLIMATE1229, 360-364. Katsman, C. A., and G. J. van Oldenborgh (2011), Tracing the upper ocean's "missing heat", <i>Geophys. Res. Lett.</i>, 38, L14610, doi:10.1029/2011GL048417. [Government of China]</p>	
3-7	3	0	0	0	0	<p>The link between decadal variability and trends needs to be better assessed throughout the chapter. What can we say about the low rate of warming since 2000 from ocean data? Is it consistent with observations, and can it be explained. When decadal variability is mentioned, can it be explained in the context of known information (e.g. multi-decadal variability estimated from coral d18O? The chapter mentioned a lot of variability in the ocean, but does not point out when or why. The introduction could explain what we know about the cold and warm periods since 1900, and sections can then refer to the introduction to explain various phenomenon. [European Union]</p>	<p>Rejected. Coral d18O is the remit of paleo-oceanography, and this chapter is focused on non-proxy temperature measures.</p>
3-8	3	0	0	0	0	<p>The link between decadal variability and trends needs to be better assessed throughout the chapter. What can we say about the low rate of warming since 2000 from ocean data? Is it consistent with observations, and can it be explained. When decadal variability is mentioned, can it be explained in the context of known information (e.g. multi-decadal variability estimated from coral d18O? The chapter mentioned a lot of variability in the ocean, but does not point out when or why. The introduction could explain what we know about the cold and warm periods since 1900, and sections can then refer to the introduction to explain various phenomenon. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]</p>	<p>Rejected. Coral d18O is the remit of paleo-oceanography, and this chapter is focused on non-proxy temperature measures.</p>
3-9	3	0	0			<p>A better explanation of how CO2 causes the ocean acidification would be welcome, including chemical reaction formulas explaining this system. [Line van Kesteren, the Netherlands]</p>	<p>accepted, FAQ 3.2 now includes chemical reaction formulas; section 3.8.2 and Box 3.2 have been revised</p>
3-10	3	0	1			<p>Consistency in assessment numbers: Because chapter assessments continue to be refined, please check carefully all values (and the uncertainty ranges) carefully between tables, figures, main text, and summary text within your chapter. If numbers are taken from other chapters, please also ensure the latest results are used. Specific examples will be highlighted in our chapter comments. [Thomas Stocker/ WGI TSU, Switzerland]</p>	<p>accepted text and numbers checked and if applicable updated</p>
3-11	3	0	2			<p>Treatment of Uncertainty: please follow the IPCC guidance note carefully; use italics to highlight formal uncertainty assessments; use likelihood in conjunction with high/very high confidence only (except in exceptional cases); if likelihood is given for situations where confidence is less than 'high', we recommend to put confidence in brackets at the end of the sentence rather than combining both confidence and likelihood in text. Please note - usage of the formal terms from the uncertainty guidance note, (egg. "likely", "confidence" etc) should be restricted to the use within statements which report assessment findings. [Thomas Stocker/ WGI TSU, Switzerland]</p>	<p>accepted uncertainty statement checked and modified accordingly. Footnotes from TSU explaining likelihood and confidence levels added, use of "confidence" and "likely" restricted to its use as calibrated language</p>
3-12	3	0	3			<p>Format of Executive Summary (ES): As agreed at the third lead author meeting, we would ask that all chapters follow a consistent style for the ES. 1) The first sentence (or two) of each paragraph should be bolded to highlight the key message, with the subsequent sentences providing the detailed quantitative assessment. 2) Statements should incorporate the IPCC Uncertainty Language 3) Each paragraph must include a traceability to the underlying sections/subsections where the key message was drawn from (to the second level section heading), indicated using square brackets at the end of each paragraph. 3) Paragraphs should be grouped together under subtitles. The use of bullets should be avoided. 4) Finally, because the ES should be short and concise, lengthy textbook or chapeau type introductory text should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]</p>	<p>accepted traceability of exec summary statements checked, subtitles to exec summary added.</p>
3-13	3	0	4			<p>Cross-chapter references AR5: suggest to update cross-chapter references to not just refer to Chapter number but to refer to specific section if appropriate. [Thomas Stocker/ WGI TSU, Switzerland]</p>	<p>accepted subsection added if appropriate</p>

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-14	3	0	5			References to AR4 and earlier IPCC assessments: be as specific as possible. Writing just AR4 without any reference is not useful to the reader. Please refer to specific chapter where possible. [Thomas Stocker/ WGI TSU, Switzerland]	accepted subsections added if appropriate
3-15	3	0	6			Use of acronyms: In order to improve overall readability of the report, we would like to suggest that you please avoid acronyms that are not needed and/or are not used in more than one section of your chapter. [Thomas Stocker/ WGI TSU, Switzerland]	accepted text checked and acronyms that are not needed removed
3-16	3	0	7			Personal pronouns: our strong preference is to minimize the usage of personal pronouns, e.g., we/us/our to the extent possible. Exceptions to this would be when the Chapter's assessments conclusions are presented as clear summary statements. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Ch3 does not use personal pronouns
3-17	3	0	8			Please make sure to provide updates of relevant data from your chapter that will be collected in Annex II - Climate System Scenario Tables, to the Annex II Chair. Also, please take the time to critically check all the entries in Annex II that are based on your Chapter assessment or that you are using in your chapter assessment. [Thomas Stocker/ WGI TSU, Switzerland]	noted
3-18	3	0				general comments, I found a strong improvement from the FOD, but one more effort have to put on the conclusion of each sections, these sections can be an opportunity for the discussion on the mechanisms and processes that could be changed as consequence of the observed ocean variability and change, otherwise the risk is that each chapter will be very difficult to follow and read easily, becoming a long list of numbers without capture the deep physical meaning of the variability that we are observing [VINCENZO ARTALE, ITALY]	accepted Ch3 clarified the statements
3-19	3	0				Even when the text has greatly improved from the first version still 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. For instance, almost half of the biological productivity of the ocean depend on shelf waters, a disproportionately low fraction of the total ocean. [Antonio Bode, Spain]	rejected. Coastal waters are not subject of Ch3, this topic will be covered in WG II
3-20	3	0				I have only reviewed the section and box on ocean acidification. They are excellent overall. [Jean-Pierre Gattuso, France]	noted. Thank you
3-21	3	0				The citations are not always ordered chronologically [Jean-Pierre Gattuso, France]	Editorial - will correct.
3-22	3	0				One may wonder about the slope of the satellite ENVISAT data which have been multiplied in April 2012 from 0.5 mm per year to 2.3 mm/year, a factor 4.6 ! How to convince of the reliability of the new slope which intrinsically would imply an enormous error on the preceding one ? Anyway, on the website ftp://ftp.aviso.oceanobs.com/pub/oceano/AVISO/indicators/msl/ , it is instructive to consider the file MSL_Serie_MERGED_Global_IB_RWT_NoGIA_NoAdjust.txt, which is the average of altimeter sea level data. A simple linear regression to fit the 1993-2006 data gives a slope of 3.2 mm per year for the sea level rise, which is consistent with the conclusions of this chapter. However, note that the slope is reduced to 2 mm per year in the period 2006-2012 and even to 1 mm per year in the period 2008-2012, viz. less than during the 20th century. Please check. I suggest addressing and discussing in AR5 the deceleration of the sea level rise observed recently, because it is inconsistent with the model projections. [François Gervais, France]	Rejected - there has been no scaling of the ENVISAT data in May 2012 that the Lead Authors are aware of, and there is no note on the AVISO web-site regarding this.
3-23	3	0				The long-term studies cited in chapter 3 need to make various assumptions about a sometimes poorly known seasonal cycle of temperature, salinity, heat content or dynamic height. To get around the problem of computing reliable seasonal cycles, Freeland and Gilbert (2009, Atmosphere-Ocean, 47: 292-298) estimated the steric contribution to global sea level rise from a comparison of the WOCE one-time survey with 2006–2008 Argo Observations. All comparisons between the two datasets were made at the WOCE locations and yeardays of data collection by interpolating Argo temperature and salinity data to exactly the same locations and yeardays, thus eliminating the need to compute seasonal cycles. Section 3.7.2 might be an appropriate section to introduce the findings of Freeland and Gilbert (2009), who estimated a steric component to global sea level rise of 2.2 mm per year. [Denis Gilbert, Canada]	Taken into account - all studies do account for seasonal variations in climatology, although each uses different estimates. This is covered in Section 3.2. Ch 3.7 also adds a statement: "Comparison of sparse but accurate temperature measurements from the World Ocean Circulation Experiment in the 1990s with Argo data from 2006–2008 also indicates a significant rise in global thermosteric sea level, although the estimate is uncertain owing to relatively sparse 1990s sampling (Freeland and Gilbert, 2009). "
3-24	3	0				The role of data assimilation products varies between the different sub-sections. For some quantities (e.g.,	rejected. The assessment of ocean temperature and

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						ocean heat content, 3.2.1, p. 3-7, line 32) observations are compared against results from data assimilation. I have found no systematic description, when data assimilation products are considered and when not. It might make sense to discuss this in the introduction once, as most references to data assimilation do mention that the results differ between the different products. Possibly references could be added, though I most obviously see the need for integrated quantities (such as the AMOC, 3.6.3, e.g. Munoz et al., 2010). [European Union]	heat in Ch3 is based solely on observations.
3-25	3	0				A link with Chapter 13 may be shown (for instance 13.7 on extreme sea level). [Government of India]	Taken into account - Section 3.7 includes relevant links to Chapter 13.
3-26	3	0				The authors mention the changes in surges due to inter-annual variability. There are studies which point out the changes in surges with multi-decadal oscillations such as AMO (see Park et al. (2011) Climatic Change (2011) 107:109–128 DOI 10.1007/s10584-011-0079-8). [Government of India]	Accepted - reference added.
3-27	3	0				The information contained in the Likelihood table (Table 1.1) and Confidence figure (Figure 1.12) is critical to interpreting conclusions throughout the document. This information should be repeated in the front of the SPM, the TS and each Chapter and the terminology should be applied consistently. [Government of United States of America]	accepted Ch3 includes in the exec summary footnotes explaining likelihood and uncertainty levels
3-28	3	0				The Authors should ensure that the terms discussed in this chapter are defined in the glossary. If not, they should consider a more elaborate description in the text (sentence or two). There were multiple reviewer comments about the need to define terms [Government of United States of America]	accepted terms used in Ch3 are checked against the glossary
3-29	3	0				It could be helpful to explain that satellites were the reason that observations improved in 1971 [Government of United States of America]	Rejected. The amount of temperature observations increased after 1967 owing to the introduction of the expendable bathythermograph (XBT). This fact is discussed in Section 3.2.3
3-30	3	0				There are only passing comments about ENSO. If the intent is to rely on discussions elsewhere in the document, consider making reference to those locations. For example, section 9.5.3.4.1, chapter 13, section 14.4. [Government of United States of America]	noted. Ch2 added a box 2.5 summarizing informations about modes and the definition of the indices. Ch3 refers to this box if appropriate
3-31	3	0				The Executive Summary is useful but there should be more definitions of technical terms in the text. [Government of United Kingdom of Great Britain & Northern Ireland]	accepted. Ch3 added footnotes in the exec summary to explain terms
3-32	3	0				This chapter presents the findings clearly and has a logical, well presented structure, including conclusions for each main section and a final synthesis summary. A good, reader-friendly format is used in the Executive Summary, with key points in bold and cross referencing to the relevant parts of the chapter for further detail. The chapter also includes some useful referencing to other chapters eg 2 and 4, which helps to illustrate important cross linkages between a range of atmosphere, ocean and cryosphere observations. The three FAQs cover a suitable range of key topics relating to ocean observations and current changes to ocean conditions. [Government of United Kingdom of Great Britain & Northern Ireland]	noted thank you
3-33	3	0				I would reduce the size of the sections on fluxes in this chapter and replace it would more figures documenting the variability of ocean heat content (OHC) which I believe should be the main thrust of this chapter. Estimates of OHC are one of the most important indicators of climate change and one of the most important quantities for assessing future climate. This is not just my view but the view of many others who study earth's climate system. The chapter reviews too much material that is not important because of great uncertainties in our knowledge. The chapter reads like a textbook and not an assessment in some sections. [Sydney Levitus, U.S.A.]	Partly accepted. An additional panel of deep (700–2000 m and 2000 - 6000 m) ocean heat content estimates has been added to Figure 3.2. Already figures 3.1, 3.2, and 3.3 are completely devoted to ocean temperature and heat content changes. They are also prominently featured in Box 3.1 Figure 1, with Figure 3.9 having another view. They are also prominently featured in synthesis Figures 3.20 and 3.21, as well as in FAQ 3.1, Figure 1.
3-34	3	0				The IPCC emphasizes linear trends. It should also emphasize the per cent variance accounted for by linear trends. Chapter 3 neglects the per cent variance accounted for by linear trends. I recommend rectifying this in the next draft. [Sydney Levitus, U.S.A.]	Rejected. Chapter 3 instead computes the uncertainties of these linear trends, as specified in the text. This is the IPCC convention.
3-35	3	0				Some general concerns with this Chapter as it stands include: 1) When referring to changes in a given variable it is important, in all cases, to provide the time period over which the change has been identified (e.g.	noted. The time periods are clarified, and according to IPCC guidelines, the 90% confidence level is used

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						1990-2005 rather than just "since the 1990s"); 2) There is a lack of consistency in terms of whether changes discussed are significant or not - ideally significant changes should be identified; 3) Following on from that point, where significance levels are provided there is a lack of consistency in level used (e.g. 90% confidence level in Fig 3.9 but 99% used in Fig 3.4). [Janice Lough, Australia]	
3-36	3	0				Figures are more attractive and informative, thanks for the effort [Belén Martín Míguez, Spain]	noted. Thank you. For the final draft many figures have been improved and some figures have been extended
3-37	3	0				[No comment - merely entry on blank line to ensure that processing doesn't abort at this point.] [John McLean, Australia]	noted
3-38	3	0				Given its range and scope I have not reviewed the whole of this chapter but have focussed on those parts that concern most directly my field of expertise - the oceanography of the North Atlantic - although I have looked a bit wider than that in places. Overall I believe that the report is both comprehensive and generally presents a compelling case for anthropogenic climate change. I have some thoughts of both a specific and a general nature. [Toby Sherwin, United Kingdom]	noted
3-39	3	0				I believe that the chapter would benefit from a strong editorial review to make it more accessible and consistent. Compared with Chapter 1 this chapter seems to lack a clear narrative structure and I felt that the authors weren't entirely sure who they are writing it for - is it for their peers, government scientific advisors or the educated layman? There is also a clear difference in style between sections with some contributions reading more like a literature review of observations than an expert overview. In my view some of the sections are very good, such as section 3.3 on sea level rise which has a nice expansion of the problems of making accurate measurements of sea level change. On the other hand section 3.6 about ocean circulation largely overlooks such considerations. I also think that Section 3.2 on ocean heat content is weak, with some poorly structured sentences (see below). [Toby Sherwin, United Kingdom]	Noted. Section 3.6.1 summarizes global observations of ocean circulation.
3-40	3	0				In Annex III the concept of ventilation is defined as "The exchange of ocean properties with the atmospheric surface layer such that property concentrations are brought closer to equilibrium values with the atmosphere (AMS, 2000)." However, this definition does not give the sense in which "ventilation" is used in Chapter 3. Here it is used to describe "The refreshing of deep water at high latitudes by downwelling surface waters that have become dense by heat loss to the atmosphere" This qualification to the AMS definition needs to be added to Annex III. [Toby Sherwin, United Kingdom]	accepted. The glossary has added an oceanographic definition
3-41	3	0				There is inconsistency in the way that range of values are quoted: e.g. P4, L54 "155 [125 to 185]", but P25 L28 "15.5 Sv +/- 2.2" and P25 L16 "18 +/- 1.3 Sv". The inconsistencies need weeding out. [Toby Sherwin, United Kingdom]	accepted. Ch3 follows IPCC guidelines reporting confidence levels, but they are not available from all papers. Sometimes alternative ways to report uncertainties are used, based on the cited papers
3-42	3	0				I would have a list of acronyms at the end of Chapter 3. [Toby Sherwin, United Kingdom]	noted. Ch3 has to follow the editorial guidance of IPCC
3-43	3	0				I felt that the chapter as a whole frequently alluded to models and / or model expectations which arguably begins to blur boundaries with later chapters looking at the models and their performance. This increases the chances of pedants / critics finding issues with the report whereby this chapter's characterization differs from that of latter chapters. Hence I would personally remove as many cases of discussions of models / model expectations from the chapter as is possible, and preferably all such references. [Peter Thorne, United States of America]	rejected. The assessment of Ch3 is based on observations.
3-44	3	0				Chapter is inconsistent in what range of uncertainty should be used. In most places 5-95% Cis are reported as per TSU guidance but in some places its 2.5-97.5 or 0.5-99.5%. This should be rectified and consistent confidence used commensurate with the TSU stated guidelines so the chapter is consistent with the analysis of remaining chapters in this regard. [Peter Thorne, United States of America]	noted and partly rejected. In some cases the publications use different confidence levels, and the authors of Ch3 cannot recalculate all of them
3-45	3	0				The chapter as a whole felt very uneven. It was very obvious that different sections had been written by certain authors and this made it tougher reading than absolutely essential. Greater efforts to make the style and depth of the information consistent across the chapter as a whole would be very beneficial. [Peter Thorne, United States of America]	noted. CLAS and LAs made an effort to homogenize style and wording of Ch3

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						States of America]	
3-46	3	0				There are two initiatives currently underway (one at NASA-JPL the other ESA-CCI) to produce fundamental climate data records (satellite observations converted and gridded to climate data) for use in climate monitoring and climate studies. It would be appropriate to cite them in this chapter given their significance to climate observations - and subsequent climate research. Of relevance to this chapter is the work on SST, sea surface height and ocean colour. The citation for the ESA-CCI work is: Hollmann R., C. Merchant, R. Saunders, C. Downy, M. Buchwitz, A. Cazenave, E. Chuvieco, P. Defourny, G. de Leeuw, R. Forsberg, T. Holzer-Popp, F. Paul, S. Sandven, S. Sathyendranath, M. van Roozendaal and W. Wagner. 2013: The ESA Climate Change Initiative: satellite data records for essential climate variables, BAMS, submitted. [Paul van der Linden, Great Britain]	Noted - this chapter only deals with sea level out of the 3 variables. Scientific results published in the peer reviewed literature that are part of these initiatives are considered and referenced.
3-47	3	0				There are two initiatives currently underway (one at NASA-JPL the other ESA-CCI) to produce fundamental climate data records (satellite observations converted and gridded to climate data) for use in climate monitoring and climate studies. It would be appropriate to cite them in this chapter given their significance to climate observations - and subsequent climate research. Of relevance to this chapter is the work on SST, sea surface height and ocean colour. The citation for the ESA-CCI work is: Hollmann R., C. Merchant, R. Saunders, C. Downy, M. Buchwitz, A. Cazenave, E. Chuvieco, P. Defourny, G. de Leeuw, R. Forsberg, T. Holzer-Popp, F. Paul, S. Sandven, S. Sathyendranath, M. van Roozendaal and W. Wagner. 2013: The ESA Climate Change Initiative: satellite data records for essential climate variables, BAMS, submitted. [Paul van der Linden, Great Britain]	Noted - this chapter only deals with sea level out of the 3 variables. Scientific results published in the peer reviewed literature that are part of these initiatives are considered and referenced.
3-48	3	1	10	1	18	Countries are not specified for the Contributing authors as they are in all other chapters. [Government of Australia]	accepted. Country of present affiliation are added,
3-49	3	1	10	1	18	Add in contributing authors' contry, as in other chapters? [Xiaolan Wang, Canada]	accepted. Country of present affiliation are added
3-50	3	1	63	9	70	Most of the graphs treat the time series as if were based on constants. In reality all of them have large uncertainties, which, if added, would destroy the saististical vakidity of many of the claimed trends [Vincent Gray, New Zealand]	rejected the time series assessed in Ch3 are presented together with the uncertainties, following IPCC guidelines
3-51	3	1			100	To make future projections more convincing, it will be nice to consider future scenarios of volcano activity, for example,a simple extrapolation of present-day observations. [Joshua Xiouhua Fu, United States of America]	rejected. Ch3 assesses ocean observations and no projections
3-52	3	1			200	10. This paragraph refers to the entire Chapter 3. Chapter 3 reviews some of the published information on the topic "Ocean". However, the motivation for the reviewed research effort and the logic behind it is more often fraudulent than not, as the respective research frequently follows the pseudo-scientific reasoning that "more corroborating evidence produces a stronger case for the AGW hypothesis". In fact, nothing can be further from the truth, as shown in my Paragraph 3. Indeed, no amount of corroborating evidence can prove a hypothesis, while a single piece of contradictory evidence is sufficient to reject a hypothesis. In effect, the only (dubiously) useful result of this research effort is the "general progress of science", resulting from wasteful usage of public money on climate studies, where no real problem requiring study may be found. Even the PhD degrees earned as a result of such research are of dubious (in the very least) value, as we are producing more pseudo-scientists certified as scientists, in addition to the already existing pseudo-scientists. Research based on the AGW hypothesis, known to be wrong, may provide no valid scientific results, as its conclusions are already known before the research even began - these conclusions being "AGW is happening, and we are to blame for it". Additionally, the data interpretation in the publications is frequently done based on the same climate models, which are demonstrably wrong (as shown in my Paragraphs 2 to 8), and therefore constitutes a fraud. [Igor Khmelinskii, Portugal]	rejected. In Ch3 ocean observations published in internationally peer reviewed journals are assessed carefully and the statements are based on these results. Political statements and insults are not part of Ch3
3-53	3	3	1	5	17	These are all merely the opinions of so-caljed :experts: who have a conflict of interest and therefore should not be taken seriously. It is a shame that there are no results that can be considered to be confirmed by scientific study of future behaviour [Vincent Gray, New Zealand]	rejected. In Ch3 ocean observations published in internationally peer reviewed journals are assessed carefully and the statements are based on these results. Political statements or projections are not part of Ch3
3-54	3	3	1	89	50	General comment: I agree that observed tendencies of water mass propertires reflect the joint effect of long-	partly accepted. Ch3 includes now longer time series

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						term trends and interannual-to-multidecadal variability. But this is also true for the global temperature characteristics of thermocline/deep waters. The authors analyzed the variability of these characteristics after 1970 and emphasized that there is sampling problem before that. So, we have around 40yrs (more or less reliable) time series. Taken into account that there are natural large-scale signals with typical time scales from interdecadal to multidecadal (PDO and AMO) and different subdecadal modes one can repeat above conclusion for the variability of temperature characteristics of thermocline/deep waters. (Alexander Polonsky, Marine Hydrophysical Institute). [Alexander Polonsky, Ukraine]	when available.
3-55	3	3	1			Executive Summary: As outlined in the general guidance, paragraphs in the ES should be grouped together under subtitles. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Subtitles are included.
3-56	3	3	3	3	12	This statement would be stronger if the average temperature change for the top 700 m was given. As it is written, it could be interpreted as if mostly the upper 75 m has warmed and that 75-700 m barely did, which is inaccurate. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	partly accepted. The statement is reworded and expanded.
3-57	3	3	3	3	31	The choice made to present temperature trends from 1971 only looks like a step backwards from AR4, where trends from 1961 were presented. The authors argue that the data was not good enough. This may be true globally, but there is information at least in the Northern Hemisphere that could be presented. Given that the chapter highlights in many instances the role of decadal variability, it is critical that where data exists for longer time intervals, it is used. [European Union]	Accepted. While the global analysis starts in 1971, studies that extend it back to earlier periods (as early as the 1870s) are discussed in section 3.2.
3-58	3	3	3	3	31	The choice made to present temperature trends from 1971 only looks like a step backwards from AR4, where trends from 1961 were presented. The authors argue that the data was not good enough. This may be true globally, but there is information at least in the Northern Hemisphere that could be presented. Given that the chapter highlights in many instances the role of decadal variability, it is critical that where data exists for longer time intervals, it is used. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. While the global analysis starts in 1971, studies that extend it back to earlier periods (as early as the 1870s) are discussed in section 3.2.
3-59	3	3	6	3	6	I guess the reference to (2.2.2) about Sea Surface Temperature has to be corrected by (2.4.2) [BERNARD BOURLES, France]	Accepted. Reference changed
3-60	3	3	6	3	6	[2.4.2] not [2.2.2] - Chapter 2 rejigged its ordering after the FOD. [Peter Thorne, United States of America]	Accepted. Reference changed
3-61	3	3	6	3	6	Relevant section in Ch2 to be referred to is 2.4.2. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Reference changed
3-62	3	3	7			"The largest warming ..." [Toby Sherwin, United Kingdom]	accepted the statement is reworded
3-63	3	3	8			Remove "time" [Toby Sherwin, United Kingdom]	accepted the statement is reworded
3-64	3	3	11	3	12	The spurious decadal variation is raised without any context which will lead a reader to question in which decade and how it was spurious. The uninitiated reader would undoubtedly conflate this with the far more well known 'has the warming stopped since 1998?' issue in surface temperatures discussed in Chapters 2 and 10 (and possibly elsewhere). I think to avoid such conflation and confusion this text needs to be crystal clear that the decadal variation applied in the 1970's/1980s here and in general this text could be far more clear. [Peter Thorne, United States of America]	Accepted. "prominent during the 1970s and 1980s" has been added to the text.
3-65	3	3	14	3	15	Spell out acronym TW [Government of Australia]	Accepted. The unit is defined parenthetically as 10 to the 12th watts.
3-66	3	3	14	3	15	What is the meaning of the two brackets qualifying the ends of an uncertainty range ? Brackets with the very same figures are quoted on lines 1 and 2 of page 9, as uncertainties on particular estimates. The figures 74 and 137 quoted as the limits of the uncertainties range are the mean value of those estimates. What is the rationale behind this statement ? [Government of France]	Accepted. The executive summary has been rewritten to discuss only the global heat uptake estimates presented in Box 3.1
3-67	3	3	14	3	15	You state that upper ocean heat content (OHC) "very likely" increased at a rate ... during 1971-2010. However in lines 3-4 of this page you state that it is "virtually certain" that the upper ocean has warmed since 1971. This seems inconsistent to me. [Sydney Levitus, U.S.A.]	partly accepted it is virtually certain that the ocean warmed, but the quantification of that trend is assessed as very likely. The text has been reworded to clarify that
3-68	3	3	14	3	15	This sentence is not clear: you need to explain why there are two numbers each with a range (Looking in the	Accepted. The executive summary has been

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						chapter, I assume something like "depending on studies", but this should be made clear in the ES, or a single range should be provided) [Philippe Marbaix, Belgium]	rewritten to discuss only the global heat uptake estimates presented in Box 3.1
3-69	3	3	14	3	15	What is the meaning of the two brackets qualifying the ends of an incertitude range ? Brackets with the very same figures are quoted on lines 1 and 2 of page 9, as uncertainties on particular estimates. The figures 74 and 137 quoted as the limits of the uncertainties range are the mean value of those estimates. What is the rationale behind this statement ? [Michel Petit, France]	Accepted. The executive summary has been rewritten to discuss only the global heat uptake estimates presented in Box 3.1
3-70	3	3	14	3	15	Two lots of ranges are given here 74[43 to 105) and 137 [120 to 154]. So the very likely goes from 74 to 137 but could go from something else to something else ? (depending on what the numbers in the bracket mean) Shouldn't the very likely range be reduced to a lower and upper bound based on the overall assessment ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. The executive summary has been rewritten to discuss only the global heat uptake estimates presented in Box 3.1
3-71	3	3	14	3	18	Why are there two ranges here for just one quantity? [Olivier Boucher, France]	Accepted. The executive summary has been rewritten to discuss only the global heat uptake estimates presented in Box 3.1
3-72	3	3	14	3	18	What do the ranges correspond to? 90% confidence level? [Olivier Boucher, France]	Accepted. Yes, the correspond to 5 to 95% confidence limits throughout the report unless otherwise specified.
3-73	3	3	14	3	18	This statement needs to be strengthened. As it is written, it is very weak because it lumps all methods and regions together, without picking out the solid pieces of evidence. The assessment needs to go much beyond a general statement about the sign of the trend, and pick out e.g. the regions where the trends agree well and by how much, and the regions where there is still disagreement and why. [European Union]	Partly accepted. Global averages are the focus of this report, and certainly the executive summary. There are some details on regional trends in the rest of the chapter.
3-74	3	3	14	3	18	It would be informative to give the rate of upper ocean warming also as of order 0.3 Wm-2 for the global ocean surface of 360 10**12 m2. Considering understandably less reliable estimates of deep ocean warming, this appears to be roughly consistent with model-based estimates (e.g. Hansen et al 2005) of total radiative forcing of the climate system, mostly by intensification of the greenhouse effect. This might be worth mentioning here. [Robert Kandel, France]	Partly accepted. A warming rate of 0.4 W m-2 applied over the entire Earth is now mentioned here for the total heat storage budget.
3-75	3	3	14	3	18	This statement needs to be strengthen. As it is written, it is very weak because it lumps all methods and regions together, without picking out the solid pieces of evidence. The assessment needs to go much beyond a general statement about the sign of the trend, and pick out e.g. the regions where the trends agree well and by how much, and the regions where there is still disagreement and why. [Corinne Le Quééré, United Kingdom of Great Britain & Northern Ireland]	Partly accepted. Global averages are the focus of this report, and certainly the executive summary. There are some details on regional trends in the rest of the chapter.
3-76	3	3	14		31	Give rate of ocean heat content increase in W m-2 (per meter squared of globe) so that it can be compared to forcing. Give uncertainties; Indicate increase in heating rate with time and any degree of confidence associated with that. This is a really important quantity and needs to be addressed up front in the summary. Might even indicate why it is important; it is subtractive from forcing to get equivalent forcing needed to relate obsd temp increase to sensitivity. Heat content increase is impt as an indep measure of global heating, which comes through, but the quantitative heating rate is missing from the discussion. It is available in the box page 3-11, lines 23, 24, but without uncertainty and expression of level of confidence. Also 3-17, line 27. The language page 3-20, lines 8-9 seem a good place to start. The global mean net heat flux signal expected from observed ocean heat content changes is extremely small (about 0.5 W m-2) and beyond the detection ability of currently available observational surface flux datasets. So I would urge you to get this into the exec summary. [Stephen E Schwartz, United States of America]	Partly accepted. The heating in W m-2 over Earth is now mentioned. Uncertainties are given for the Energy inventory change. No attempt is made to address any change in heating rates with time, given the size of the uncertainties in the inventory estimates.
3-77	3	3	17	3	18	This a bold and simplistic statement. In fact, there are more differences between estimates. I suggest some caution. With respect to: "the smaller trends are from estimates that assume no change in these regions". This is incorrect in case Palmer et al. 's estimate uses a representative average approach. Palmer et al.'s estimate is 108 TW compared to 118 TW from Levitus et al. In fact, the rates actually depend on the trend period analysed and it is not universally true that estimates which do not assume change in sparse areas have	Accepted. This discussion has been eliminated from the Executive Summary.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						always smaller trends than estimates that used some sort of infilling. [Catia Domingues, Australia]	
3-78	3	3	17	3	18	This could be presented more clearly, particularly the last sentence. Its not clear that the largest difference in estimated rates comes from the extreme ends of the methodological spectrum . Also, if the smaller trends assume no change in certain regions, then what do the large trends do? [Government of United States of America]	Accepted. This discussion has been removed from the executive summary.
3-79	3	3	18	3	18	I would make clear that the assumption of no change in unsampled regions is a grossly naïve assumption. It is currently implied to a science audience but the audience will not all be scientists so I think you need to spell this out explicitly here. [Peter Thorne, United States of America]	Accepted. This discussion has been removed from the executive summary, but the point is made in Section 3.2.3.
3-80	3	3	19	3	19	No mention is made of temperature change between 700m-3000m. [Government of Australia]	Accepted. This paragraph of the summary now covers all depths below 700 m (the "deeper ocean").
3-81	3	3	20	3	22	"early 1990s" would make the point more strongly than "1990s" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. The more accurate phrase circa 1992–2005 is now used.
3-82	3	3	20	3	25	The bolded statement talks about 300m depth, while the following text talks about 4000m, with no reference to 3000m. This is confusing - consistency needed or a better explanation. [Government of Australia]	Partly accepted. This paragraph of the summary now covers all depths below 700 m (the "deeper ocean"), although only the very likely results from 700–2000 m are in bold.
3-83	3	3	20	3	26	In the bold text talks about 3000 meters but in the subsequent text it only talks about 4000 meters and below, suggest making this more consistent, I was left wondering what was happening in the other 1000 meters. [Government of United States of America]	Partly accepted. This paragraph of the summary now covers all depths below 700 m (the "deeper ocean"), although only the very likely results from 700–2000 m are in bold.
3-84	3	3	20	3	26	In bold is 3000m yet this number is never mentioned in the summary text. Please correct. [Terrence Joyce, United States of America]	Accepted. This paragraph has been rewritten.
3-85	3	3	20	3	26	The many depth intervals mentioned in the executive summary (75m, 700m, 1000m, 3000m, 4000m), without any mention of what happened between 700m and 3000m is difficult to follow. This bullet needs to capture warming over the entire ocean in a simpler form. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Partly accepted. This paragraph of the summary now covers all depths below 700 m (the "deeper ocean"), although only the very likely results from 700–2000 m are in bold.
3-86	3	3	20	3	26	The first time I read this paragraph it sounded quite confusing. When I got to the right place in the chapter it made more sense. With all the numbers it is easy to loose the overview. [Jan Sedlacek, Switzerland]	Accepted. This paragraph of the summary now covers all depths below 700 m (the "deeper ocean"), although only the very likely results from 700–2000 m are in bold.
3-87	3	3	20	3	26	In the bolded sentence the ocean depth is mentioned as below 3000m depth, but in the following text it is discussed to be below 4000m depth. Please clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Partly accepted. This paragraph of the summary now covers all depths below 700 m (the "deeper ocean"), although only the very likely results from 700–2000 m are in bold.
3-88	3	3	21	3	21	The global ocean has warmed at a rate of <0.01 C per decade...: 0 is also < 0.01! Please indicate a value or write: at least at a rate (e.g.) of 0.005 C! The same for TS and SPM! [Government of Germany]	Accepted. This portion of the Executive summary has been deleted, but the change has been made elsewhere.
3-89	3	3	21	3	22	Suggest noting that the Southern Ocean warming rate is 3 times the global deep water warming rate [Government of United States of America]	Accepted.
3-90	3	3	24	3	24	Check grammar. Do you mean "amounts"? [Olivier Boucher, France]	Accepted. This number is removed.
3-91	3	3	24	3	25	So here a warming is given as a number with a range in brackets 48[21 to 75]. How does this formulation relate to likelihood levels ? On the face of it even though there is less confidence in estimates of heat content change in the upper ocean than the lower ocean the upper ocean heat content change numbers are given with less precision (double ranges) than the lower ocean heat content change numbers (single ranges) [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Accepted. The executive summary has been rewritten to discuss only the global heat uptake estimates presented in Box 3.1

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-92	3	3	29	3	31	"Earth" is somewhat anthropomorphized by the statement that extra energy is stored "by" Earth. Suggest completing the last sentence so that it reads "...account for the remainder OF THE CHANGE IN ENERGY." (so as not to sound like it makes up for the remainder of the entire inventory, but just the extra energy) [Government of United States of America]	Accepted. Text has been reworded
3-93	3	3	33	3	35	Based on the work of Durack Wijffels and Boyer et al. (2005) I believe that it is "very certain" not just "very likely" that ocean saline waters have become more saline and relatively fresh ocean waters have become fresher on gyre and basin scales. Boyer et al. showed that the percent variance accounted for the linear trend of salinity changes is very high in some regions which is evidence for "very certain" to me. [Sydney Levitus, U.S.A.]	Taken into account. We have gone carefully through the assessment language and changed these statements. The issue with changing all assessment to "virtually certain" (there is no category of "very certain") are that 'virtually certain' means 99%, which we can't claim in a global sense, given that sampling is very poor in some regions. . We have split out more highly sampled regions, to comment that the trends there are "very likely" (90 to 100%), whereas we have reduced the confidence for the globe to "likely" (60 to 100%) because of uneven sampling.
3-94	3	3	33	3	41	It would be helpful to include discussion of how this affects ocean stratification. [Government of United States of America]	Rejected. This paragraph is about surface salinity. We don't have an analysis to draw on regarding its effects on stratification. We have added statements in section 3.3 about how changing stratification can affect surface salinity.
3-95	3	3	33	3	46	Consider combining the points about salinity. Also the phrase "pattern of sea surface salinity has been enhanced" is not intuitive. Re-word for clarity such "changes in salinity vary on regional and temporal scales". [Government of United States of America]	Taken into account. The SSS and subsurface salinity points have been substantially rewritten.
3-96	3	3	34	3	34	"..sea surface salinity has been enhanced since the 1960s.." The reference Durack & Wijffels (2010; Journal of Climate) suggests this change has occurred since 1950, so uncertain about where the 1960s year comes from [Paul Durack, United States]	Accept. Text modified
3-97	3	3	37	3	39	The statement that enhanced salinity patterns are very likely to be due to an intensification of the fresh water cycle is too strong. It is not supported by observations (page 6, lines 48 to 52). It is inconsistent with many of the other statements concerning the water cycle in the Technical Summary (see my comments on that section). It also appears to depend primarily on the paper by Durack et al. (2012). My concerns about that paper are discussed later. [David Webb, United Kingdom]	Accept. We have reduced the likelihood statement to "medium confidence" from "very likely" as we cannot make a quantitative statement, but we are also convinced that other hypotheses (stratification and circulation changes) are unlikely to be solely responsible for the observed SSS changes. This is based on quantitative assessment of freshwater content in the water column, which avoids the issue of stratification (Fig. 3.5). Over very large regions, circulation changes cannot be responsible. Therefore air-sea fluxes (P-E+R) changes are the principle hypothesis. How these relate to the hydrological cycle is rather direct.
3-98	3	3	37			"over the ocean" should be inserted after "intensification of the water cycle". The salinity observations indicate increases in precipitation and evaporation over the ocean, but do not necessarily imply intensification of the water cycle over continental interiors. This could be the case, but "very likely" would be too strong a way of putting it. Please also comment 298 on Chapter 10, and the sentence spanning lines 46-48 of page 10-33. [Adrian Simmons, United Kingdom]	Taken into account text was reworded
3-99	3	3	38	3	39	In relation to this, the water vapour increase in the lower atmosphere over land has not kept pace over the last decade with the increase in temperature, in the sense that relative humidity has declined (see section 2.5.5 and several of the above comments on Chapter 2), so again caution is needed as regards the water cycle over	Taken into account. Chapter 2.5 states that the changes have kept pace with the warming. However, we have changed our confidence language to

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						land. [Adrian Simmons, United Kingdom]	"medium confidence", which should satisfy your concern.
3-100	3	3	41			In view of the preceding two comments, the use of the word "global" in this line may be questioned. [Adrian Simmons, United Kingdom]	Taken into account. Sentence was removed for other reasons.
3-101	3	3	48	3	48	It is not clear what "surface flux products" are. [Government of Canada]	Taken into account. 'surface flux products' replaced with 'air-sea heat flux datasets'.
3-102	3	3	48	3	49	the very small change in global mean net heat flux' is only 'very small' compared to the total heat flux, but 0.5 W/m2 is actually a large number compared to equilibrium. The qualification of 'very small' is not needed here. Consider deleting lines 48-51, which do not provide useful information for policy. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The text 'Very small' has been removed here. Lines 48-51 have been retained as the result that flux dataset uncertainties are currently too large to allow detection of the signal inferred from ocean heat content change is useful information.
3-103	3	3	49	3	49	Suggest changing "expected from" to something like "required for consistency with" [Government of Australia]	Accepted. Text changed.
3-104	3	3	49	3	49	"very small change in global mean net heat flux expected...": Clarify that this is heat flux into ocean. [Sonya Legg, United States of America]	Accepted. Point clarified by replacement of 'mean net heat flux' with 'mean net air-sea heat flux'.
3-105	3	3	49			Change "(order 0.5 Wm-2...." to "(on the order of 0.5 Wm-2...." [Government of United States of America]	Accepted. Text changed.
3-106	3	3	52	3	52	Again 1960s not 1950 as noted in the above comment [Paul Durack, United States]	Accepted. Text changed to 1950.
3-107	3	3	52	3	56	Consider raising the visibility of statements on changes in wind stress and wave height, because they are important both for the strength of the carbon sink in the ocean and they have policy relevance for impact studies. [European Union]	Accepted. Statements on changes in wind stress and wave height are now covered in separate paragraphs.
3-108	3	3	52	3	56	Consider raising the visibility of statements on changes in wind stress and wave height, because they are important both for the strength of the carbon sink in the ocean and they have policy relevance for impact studies. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	See 3-107.
3-109	3	3	52			what about poleward shifts in westerlies in N. Hemisphere? [Terrence Joyce, United States of America]	N Atlantic wind stress curl changes are treated in Sec 3.4.4 para 2.
3-110	3	3	54	3	55	You state that "mean significant wave height: has "likely" increased since the 1950s... You then state that "confidence is limited by the lack of observations. Do you really want such uncertain and contradictory statements in the Executive Summary or any place else in the text? [Sydney Levitus, U.S.A.]	Accepted. Text revised to avoid confusion arising from multiple use of uncertainty language.
3-111	3	3	54	3	56	Move the acronym from significant wave height (SWH) from line 56 to the first time it is used (line 54) [Government of United States of America]	Acronym no longer employed.
3-112	3	3	55	3	55	insert "North Atlantic" before "trends" to express Wang et al. (2009)'s results better. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Reference to North Pacific no longer included.
3-113	3	3		4		I found the [x to y] notation for the ranges to be very chunky and significantly break the flow. Particularly as they did not specify what they implied - I believe a 90% range - or why in some cases they were asymmetric about the best estimate. If I were reading the ES w/o reading the main chapter text this would leave me incredibly confused. [Peter Thorne, United States of America]	[x to y] notation is the IPCC standard notation and clearly defined elsewhere.
3-114	3	4	1	4	1	the trends are described in page 19 as % per year here but in the conclusions (page 20) and in the executive summary (page 4) the rates are cited as % per decade. I suggest using the same units for these rates through the text. [Antonio Bode, Spain]	Text no longer present in final draft. Units of cm per decade now employed.
3-115	3	4	3	3	4	The statement "Observed changes in water mass properties "likely" reflect the combined effect of long term trends... and interannual-to-multidecadal variability related to climate modes... but these observations are too sparse in space and time to distinguish trends from natural variability." If this statement is true then you may as well eliminate the entire chapter. For example, changes in water mass properties reflect changes in ocean heat content that you previously stated were "virtually certain" in Line 3, Page 3. At best the entire paragraph from which this statement is taken (Lines 4-12, Page 4) is poorly written since it appears to be unrelated to the	accepted text of statement changed

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						rest of the chapter. [Sydney Levitus, U.S.A.]	
3-116	3	4	6	4	6	What about decadal modes such as PDO/IPO? [Janice Lough, Australia]	noted
3-117	3	4	6	4	7	"distinguish trend from natural variability" -- does this refer to trends of anthropogenic origin? This seems implied... [Thomas Stocker/ WGI TSU, Switzerland]	accepted text clarified
3-118	3	4	7	4	7	What is "strong" variability mean? Presumably "high" amplitude; relative to what? [Janice Lough, Australia]	accepted text changed
3-119	3	4	9	4	9	The observed weakening in the formation rate...: Weakening of what? Explanation for "formation rate" [Government of Germany]	accepted definition of formation rate added
3-120	3	4	11	4	11	Change "lower" to "diminished" ["lower" could imply "deeper"; "reduced" would suggest a chemical interpretation!] [David Parker, United Kingdom of Great Britain & Northern Ireland]	partly accepted text of statement changed
3-121	3	4	14	4	20	So wind shifts are part of natural variability and trends since 1993, which is 19 years??? Or are the data sets just not reliable enough to say whether this is part of a longer term signal. This is an important distinction to make clear. This statement, which I believe to be true is in stark contrast to other studies in the Southern Ocean which will be critiqued below. [Terrence Joyce, United States of America]	Taken into account. The text is re-worded for greater clarity on this issue.
3-122	3	4	17	4	18	What "modes" of variability and what time scales are being referred to? [Janice Lough, Australia]	Accepted. Text deleted: "including changes in the winds associated with the modes of climate variability."
3-123	3	4	18	4	20	This statement needs to provide information on the time frame where AMOC and large current are available. Is there no evidence because there is a lot of data that do not show a trend, or because there are few data with large variability? There are very different reasons. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text added: "over the past decade" is the timeframe for continuous measurements of the AMOC.
3-124	3	4	18			define AMOC [Government of France]	Accepted. Atlantic Meridional Overturning Circulation is spelled out.
3-125	3	4	22	4	22	While it is useful to record that it is virtually certain that sea level is rising, for consistency across the AR5, it would be appropriate to give the very likely range on all trends. [Government of Australia]	Accepted text changed accordingly
3-126	3	4	22	4	23	This statement differs from the equivalent statement in Ch 13 Executive Summary. Here global mean sea level (GMSL) rise is given as a mean rate of 1.4 to 2.0 mm yr ⁻¹ over the 20th Century (virtually certain), while Chapter 13 gives ~1.7 [1.5 to 1.9] mm yr ⁻¹ (very likely). For the period from 1993, this chapter gives 2.7 to 3.7 mm yr ⁻¹ (virtually certain) and Ch 13 gives 2.8 to 3.6 mm yr ⁻¹ (very likely). As might be expected, widening of the range increases confidence, but collectively the messages conveyed are confusing. [Timothy Carter, Finland]	Noted - text has been changed to reflect it is virtually certain that global mean sea level has risen, and that the rates are very likely (90% confidence), to be in agreement with Chapter 13.
3-127	3	4	22	4	23	The very likely range for slr is 2.7 - 3.7 since 1993 but in chapter 13 it is different at 2.8-3.6 (13-3 line 27-28). [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted - text has been changed to reflect it is virtually certain that global mean sea level has risen, and that the rates are very likely (90% confidence), to be in agreement with Chapter 13.
3-128	3	4	22	4	24	Can the higher rates of sea level rise since 1993 be explained in the context of recent trends in other variables (in particular the low warming rate of surface temperature)? The paragraph mentions the presence of decadal variability in sea level, but does not specifically discuss factors that could explain the recent high rates. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account - discussed in Sections 3.7.2 and 3.7.3. These sections are referenced at the end of the paragraph.
3-129	3	4	22	4	30	Regional average sea level rise slowed down or even dropped from 1993-2010. Although it is mentioned at Line 34 of Page 11 in TS, section 3.3.7 in Chapter 3, it is missing in ES of Chapter 3. It is recommended to add the following contents between Line 22-30 or Line 41-49 of Page 4, ES in Chapter 3.: "while rates over much of the Eastern Pacific from 1993–2010 are near zero or negative" at Line 34 of Page 11, TS. [Government of China]	Noted - A general comment has been added to state: "Rates of sea level rise over broad regions can be several times larger or smaller than that of GMSL for periods of several decades due to multidecadal fluctuations in the ocean circulation."
3-130	3	4	22	4	30	The sentence "It is virtually certain that globally mean sea level (GMSL) has risen at a mean rate between 1.4	Noted - text has been changed to reflect it is virtually

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						and 2.0 mmyr ⁻¹ over the 20th century and between 2.7 and 3.7 since 1993 (99% conf limits)" is misleading and should be avoided by IPCC. My reasoning touches a lot of text on global sea level rise which suggest an acceleration. With acceleration is meant: an increase which is more than linear, or in math terms: the second derivative is positive. If I closely study all important articles on (global) sea level rise, there is only one conclusion possible: global sea levels are rising linearly. I will follow the three type of series presented in the literature: (1) satellites, (2) tide gauge data, (3a) combination of tide gauges and satellite data, and (3b) combination of tide gauge data and ocean models. Ad (1): all official websites showing the well-known satellite data from 1992 onwards show linear trends. One cannot find any quality site which shows an acceleration over 1992-2011. Ad (2): there are numerous articles showing long tide gauge series. About 95% of these series are perfectly linear, or even deceleration (e.g. San Francisco USA, at page 3-75, middle panel). Good examples from the literature are Holgate (2007), P.J. Watson, 2011, Is there evidence yet of acceleration in mean sea level rise around Mainland Australia?, J. of Coastal Research, or F. Baart et al. 2012, "The effect of the 18.6-year lunar cycle on regional sea-level rise estimates", J. of Coastal Research. Ad (3a): this is the work of Church and White (2006, 2011). It is suggestive to fit a second order polynomial through the data since 1880. (NB: applying a polynomial is not advised in Chapter 2, pages 20/21). Indeed, thanks to the inflexion point around 1930, the acceleration is statistical significant. If one fits a linear and second order polynomial through the data from 1930 onwards, thus 1930-2009, both trends appear to be identical. In other words: GMSLR is perfect linear over the period 1930-2009. Another remark: the interpolation method of Church and White implies tremendous interpolation, using a lot of short tide gauge data. Therefore, their reconstruction should be regarded as 'model output', rather than 'measurements'. Ad (3b): this approach is followed by Ray and Douglas (2011). If we study their Figure 18, it is easily seen (without any statistics) that trends are linear! My conclusion is that IPCC should not suggest an acceleration over recent decades as long as the data, or a combination of data and satellites/models, show a linear pattern. In this context the review article Woodworth, White, Jevrejeva, Holgate, Church and Gehrels, 2009, Int. J. of Climatology 29, p. 777-789, is important. These authors do not conclude any unprecedented acceleration over the period 1990-2009. Note that, if you agree with my vision, a lot of text should be adapted (SPM, TS and Chapter 13, page 13-3 lines 24-30, or page 5-131, final two bars in the graph)! [Hans Visser, The Netherlands]	certain that global mean sea level has risen, and that the rates are are very likely (90% confidence). This statement does not imply acceleration, only different rates over different times and the differences are discussed in Section 3.7.
3-131	3	4	22	4	49	There is a lack of consistency between statements in lines 22-23 and statements in lines 41-43: If you follow line 22+, it appears "virtually certain" that the rate of SLR increased (century max 2 mm/Y, minimum since 1993 2.7 mm/Y, so the recent trend is above the century trend, "with 99% certainty"). By contrast, lines 41 - 43 suggests that it is only "likely", and worse, that there is one reconstruction of GMSL that does suggests that there was no acceleration [but a reply to it was published]. There are problems in the chapter on this issue, as I noted in my comments on page 31 - 32, and corrections in the chapter will need to be reflected here. [Philippe Marbaix, Belgium]	Noted - text has been changed to reflect it is virtually certain that global mean sea level has risen, and that the rates are are very likely (90% confidence). Acceleration is different from the mean rate, and it can only be considered likely (66% confidence), since only 2 out of 3 studies agree.
3-132	3	4	22			remove "globally" insert "global" [Toby Sherwin, United Kingdom]	Editorial - will correct.
3-133	3	4	23	4	23	Missing central value for rates. [Catia Domingues, Australia]	Noted -no central value is given as it is the spread from 3 different estimates.
3-134	3	4	23	4	23	TSU guidance was that where confidence was assessed this should be the 5-95% range [Peter Thorne, United States of America]	Accepted - will change
3-135	3	4	25	4	27	The presence of high rates of increase in sea level during 1930-1950 appears to undermine the attribution of sea level trends to climate change, when it fact it is simply highlighting that the recent high rates of rise could be due to natural variability. This needs to be clarified so there is no confusion. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this is discussed in referenced sections (3.7.2,3.7.3).
3-136	3	4	25			Salt marsh data give a similar conclusion to tide gauges and altimetry - see Gehrels and Woodworth, GPC, in press. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Noted - Salt marsh records are discussed in Chapter 5, as noted in the introduction to Section 3.7.1.
3-137	3	4	32	4	32	Main range for sea level is given to be 1.4 to 2.0 mm/yr whereas in the chapter 3, 28, line 45 it is given at 1.5 to 1.9, also used in chapter 13, page 3, line 27. Please clarify: between chapters and, if 1.4 to 2.0 has to be used, why 1 mm/yr is added since it is not straightforward. [SYLVIE JOUSSAUME, France]	Noted - text has been changed to reflect rates at 90% confidence instead of 99%.
3-138	3	4	32	4	32	I would change "very likely" to "virtually certain" since the change in upper ocean heat content is "virtually	rejected the temperature and heat increase is

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						certain" as you have stated on Page 3, Line 3. The thermosteric component of sea level change is directly related to changes in ocean temperature. [Sydney Levitus, U.S.A.]	virtually certain, but not the quantified trend
3-139	3	4	32	4	33	It is difficult to relate the information on processes to the information of rate of sea level rise (previous bullet) because the time scales are different. What we need to know is if the high sea level rise observed since 1993 can be explained by known variability in the ocean-climate system (e.g. ocean mass or PDO state), or by processes that are mainly related to global climate change (e.g. snow and ice melt) [European Union]	Noted - Chapter 3 discusses ocean observations only. Chapter 13 discusses processes that cause the observed sea level change.
3-140	3	4	32	4	33	The bold part of this paragraph could provide a wider overview of our capability to account for the sea level rise, rather than focus immediately on one component. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Noted - only thermosteric sea level change can be measured by ocean observations, the focus of this chapter. Ice mass contributions are discussed in Chapter 4, and Chapter 13 brings all observations together to discuss contributions of sea level change.
3-141	3	4	32	4	33	It is difficult to relate the information on processes to the information of rate of sea level rise (previous bullet) because the time scales are different. What we need to know is if the high sea level rise observed since 1993 can be explained by known variability in the ocean-climate system (e.g. ocean mass or PDO state), or by processes that are mainly related to global climate change (e.g. snow and ice melt) [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Noted - Chapter 3 discusses ocean observations only. Chapter 13 discusses processes that cause the observed sea level change.
3-142	3	4	32	4	39	Numbers here are quoted with different precision (0.1 mm per yr then 0.11 mm per yr). Also the components of the budget over similar time frames are not given in this ES nor are they given in the Chapter 13 ES despite a statement there that the budget is pretty well understood. If we are going to quote all these numbers in the ES it would be good to see how the contributions add up to the overall SLR wouldn't it ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	rejceted. Ch3 only assesses the ocean part of sea level rise. This is a topic for Ch13
3-143	3	4	32	4	39	If there is no acceleration since 1930, this conclusion seems a bit strange (although correct). If the upper 700 mm yields an extra contribution since 1971, there should be another contribution which is diminishing since 1971. If not, there should be an acceleration since 1971 due to this phenomenon. But the data do not support that. More clarification needed here. [Hans Visser, The Netherlands]	notrd. the discussion and assessment of sea level acceleration has been expanded and clarified
3-144	3	4	36	4	37	An increase in ocean mass should not be given in units of mm/year. I suggest rewriting to "...the ocean mass has increased, at a rate corresponding to a surface flux into the ocean in the range [0.8 to 1.6] mm yr ⁻¹ since 2005, ..." [Arne Melsom, Norway]	Noted - text will be changed to reflect the mass is in terms of equivalent water thickness.
3-145	3	4	37	4	38	Which 6-year period? [Janice Lough, Australia]	Noted - time-period will be described in previous statement (2005-2011).
3-146	3	4	37			The text states "since 2005" but please check to see if this should read "between 2005 and 2011" based on data availability. [Government of United States of America]	Noted - discussion of trends between 2005-2011 has been eliminated from draft.
3-147	3	4	39			Same comment as the one immediately above, concerning the use of the word "global" in this line. [Adrian Simmons, United Kingdom]	Noted - unclear what this refers to, as there is no "global" on this line.
3-148	3	4	41	4	43	What do the numbers in square brackets mean ? [-0.002 to 0.019] ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	Noted - the brackets refer to 90% confidence, using the style required by TSU for the entire document.
3-149	3	4	41	4	43	In this Executive Summary, the point is made that it is likely that GMSL has accelerated since the early 1900s. I think this statement requires careful qualification because all studies to date looking at global or regional scale sea level rise have invariably fitted a 2nd order polynomial (or quadratic) function to a data set in order to estimate the "acceleration" term. In reality such a curve fit describes the "average acceleration" over the entire time period rather than accurately reflect the considerable temporal sensitivity in relative acceleration evident throughout the records. For eg, the sharp rates of rise evident between approximately 1920 and 1940 and again from 1990 in most records with a distinct tapering from 1960 to 1980 are not highly reflective of smooth curvilinear (quadratic) functions. Discussions in section 3.7.4 more correctly note the sensitivity of acceleration analyses to the time period of the record.	Noted - the word "average" will be added to the text to clarify this.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Phil Watson, Australia]	
3-150	3	4	41	4	49	Interesting new piece of work added since last time - or at least seems so to me! [Terrence Joyce, United States of America]	Noted thank you
3-151	3	4	41	4	49	As above - these conclusions also supported by salt marshes. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Noted - Salt marsh records are discussed in Chapter 5, as noted in the introduction to Section 3.7.1.
3-152	3	4	42	4	42	...non-zero acceleration. of GMSL. [Government of Germany]	Noted - will move "GMSL" from first part of sentence to end.
3-153	3	4	42	4	42	"non-zero" acceleration - jargon? [Janice Lough, Australia]	Noted - will refer instead to a small, average acceleration
3-154	3	4	45	4	45	into the 20th Century - do you mean "through the 20th Century" or even "into the 21st century"? [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted - paragraph text has been revised to use specific dates.
3-155	3	4	46	4	47	I think this is a partial statement and can be misleading. There is no comment where sea level is not rising and why. [Catia Domingues, Australia]	Noted - All records of sea level that extend back to 1900 show an average positive rate of sea level rise, after correcting for vertical land motion. A statement has been added to clarify the vertical land motion correction.
3-156	3	4	47	4	48	Does this mean the magnitude of "extreme sea levels" and/or their frequency of occurrence - unclear. [Janice Lough, Australia]	Change to: Noted - height of extreme water level was meant. This will be revised to read: "it is likely that the magnitude of extreme high sea levels increased since 1970."
3-157	3	4	48	4	49	Please quantify the change in extreme sea levels here and in the main text. [European Union]	Noted - a new figure and discussion as been added to 3.7.5.
3-158	3	4	48	4	49	Please quantify the change in extreme sea levels here and in the main text. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Noted - a new figure and discussion as been added to 3.7.5.
3-159	3	4	48			"mainly attributable" -- suggest to quantify what is meant here with mainly, i.e., more than 50% or something else? Also suggest to add confidence/likelihood if appropriate [Thomas Stocker/ WGI TSU, Switzerland]	Noted -Confidence/likelihood is difficult to quantify, as there is only a single recent global study. However, we can quantify the % of tide gauges with no significant trend, small residual trends, and higher residual trends. This has been added.
3-160	3	4	51	4	51	It isn't clear in Section 3.8 how nutrient data have been used to estimate the anthropogenic carbon inventory. [David Parker, United Kingdom of Great Britain & Northern Ireland]	rejected the space limitation does not allow to discuss details of the various methods (C*, TTDs, Green's function etc) applied to calculate Cant. The reviewer is referred to the cited references
3-161	3	4	51	4	56	What do the ranges correspond to? 90% confidence level? [Olivier Boucher, France]	Yes, this has been clarified
3-162	3	4	51	4	56	This statement needs to be clarified. It states 'very high confidence' for an increase in carbon uptake between 1994 and 2010, but the two numbers stated overlap and in fact show no significant difference between the two time periods. The reason why there is high confidence that carbon uptake continued is not because of the inventory surveys, but because there is a wide range of data (pCO ₂ , atmospheric O ₂ /N ₂ , atmospheric d ¹³ C, tracer-based ocean inversions, patterns of change of atmospheric CO ₂ concentration) which support a positive CO ₂ at least since 1990, with an amplitude of around 2 PgC/y. [European Union]	partly accepted. The statement has been rewritten to clarify that although the inventory calculated with different methods are different, all methods show an increase in the inventory, and the respective Cant flux is compatible with other results from independent methods.
3-163	3	4	51	4	56	Here and in the main text there is no assessment of the efficiency of the ocean CO ₂ uptake, which has been extensively studied since AR4. Is the available information sufficient to assess if the ocean CO ₂ sink has weakened in the past 2 decades in some regions of the world or not? [European Union]	rejected. this discussion was and is part of subchapter 3.8.1.2, and the para is now slightly expanded: "Ocean observations are insufficient to assess whether there has been a change in the rate of total (anthropogenic plus natural) carbon uptake by the global ocean.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							Evidence from regional ocean studies (often covering relatively short time periods), atmospheric observations and models is equivocal, with some studies suggesting the ocean uptake rate of total CO ₂ may have declined (Le Quéré et al., 2007; Schuster and Watson, 2007; McKinley et al., 2011) while others conclude that a decline is rather unlikely (Knorr, 2009; Gloor et al., 2010; Sarmiento et al., 2010). A recent compilation of observed CO ₂ uptake by land and oceans found a significant increase of this uptake from 1960 to 2010, implying that it is unlikely that on a global scale both land and ocean sinks decreased (Ballantyne et al., 2012).
3-164	3	4	51	4	56	This statement needs to be clarified. It states 'very high confidence' for an increase in carbon uptake between 1994 and 2010, but the two numbers stated overlap and in fact show no significant difference between the two time periods. The reason why there is high confidence that carbon uptake continued is not because of the inventory surveys, but because there is a wide range of data (pCO ₂ , atmospheric O ₂ /N ₂ , atmospheric d13C, tracer-based ocean inversions, patterns of change of atmospheric CO ₂ concentration) which support a positive CO ₂ at least since 1990, with an amplitude of around 2 PgC/y. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted text rewritten: all methods show an increase in Cant
3-165	3	4	51	4	56	Here and in the main text there is no assessment of the efficiency of the ocean CO ₂ uptake, which has been extensively studied since AR4. Is the available information sufficient to assess if the ocean CO ₂ sink has weakened in the past 2 decades in some regions of the world or not? [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	see 3-164
3-166	3	4	53	4	53	Why no best estimate for the 93-137 PgC number for 1994 when best estimates are given for next numbers for 2010 ? [Peter Stott, United Kingdom of Great Britain & Northern Ireland]	taken into account text is clarified
3-167	3	5	1	5	2	Why is confidence used in this statement but likelihood is used in all other statements in this exec summary. Please be consistent [Government of Australia]	accepted. Statements reworded
3-168	3	5	1	5	2	"gradual acidification of seawater and decreasing pH"? Doesn't the term "ocean acidification" not directly refer to the decrease in pH? Suggest to revise formulation to, e.g., "seawater as expressed by decreasing pH"? [Thomas Stocker/ WGI TSU, Switzerland]	accepted text changed and clarified
3-169	3	5	1	5	5	Page 5 , line 1 to 5. why is there no likelihood on oceanic uptake here, and virtually certain in section 3.8.2? [Nathaniel Lee Bindoff, Australia]	accepted statement has been reworded and checked for consistency with 3.8
3-170	3	5	1	5	5	Page 5, lines 11 to 12. In the detection and attribution chapter it turns out to be easier to attribute the changes outside of the equatorial oxygen minima because the models have better realism. [Nathaniel Lee Bindoff, Australia]	noted
3-171	3	5	1	5	5	Ocean acidification is given a lot of space in the document but this key point does not have much detail. OA is one of the most easily detected impacts of CO ₂ (SO _x and NO _x) emissions, because the chemistry is straightforward. The key finding mentions -0.0015 and -0.0024 as the range, which may seem insignificant unless it is explained that pH is a log scale. Consider restating as a reduction in average pH of 30% and noting more significant differences in some regions. [Government of United States of America]	accepted text changed and the change in hydrogen ions (26%) added
3-172	3	5	2	5	2	Ocean acidification has not occurred only in surface waters, but throughout the water column. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted. Text reworded to clarify this point
3-173	3	5	3	5	4	The sentence "In the ocean interior...decadal time scales" seems vague and without context. [Government of United States of America]	partly accepted statement is rewritten to make the connection between the two sentences more clear
3-174	3	5	3	5	4	Why is this important? Might natural physical/biological processes also affect decadal variability of surface water pH? [Janice Lough, Australia]	noted in the ocean interior, other processes than uptake of Cant could lead to a changing pH

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-175	3	5	7	5	7	Is 'limited' necessary here? Although there are fewer O2 measurements than temperature, there are many more than e.g. nutrients. Please use the IPCC language to assess the value of the information. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	rejected. The limited oxygen data set is setting the level of confidence
3-176	3	5	7	5	12	Given the new results of Stramma et al. (2012), I would broaden the uncertainty range to 0.7 - 5 µmol per kg per decade rather than 3 - 5 µmol per kg per decade. [Denis Gilbert, Canada]	accepted the results from Stramma et al., 2012 have been included in Ch 3.8
3-177	3	5	7	7	8	"..oxygen observations available since 1960.." The Helm et al., 2011 (Geophys Res Lett) ref notes their analysis extends from 1970s to 1990s, not 1960. They note their data extends from 1940 to 1988 [Paul Durack, United States]	accepted text changed
3-178	3	5	14	5	15	This is a good example of an excellent summary sentence which would be appropriate for the SPM. Clear, plain English and with a simple message for policy makers to use. Suggest putting into SPM as well. [Government of Australia]	noted
3-179	3	5	14	5	17	It is noted that more emphasis has been given for change of physical or biogeochemical state of ocean during the past 40 years without adequate reference to Bio-geochemical state of the Ocean in the Chapter. Section 3.8 is on Ocean- Biogeochemical change, including anthropogenic oceans acidification. However, discussion on Bio-geochemical state of Ocean has not been reflected significantly. In view of this, adequate reference may be given to this affect. [Government of India]	rejected Ch 3.8 assesses the changes of the relevant parameters (carbon, oxygen, nutrients)
3-180	3	5	14	5	17	After all that work you make the astounding discovery that the oceans are changing!!! [Vincent Gray, New Zealand]	accepted the statement is reworded, including confidence levels
3-181	3	5	14	5	17	would it be possible to actually add the level of confidence explicitly rather than just referring to an enhancement of the level of confidence without providing from what to what? [Thomas Stocker/ WGI TSU, Switzerland]	accepted the statement is rewritten including levels of confidence
3-182	3	5	14			The consistency between different observations is important. Could this statement also say that the overall picture in the ocean is consistent with a warming world in general, and with a penetration of the signal from the surface to depth? See Sedlacek and Knutti 2012 GRL for a recent discussion. [Reto Knutti, Switzerland]	accepted, text revised
3-183	3	5	16	5	16	Please clarify what is meant by 'enhances the level of confidence associated with this conclusion' [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted statement is reworded
3-184	3	6	1	6	51	General Comment: Introduction seemed lengthy, and could be sharpened to more clearly show the progress in oceanography on discovery of change in this report relative to AR4. [Nathaniel Lee Bindoff, Australia]	accepted introduction is reworded
3-185	3	6	3	6	18	These two paragraphs need strengthening. Line 12-13 is a powerful sentence, and it would be nice to have a sentence like this at the very beginning of the paragraph and introduction. The use of the word "therefore" in line 17 is odd-- it is not clear that "A" proves "B". This whole sentence is a restatement of the last sentence of the first paragraph (lines 12-13). [Government of United States of America]	accepted text of introduction changed and rearranged
3-186	3	6	5	6	6	Consider rewording the phrase "... circulation connects the surface and interior ocean..." As written it describes a surface-to-interior connection. [Government of United States of America]	accepted text of introduction changed and rearranged
3-187	3	6	5			Reading the sentence, one may believe that what counts is the factor of 4 for the Cp of seawater relative to air in J kg-1 oK-1. The huge thermal inertia of the ocean relative to the atmosphere comes from rCp in J m-3 oK-1 (3 orders of magnitude difference). [Government of France]	accepted text of introduction changed and rearranged
3-188	3	6	6			change "total exchange of water" to "total exchange of energy" [Karina von Schuckmann, France]	rejected. The sentence refers to exchange of water via evaporation and precipitation
3-189	3	6	8	6	10	"acting to slow the rate of climate change" -- as this specifically refers to the CO2-induced climate change, we suggest to explicitly refer to the atmospheric CO2 increase first and then connect to the changing climate. Proposal: change to "acting to slow the increase in atmospheric CO2 and the rate of a climate change" [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text rewritten to clarify that ocean slows the rate of atmospheric CO2 increase

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-190	3	6	8	6	13	there is no comment on freshwater, although it is listed in line 3 [Karina von Schuckmann, France]	accepted text modified
3-191	3	6	9	6	10	This sentence should be removed. No credible empirical has ever been presented to support the assertion that carbon dioxide causes significant climate change and conversely that natural forces cannot account for them. Models can't provide that clarity because natural forces are not simulated with 100% accuracy, besides which all that models can do is provide predictions according to the assumptions in their software. The absence of warming over the last 15 years despite the additional carbon dioxide should also tell you that if CO2 is causing any warming at all then it must be minor. [John McLean, Australia]	rejected .Literature quote in this and other AR5 chapter provides evidence of the role of carbon dioxide in climate change. Changes in temperature over the last 15 years are discussed in this chapter and in Box 9.2
3-192	3	6	10			This sentence makes reference to the ocean slowing the rate of climate change. This should be clarified since the oceans are part of the climate system, not some external force. [Government of United States of America]	accepted. Text rewritten to clarify that ocean slows the rate of atmospheric CO2 increase
3-193	3	6	11	6	12	Add the words "whatever the direction of change" to this sentence. [John McLean, Australia]	rejected. Sentence already states possibility of increasing or decreasing rates of change
3-194	3	6	13	6	13	Suggest rewording this line to read "...therefore closely tied to the evolution of the ocean." [Government of Australia]	accepted text of introduction changed and rearranged
3-195	3	6	15	6	16	Needs rewording. When have the oceans not provided a clearer signal? If you cannot answer this question then the word "often" is irrelevant. [John McLean, Australia]	accepted text of introduction changed and rearranged
3-196	3	6	20	6	21	Consider rewording the phrase "paucity of long-term measurements of the global ocean" to better represent that the long-term measurements are not spatially consistent, or well distributed geographically. [Government of United States of America]	accepted text of introduction changed and rearranged
3-197	3	6	23	6	24	Include a reference to the oceans chapter in AR4 to help readers look at past reports. [Nathaniel Lee Bindoff, Australia]	accepted
3-198	3	6	23	6	24	Lines 23-24 you list the trends reported in AR4, but don't include SST. [Government of United States of America]	rejected the assessment of SST is part of Ch2
3-199	3	6	23			References to the AR4 should be as specific as possible. Here, at least the full report needs to be referenced rather than not having any reference at all. [Thomas Stocker/ WGI TSU, Switzerland]	accepted reference added
3-200	3	6	25	6	27	please add reference for ARGO array of profiling floats [Thomas Stocker/ WGI TSU, Switzerland]	accepted reference added
3-201	3	6	25	6	29	could be relevant here adding "strong efforts to maintain and extend in situ moored systems (eg in the tropics TAO/TRITON, PIRATA and now RAMA in the Indian Ocean from 2004; reference: McPhaden, M. & Co-Authors (2010). "The Global Tropical Moored Buoy Array" 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.61). [BERNARD BOURLES, France]	rejected. We agree about the importance of these measurements but they are not used in the studies assessed here and so are omitted given space limitations.
3-202	3	6	26	6	26	In addition to the comment that the Argo floats are NOW providing measurements, it would be helpful to know how when this record become globally available. [Government of United States of America]	accepted (near global coverage achieved in 2005)
3-203	3	6	26			It would be helpful to provide some explanation of the Argo array. The first time that Argo (and Pentadel on page 3-9) are mentioned, it would be helpful to provide a citation for those unfamiliar with these. [Government of United States of America]	partly accepted a reference for the ARGO data added. Pentadal is not a programme but an average over 5 years (a pentade)
3-204	3	6	27	6	27	Suggest rewording this line to read"...upper 2000m for the first time in the ice free open oceans." [Government of Australia]	accepted. Now state "near-global"
3-205	3	6	27	6	27	I would change "2000 m" to "2000 db" for accuracy. [Sydney Levitus, U.S.A.]	rejected Ch3 throughout follows the convention to use m instead of db
3-206	3	6	27			add "of the global ocean" between "upper 2000m" and "for the first time" [Karina von Schuckmann, France]	accepted
3-207	3	6	29	6	31	This sentence could be reorganized for clarity (e.g. "...lack of historical data, they help estimate...observational record by documenting seasonal and interannual variability."). [Government of United States of America]	accepted text of introduction changed and rearranged

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-208	3	6	30			add "and interpreting" after "they help estimate" [Karina von Schuckmann, France]	accepted text of introduction changed and rearranged
3-209	3	6	31	6	31	Suggest addition of word 'understanding' between 'been made in' and 'and reducing biases...' [Government of Australia]	accepted text of introduction changed and rearranged
3-210	3	6	36	6	51	Consider reorganizing that paragraph as a list of bullets. [Government of United States of America]	partly accepted text of introduction changed and rearranged
3-211	3	6	36	6	51	This paragraph is mostly a prosaic repeat of the chapter's table of contents and is unnecessarily redundant, in my opinion. In comparing to other chapters, the final introduction paragraphs tend to contrast the findings with previous ARs, describe the chapter's context as it relates to other chapters in the volume, or give other key highlights. Such a revision of this paragraph would be more useful. [Gary Lagerloef, United States of America]	partly accepted text of introduction changed and rearranged
3-212	3	6	38	6	38	Please cite the relevant section of chapter 2 (in this case section 2.4.2.) [Thomas Stocker/ WGI TSU, Switzerland]	accepted 2.4.2 added
3-213	3	6	39	6	41	This may overemphasis the role of air-sea fluxes. Changes in advection, diffusion, and the effect of the observed increase in stratification on diffusion may be equally important. [David Webb, United Kingdom]	accepted text of introduction changed and rearranged
3-214	3	6	49	6	51	where is the comment ?	noted
3-215	3	6	53	6	53	Sea surface temperature research is mostly reviewed in Chapter 2, but it is still essential to highlight the role of ocean circulation and ocean dynamics on the trends and decadal variations of the SST in this chapter. [Government of Australia]	Rejected. Given space limitations, duplication must be eschewed.
3-216	3	6	53	11	24	Section 3.2As with my comments on the first draft of this report, I still dont think it makes sense to limit discussion of upper ocean heat content to the period 1971 to present. There is some statistical evidence from Lyman and Johnson that this is the most certain time period for results on upper ocean heat content based on yearly averages. However, I dont think this is sufficient to ignore results for years prior to 1971. For example, there is no mention in the ocean heat content section of longer compositing periods which do have sufficient data for years prior to 1971. The sea level section does discuss Levitus et al. 2012 five-year compositing period results, but the ocean heat content section does not. Further, recent results by Roemmich et al regarding differences between the Challenger Expedition (1870s) and more recent data and Gouretski et al. (2012) discussion of change prior to 1971 do discuss earlier heat content changes. The freshwater section discussed 50 years of change from in situ salinity data. These data are much sparser than temperature data before and after 1971. There are different gradations used in the IPCC report to classify evidence for change. I think the report does itself a disservice by not discussing change prior to 1971 for ocean heat content. Different gradations of certainty could be used for different time periods, but at least putting forth the existing evidence for ocean heat content change prior to 1971 is important for this report. [Tim Boyer, United States of America]	Accepted. Roemmich et al. (2012) and Gouretski et al. (2012) are now discussed in Section 3.2.2. Box 3.1 uses the pentadal estimates of Levitus (2012) from 700–2000 m, and now notes that that for this depth ranges, Section 3.2.2 notes that these estimates are less likely to contain aliased by interannual variability in either ocean. However, 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), so Chapter 3 does not discuss pre-1970 estimates extensively, although they are shown in Fig. 3.2.
3-217	3	6	53			Section 3.2: There is a discussion of the water masses and circulation to explain the heat content, especially in 3.2.4. This is somewhat redundant with the water mass and circulation section. For example p. 3-9 line 53-57 essentially repeats a lot of the information about the NADW. This would be avoided by cross referencing. [Government of United States of America]	Rejected. This section focuses on the impact of NADW temperature variability on heat content, the water-mass section discusses NADW circulation and formation rates.
3-218	3	6	55	6	55	This section is really nice and well written. However, its really about more than implied by this section title. Can	Accepted: subtitle formerly "Background: Instruments

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the authors think up a title that better reflects what the text section contains? [Peter Thorne, United States of America]	and Sampling" is now "Instruments, Sampling, and Ocean Heat Content Estimates".
3-219	3	6		10		Section 3.2 is a good compendium of contemporary knowledge about the changes of the ocean temperature and heat content. [Government of Poland]	Noted. Thank you.
3-220	3	6		14		The text is very comprehensive and also interesting. Nevertheless called my attention the almost complete lack of data on the South Atlantic Ocean, which makes the global projections weaker. Then, I think the text should contain specific recommendations that address research policies in unexplored regions, such as South Atlantic Ocean. [Government of Brazil]	Rejected. This assessment must focus on findings, not recommendations for study of specific regions.
3-221	3	6				I'm really surprised that ENSO is not discussed at all in Section 3.2 since my understanding why global temperatures have not increased as expected in the last few years is because the Equatorial Pacific is stuck in a La Nina / neutral phase. [Toby Sherwin, United Kingdom]	Accepted, the influence of interannual variability (e.g. Roemmich and Gilson, 2011) is now mentioned in Section 3.2.1.
3-222	3	7	1	7	1	Property is an ocean community term. variable might be a better term. [Nathaniel Lee Bindoff, Australia]	Accepted. Text is revised.
3-223	3	7	1	7	1	I would change "Temperature is" to "Temperture has been". With the introduction of profiling floats we are now measuring salinity as frequently as temperture. [Sydney Levitus, U.S.A.]	Rejected. Temperature is still measured more frequently than salinity because expendable bathythermographs are still being used.
3-224	3	7	1	7	43	This discussion of instruments and sampling doesn't show the sampling and network of instruments globally. I wondered if a figure showing the increase in profiles and coverage would be worthwhile (perhaps as an appendix). You might consider making a more confident statement on level of reliability of the netowrk. My interpretation of the recent Abrahams et al paper that much of the differences is not from sampling per se, but rather from methods and approaches of the different studies. They may even use the same data and get different answers. This text need to reflect this broader range of the different contributions to ocean heat content variability than just attributing to sub-sampling. While the subsampling is an issue in the literature, methods and approaches and data quality will be bigger issues for the anthropogenic carbon, and also for sea level because there are probably only 4 records that extend back to 1880. I certainly think sea-level level is reliably estimated. In my mind the issue here is to be explicit about the coverage across all of the different climate variables. I would suggest moving this discussion to appendix, and cover sea level, ocean temperature, oxygen, salinity, and anthropogenic carbon, ocean acidity (just a few time series). [Nathaniel Lee Bindoff, Australia]	Sampling issues are now addressed in an appendix
3-225	3	7	5	7	6	and Argo was also a problem in the last IPCC because of pressure errors in some floats, wasn't it? [Terrence Joyce, United States of America]	Accepted. Argo and Barker et al. (2011) are added to the text.
3-226	3	7	5	7	14	This paragraph should include a short explanation about the correction of the Argo bias. This paragraph talks about measurement biases from various instruments, but does not touch on Argo. [Government of Australia]	Accepted. Argo and Barker et al. (2011) are added to the text.
3-227	3	7	5			comment on "some of these instruments": 's and MBT's made up the majority of instruments for historical in situ temperature measurements [Karina von Schuckmann, France]	Accepted. "Widely used" has been added to this sentence.
3-228	3	7	9	7	10	Partial list of XBT/MBT bias corrections. It would be nice to have all citations or instead cite this website: http://www.nodc.noaa.gov/OC5/XBT_BIAS/xbt_bias.html [Catia Domingues, Australia]	Rejected. This is an assessment, not a review.
3-229	3	7	10			another work on XBT correction could be added: Hamon, M., G. Reverdin and P.-Y. Le Traon, 2012: Empirical correction of XBT data, J. Atmos. Oceanic. Technol., 29, 960-973 [Karina von Schuckmann, France]	Accepted. Reference added
3-230	3	7	11			"upper ocean heat storage": confusing to mix up ocean heat content and ocean heat storage [Karina von Schuckmann, France]	Accepted. Word changed from "storage" to "content" here.
3-231	3	7	13	7	13	After the word "spurious", insert "signal that indicated ...". [Gary Lagerloef, United States of America]	Accepted. Inserted "signal suggesting".
3-232	3	7	17	7	17	Some of this decadal variability or error is for a specific period, and so may want to clarify that period of data bias in this text. [Nathaniel Lee Bindoff, Australia]	Accepted. Added the words "during this time period" to the text.
3-233	3	7	17	7	17	To avoid confusion, "... result in reduced decadal variability" is better changed to "... result in reduced artificial	Accepted. Text modified

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						decadal variability" [Government of Australia]	
3-234	3	7	19	7	19	Remove 'Recent' - this issue has always been a source of variance among OHC estimates - for AR4 as well as AR5 [Government of Australia]	Rejected. "Recent . . . also" clearly reflects that this issue has always been a source of variance among OHC estimates and ties this paragraph to the previous one.
3-235	3	7	19	7	28	Which regions of the upper ocean are sub-sampled/unsampled, which regions of the ocean are considered "well-sampled"? [Leticia Cotrim da Cunha, Brazil]	Sampling issues are now addressed in an appendix
3-236	3	7	19	7	28	Sensitivity of the use of the climatology could be mentioned, which is especially high in undersampled regions. Problem: Warming signal in the climatology ==> biased to this signature in under-sampled regions, most pronounced in the the southern ocean sector [Karina von Schuckmann, France]	Accepted. This is noted in the text.
3-237	3	7	20	7	20	I would add "Levitus et al. (2012) to the citations given in this line. [Sydney Levitus, U.S.A.]	Accepted. Reference added
3-238	3	7	20	7	20	Smith and Murphy (2007) is described as "effectively assume no temperature anomaly in these regions". I think this is incorrect. The Smith and Murphy (2007) methodology uses global covariances from a climate model to infill data, and has more in common with the Domnigues et al. (2008) approach. Since Doug Smith is a contributing author I suggest that he is alerted to this issue. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. The change has been made.
3-239	3	7	20	7	21	Do these analyses assume "no temperature anomaly" or "a temperature anomaly of zero"? [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed to clarify the issue
3-240	3	7	23	7	23	I would replace "anomalies" with "ocean heat content". [Sydney Levitus, U.S.A.]	Rejected. However, changed "anomalies" to "temperature anomalies" for increased clarity.
3-241	3	7	26	7	28	Even well sampled such as the North Atlantic, for instance, there are significant differences between basin-averaged estimates. See Gleckler et al. (2012), supplementary material. [Catia Domingues, Australia]	Accepted. The word "consistent" has been replaced with "closer".
3-242	3	7	27	7	27	You should explain how these results are consistent. Are they static? Do multiple analyses produce the same results and if so, are the analyses essentially the same? [John McLean, Australia]	Accepted. The words "consistent results" has been replaced with "results in closer agreement".
3-243	3	7	32	7	35	Is there any study that underpins the "virtually certain" statement? [Catia Domingues, Australia]	Accepted. That statement is removed, and newly appeared studies on centennial variability (Roemmich et al., 2012; Gourtetski et al., 2012) are added.
3-244	3	7	34	7	34	Given that ocean temperature varies, it is absolutely certain, not "virtually certain" that ocean heat content would also vary. [John McLean, Australia]	Accepted. This statement has been removed.
3-245	3	7	38	7	38	"globally averaged temperature changes" of what? Surface ocean? [Janice Lough, Australia]	Accepted. "upper ocean" has been added.
3-246	3	7	39			add "and the use of well resolved in situ data during the 20th century (Argo)" after "... its relation to UOHC" [Karina von Schuckmann, France]	Rejected. While this is a good suggestion, length limits do not permit extensive discussion. In fact, the new text simply notes that the uncertainties have been estimated in three different studies.
3-247	3	7	40	7	40	What purpose? [Janice Lough, Australia]	Rejected. The purpose is clearly listed in the previous sentence, now as "for estimating globally averaged subsurface ocean temperature changes".
3-248	3	7	41	7	41	I would replace "Domingues et al. (2008)" with "Levitus et al. (2012)". My reason is that the estimate of ocean heat content by Domingues et al. is very indirect since it begins the computation of ocean heat content with estimates of sea level change from satellite altimeters, etc. As shown by Levitus et al. (2009) figure S10, this leads to unrealistic features when you plot yearly time series of ocean heat content which Domingues et al. eliminated by smoothing over 3-year intervals. Furthermore in my mind the procedures used by Domingues et al. cast a great deal of doubt on the interannual variability in their time series of ocean heat content. The work of Levitus et al. (2012) also shows the decrease in uncertainty with increasing time (and number of	Rejected. While the Domingues et al. (2008) technique is indirect, its superiority in filling in areas of sparse data coverage and generating a much larger and more realistic uncertainty estimate outweigh its indirect nature, especially in the pre-Argo years where data are sparser This is a long-term assessment. Results from many groups, including those in Levitus

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						observations). [Sydney Levitus, U.S.A.]	et al. (2012) are shown in Figure 3.2.
3-249	3	7	41	7	42	It is incorrect to refer to Palmer and Brohan (2011) as using annual values. Their work is based on monthly values – but still shows the dramatic reduction in uncertainty following the introduction of XBTs into the observing system. Suggest the word “annual” is removed and simply refer to the reduction in uncertainty. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text changed
3-250	3	7	41	7	43	This sentence is ambiguous. I would suggest rewording as "Uncertainties in the annual average UOHC are smaller after 1970 as a result of improved sampling,..." [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. The text has been changed to read "UOHC uncertainty estimates shrink after 1970 with improved sampling, so this assessment focuses on changes since 1971"
3-251	3	7	42			"..., with annual values that shrink ..." There is some ambiguity here. Doe this refer to temperature or error values? Suggest rewording to "error values" [Government of United States of America]	Accepted. The text has been changed to read "UOHC uncertainty estimates shrink after 1970 with improved sampling, so this assessment focuses on changes since 1971"
3-252	3	7	43	7	43	Nice justification for using this period. [Terrence Joyce, United States of America]	Noted. Thank you.
3-253	3	7	45	7	45	The upper ocean temperature trend is mentioned for the 0-700 m layer and for the period from 1970-2010. It can further be subdivided into 0-20 m, 0-400 m and 0-700 m, and covering the time period from 1900-2010, with reasonable confidence in view of a recent paper by Gouretski et al, 2012 . [Government of India]	Partly accepted. The Gouretski et al. (2012) paper is discussed in revision. However, since the bulk of published estimates in the upper ocean are for 0-700 m, that remains the focus of the assessment.
3-254	3	7	47	7	47	If this statement is about surface temperature then say so. If it is about temperature deeper in the ocean then you have a conflict with lines 20 to 34 of page 6, and the conflict either warrants a clarification or removal of text. [John McLean, Australia]	Rejected. While the historal data are sparse in some times and places, studies assessed herein show that changes discussed are statistically significant.
3-255	3	7	47	8	4	Make it clear whether these changes are significant or not? Also, provide reference to statement that much of the warming of the Southern Ocean occurred between the 1930s and 1970s. [Janice Lough, Australia]	Accepted. A second citation to Gille (2008) has been added to the end of this sentence.
3-256	3	7	47			Gouretski et al., 2012 (GRL) should be cited here (and in section 3.2.3) [European Union]	Partly accepted. Gouretski et al. (2012) is now cited here, but not in 3.2.3 because it does not report ocean heat content changes, only temperature changes.
3-257	3	7	47			This paragraph apparently is referring to Levitus et al. (2009); it would be helpful to include that citation in the first sentence. [Government of United States of America]	Accepted.
3-258	3	7	48			It is not clear what is meant by "four small bands of cooling" Can the authors please amplify? [Government of United States of America]	Accepted. Phrase changed to "a few small regions of cooling" as "bands" may be overly technical.
3-259	3	7	49			The language is not clear, what is meant by "a result robust with respect to..." Suggest rewording. [Government of United States of America]	Accepted. The sentence has been modified to read "This result holds in different analyses, over varying multi-decadal time periods, with different bias corrections."
3-260	3	7	50	7	50	"...time periods.." This should be qualified as a 10 year trend can easily be dominated by local NAO variability. Do you mean 'starting year for the multidecadal trend estimate'? [Government of Australia]	Accepted. The sentence has been modified to read "This result holds in different analyses, over varying multi-decadal time periods, with different bias corrections."
3-261	3	7	50	7	50	You probably mean Figure 3.9 not 3.10 [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted.
3-262	3	7	51	7	51	Maximum warming south of 30S? Maximum since when? And surely that should be "temperature", unless you are referring to the rate of warming?? Please clarify. [John McLean, Australia]	Accepted. "warming" is changed to "warming maximum".
3-263	3	7	51	7	52	The start of this sentence, up to the first comma, seemns very odd phraseology. I had to parse it three times and yet I'm still not convinced I understood it. Can it be rewritten in a clearer manner? [Peter Thorne, United States of America]	Accepted. The sentence has been expanded.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-264	3	7	53	7	54	The phrase 'much of the Southern Ocean warming occurred between the 1930s and 1970s' sound funny to me. Do you mean of the warming over much of the area or much of the time? And this statement seems to conflict with that on P9 L46 which implies that the warming took place between 1992 and 2005. [Toby Sherwin, United Kingdom]	Accepted. The phrase has been changed to "much of the warming in the upper 1000m of the Southern Ocean occurred between the 1930s and the 1970s." With the upper ocean specification, the lack of conflict between this statement and the statement regarding deep ocean changes should be more clear.
3-265	3	7	53	7	54	comment on "because much of the Southern Ocean warming occurred between the 1930s and the 1970s": which analysis has shown this? And this is in conflict with what is said later on page 8, l 55-l 56 [Karina von Schuckmann, France]	Noted. The result is presented in Gille (2008). There is no conflict owing to the presence of the word "much", which is not the same as "all". The latter word choice would imply a conflict.
3-266	3	7	54	7	57	1. Consider combining the sentence starting with "Another" and the one starting with "Both". 2. "the warming of the upper Southern Ocean" Does "upper" refer to meters or latitude? [Government of United States of America]	1. Partly accepted. The short sentence starting with "Both" has been combined with the sentence that followed it. 2. Accepted. "upper" has been changed to "upper 1000 m" to make it clear that it refers to a depth range.
3-267	3	7	55	8	4	"broadly consistent" is an extremely vague term. Replace it with proper quantification. I also suggest that the observation is consistent with the ENSO being dominated by conditions on the El Nino side of absolutely neutral (ie. SOI = 0) and the various magnitudes of forces associated with El Nino conditions, the ENSO is after all a continuum, not three states separated by artificially imposed thresholds. [John McLean, Australia]	Partly accepted. The word "broadly" has been removed, however, the higher extra-tropical warming signatures being discussed here are probably not associated with ENSO.
3-268	3	7	56	7	57	that are ALSO in turn by poleward shifts in the westerlies and zonal currents in the Northern hemisphere. (Wu et al 2012, Nature Climate Change) [Terrence Joyce, United States of America]	Accepted. This work is now discussed in the revised Section 3.6.
3-269	3	7	56	8	1	The references in this sentence are misleading. All three papers document changes in the ocean. None of these papers shows any observations of the wind. All three papers cite modelling simulations to support what are rather broad connections drawn between winds and ocean properties. The three references should be put after "Antarctic Circumpolar Current" and modelling studies (Cai et al, 2005; Fyfe and Saenko, 2005, 2006; Bi et al., 2002 ;Saenko et al.,2005) [Melissa Bowen, New Zealand]	Partly accepted. This discussion has been moved to Section 3.6, and Swart and Fyfe (2012) is cited for changes in the surface westerly winds.
3-270	3	7	56	8	2	The Southern Ocean is only weakly stratified and as a result the path of the Antarctic Circumpolar Current is strongly controlled by bottom topography - so it is difficult to 'drive the current southwards'. In models the ACC usually has the form of a braided river so it would be more accurate to say that the mean path has moved southwards with the more southerly braids becoming stronger. [David Webb, United Kingdom]	Rejected. This is not what the observational work suggests.
3-271	3	8	1	8	2	Need to consider Swart et al. 2012 findings. And what about Morrow et al. 's citation? [Catia Domingues, Australia]	Accepted. These changes are discussed in Section 3.6, where Swart and Fyfe (2012) is now worked into the revision.
3-272	3	8	2	8	4	This argument is not rigorous scientifically. Reference should be added when drawing this conclusion. [Xianyao Chen, China]	Rejected. This argument is geometrical, and does not require a reference.
3-273	3	8	3	8	4	The last sentence in the paragraph is confusing because the reader has to have a picture in their head of the mean temperature structure with depth. Consider expanding the previous sentence to explain the pattern "Other zonally-averaged temperature changes are also consistent with a poleward displacement of the mean temperatures. For example, cooling at depth between 30S and the equator is consistent with a southward shift of cooler water nearer the equator." [Melissa Bowen, New Zealand]	Accepted. Text changed to clarify
3-274	3	8	6	8	7	Again you have a conflict with lines 20 to 34 of page 6, in which you said that very little data was available. [John McLean, Australia]	Rejected. The word "relatively" is used to indicate that the data coverage is not ideal.
3-275	3	8	6	8	9	But positive anomalies not evident till ~1990s and certainly not through to 700m till the late 1990s? [Janice Lough, Australia]	Rejected. The figure shows a trend from colder to warmer over the entire time period. In an ocean that warms with time, the mean (1971–2000 as noted in

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							the figure caption) from which the anomaly is calculated naturally leads to anomalously cold conditions early in the record and warm condition later in the record.
3-276	3	8	8			remove "time-" [Toby Sherwin, United Kingdom]	Accepted.
3-277	3	8	11	8	12	Roemmich et al (2012) do not write exactly this... To be more precise, I'd write: "An analysis from the Challenger expedition in the 1870s to Argo (2004-2010) data sets indicates that globally the oceans have been warming at least since the late-nineteenth or early-twentieth century." [BERNARD BOURLES, France]	Partly Rejected. The suggested rewording is not correct because the Roemmich et al. (2012) analysis only shows is that there was warming sometime between the 1870s and the second half of the twentieth century. However, Gouretski et al. (20120) is now discussed here, and they do find a warming trend in the upper 400 m of the word ocean from 1900 to 1945.
3-278	3	8	11	8	12	Need to consider Gouretski et al. 2012 citation. [Catia Domingues, Australia]	Accepted. Reference added
3-279	3	8	11	8	12	Also see paper by Gouretski et al. (2012) looking at longer term warming in upper 400m [John Kennedy, United Kingdom of Great Britain & Northern Ireland]	Accepted. Reference added
3-280	3	8	11	8	12	I would add "may" after "warming". I say this because it is a long time between the Challenger observations and 1970 when you state that estimates become reliable. I would be conservative here. [Sydney Levitus, U.S.A.]	Rejected "suggests" is sufficiently conservative, and the long time between estimates is accounted for in the sentence construction.
3-281	3	8	11	8	12	Has the method of water temperature sampling on the Challenger expedition been taken into account? [John McLean, Australia]	Rejected. The Roemmich et al. (2012) study addresses this issue in detail and shows that all known possible biases arising from Challenger expedition temperature measurement methodology all make the warming estimate a lower bound.
3-282	3	8	11	8	12	While this sentence is true it feels like a very substantive hostage to fortune. My feeling coming from a background in radiosondes is that the Challenger data did not sample enough of the ocean long-enough to know whether it were sampling noise or the true mean state. Also, the instrumentation and methods would have been distinct. I just think that unless this is of imperative import to the section its better off deleted otherwise all the discussion would likely occur around this one sentence and the rest of the very good analysis and summary in this section be forgotten / discounted. To my mind the risk of retaining this sentence far outweigh any possible benefits to its retention. [Peter Thorne, United States of America]	Rejected. While early sampling is sparse, the study findings are bolstered by a new analysis of Gouretski et al. (2012) that starts in 1900. This longer-term warming is of sufficient interest to retain.
3-283	3	8	14	8	17	Caption Figure 3.1 (d): Can we define the ocean surface? Is it top 5 m or 10 m or mixed layer? [Government of India]	Rejected. The ocean surface is the 0 m level in the World Ocean Atlas Climatology.
3-284	3	8	14	8	17	What is the dynamic and climatic significance of the observed increase in upper ocean thermal stratification evident in the 0-200m temperature data (Fig 3.1d)? [Gary Lagerloef, United States of America]	Rejected. This topic is difficult to explore in an observational chapter.
3-285	3	8	14	8	17	While this is all quite clear to an oceanographer, it seems to me that it needs to be said that one would likely not expect the difference to keep growing over time given how wind mixing and seasonal variations do provide some linking of these two levels, so the difference grows as warming starts but then levels off as the heat transport to the deep ocean gets going. [Michael MacCracken, United States of America]	Rejected. Length limits preclude lengthy discussions of future outcomes. Also, this is a chapter on ocean observations, not projections.
3-286	3	8	14	8	17	This sentence doesn't need to mention stratification; the meaning would be clearer if you said that the difference between surface temperature and those at 200m increased from 1975 to 1995 [John McLean, Australia]	Partly accepted. The term density stratification is used here to support discussions throughout the chapter. However, the wording suggested has also been incorporated in the revision.
3-287	3	8	15	8	15	Do not discuss temperature variations in terms of percentages. The percentages are different if one uses Celsius or more correctly in physics, Kelvin, and what are American citizens to think regards Fahrenheit? [John McLean, Australia]	Partly Accepted. This is a variation in a vertical temperature gradient, not a temperature, so it can certainly be expressed as a percentage. Nonetheless,

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							the wording has been modified in revision.
3-288	3	8	20	8	25	Figure 3.1: 1) are data all annual values? 2) panel b) indicate N or S on x axis rather than +ve and -ve) 3) panel d) extend y axis to include lowest data point; why is 5-year RUNNING mean used - weighted filter would be better [Janice Lough, Australia]	1. Yes, the annual values are mentioned in the last sentence of the figure caption. 2. Accepted. 3. Rejected. The lowest data point can be inferred from the graph, and a five-year running mean is simple a well-accepted smoother that is adequate for the purpose here.
3-289	3	8	27	8	39	You should also state the possibility that changes in cloud cover have impacted polar temperatures, and that winds and storms breaking up Arctic ice have caused overall ice loss. [John McLean, Australia]	Rejected. That is the purview of Chapter 4.
3-290	3	8	27	8	39	This whole paragraph felt like very substantive cross-chapter talk and better (more relevant) in Chapter 4 than here. If it is also discussed in Chapter 4 and that either in worst case characterizes it differently or in best case discusses it distinctly then it is an open invitation to language pedants to suggest that IPCC is contradictory. What would be the cost to the section to deleting this text so it is only discussed in the one place? [Peter Thorne, United States of America]	Partly accepted. The discussion of a warming ocean on glacial ice sheets and ice sheet dynamics has been deleted in revision, and is left to Chapter 4. The discussion of the warming Arctic Sea has been retained, as Chapter 4 does not treat this topic.
3-291	3	8	27	8	39	This paragraph seems to drift beyond the remit of chapter 3, into the attribution of changes in the cryosphere, and this should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	Partly accepted. The discussion of a warming ocean on glacial ice sheets and ice sheet dynamics has been deleted in revision, and is left to Chapter 4. The discussion of the warming Arctic Sea has been retained, as Chapter 4 does not treat this topic.
3-292	3	8	33	8	37	The time frame analysed are so short that it is difficult to evaluate the importance of this information. Can this variability be related to the overall trends in climate of the past decade? [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Partly Accepted. This material has been deleted as it is within the remit of Chapter 4, not Chapter 3 (see two comments immediately prior to this one).
3-293	3	8	34	8	38	Inconsistent formatting. L34 has "2003-2005" whilst L38 has "1993 to 2007". On the same page there is "0 to 200 m" (L36) and "0-700-m" (L43). More weeding is needed. [Toby Sherwin, United Kingdom]	Accepted.
3-294	3	8	43	8	43	UOCHs - remove 's' [Government of Australia]	Accepted.
3-295	3	8	43	9	6	A comment is missing for contributions polewards of 60° latitude, as those regions are not included in the ocean global estimations. [Karina von Schuckmann, France]	Accepted. This comment has been added to the text just prior to the UOHC section.
3-296	3	8	43			UOHC not defined - it's in the title but need 'Upper Ocean Heat Content (UOHC)' in the text. I noticed nothing else - an excellent chapter. [Michael Sparrow, United Kingdom of Great Britain & Northern Ireland]	Rejected. UOHC is defined earlier in the chapter.
3-297	3	8	47	8	47	Please clarify what is meant by 'convergence towards instrument bias correction'. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. The reader is referred to section 3.2.1 for a more extensive discussion.
3-298	3	8	50	8	50	Need to add Palmer et al. 2010 (oceanobs white paper) [Catia Domingues, Australia]	Rejected. Palmer (2010) is not published in a refereed scientific journal.
3-299	3	8	55	8	55	This statement is false. Some estimates show a later start year. [John McLean, Australia]	Accepted. The sentence has been changed to note the increase between 1971 and 2010 in all the estimates.
3-300	3	8	55	9	2	Suggest that the authors consider reporting the range in trend estimates across all sources instead of listing the actual trend and range from each source. [Government of United States of America]	Rejected. The different estimates are valuable to show that while estimates are uncertain, they all show increases in UOHC at similar rates.
3-301	3	8	55	9	6	Please identify more specifically solid information in heat content from the uncertain trends by picking out regions where there are more data from regions where the methods need to infer trends from limited information. Figure 3.2 could show different latitude bands and/or ocean basins. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Rejected. The global integral is the quantity of most interest for climate, and the work suggested is not found in the literature. Also, other figures (Figures 3.1 and 3.9) show trends for different latitude bands and ocean basins.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-302	3	8	55	64		This is all very confusing and weak. In the first place Fig 3.2 is extremely difficult to read particularly as most of the lines are masked by the swath of purple error band assigned to L12. All in all I can't make head nor tail of it. But in many ways the problem stems from section 3.2.3 para 2 which is equally difficult to understand because of all the [Js and references which get in the way of the flow. (In fact if I were in charge of these reports I would reformat the references by assigning them numbers in the same way as Nature, for example). Anyway, all that the authors have done here is quote a range of trends, with error bars without any judgement on the quality of the results they are presenting. This is frustrating because the range of UOHCs is about at least a factor of 2 (from 74 to 137, or 43 to 154 if we include error bars) and I get no advice what to make of them. Surely if each paper has equal value why not calculate and plot averages of them all (after all this is what the IPCC does to the models), and if some have greater credence than others then why not say so and perhaps just plot the most robust one? [Toby Sherwin, United Kingdom]	Partly accepted. Fig. 3.2 has been expanded from single-column-width to double and the uncertainty envelopes have been made more transparent to make it easier to read.
3-303	3	8	57	8	57	"yields a poer" - what does this mean? [Janice Lough, Australia]	Accepted. The phase has been expanded to note that the numbers that follow are estimates of the power required to account for this warming.
3-304	3	9	1	9	1	Ishii's estimate is being revised. Need to consult with Ishii. [Catia Domingues, Australia]	Accepted. Ishii has been written and says he has no new estimate at this time.
3-305	3	9	6	9	6	the sentence is obvious I suggest to delete [VINCENZO ARTALE, ITALY]	Rejected. This is an important point that helps to assess the estimates.
3-306	3	9	6	9	6	Make clear that this assumption is a naïve / invalid assumption. Its impossible that the anomaly is zero everywhere we don't sample. The ocean is a fluid body and such implied sharp gradients could not possibly be retained so this is a naïve / conservative estimate that will tend to damp both trends and variability by construction. [Peter Thorne, United States of America]	Partly accepted. A sentence has been added to the end of this paragraph noting why one of the estimates is chosen for Box 3.1.
3-307	3	9	9	9	14	Figure 3.2 - unclear to me what these values are anomalies from; also last sentence of figure caption is very unclear. [Janice Lough, Australia]	Rejected. Space is limited to discuss this subject. The last sentence of the figure caption is clear to us.
3-308	3	9	16			Section "3.2.4 Deep Ocean Temperature and Heat Content": The limited amount of data (correctly pointed out by the authors) necessitates description of where these data sets are spatially - perhaps mark repeat ship tracks and/or long-term ocean station data on Fig. 3.3b. This would clearly show where the changes in deep ocean temperatures are more likely, and where spatial gaps are large. [Government of United States of America]	Accepted. The ship tracks are included in the revised version of Figure 3.3b.
3-309	3	9	18	9	29	In addition to stating their presence, t would be helpful to have the authors include a discussion of why these stratified trends exist. [Government of United States of America]	Accepted. Some text has been moved up from the following paragraph to this one, and material about why there is no change from 2000-3000 m added.
3-310	3	9	19	9	19	I would change 1500 m to 1750 m because it would be more accurate. [Sydney Levitus, U.S.A.]	Partly accepted. A phrase has been added to note that the well sampled depth is approaching 2000 m in recent years.
3-311	3	9	20	9	20	Here, and elsewhere, why not just use "5-year" rather than "pentadal"? [Janice Lough, Australia]	Accepted.
3-312	3	9	20	9	20	pentadal should be defined as (five year average) or (five day average) on first use, its not clear which from the text and also its 'inside baseball' terminology that will only make sense to a very few climate scientists let alone members of the broader public. [Peter Thorne, United States of America]	Accepted.
3-313	3	9	20	9	21	Given that we only have data for about 7 years it's entirely unreasonable to have any confidence in estimates that go back to 1957 (more than 50 years). [John McLean, Australia]	Rejected. There are sub-surface ocean data going back to the Challenger expedition in the 1870s. There were many data taken in the 1950s, some associated with the International Geophysical Year. The referenced literature discuss these data in detail.
3-314	3	9	21	9	21	If possible Fig. 2 (0-2000 m) UOHC linear trend as a function of latitude) from Levitus et al. (2012) should be shown here and Fig. S3 (per cent variances accounted for by the linear trends shown in Fig. 2 from the same	Partly accepted. The 700-2000 m time-series of globally integrated OHC from Levitus et al. (2012) is

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						paper. I think these figure(s) would add a great deal to this chapter. If not here, then these figures should be added and discussed at some place in the manuscript. [Sydney Levitus, U.S.A.]	now shown in the revised Figure 3.2b and discussed here.
3-315	3	9	24	9	26	the paper ponter2012 could be cited here: Ponte, R.M., 2012: An assessment of deep steric height variability over the global ocean, GRL, 39, L04601, doi:10.1029/2011GL050681 [Karina von Schuckmann, France]	Accepted.
3-316	3	9	26	9	29	This time you are claiming that accurate (or reasonably accurate) data exists back to 1992, 3 times as far back as we have data for. This is unreasonable. [John McLean, Australia]	Rejected. There have been many expeditions since the 1990s, first associated with the World Ocean Circulation Experiment and more recently with CLIVAR to collect accurate full-depth oceanographic data. The referenced literature discusses these data in detail.
3-317	3	9	26	9	29	This sentence is almost incomprehensible. Reword. [Toby Sherwin, United Kingdom]	Accepted. The sentence has been expanded and broken into two.
3-318	3	9	27	9	28	The statement 'warming of the global ocean from circa 1992-2005 is likely not distinguishable from zero between 200 and 3000 m depth' is clunky - suggest reversing and simplifying i.e. 'It's likely that there was no warming between 2000-3000m depth'. [Government of Australia]	Accepted. Text revised and clarified
3-319	3	9	27	9	29	The chapter needs to explain if the lack of temperature trends in the 2000-3000 depth is expected or not, either from known ocean circulation processes or from decadal variability in the water masses. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. Text revised and clarified
3-320	3	9	27			This sentence seems to contradict the rest of the paragraph. The statement here is that there has been no warming between 2000-3000 m, but the previous discussion says the data are too sparse to draw conclusions. [Government of United States of America]	Accepted. Text revised and clarified
3-321	3	9	28	9	29	Yes, this is supported by Fig. 3.3a, while what is discussed below is NOT. Please re-work this discussion. [Terrence Joyce, United States of America]	Accepted. The discussion has been reworked substantially.
3-322	3	9	28	9	29	If you claim that the ocean has warmed from 3000m to the ocean floor (what depth are you assuming because depth varies greatly?), then where is the explanation of how this water could warm? What are the physical process and the physics to support such a contention? [John McLean, Australia]	Accepted. The text has been reworked to include discussion of why the mid-depths have not warmed while the deepest waters have.
3-323	3	9	31	9	37	Figure should be redone using 90% Cis as per TSU guidance if possible. [Peter Thorne, United States of America]	Accepted.
3-324	3	9	32	9	32	Fig.3.3a: "during 1992-2005" should be indicated. [Dongxiao Wang, China]	Accepted.
3-325	3	9	32	9	37	Figure 3.3: Example where period over which "warming rates" have been calculated should be provided. [Janice Lough, Australia]	Accepted.
3-326	3	9	38	9	39	Shouldn't it be mentioned that the data of Purkey and Johnson 2010 and Kouketsu et al. 2011 on abyssal warming (Figure 3.3) uses not only temperature measurements but also sea level data? The text of the paragraph above this statement does not mention the sea level method. Only in subsection 3.2.5 which concludes the section, one learns about the method but it is not mentioned that the abyssal warming data is calculated using sea level. [Government of Poland]	Rejected. These studies do not use sea level data. Section 3.2.5 notes that rising sea level is consistent with warming of the ocean over all depth ranges.
3-327	3	9	39	9	40	Do you really think you can give one paper and sparsely sampled data much credibility? If not then remove it. [John McLean, Australia]	Rejected. This is a very strong signal, and while Purkey and Johnson (2012), and Kouketsu et al. (2011) are the global syntheses, there are many other studies that they reference which support their conclusions.
3-328	3	9	39	9	40	This is in contrasts to what is said on page 7, l 53-54 [Karina von Schuckmann, France]	Accepted. The earlier text has been rephased to read "and because warming in the upper 1000 m of the Southern Ocean was stronger between he 1930s and

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							the 1970s than between the 1970s and 1990s (Gille, 2008)."
3-329	3	9	39	9	43	It is not clear what this paragraph is getting at. What does it mean? [Government of Australia]	Accepted. This paragraph has been combined with the previous paragraph, condensed and rewritten.
3-330	3	9	39	9	43	Should note that the ventilation age of AABW is young enough that the temperature during formation was affected by a warmer surface ocean (The flow of Antarctic bottom water to the southwest Indian Ocean estimated using CFCs, Haine et al. 1998). Because water mass formation also integrates over short term variability stresses the idea that the observed warming trend was likely a result of a climate signal. [Andrew Shao, United States of America]	Partly Accepted. Much of the discussion of AABW has been moved to Sections 3.5 and 3.6, where the work of Huhn et al. (2013) and Orsi et al. (1999) with transient tracers are discussed, including changes in ventilation detailed in the more recent work.
3-331	3	9	39	9	51	Please reference figure 3.3a after the first or second sentence (lines 39-41) – only figure 3.3b is cited currently. The text states that global ocean warming is largest near the sea floor while the figure shows significantly greater warming between 1000m and 2000m. It is also not clear whether the global numbers include the southern ocean or are global minus the southern ocean. [Government of United States of America]	Accepted. These paragraphs have been reworked, but Fig. 3.3a is referenced early in them. The text states that the deep warming is largest near the sea floor.
3-332	3	9	40			Near 4500m. There is NO information about the depth of the sea floor in the figure, and what is plotted clearly shows the max warming is ABOVE the deepest levels shown. Suggest re-wording. [Terrence Joyce, United States of America]	Accepted. These paragraphs have been reworked.
3-333	3	9	42	9	42	Suggest inserting the word 'more' between "...in basins that are' and 'effectively ventilated...." [Government of Australia]	Partly accepted. The "ventilated" portion has been reworked as that word could be thought of as jargon.
3-334	3	9	45	9	45	You are relying on that one paper again, but this time stating a rate of warming without specifying the whether the figure was an average rate per year or decade or ???, and you don't state what happened after 2005 or even if the data was included in the 2010 paper. This needs clarification. [John McLean, Australia]	Rejected. While this particular portion has been deleted in reworking the figure, Watts are a measure of power, in this case a rate of heat accumulation. It is clear in the text that this rate is calculated as a trend over the measurement period.
3-335	3	9	45	9	46	Change to "The combined warming....below 1000m [removed combined] amount to a heating rate...." [Government of Australia]	Partly accepted. The paragraphs in this section have been reworked, and the heating rate has been left out of the box.
3-336	3	9	45	9	57	This is quite confusing. In the S. Ocean, 'trends' are calculated for the short period between 1992 and 2005 and are interpreted as global warming, yet a much longer (multi-decadal) period for NADW is associated with (natural?) variability. This is quite uneven and probably reflects the amount of data and the conclusions in the different citation sources. Yet for this report, there has to be some regularization! [Terrence Joyce, United States of America]	Accepted. The recently published deep (> 2000 m) heat content change estimates of Mauritzen et al. (2012) for the N. Atlantic are now discussed and compared to the global estimates of Purkey and Johnson (2010) in the revision. There is no direct attribution of either change to "global warming".
3-337	3	9	49	9	49	Why use atmospheric jargon like "teleconnections" when the physics is actually described? "teleconnections established" could be replaced by "communication" [Melissa Bowen, New Zealand]	Accepted.
3-338	3	9	53	9	57	Does PDO/IPO play a role in Pacific? Or do we not know? [Janice Lough, Australia]	Rejected. Unlike in the Atlantic, deep and bottom water is not formed in the Pacific, so the PDO/IPO are not really germane to this discussion.
3-339	3	9	55	9	57	I get the sense that the authors wanted to go home at this point and just wrote something to fill the gap. You could at least give us a sense of the scales of the decadal variability and trend of NADW temperatures. [Toby Sherwin, United Kingdom]	Accepted. The recently published deep (> 2000 m) heat content change estimates of Mauritzen et al. (2012) for the N. Atlantic are now discussed and compared to the global estimates of Purkey and Johnson (2010) in the revision.
3-340	3	10	3	10	9	As one of the three pieces of evidence for warming of the upper ocean since the 1970s is sea-surface temperature data, might a statement be added that warming of the upper ocean is likely (or some such word) to have occurred since much earlier than the 1970s, due to warming seen in the sea-surface temperature	Accepted. Text has been revised and temperature data since 1870 are assessed

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						record? [Adrian Simmons, United Kingdom]	
3-341	3	10	5	10	5	same remark that n°1: Sea Surface Temperature in Section 2.4.2 [BERNARD BOURLES, France]	Accepted. Correction made.
3-342	3	10	5	10	5	This is Section 2.4.2 and not Section 2.2.2 [Peter Thorne, United States of America]	Accepted. Correction made.
3-343	3	10	10	10	11	It is strange that this conclusion appears here but was not stated in the main text discussion. [Government of United States of America]	Rejected. This conclusion is discussed in section 3.2.2.
3-344	3	10	11	10	12	There are still unresolved issues with XBT(MBT) biases and particularly near-surface. I suggest some caution w/ respect to the 4%. [Catia Domingues, Australia]	Accepted. The uncertainty language "very likely" is employed in the revision. Note that "about 4%" is used here as well.
3-345	3	10	15			again inconsistent with page 7, l 53-54 [Karina von Schuckmann, France]	Accepted. The earlier text has been rephased to read "and because warming in the upper 1000 m of the Southern Ocean was stronger between the 1930s and the 1970s than between the 1970s and 1990s (Gille, 2008)."
3-346	3	10	16	10	17	see comments above: in text multi-decadal variability is mentioned for NADW, but AABW conclusions are only based on ca. a decade of data! [Terrence Joyce, United States of America]	Accepted. To be more regular, simply the observed changes are discussed in the conclusion, without mention of multi-decadal variability.
3-347	3	10	21	10	29	The use of the word "effectively" might be reconsidered or reworded to more precise. [Government of United States of America]	Accepted. "effectively" has been changed to "readily"
3-348	3	10	21	11	26	Box 3.1 Change in Global Energy Inventory; The findings from the study "Importance of density-compensated temperature change for deep North Atlantic Ocean heat uptake" by Mauritzen et al. should be reflected in Box 3.1. Especially the fact that it explains the decadal variability in the ocean heat uptake and how it compares the recent decade with the 1960s is interesting because of the link to global temperature development. http://www.nature.com/ngeo/journal/vaop/ncurrent/full/ngeo1639.html [Government of NORWAY]	Partly accepted. This newly published work is now discussed in section 3.2.4, and its findings compared with the global assessments.
3-349	3	10	21	11	26	This box is really nice overall but I fear that the use of esoteric and scarily large numbers of ZJ etc. will be hard for scientists and non-scientists alike to consume and interpret. Can an analogy be given in terms of e.g. equivalence to US national annual energy usage or how high some object would be lifted or some other analogy that actually makes these numbers more accessible to both science and lay audiences? [Peter Thorne, United States of America]	Rejected. That sort of analogy would be very appropriate to a FAQ, but this is a box, which is supposed to address cross-chapter issues.
3-350	3	10	23	10	24	Don't talk about radiation imbalance as if it is a new phenomena. Earth is always in radiative imbalance and is always trying to redress that imbalance. This is easily demonstrated by the diurnal cycle of heating and cooling, and by seasonal variations. Cloud cover isn't constant around the world, nor are winds (which distribute heat energy), nor does radiative transfer occur equally over land and sea, so your statement about energy balance is false. [John McLean, Australia]	Rejected. There are many refereed scientific studies (Church et al. 2012; Murphy et al. 2009 J. Geophys. Res.) that demonstrate that Earth has been in substantial radiative imbalance over at least the past few decades. This decadal imbalance is a climate phenomena, very different from diurnal, seasonal, or even interannual imbalances.
3-351	3	10	23	10	24	This opening sentence could be misconstrued as implying there has been an increase in solar radiation recently by the unwary. It should be redrafted to avoid the potential for such an interpretation. Solar radiation has changed little and may have actually decreased from a relative maximum attained in the late twentieth Century. [Peter Thorne, United States of America]	Accepted. The sentence has been redrafted.
3-352	3	10	23	11	26	Box 3.1 – This box is full of jargon and would be more effective if it was shortened and in plain language. [Government of United States of America]	Rejected. This box cannot be shortened without loss of traceability. Suggestions for reducing jargon would have been welcomed. Revisions are not obvious in the absence of specific suggestions along these lines.
3-353	3	10	24	10	24	Should also cite Murphy et al. 2009 JGR? [Catia Domingues, Australia]	Accepted.
3-354	3	10	24	10	24	I think the pioneering paper: Levitus, S., Antonov, J. Wang, T. L. Delworth, K. W. Dixon, and A. J. Broccoli,	Rejected. This assessment has been directed to

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						2001: Anthropogenic warming of earth's climate system. Science, 292, 267-270. should be cited here also. [Sydney Levitus, U.S.A.]	focus on advances since the last assessment.
3-355	3	10	24			Change "useful" to "essential". [Stephen E Schwartz, United States of America]	Accepted.
3-356	3	10	25	10	25	Insert "radiative" before the word forcing in line 25. [Government of United States of America]	Accepted, but what about eliminating jargon (see comment 3-352)?
3-357	3	10	25	10	27	The oceans warm by insolation. Any downwelling radiation is absorbed within the first few microns of the oceans' surface and disappears rapidly in evaporation. Heat rises in the ocean and is transferred to the air above, where it is carried aloft by convection. If you are going to try to claim that some "excess energy" warmed the ocean below the surface then you need to show a convincing account of the physics involved. [John McLean, Australia]	Rejected. First the ocean heat gain occurs as a result of an imbalance in the net exchange of heat across the ocean surface. This could mean less heat is leaving the ocean because of a warmer atmosphere above. Second, longwave, sensible, and latent heat fluxes are all mediated by the ocean mixed layer, where turbulence keeps ocean properties vertically homogenous, usually to depths of at least 20 m, but sometimes (owing to convection) to depths of 2000 m or more in winter.
3-358	3	10	26	10	27	Line 26 says "the oceans" and line 27 says "the ocean". Assume that there is a style rule for this. [Government of United States of America]	Accepted.
3-359	3	10	27	10	28	State the heat content of the ocean and the atmosphere. [John McLean, Australia]	Rejected. That information easily available and not central to assessing the energy imbalance.
3-360	3	10	28	10	29	Consider changing the last sentence to "...have a very low albedo and absorb solar radiation more effectively than ice." [Government of United States of America]	Accepted.
3-361	3	10	28	10	29	It's not as simple as saying that ice free ocean absorbs more solar radiation. The polar regions receive little or no radiation at some times of year, radiation striking at a shallow angle and therefore mainly reflecting at other times and, for just a brief period, long days where the angle of incidence doesn't mean a large loss and the total daily solar irradiance is reasonable. The times of lowest ice cover occur near the equinoxes, when incoming radiation strikes at a low angle of incidence. [John McLean, Australia]	Rejected. The statement is accurate on the global average, and this box is about a global average.
3-362	3	10	28			more effectively absorb ... insert "more" [Terrence Joyce, United States of America]	Accepted. This sentence has been rewritten.
3-363	3	10	31	7	12	Box 3.1: Loeb et al., 2012 in Nature Geoscience estimates the observed changes in top-of-atmosphere radiation and upper-ocean heat content, derived from various resources (satellites, climate models and in situ measurements), and conclude the extra energy is continuing to increase into the sub-surface ocean. His work should be added in this section. [Xianyao Chen, China]	Rejected. The work of Loeb et al. (2012) discusses interannual variability since 2000, whereas this box focuses on changes integrated over 40 years.
3-364	3	10	32	10	38	Caption of Box 3.1 Figure 1 has various objects 'starting', for example: "with below 2000 m starting", "Greenland starting", etc. I would add the word "data" before every instance of "starting". [Government of Poland]	Accepted, except "estimate" is used, not "data".
3-365	3	10	40	10	44	It seems that there are lost bits of text in this passage. [Leticia Cotrim da Cunha, Brazil]	Rejected. The sentence is long, but complete.
3-366	3	10	40	11	5	This section needs to be placed elsewhere. It is not a conclusion of anything previously presented - in fact this is the first time I have seen it! It definitely does NOT belong here. I am aware that it is a 'box' but it seems out of place and unsupported in this section, at least for the atmosphere and solid earth. Perhaps it can be retained but a reference must be made to sections elsewhere in this report where this is discussed. [Terrence Joyce, United States of America]	Rejected. This is a box, which is cross chapter. Furthermore, references for all estimates outside the ocean are either given in the box, or made other chapters.
3-367	3	10	42	10	42	change "but version 3.3" to "updated to version 3.3". [Government of United States of America]	Accepted.
3-368	3	10	42	10	43	Confusing text - what is version 3.3? This suffers from the same problem as my comment above. [Terrence Joyce, United States of America]	Partly accepted. "But version 3.3" has been changed to "updated to version 3.3".
3-369	3	10	43	10	43	The statement "by the ratio of the portions..." is confusing. Consider adding the word "weighted" to clarify?	Accepted.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[Government of United States of America]	
3-370	3	10	48	10	48	Change "are neglected here" to "are considered negligible". [Government of United States of America]	Accepted.
3-371	3	10	53	10	54	Needs to state the period over which the 6TW applies (annual average? Decadal average? Total?) [John McLean, Australia]	Rejected. The 6 TW applies to the entire time period, 1950–2000, which is clear in the text as written.
3-372	3	10	54	10	56	This statement is unsustainable because HadCRUT4 shows no warming over the last 16 years (and the HadCRUT3 trend from January 1997 to July 2012 is flat). [John McLean, Australia]	Rejected. The thermal inertia of the earth means that it will continue to absorb heat from an atmosphere if it is warmer than for previous decades or centuries. It does not have to warm continuously.
3-373	3	10	55	10	56	In Chapter 2 Land Surface Air Temperatures (LSAT) are used. This distinguishes from Land Surface Temperatures which are the temperature of the land surface itself sensed by satellites. Hence air should be added here. Unless discussion is really about the skin temperatures observed by e.g. AQUA in which case as these are not discussed in Section 2.4.1 (not 2.2.1) the reference to the Chapter 2 section text should be dropped and a suitable reference to a paper describing the AQUA LST records appended. [Peter Thorne, United States of America]	Accepted.
3-374	3	10	56	10	56	Relevant section in Ch2 to be referred to is 2.4.1. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.
3-375	3	11	2	11	5	The long second sentence in this paragraph ("The heat of fusion...are neglected here") is awkward and confusing. Suggest changing "are neglected" to "are considered negligible". [Government of United States of America]	Accepted.
3-376	3	11	7	11	14	This paragraph should mention error margins for all papers, not just one. [John McLean, Australia]	Rejected. One upper ocean heat content estimate is selected because it uses ocean statistics to infill sparsely sampled regions and to estimate uncertainties. These selection criteria are now noted in section 3.3.3.
3-377	3	11	7	11	24	This contains bad English and needs a rewrite. On line 9, 137 TW is not a linear trend, it is an estimate of a heat flux. Similarly on line 13. On line 16, 273 TW is not a first-difference change it is an estimated heat flux. (Ask what a second difference change would be?). Also 273 is not "with" anything but it might correspond to average flux of 213 TW over the same time period. For the period 1993-2000 the estimated heat gain might be 163 ZW, corresponding to a flux of 270 TW (not 27!) [David Webb, United Kingdom]	Accepted. The paragraph has been rewritten.
3-378	3	11	9	11	9	Please explain what 137 TW is used here rather than an average of the estimates presented on p.9 line 1. 137 is at the top of the range, is it more believable than the other estimates? if yes this needs to be mentioned on p.9. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. One upper ocean heat content estimate is selected because it uses ocean statistics to infill sparsely sampled regions and to estimate uncertainties. These selection criteria are now noted in section 3.3.3.
3-379	3	11	12	11	13	Why assume that there was no warming below 2000m prior to 1992? Besides which, at some point in history (over the last 6 billion years) there must have been. [John McLean, Australia]	Partly accepted. There are very few deep data globally prior to the 1990s on which to estimate trends. There are some data in the Weddell Sea which suggest trends may have started earlier, but data in the South Atlantic suggest a 1991 start to deep warming there. These results are added to section 3.2.4. As to the billions of years comment, this box starts with 1971, not six billion years ago.
3-380	3	11	13	11	14	You need to explain how the ocean below 700m warms. The principal source of ocean warming is insolation, but it doesn't penetrate to 700m. Downwelling radiation from CO2 is absorbed at the ocean surface and disappears in evaporation. Warm water rises and transfers its heat to the atmosphere where the heat is	Rejected. First the ocean heat gain occurs as a result of an imbalance in the net exchange of heat across the ocean surface. This could mean less heat is

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						carried away by evaporation and convection. If you can't satisfactorily explain the physical processes by which the ocean below 700m warms then large parts of the text should be removed. [John McLean, Australia]	leaving the ocean because of a warmer atmosphere above. Second, longwave, sensible, and latent heat fluxes are all mediated by the ocean mixed layer, where turbulence keeps ocean properties vertically homogenous, usually to depths of at least 20 m, but sometimes (owing to convection) to depths of 2000 m or more in winter. Around Antarctica and in the Northern N. Atlantic, cold water sinks to the bottom in dense plumes. Third, in the ocean currents carry colder water from the surface high latitudes poles to deeper in the ocean at lower latitudes. As the ocean surface warms, these sinking waters at high latitudes become warmer, and transmit this signal to the subsurface ocean.
3-381	3	11	16	11	24	Confusing: in alternating sentences the period is changed from 1971-2010 to 1993-2010 and then back and then forward again. RE-write such as the rate of global energy change is estimated to be (xxx) from 1971-2010 and (yyyy) from 1993-2010, suggesting And please check your numbers: results indicate a reduction in rate of energy increase in the latter period (213TW vs 27TW). [Terrence Joyce, United States of America]	Accepted. The 27 TW is a typo, it has been corrected to 270 TW.
3-382	3	11	16	11	24	Where are the references that support these claims and what do other references say about the situation? [John McLean, Australia]	Rejected. The references are given in the paragraph above, and the other references are discussed in Section 3.2
3-383	3	11	16			Please define "first-difference change." [Government of United States of America]	Accepted. This terminology has been expunged from the revision.
3-384	3	11	17			"change of 273 ZJ". Is this cumulative? Is it accelerating? [Government of United States of America]	Partly accepted. The text has been rewritten to clarify that this is an increase between two specific years, 1971 and 2010.
3-385	3	11	18	11	19	"the estimated energy gain is from a first difference is 163ZJ" - one "is" should be deleted. [Sonya Legg, United States of America]	Accepted. This text has been rewritten.
3-386	3	11	19	11	19	What is a "linear rate estimate"? [Janice Lough, Australia]	Accepted. This has been rewritten.
3-387	3	11	19	11	19	27TW should be more like 257 TW or 270TW. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. This was a typo, which has been corrected to 277 TW.
3-388	3	11	20	11	20	I would cite Levitus et al. (2012) who published that the world ocean accounts for 93% of the increase in heat content of the earth system. [Sydney Levitus, U.S.A.]	Accepted. The reference is now cited at the start of the Box 3.1
3-389	3	11	23	11	24	When reporting heat fluxes crossing the ocean surface, in units of W/m ² , the authors have not specified if the area normalizing the fluxes refers to total area of the earth's surface, including land and ocean, or just the ocean area. When atmospheric scientists report heat fluxes, they generally refer to the total surface area of the earth. However, many ocean scientists consider just the ocean area since this is the appropriate area for performing heat budgets for the ocean. It is critical to note what is the convention being used, since the heat flux will differ by a factor of roughly 0.7. [Stephen Griffies, United States of America]	Partly accepted. While "air-sea flux" implies a flux across the ocean surface, this has been made more explicit in revision.
3-390	3	11	25	11	26	"which is dominated by decadal variability from atmospheric modes like the North Atlantic Oscillation (NAO)". This is arguable. NAO does have decadal variability and it does influence ocean parameters but whether the decadal variability in overturning circulation (influencing salinity practically by definition) is NAO forced is not a closed case. On the contrary, models showing that NAO is better predicted by models with better SST (Scaife et al. 2011, doi:10.1029/2011GL049573) are a hint of an inverse relationship: possible influence of AMO on NAO on decadal scales or possibly the two indexes being manifestations of the same multidecadal variability involving Atlantic Ocean and North Hemisphere atmospheric circulation. This does not mean an AMO -> NAO influence is proven but simply that "the dominance" of atmospheric parameters on salinity is not certain basing on present knowledge. Actually the text here should be made compatible with the one in page 3-13 line 54-56	rejected. In 3.3.2.1 salinity is discussed and not whether there is a relationship between salinity and AMOC. Atmospheric modes like the NAO are responsible for changing circulation pattern (for instance zonal extension of the subpolar gyre), modifying the salinity distribution in the upper ocean. the statement does not exclude other atmospheric modes from contributing, the NAO is named as an example

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						which says "Decadal and multi-decadal variability in the subpolar gyre and Nordic Seas is vigorous and has been related to various climate modes such as the NAO, the Atlantic multidecadal oscillation(AMO), and even ENSO (Polyakov et al., 2005; Yashayaev and Loder, 2009), obscuring long-term trends. [Government of Poland]	
3-391	3	11	28			Section "3.3 Changes in Salinity and the Freshwater Budget": As was done in the section on SST, it would be appropriate to include a few sentences about how salinity measurements have been made over the last 50 to 100 years. This addition should not be lengthy, would make this section consistent with others in this chapter. [Government of United States of America]	Accept. Changed to "...and Freshwater Content", which is even more conservative
3-392	3	11	32	11	32	The analysis of Trenberth et al., 2007 suggests these numbers are 77% and 85% respectively, when considering up-to-date terrestrial P and E estimates. These numbers are presented graphically in the Durack et al., 2012 (Science) supplementary material, supplementary figure 1. [Paul Durack, United States]	Accept. Changed the numbers and add the reference of Trenberth et al.(2007). We retain the Schmitt (2008) reference because it also provides an introduction to the whole subject, including concept of salinity as rain gauge.
3-393	3	11	32			Are the percentages quoted in this line known to an accuracy of a percentage point? Can an estimate or at least a verbal indication of uncertainty be given? [Adrian Simmons, United Kingdom]	Accept. Added the word "about" and another reference.
3-394	3	11	33			This reference (Schmitt) and the figures seem redundant - I recall seeing them earlier in the chapter. Please consolidate. [Terrence Joyce, United States of America]	Reject. Statement in Introduction (p. 3-6, line 7-8) that cites Schmitt (2008) did not include actual percentages.
3-395	3	11	35	11	35	after "precipitation" maybe add "and large river discharges, (eg. Amazon and Congo)" [BERNARD BOURLES, France]	Accept.
3-396	3	11	35			Fig. 3.4ab. I know these are from original references, but it would have been better to plot them as annual rates of change, since the time periods are different. This would then allow a better comparison of possible acceleration or reduction in rate change. [Terrence Joyce, United States of America]	Accept. Since these are not derived in the same method, contours and colors are changed to be equivalent.
3-397	3	11	39	11	39	Please add a sentence " A temporary shift in salinity can also occur due to change in terrestrial water storage capacity in response to coupled ocean-atmospheric interactions (Boening et al., 2012)". [Government of India]	Taken into account. Sentence modified.
3-398	3	11	41	11	43	"The water cycle is expected to intensify in a warmer climate because warm air can contain more moisture". No. See page 9, lines 16 and 17 which state that "precipitation will have a smaller fractional change than the water content of air as the climate warms". I would agree that the humidity is expected to increase, but once it has increased there is no reason to expect evaporation to remain high unless there is more rainfall. There is no evidence of more rainfall and model of clouds and rainfall are not good - see the following lines. "The dominant effect is due to the Clausius-Clapeyron relation: equilibrium water vapour pressure increases by about 7% per degree (at the current global temperature of about 14C)". 7% is the figure given by Durack et al. (2012) assuming the humidity is fixed. However as stated in lines 45 to 47 the "water vapour content of the atmosphere has increased since the 1970s at a rate consistent with the observed warming". Thus the 7% figure is invalid for the real world and should not be used here. [David Webb, United Kingdom]	Accept text change. All of Clausius-Clapeyron text deleted, reader referred to relevant FAQ 3.3 and Sections 2.5 and 12.4.5. Our usage of "water cycle" was not inconsistent with these 2 chapters.
3-399	3	11	41	11	50	The discussion of water cycle in relation to atmospheric warming is inappropriate. The water cycle is not expected to "intensify" as a result of warming or increasing amounts of water vapor in the atmosphere. As a matter of fact, the turnover of water vapor in the troposphere is faster in winter than in summer simply because the amount of water vapor stored in the atmosphere is larger in summer while the fluxes (either total evaporation or total precipitation) remain essentially constant. Furthermore, the rate of the global cycle (as measured by global precipitation for example) is only weakly affected by atmospheric warming, not on account of "feedbacks and atmospheric dynamics" but because of the thermodynamical fact that evaporation is governed by the energy available at the surface (net surface radiation budget), not by the amount of moisture in the atmosphere above. Thus, to first order, the rate of the global water cycle has nothing to do with	Accept text change. All of Clausius-Clapeyron text deleted, reader referred to relevant FAQ 3.3 and Sections 2.5 and 12.4.5. Our usage of "water cycle" was not inconsistent with these 2 chapters.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Clausius-Clapeyron and the like. For this reason, lines 41 to 50 of Section 3.3.1 are irrelevant to a discussion of oceanic processes and inconsistent with the judgment of the cloud and radiation experts, expressed in Chapter 7 Executive Summary (Chap.7, page 5, lines 30-32 and sections 7.6.2, 7.6.3). For these reasons. The corresponding statements lines 41-50 from "The water cycle..." to .. "evaporation observations (Section 3.4)." should be DELETED. The remaining should be retained, beginning: "Ocean salinity naturally integrates...". [Government of France]	
3-400	3	11	42	11	42	I would replace "due to" with "governed by". [Sydney Levitus, U.S.A.]	Accept.
3-401	3	11	43	11	45	It is not sufficient to simply say "feedbacks", they need to be described. [John McLean, Australia]	Accept. Removed phrase altogether as it was too vague and it was not deemed important to expand.
3-402	3	11	44	11	44	...increase in total global precipitation per unit warming'. It would be helpful in this early paragraph to immediately head off confusion caused by what the readers think of as the water cycle: either global average P (= global average E) or the regional patterns and lateral cycling of moisture. As salinity patterns respond to the latter, that is what you should focus on and state as the aspect of the water cycle we can consider. The theoretical discussions on global average P and E (theoretical as these are unmeasurable to the few per cent required and no one lives under them anyways) should be recognised but then sidelined here as it has little relevance to salinity changes. [Government of Australia]	Accept. Language is clarified. Our usage of "water cycle" was not inconsistent with usage in chapters 2 and 12.
3-403	3	11	45	11	47	The sentence spanning these lines needs to be qualified. It does not apply at low levels over land. See section 2.5.5 and Figure 2.30, and several earlier comments on Chapter 2. [Adrian Simmons, United Kingdom]	Taken into account. We did not mean to imply anything about conditions over land.
3-404	3	11	45	11	48	Given the uncertainties associated with process measurements, it is confusing to have this preceded by the declarative statement regarding atmospheric water vapor. The word "However" may exacerbate this confusion. [Government of United States of America]	Accept. Sentence reworded
3-405	3	11	47			It might be more precise to say that "observations of precipitation and evaporation are sparse over most of the ocean." Many areas over land have dense observing networks... [Government of United States of America]	Accept. Sentence reworded
3-406	3	11	50	11	51	This is very nice, and it should be mentioned that ocean heat content change provides a better measure of increased greenhouse forcing than direct flux measurements in a similar fashion. [Terrence Joyce, United States of America]	Noted. No additional heat content language added here.
3-407	3	11	52	11	52	A number of studies preceding Yu (2011) address the concept of the ocean as a rain gage and salinity trends as an indicator of the changing water cycle. Lagerloef, et al (2010, Oceanography, 23(4) provide a partial review, and discuss the challenges of closing the marine freshwater budget. [Gary Lagerloef, United States of America]	Taken into account. Schmitt 2008 now listed. AR4 (2007) certainly referred to the concept but didn't use the phrase "rain gauge". Article Terray et al. (J. Clim. 2012) also added as it includes comparison between Atlantic and Pacific.
3-408	3	11	54	15	16	I. As it is stated in the paper (Chapter 3, p.3-11, lines 54-56) "salinity changes affect sea level". However there is no clear description given for the effect of the salinity contraction of sea water. It would be beneficial to add a sub-section reflecting an importance of the halo-steric contribution to steric height (SH) variability. In common case the contribution is opposite to the thermo-steric one and could correct SH trend toward the observed sea surface height (SSH) trend. Some evidences for that have been collected in certain regions of the Tropical Pacific and Southern Ocean (Maes, 1998), and in low and mid-latitudes (Sato et al. 2000). More recently, Argo floats data imposed the opportunity to estimate relative contribution thermosteric and halosteric components on global/regional scale, i.e. for North Atlantic (Ivchenko et al. 2008). This effect is essential in a context of balanced view on the observed trend of the SSH. References with the observation results: Maes, C. 1998. doi:10.1029/98GL02758 Sato, et al. 2000. doi:10.1029/1999GL011003 Ivchenko, et al 2008. doi:10.1029/2008JC004836 [Dmitry Aleynik, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The sea level section 3.7 discusses salinity contributions to sea level change.
3-409	3	11	55	11	55	I would add the citation to the following paper: Antonov, J. I., S. Levitus, T. P. Boyer, 2002: Steric sea level variations during 1957-1994: Importance of salinity. J. Geophys. Res.- Oceans, 8013,	Reject. Rather standard concept and many different references would suffice.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						doi:10.1029/2001JC000964. [Sydney Levitus, U.S.A.]	
3-410	3	11	55	11	56	I agree but salinity variations also strongly influence the mixed layer in the tropics (large precipitation and river discharges) and thus air-sea exchanges. I'd mention also the tropics (eg reference: Mignot, J., C. de Boyer Montégut, A. Lazar and S. Cravatte, Control of salinity on the mixed layer depth in the world ocean: 2 tropical areas; Journal of Geophysical Research, 112, C10010, doi:10.1029/2006JC003954, 2007) [BERNARD BOURLES, France]	Taken into account. We deleted the sentence that highlighted higher latitude salinity effects as reviewer is correct that salinity change can importantly affect stratification in lower latitudes as well, and the sentence actually served no purpose.
3-411	3	11		15		Section 3.3 describes the oceans salinity changes as the result of the global water cycle changes, do not discuss the 'changes in ocean salinity and implications for the global water cycle' (line 39 page 6) [Government of Poland]	Reject. Section3.3 should touch on the implications for changes in E-P. Language is being narrowed.
3-412	3	12	1	12	3	The data over 44 years (1955-1998) is hardly likely to be precisely linear, so where is the data, what exactly was the trend and what is the R squared value for the trend? [John McLean, Australia]	Reject. This sentence summarizes conclusions in AR4, and is not a statistical statement.
3-413	3	12	5	12	5	I would add relevant text from, and a reference to, the important paper by Pierce et al. (2012, GRL) [Sydney Levitus, U.S.A.]	Noted. Reference is really appropriate for Ch. 10, which has cited it, but it is added here in the intro paragraph about usefulness of salinity
3-414	3	12	11			Please define Practical Salinity Scale 1978. [Government of United States of America]	Accept. Added Lews and Fofonoff 1979 reference. Definitions are moved up to the end of section 3.3.1.
3-415	3	12	15	10	15	I suggest remove the word "robust" (and all of the other instances). This word is not used to OHC/Temp and there are many more thermal observations than salinity and considering that only half of the global ocean surface has statistically significant trends. [Catia Domingues, Australia]	Taken into account. This word is used in the IPCC guidelines for uncertainty language ("robust evidence") so we do not feel compelled to remove it, but we will attempt to use it consistently.
3-416	3	12	15	12	27	Again you state that trends exist but fail to state what how frequently the salinity was sampled, where they were sampled, what the raw data was, what the trends are and what their R-squared value is. [John McLean, Australia]	Accept. Characteristics of data distribution are now described in a new appendix.to Chapter 3.
3-417	3	12	16	12	16	Is the Roemmich and Gilson 2009 only about the Argo period (and not about multi-decadal trends)? [Catia Domingues, Australia]	Reject. R&G has extensive comparison with the WOA01 climatology and draws many conclusions about ocean change from the difference. (Paper is not just about Argo era data.)
3-418	3	12	17	12	20	The sentence ' The spatial pattern of surface salinity change is similar to the distribution of surface salinity itself: salinity tends to increase in regions of high mean salinity, where evaporation exceeds precipitation, and tends to decrease in regions of low mean salinity, where precipitation dominates (Figure 3.4)' is valid for the Figure 3.4 C. For the Figure 3.4 D this rule is not so apparent. [Government of Poland]	Noted. Panel d is based on analysis where the beginning period for the difference spans almost 30 years and hence has variability within it; the patterns are thus less clear. We have extended the discussion.
3-419	3	12	18	12	20	Again redundant - this was just mentioned on the previous page! [Terrence Joyce, United States of America]	Reject. Does not duplicate previous page although wording is very similar - previous page talks about how the mean salinity pattern relates to the mean E-P pattern. This page talks about how the salinity trends look like the mean E-P pattern. These are importantly different statements.
3-420	3	12	20	12	21	This sentence reads awkwardly. "in salinity" could be dropped from the end of it, as the subject is "the surface salinity maxima". [Adrian Simmons, United Kingdom]	Accept. Sentence reworded
3-421	3	12	26	12	27	Presumably the 43.8% includes both significant increases and decreases? [Janice Lough, Australia]	Accept. Language clarified.
3-422	3	12	26	12	27	This statement is very ephemeral. What was calculated? How was it calculated? What were the results? And what was the variation in the annual(?) data? [John McLean, Australia]	Noted.We think the review comment refers to the Durack and Wijffels 43.8%. Clarified.
3-423	3	12	30	12	30	The World Ocean Atlas 2009 includes data extending up to 2008 and incorporates data back to 1874 - I would recommend the period noted period "1955-2005" be updated to reflect this correct information [Paul Durack,	Taken into account. Figure caption is now much more precise.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						United States]	
3-424	3	12	30	12	30	Instead of "World Ocean Atlas 2009: the citation both here and in Fig. 3.4 should be: Antonov, J. I., et al., 2010: World Ocean Atlas 2009, Volume 2: Salinity. S. Levitus, Ed. NOAA Atlas NESDIS 68, U.S. Gov. Printing Office, Wash., D.C., 184 pp. [Sydney Levitus, U.S.A.]	Accept. Inserted in text and figure caption
3-425	3	12	30	12	34	Figure 3.4 Need to make clear what is being plotted in c. and d. My reading of Hosoda et al (2009) is that the Argo average for the 5-years 2003-2007 was compared to the WOD05 data which is for the period 1960-1989; i.e. not comparison of 2 years as suggested in caption. Also check the Durack & Wijffels (2010) - is this just comparing 2 years or averages for 2 time periods? [Janice Lough, Australia]	Accept. Figure revised and figure caption adapted to revised figure
3-426	3	12	30			Figure 3.4: the color bar is missing in the figure. In addition, it's not clear from the caption what the white regions represent? Are these regions with no or not sufficient data? Please specify. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Color bars added and description is more specific.
3-427	3	12	32	12	32	Are you sure that 1950 salinity sampling was widespread and comparable to year 2000 sampling? If not this should be removed or greatly clarified. [John McLean, Australia]	Taken into account. More discussion of sampling is now included. Similarity of DW and Hosoda results partially addresses this since Hosoda does not use 1950s data. Both paper include extensive discussion of sampling issues an appendix is added showing the observations the statements are based on.
3-428	3	12	33	12	33	The Hosoda et al., 2009 (Journal of Oceanography) analysis generated two epoch climatologies. One centred on 1975, and one centred on 2005. However the 1975 climatology ingests hydrographic data which ensures that the median year of their climatologies is not specifically representative of the year 1975 due to data sparseness. I would be cautious in suggesting their change maps are then 30-year analyses, as the period of which their changes are estimated changes depending on the region of analysis. This issue is not apparent with the Durack & Wijffels (2010; Journal of Climate) analysis, however their spatial scale over which their change estimates are generated is spatially varying due to data sparseness (Durack & Wijffels, 2010; Figure 2). The Hosoda et al., 2009 description should be reviewed [Paul Durack, United States]	Taken into account. We include more detail and analysis of these two products. Having at least 2 independent realizations/analyses of what is essentially the same data set is important in increasing the confidence in our conclusions. The Boyer et al. update has been added for the same reason.
3-429	3	12	36	12	36	Should this be "sub-surface" rather than "upper ocean"? [Janice Lough, Australia]	Accept. Change subtitle
3-430	3	12	36			Section 3.3.2.2: This discussion seems quite long relative to the preceding section on sea surface salinity, and yet the conclusion is less clear. [Government of United States of America]	Taken into account. The previous surface salinity section is now longer to deal with issues of sampling, uncertainty.
3-431	3	12	38	12	38	I would add "convection" to the processes that transfer changes of salinity to the ocean interior. For example changes in convection in the Labrador Sea clearly documents this process. [Sydney Levitus, U.S.A.]	noted. Para has been reworded
3-432	3	12	38	12	39	As I understand it "subduction" involves "flow along ventilation pathways". Diffusion is also important. [David Webb, United Kingdom]	Accept. Para has been reworded
3-433	3	12	41	12	48	Are you sure that 1950 salinity sampling was widespread and comparable to year 2000 sampling? If not this should be removed or greatly clarified. [John McLean, Australia]	Taken into account. More information about sampling is reported. And more information is added in an appendix. The cited references contain extensive analysis of the uncertainties arising from sampling changes.
3-434	3	12	42	12	42	It is hard to understand why salinity measurements are reliable enough to cover the 1950s, whereas temperature measurements can only start in 1971. This needs to be better justified. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account. The uncertainty in these trends is now discussed. For salinity one is simply documenting that there has been a trend. For heat content, the discussion has moved beyond this to being specific about rates.
3-435	3	12	50	12	52	The word "change" or "changes" appears many times in this sentence. [Leticia Cotrim da Cunha, Brazil]	Noted. Paragraph rewritten to focus on results rather

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							than explanation.
3-436	3	12	50	13	6	This is very non-specific. Please state the ocean locations for this work because without it the reader has no idea if the described changes were local or widespread. [John McLean, Australia]	Accept. Paragraph rewritten to focus on results rather than explanation.
3-437	3	12	52	12	54	This sentence does not give information about the discussed salinity changes but about the technical aspects of the data analyses and seems to be unnecessary. [Government of Poland]	Accept. Paragraph rewritten to focus on results rather than explanation.
3-438	3	12	54	12	54	A footnote description of 'isopycnals' would be useful, as it is a very specific scientific term. [Government of United Kingdom of Great Britain & Northern Ireland]	Noted. Paragraph rewritten to focus on results rather than explanation so footnote is unnecessary.
3-439	3	12	54	12	54	I would cite Levitus (1989) here because he introduced this procedure. Levitus, S., 1989: Interpentadal variability of temperature and salinity at intermediate depths of the North Atlantic Ocean, 1970-74 versus 1955-59. J. Geophys. Res.-Oceans, 94, 6091-6131. [Sydney Levitus, U.S.A.]	Noted. Paragraph rewritten to focus on results rather than explanation so footnote is unnecessary.
3-440	3	12	55	12	56	It would be helpful to have a footnote definition of what is meant by 'ventilated' in this context. [Government of United Kingdom of Great Britain & Northern Ireland]	noted. Ventilation is explained in the glossary
3-441	3	12		15		In several instances (text and figures) the units of time rates of salinity change are not given (e.g figure 3-9). [Government of France]	Accept. Added.
3-442	3	13	1	13	6	I find this section to be very unclear. Is it really necessary to keep this? I would eliminate it. [Sydney Levitus, U.S.A.]	Accept. Paragraph rewritten to focus on results rather than explanation.
3-443	3	13	2	13	4	the phrase beginning with "salinity has increased...." does not make sense [Government of United States of America]	Noted. Paragraph rewritten to focus on results rather than explanation.
3-444	3	13	8	13	14	Finally you quantify trends and error margins. So that the reader does not get a false impression of the significance of certain findings, this paragraph should be moved to prior to descriptions of those findings. [John McLean, Australia]	Taken into account. These are the "globally-averaged" values. We are now much more precise about how this information differs from that in the previous paragraphs. The previous paragraphs are now more quantitative.
3-445	3	13	10	13	12	Is this a statement from AR4 or one from the AR5? If the latter, then perhaps the situation using ocean for salinity change as an estimate of hydro cycle change is not as firm as for temperature, and my comments 2 lines above about this should be taken with a grain of salt! [Terrence Joyce, United States of America]	Taken into account. This paragraph's intent was obscure and is now hopefully clearer.
3-446	3	13	11	13	11	It would be useful to have the trend in high salinity waters versus low salinity waters separately, rather than the global averaged alone as a way to quantify the intensification of the water cycle mentioned in this section. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Taken into account. This paragraph is about the global average. We have added a statement in a previous paragraph about high vs. low as shown in Figure 3.20 (which we had not cited before), but we do not have an analysis that we can cite that provides the information requested by this reviewer.
3-447	3	13	11	13	11	"SSS" appears with no description in the previous texts, if it is not mentioned in the previous chapter [Dongxiao Wang, China]	Accept. (Acronym deleted)
3-448	3	13	12	13	14	Not really clear what this sentence means. Can the land ice mass loss since 2000 be related to recent climate trends or known decadal variability? please clarify. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accept. Paragraph has been edited to be clearer.
3-449	3	13	12	13	14	Please ensure this statement remains consistent with Chapter 4, and the confidence level that they associate with the acceleration of land ice mass loss. Not sure if the uncertainty is due to sparse sampling, given that these observations are remotely sensed, and therefore geographical coverage is not the issue. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. Chapter 4 is now referenced instead of specific papers. Phrase about sparse sampling is removed as it was irrelevant.
3-450	3	13	16	14	36	There is a lot of descriptive information provided on regional changes in salinity, and it is difficult to understand if the observed changes are consistent with climate change, dominated by variability, or both. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Noted. This section is intended for locally significant changes on decadal or longer time scales (not just trends), that have large-scale implications. Hence they

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							are broadly consistent with global picture but include local variabiliiies as well. Hence top sentence was changed from "reinforce" to "broadly consistent".
3-451	3	13	18	13	19	This statement cannot be sustained without quantified trends and error margins, and you don't seem to do that in the subsequent paragraphs and the few figures that you give have no context in which to determine the magnitude and significance of the change. [John McLean, Australia]	Taken into account. This statement was merely a summary lead-in for the sections that follow. It now includes uncertainty estimates and likelihood statements. Refers now to figures that show uncertainties and discussion that follows.
3-452	3	13	18	14	29	Nice regional summary [Terrence Joyce, United States of America]	Noted.
3-453	3	13	19			Probably better to talk about precipitation increasing where it exceeds evaporation and decreasing where evaporation dominates. As this is a chapter on oceanic observations, words like "wetter" and "drier" perhaps should be avoided. [Adrian Simmons, United Kingdom]	Accept. We now avoid the words wetter and drier.
3-454	3	13	26	13	42	It would be helpful to readers such as myself who are more versed in atmospheric or terrestrial measurements, or modelling, if the measure used for salinity could be mentioned here. An incese of "0.1 to 0.3" does not convey much by itself unless one knows what typical absolute values are. FAQ3.3 later defines salinity as "the weight of dissolved salts in a kilogram of seawater". If salinity is measured by a weight, it has dimensional units, so the units that apply to the "0.1 to 0.3" should be quoted in this paragraph. [Adrian Simmons, United Kingdom]	Accept. Salinity is defined now at the end of 3.3.1 (introduction), moved from 3.3.2 to clarify it applies throughout Section 3.3.
3-455	3	13	27	13	27	Cravatte et al., 2009 (Page 572) note peak freshening values of >0.6 PSS-78 are noted in the warm water pool bounded by >29.0C [Paul Durack, United States]	Accept. More details now in text.
3-456	3	13	51	13	52	Should be Vargas-Yáñez et al., 2010. Also in the references list. [Carles Pelejero, Spain]	Accept. Reference added
3-457	3	13	52			"During the time period between 1955–1959 and 2002–2006" This approach of specifying the endpoints of a time periods with time periods is confusing. The consistent repetition of this approach makes it look intentional, but to the non-initiated, it is confusing. Please clarify. [Government of United States of America]	Taken into account. Each of these hyphenated year ranges indicates that data were averaged over those years ("pentadal" averages) and comparisons were then made between averages in the different pentads (5-yr averages). Attempted to clarify.
3-458	3	14	6	14	6	Add reference: Valdimarsson, H., Astthorsson, O. S., and Palsson, J. Hydrographic variability in Icelandic waters during recent decades and related changes in distribution of some fish species. – ICES Journal of Marine Science, doi:10.1093/icesjms/fss027. [Government of Iceland]	Accept.reference added
3-459	3	14	10	14	10	The Durack & Wijffels (2010; Journal of Climate) analysis presents changes for the period 1950-2000, not "1950-2008" as noted. Their analysis uses data for the period 1950-2009 (April) however their trends are presented for 1950-2000 [Paul Durack, United States]	Taken into account. This is complicated to report because of the choice made in Durack and Wijffels paper, to fit a trend from 1950 to 2008, and then report it as a 50-yr trend "nominally from 1950 to 2000", which frankly is less accurate than saying it's the trend from 1950 to 2008, calculated as a trend/50 yr.
3-460	3	14	14	14	14	add reference to chapter/section of AR5-WG1 report that also covers Arctic sea ice decline. [Janice Lough, Australia]	Accept. Reference to Ch4 added
3-461	3	14	14	14	14	Rather than citing a single paper here (Kwok et al. 2009), you should simply link to the relevant section of Chapter 4 [Thomas Stocker/ WGI TSU, Switzerland]	Accept reference to Ch4 added
3-462	3	14	14	14	15	There needs to be a reference here to Chapter 4 where the discussion to changes in sea ice is discussed. Putting in a specific author citation to changing sea ice from 2009 just is not helpful to readers who want to know where the changes in sea ice cover are discussed. While earlier in the chapter there is an indication that cryospheric aspects are covered in Chapter 4, there needs to be a referral here to the appropriate section. [Michael MacCracken, United States of America]	Accept. Reference to Ch4 added
3-463	3	14	14	14	17	Does this paragraph means to say that the observed trends in salinity in the Arctic are inconsistent with our	Accepted. Paragraphs have been heavily edited in

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						understanding of physical processes, or that there are a lot of competing processes and we cannot tell which way should the Arctic salinity change from theoretical principles only? please clarify. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	light of very recent results, some of which we can only present as an update of an earlier paper because the new publication did not meet the IPCC AR5 deadline.
3-464	3	14	14			add perhaps Cavalieri, D. J., and C. L. Parkinson (2012), Arctic sea ice variability and trends, 1979–2010, The Cryosphere, 6(4), 881–889, doi:10.5194/tc-6-881-2012. [Jan Sedlacek, Switzerland]	Reject. Following advice in 3-460 through 3-462, we have removed specific references and now reference Ch. 4. Ch. 4 includes this reference.
3-465	3	14	21	14	21	Include reference to Giles, K. A., Laxon, S. W., Ridout, A. L., Wingham, D. J. & Bacon, S. Western Arctic Ocean freshwater storage increased by wind-driven spin-up of the Beaufort Gyre. Nature Geoscience DOI: 10.1038/NGEO1379 (2012) along with refernce to Rabe. This paper also includes evidence for increased FW storage up to 2010. [Seymour Laxon, United Kingdom]	accepted reference added
3-466	3	14	33	14	36	This paragraph on Southern Ocean is very short compared to the information provided for other oceans. Suggest providing equivalent information. [Government of Australia]	Taken into account. Text enhanced and Refer now to the basin-scale figures analyzed in other sections.
3-467	3	14	33	14	36	For the N. Atlantic, observations suggested large multi-decadal changes of both signs. It would seem prudent to note that the S. Ocean does have the same archive of data and that thees modern changes cannot be attributed solely to 'global change'. [Terrence Joyce, United States of America]	Taken into account. It is true that more complete and longer record might reveal local characteristics. At least the changes described here are now clarified in terms of time periods, and we include a comment about relative magnitude of decadal variability vs. trend in Boning et al.
3-468	3	14	36	14	36	Should be specific about which trend period the Meijers citation refer to? [Catia Domingues, Australia]	Accept information added
3-469	3	14	38			<p>Section 3.3.4: This contains a new result which is quoted widely elsewhere in the report and so needs special consideration.</p> <p>Here it states that the observed large scale changes in salinity, "support the hypothesis" that the water cycle is intensifying. It also states that the similarities "suggest" the hydrological cycle has been enhanced. Given the evidence these qualifications may just be acceptable - but elsewhere in the document the same results are used many times to support statements that an increased water cycle is "very likely" despite the lack of any other evidence.</p> <p>The statements appear to be based primarily on the paper by Durack et al. (2012). I have been through the paper but unfortunately my time for reviewing is limited. Because it comes up so many times in all that I have read, I think an independent reviewer needs to be appointed - but in the mean time I have the following points to make.</p> <ol style="list-style-type: none"> 1. It is a good paper describing some nice work and, on that basis, I agree it should have been published. 2. Paragraph one of the paper contains the 7% figure for the increase in evaporation per degree warming. This figure is repeated elsewhere in the IPCC report, but the assumption that humidity says fixed is not reported. <p>The same paragraph also reports a 2 to 3% limit on the increase on global precipitation. There is a line in the Technical Summary which refers to a 2% limit but it is not obvious where it comes from. Elsewhere this second limit is neglected but it is an important point because evaporation cannot increase by 7% everywhere while at the same time rainfall increases by less than 2%.</p> <ol style="list-style-type: none"> 3. I have no problem with the estimates of increased salinity or the high correlation with the estimates of the current pattern of precipitation and evaporation. 4. The remainder of the analysis appears to be based on model studies. As I have argued in other comments these have been designed to fit data from the recent past but their predictions must be less reliable because of 	Taken into account. It is true that there is no observation supporting the relationship over 50 years (partially 20 years by Wentz et al., 2007). We now change the statement relating the hydrological cycle to "medium confidence". We also include the evidence of freshwater content trend: this integrates down through the stratification, and is therefore more related to overall changes in air-sea fluxes than is the salinity itself, which as the reviewer notes, can be concentrated simply by changing the near-surface stratification.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						<p>the O(2) differences in climate sensitivities.</p> <p>5. The key figure of the Durack et al (2012) paper appears to be figure S3.D. This compares the mean precipitation change over 50 years with the global surface warming over the same period. It does this for both 20th Century simulations and for predictions of future changes.</p> <p>For the future it does show precipitation changes for up to 3% per degree. However for the 20th century runs the figure is between -5% and +2% with an average around 0%. These are the runs which have been fitted to the historic data and which should be applicable during the period of the observed salinity increase.</p> <p>I note that the value of 0% is also typical of those runs which give a warming of about 0.6 degrees over the last 50 years - i.e. they agree best with the estimated warming reported in section TS.2.2.</p> <p>If you use just the 20th century runs, I suspect you would have to conclude that there is no correlation between the changes in precipitation and the observed changes in salinity.</p> <p>6. But what about the predicted changes? Unfortunately weaknesses in the cloud and rainfall are probably the main cause of the O(2) scatter in the climate sensitivities. Having a model which increases rainfall with humidity seems a sensible thing to do - but given the present problems such sub-models cannot be relied on without a lot more justification.</p> <p>7. To conclude - the evaporation argument seems irrelevant given that it will stop once the atmosphere has become more humid. The rainfall argument is also suspect given its lack of support from the 20th century runs and the scatter in model climate sensitivities. So the basic idea may be correct but it is not proven. It is a nice simple idea but it is not supported by observations and the argument based on model results could easily be wrong.</p> <p>The use of unqualified versions of the statement so widely throughout the present IPCC report is unjustified.</p> <p>[David Webb, United Kingdom]</p>	
3-470	3	14	40	14	49	<p>In the first place the water cycle is a global process that includes long-range transport by the atmospheric circulation. As the residence time of water in the troposphere is ~ 1-3 weeks, water vapor can travel considerable distance downstream (away from a particular ocean basin). For this reason, no definite conclusion about trends in the global cycle may be drawn from regional studies (such as any oceanographic observation project). Next, salinity changes result from regional changes in P-E that in no way imply, much less prove, the existence of proportional changes in the rate of the global water cycle measured by the global P or E fluxes (that exactly compensate). In effect, the observed enhancement of regional salinity anomalies are akin to enhanced precipitation observed over wet continental regions while already dry regions tend to become drier under climate warming. Both phenomena result from changes in the atmospheric circulation (changes in flow divergence or convergence). For these reasons the section 3.3.4 seems erroneous, and misplaced in Chapter 3, and should be suppressed.. [Government of France]</p>	<p>Noted. The salinity analysis is global. Therefore the observed changes in salinity must be due to changes in P-E. Changes in P-E require a change in water vapor transport in the atmosphere. These can be due to atmospheric circulation change or water vapor content.</p>
3-471	3	14	40		49	<p>The authors relate observed salinity changes to changes in the fresh water cycles. In the next paragraph 3.4, it is shown that both evaporation and precipitation changes cannot be measured accurately which I agree with. So it is disturbing to give exclusive causes that cannot be measured. However there are other causes as very small circulation changes could also be responsible for the salinity changes. It is not because these circulation changes have not been observed yet that you should rule them out. In fact the middle column of figure 3.9 shows density tendencies for the 3 oceans that will induce pressure and circulation changes. [Government of France]</p>	<p>Reject. It is not possible to create the large differences in Atlantic vs. Pacific vs. Southern Ocean freshwater content and salinity with small circulation changes. We now highlight this.</p>
3-472	3	14	42	14	43	<p>'suggests the global hydrological cycle has been enhance' - is 'suggests' a strong enough word taking recent papers (Durack and Wijffles) into account? (AM) [Government of Australia]</p>	<p>Taken into account. This is clearly controversial - see comments 3-470 and 3-471. Text is this section is based strongly on Durack&Wijffels and inferences in other Australian author's works (co-author Bindoff),</p>

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							hence Australian research. We chose the measured word "suggest" for good reason. Since "suggest" is not an official WG1 uncertainty word, we have changed it to "medium confidence" and "likely".
3-473	3	14	43	14	44	What are the assumptions involved for that to be valid? (eg., mixing/transport) [Catia Domingues, Australia]	Taken into account. Sentences have been written more specifically and now reference Held and Soden who showed the complicated path from higher water vapor to higher water transport and hence enhanced E-P patterns
3-474	3	14	43			Please see comments 145-148 concerning the use of the word "global". [Adrian Simmons, United Kingdom]	Accept. Not clear which comments are meant - it looks like it should be 3-99 and 3-100? Removed word global, changed "water" to "hydrological", specified the spatial scales that we are referring to.
3-475	3	14	45	14	46	"..amplified at a rate that is comparable or larger than expected by model simulations and the Clausius-Clapeyron relationship (Durack et al., 2012)" - I would recommend breaking up this sentence to ensure accurate statements. The statement: "..amplified at a rate that is larger than model simulations for the historical 20th century and 21st century projections. Their reported rate of surface salinity amplification is comparable to the rate expected from a water cycle response following the Clausius-Clapeyron relationship." [Paul Durack, United States]	Accept.
3-476	3	14	46	14	48	It would be helpful here (and in reference to figure 3.5) to point out any (or no) significant trends. [Government of United States of America]	Accept.
3-477	3	14	48	14	48	"..inferred change.." The salinity changes are ACTUAL oceanic water cycle changes, linking these to atmospheric flux changes is where the inference is made [Paul Durack, United States]	Accept.
3-478	3	14	48			Please see comments 145-148 concerning the use of the word "global". [Adrian Simmons, United Kingdom]	Accept. See response to 3-474.
3-479	3	14	51	14	51	Caption "freshwater content change" and its unit in they-axis should appear in the figure. [Leticia Cotrim da Cunha, Brazil]	Accept.
3-480	3	14	51	14	56	Figure 3.5 tells a profound story that is not emphasized at all in the discussion as far as I can see: The Atlantic shows significant FWC decrease across mid latitudes commensurate with a FWC increase in the Pacific. This indicates a substantial, anomalous (relative to steady state), inter-basin exchange in FWC during the latter half of the 20th century. To put it in perspective, the integrated (by eye) Atlantic FWCC between 40S to 40N, divided by 50 years, yields a transport rate ~50,000 m ³ /s. This is comparable to combined transport of all N. American rivers that discharge to the ocean. (Source: http://en.wikipedia.org/wiki/List_of_rivers_by_discharge) [Gary Lagerloef, United States of America]	Accept. This inter-basin difference was a major conclusion, also for the AR4, but apparently we did not highlight it strongly enough. We did have it highlighted in the executive summary, and overall technical summary. The transport rate estimated in the comment is very interesting, and we have now done the calculation and include it.
3-481	3	14	51			This is a nice figure - too bad nothing is said about it. It shows the Atlantic getting saltier, the Pacific fresher, consistent changes as a fct. of latitude, but no global mean signal (apparently). It deserves some discussion. [Terrence Joyce, United States of America]	Accept. Also see response to 3-480
3-482	3	15	3	15	3	Insert the word 'patterns' after "Robust changes in ocean salinity" to avoid confusion with global average salinity. [Government of Australia]	Accept.
3-483	3	15	3	15	10	Page 15, Line 3 to 10, the likelihood statements here seem inconsistent with the conclusion in the synthesis section. There the language is confidence based or rather than the stronger likelihood levels expressed here. [Nathaniel Lee Bindoff, Australia]	Accept. Confidence language has now been carefully checked and modified.
3-484	3	15	4	15	4	"..since the 1960s" As noted in comments above, the reference Durack & Wijffels (2010; Journal of Climate) suggests this change has occurred since 1950, so uncertain about where the 1960s comes from [Paul Durack, United States]	Accept.
3-485	3	15	4	15	10	Why use "very likely" when you should be able to quantify the number of papers that made these findings? Did any papers not agree with these findings and if so, why have you not mentioned them? [John McLean,	Reject. We are asked to use this specific language by the IPCC. Each of these terms has a specific

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Australia]	definition. Taken into account. We found no paper opposing to this finding.
3-486	3	15	4			This Conclusion sentence refers to changes since the 1960s. However, the previous text did not note changes since the 1960s Section 3.3. [Government of United States of America]	Accept. Changed to "since 1950s".
3-487	3	15	8	15	9	This has been mentioned repeatedly. It occurs to me that it is consistent with a change in the water cycle, but does not prove it. To do this patterns of delta (E-P) need to be related to related to E-P. Is there a simple way to prove that this is happening? [Terrence Joyce, United States of America]	Taken into account. We have added more carefully worded sentences. The changes in freshwater content (column integrated) require a change in horizontal moisture transport in the atmosphere; surface salinity changes alone do not, since increased stratification could also create enhanced salinity patterns.
3-488	3	15	8	15	9	I do not think that it is "very likely" that, on the basis of the evidence reported in the previous sections, that the patterns of enhanced salinity are due to intensification of the water cycle. [David Webb, United Kingdom]	Taken into account. See response to 3-487.
3-489	3	15	8	15	10	For the same reasons as in page 14 line 40-49 , the last sentence in section 3.3.5 should be deleted from: "It is very likely that these patterns are caused"... to "...warmer air (Section 2.3)". [Government of France]	Taken into account. We have clarified our statements. See response to 3-487.
3-490	3	15	9			In the light of the two comments immediately above, I am pleased to see here reference only to "the water cycle" not "the global water cycle"! [Adrian Simmons, United Kingdom]	Noted.
3-491	3	15	18			Section "changes in ocean surface fluxes": This section could benefit from a figure, maybe at the end of the introduction, to illustrate heat and water exchanges [Government of United States of America]	rejected Figures illustrating heat and water exchanges are available in textbook discussions so we do not feel the need to include one here.
3-492	3	15	18			Section 3.4: In this section, the fluxes are examined in terms of global averages, but one wonders if a spatial pattern of change might have been missed. Could there be some key areas where a stronger signal could be expected? [Government of United States of America]	noted. Heat flux variability at regional scales is more heavily influenced by regional processes making identification of a spatial pattern of change difficult. Hence, the focus on global averages.
3-493	3	15	23	15	29	You cannot assume that there's any need to consider anthropogenic influence on climate change while climate models still cannot fully simulate all natural forces. Delete all such references in this section because they are mere speculation. [John McLean, Australia]	No assumption about model-based need is made here. Sec 3.4 presents like all subchapters of Ch3 an observation based assessment.
3-494	3	15	26	15	28	This sentence seems to a. be out of scope and b. be an unsupported strawman assertion. My suggestions would therefore be to delete it. If retained substantive modifications would be required to better justify this text with supporting references. [Peter Thorne, United States of America]	This sentence is no longer included in the revised Introduction, instead the point is made in 3.4.2.1 para 1 last sentence with reference to Pierce et al., 2006.
3-495	3	15	34	15	35	sign convention: would it make sense to indicate here whether this is the same or whether this differs from the convention applied in, e.g., Chapter 2 and others? It seems to us that this could help the reader when, e.g., reading the cross-chapter material on the energy budget combined in Chapter 13. [Thomas Stocker/ WGI TSU, Switzerland]	Chapter 2 does not adopt a sign convention, referring only to the magnitude of the terms . Hence, there is no potential for confusion due to oppositely chosen sign conventions.
3-496	3	15	37	15	37	"air-sea freshwater flux"? If you really mean freshwater then you need to explain. [John McLean, Australia]	Freshwater' is a widely used and accepted term for the flux of water (i.e.Evaporation-Precipitation) between the ocean and the atmosphere. This is already explained in the text. See also FAQ 3.3
3-497	3	16	2	16	4	This sentence should be omitted because any downwelling radiation from CO2 is absorbed within a few microns of the sea surface and disappears very quickly into the atmosphere in evaporation and convection. There is no way that it can contribute to ocean warming. [John McLean, Australia]	The balance of processes and timescales is more complex than the reviewer suggests, see ref cited in text Pierce et al. (2006) for discussion of time evolution of each heat flux component. See also FAQ 3.1 explaining how heat is introduced into the ocean

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-498	3	16	3			This statement makes attribution to "anthropogenic climate change" while previously (p 3-15 ln 28) it is stated that that it is not possible to directly assess the impact of anthropogenic changes on surface fluxes. [Government of United States of America]	No attribution statement is made here. The intention is simply to set the wider context for our observation based assessment of surface fluxes.
3-499	3	16	6	16	29	This paragraph seems unusually dense, particularly given the authors direction of readers to Gulev et al. for details. Consider removing some of this detail. [Government of United States of America]	accepted Paragraph shortened by removing excess detail.
3-500	3	16	6	16	29	This whole paragraph felt very detailed and 'techy' and therefore distinct from much of the rest of the chapter. This served to break my reading flow and I didn't really know what I got out of it. Can this be simplified / made more consistent with the degree of detail elsewhere? Can the bottom line conclusions be better drawn out? [Peter Thorne, United States of America]	accepted Paragraph shortened by removing excess detail.
3-501	3	16	9	16	12	Replace the present sentence by: The Hamburg Ocean-Atmosphere Parameters and Fluxes from Satellite (HOAPS) data product provides turbulent heat flux (and precipitation) fields developed from observations at microwave and infrared wavelengths (Andersson et al., 2011). [Stephan Bakan, Germany]	Accepted. Text changed.
3-502	3	16	24	16	29	The discussion about CORE is nor relevant to this assessment. I would eliminate it along with much of the rest of the discussion about surface fluxes. The authors pointout on Page 17, Lines 26-29 that fluxes are not nearly as accurate as the estimates we have of ocean heat content. I would reduce the size of the section on fluxes in this chapter and replace it would more figures documenting the variability of ocean heat content. I recognize this may appear as being self-serving but the purpose of the assesment is to document climate change. Air-sea flux estimates are just too uncertain for this purpose as you point out so why devote so much space to their discussion? [Sydney Levitus, U.S.A.]	partly accepted Paragraph shortened by removing excess detail.
3-503	3	16	31	16	33	State what causes this variability. [John McLean, Australia]	It is not yet possible to establish the cause of the variability given uncertainty due to changes in the observing system. New text on the uncertainty due to observing system changes is now included.
3-504	3	16	32	16	32	It should be referred to Figure 3.6 upper panel, not left panel [Government of Australia]	Accepted. Text changed.
3-505	3	16	32			There is no left panel on Fig. 3.6 and in this context the reference to "middle panel" on line 33 is not clear. [Government of United States of America]	See 3-504.
3-506	3	16	36	16	37	The statement 'The overall time series from 1958-2010 provides no evidence for a trend in global mean evaporation' could equally well say "provides no evidence for a lack of a trend". Some further comment is needed here about signal to noise! At Clausius Clapeyron scaling, would any warming trend in this data set (2W/m2) be detectable given the stated 10-20% errors in the climatological mean fields (20W/m2) which are likely larger in single year averages. I really believe that with the large errors in these fields and the massive observing systems shifts of the deriving quantities, Figure 3.6 is next to useless and should be removed (even the decadal signals are not statistically robust). If not removed a bar scaling the expected anthropogenic change should be introduced so that the reader is clear that looking for a warming finger print in these fields is looking for a needle in the haystack. It is strange the authors state these huge error and small signal issues and then go ahead and discuss a global time series. A less confusing approach is to state that signal to noise is still too small to usefully look at global multidecadal trends. [Government of Australia]	Noted. Text revised in the light of points made by reviewer. In particular, the difference in magnitude of the uncertainty range and the signal expected from changing ocean heat content is now highlighted.
3-507	3	16	36	16	37	is this statement consistent with evaporation statements in the salinity section? While the salinity section addressed this issue regionally, it could be helpful to explain that regional differences can be significant but that globally averaged values do not show a trend. [Government of United States of America]	The text here has been revised and now states that it is not possible to establish multidecadal trends in global ocean mean evaporation. The statement that there is 'no evidence for a trend in global mean evaporation' which was the source of the reviewer's concern has been removed.
3-508	3	16	36	16	37	The sentence spanning these lines indicates no evidence for a trend in "global mean evaporation". Two comments. Firstly, since the word "global" is used in the sentence, the words "over the oceans" should be added at the end of the sentence for avoidance of doubt. Secondly, this result seems to be in conflict with the salinity results on which comment is given above. Some discussion of this conflict would be appropriate. [Adrian Simmons, United Kingdom]	Accepted. Point 1: 'global mean evaporation ' now says 'global ocean mean evaporation'. Point 2 : See response to 3-507.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-509	3	16	40	16	45	Figure 3.6: bottom panel of Fig 3.6 does not seem to be referenced in text. [Janice Lough, Australia]	Accepted. New sentence added referring to bottom panel.
3-510	3	16	49	16	51	Again, the heat fluxes in W/m ² do not specify the area over which the normalization is defined. [Stephen Griffies, United States of America]	partly accepted The heat fluxes quoted are typical values from available climatological datasets, for example OAFflux, and these products have a grid cell area of order 1x1degree. We now make this point at the location in the text where we first discuss OAFflux.
3-511	3	16	53	16	57	The use of estimates that are derived from other estimates surely warrants a clear statement about error margins. [John McLean, Australia]	The text makes it clear that the error estimates depend on a wide range of factors that are difficult to quantify and refers to the Gulev et al. (2010) paper for further discussion.
3-512	3	16	58	16	58	Saying "many sites" is inadequate because "many" has different meanings to different people. The figure needs to be quantified. [John McLean, Australia]	Accepted. A reference is now given to a map showing the distribution of sites in Wild, 2009 and the word 'many' has been removed.
3-513	3	17	0	0	0	With the study of Wu et al, 2012 the chapter should discuss the evidence for a general strengthening of the major western boundary currents along with the evidence for strengthening wind curl fields: Wu, L., Cai, W., Zhang, L., Nakamura, H., Timmermann, A., Joyce, T., McPhaden, M. J., Alexander, M., Qiu, B., Visbeck, M., Chang, P. and Giese, B. (In press). "Enhanced warming over the global subtropical western boundary currents." Nature Climate Change, doi:10.1038/nclimate1353. - [Government of Australia]	partly accepted Changes in ocean circulation are discussed in sec 3.6
3-514	3	17	2	17	4	Disagree with the statement that there are "very few in-situ measurements" of shortwave radiation over the ocean - at least for the tropical band in the last two decades. The Global Tropical Moored Buoy Array (McPhaden, et al 2010), consisting of some 75 buoys, observes the surface shortwave flux in all the equatorial oceans (between 15N to 15S). The array was established in the early 1990's in the Pacific Ocean and has expanded to include the Atlantic and Indian Oceans. Measurements are of similar quality to the land-based network (Pinker et al, 2009). REFS: McPhaden, M.J., and co-authors,2010: The Global Tropical Moored Buoy Array, In Proceedings of the "OceanObs'09:Sustained Ocean Observations and Information for Society" Conference (Vol. 2), Venice, Italy, 21-25 September 2009, Hall, J., D.E. Harrison, and D.Stammer, Eds., ESA Publication WPP-306; Pinker, R. T., H. Wang, & S. A. Grodsky, 2009: How good are ocean buoy observations of radiative fluxes?, Geophys. Res. Lett., 36, L10811, doi:10.1029/2009GL037840. [Government of Australia]	Accepted. Text modified to include reference to tropical moored buoy array.
3-515	3	17	6	17	12	By adding the flux trend numbers, you may still get a close to 1 W/m ² per decade flux trend, which does not support the statement of "there is no significant trend". [Government of Australia]	noted. Sec 3.4.2.2 has been shortened as discussion of radiative flux trends is located in Ch2. The text referred to is no longer present in the shorter revised version.
3-516	3	17	6	17	12	State error margins associated with the given values and state the values themselves. (What does "about 2" mean? 1? 1.5? 2.1?) [John McLean, Australia]	noted. Sec 3.4.2.2 has been shortened as discussion of radiative flux trends is located in Ch2. The text referred to is no longer present in the shorter revised version.
3-517	3	17	6	17	21	These rates of dimming and brightening seems inconsistent with Chapter 2. What are the uncertainty estimates on the changes? [Government of Australia]	noted. Sec 3.4.2.2 has been shortened as discussion of radiative flux trends is located in Ch2. The text referred to is no longer present in the shorter revised version.
3-518	3	17	6	17	21	global/regional solar radiation trend (dimming and brightening) is only discussed before year 2004. There are several papers showing the variation of solar radiation both globally and regionally, such as Hatzianastassiou et al., 2012: Recent regional surface solar radiation dimming and brightening patterns: inter-hemispherical asymmetry and a dimming in the Southern Hemisphere, Atmospheric Science Letters Volume 13, Issue 1, pages 43–48, January/March 2012. [Dongxiao Wang, China]	noted. Sec 3.4.2.2 has been shortened as discussion of radiative flux trends is located in Ch2. The text referred to is no longer present in the shorter revised version.
3-519	3	17	6		21	You are taking all these numbers (brightening, dimming) at face value, without any assessment of accuracy or	noted. Sec 3.4.2.2 has been shortened as discussion

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						uncertainty. I would encourage such an assessment here rather than just repeat the published numbers. [Stephen E Schwartz, United States of America]	of radiative flux trends is located in Ch2. The text referred to is no longer present in the shorter revised version.
3-520	3	17	15	17	15	Again, the heat fluxes in W/m ² no not specify the area over which the normalization is defined. [Stephen Griffies, United States of America]	partly accepted The heat fluxes quoted are typical values from available climatological datasets, for example OAFlux, and these products have a grid cell area of order 1x1degree. We now make this point at the location in the text where we first discuss OAFlux.
3-521	3	17	28	17	28	Again, the heat fluxes in W/m ² no not specify the area over which the normalization is defined. [Stephen Griffies, United States of America]	partly accepted The heat fluxes quoted are typical values from available climatological datasets, for example OAFlux, and these products have a grid cell area of order 1x1degree. We now make this point at the location in the text where we first discuss OAFlux.
3-522	3	17	29	17	29	This error bar seems large. Direct moored observations and reanalysis products usually have smaller error bars. [Government of Australia]	Extra text included to make it clear that what is being referred to here are estimates of the global ocean mean surface heat flux from observation based datasets. Direct mooring observations may have smaller errors but they do not provide estimates of the global mean due to the very limited number of deployments available.
3-523	3	17	29			20 W m-2. I am surprised. Is this TOA or ocean? I would assert that we know both much better in the aggregate (2 W m-2) even if individual terms are much more uncertain. So I think this needs to be clarified. [Stephen E Schwartz, United States of America]	partly accepted Extra text included to clarify that this is ocean not TOA and that the estimates being referred to are from observation based surface flux datasets which do have global biases of 20 Wm-2 or greater (e.g.Trenberth et al., 2009; Gulev et al., 2010).
3-524	3	17	32	17	33	Yu et al, 2007 also found interesting relationships between Qnet, mixed layer depth and SST in the tropical Atlantic (ref: Yu, L., X. Jin and R. A. Weller: Role of net surface flux in seasonal variations of sea surface temperature in the tropical Atlantic ocean, Journal of Climate, 19, 6153-6169, 2006. [BERNARD BOURLES, France]	rejected The paper noted here has interesting discussion of processes driving seasonal variability but does not consider multidecadal variability which is the subject under discussion here. Hence, it is not relevant to include a reference to it.
3-525	3	17	36			Section 3.4.3: The section would be stronger if it started off with a clear statement saying that studies are few and data are problematic, but there is some limited evidence of an increase in oceanic precipitation. [Government of United States of America]	accepted New opening sentence added.
3-526	3	17	36			Why just call out the agreement between MERRA and GPCP? Presumably Trenberth et al. (2011) looked at all of the reanalyses. [Government of United States of America]	accepted Discussion of Trenberth et al. (2011) results now broadened
3-527	3	17	38	17	38	I know it is in the Section title but make it clear in text that referring to precipitation measurements for the oceans. [Janice Lough, Australia]	Accepted. Now done in new opening sentence.
3-528	3	17	38	17	53	What are the uncertainty estimates on these numbers? [Government of Australia]	noted. Uncertainty estimates are not available in the cited publications.
3-529	3	17	45	17	46	This trend - actually 109 years not 108 when both end years are included - is too long to say anything useful and a trend reported in thousandths of a mm per day (I dare say rounded) is just as useless. Delete these statements or add to the sentence "but this should be treated with caution". [John McLean, Australia]	rejected The requested extra sentence was already present at the end of the paragraph where it was stated that 'such estimates should be treated with caution', in the final draft the same message is conveyed using the 'low confidence' measure .
3-530	3	17	45			mm per day per decade is a non-intuitive unit of change. Suggest using mm per decade [Government of United States of America]	accepted The unit has been changed too mm per month per 100yr to correspond directly with that employed in the cited publication.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-531	3	17	54			Insert a new paragraph: A similar conclusion was drawn by Andersson et al. (2011) from an evaluation study of the oceanic freshwater flux components in the purely satellite-based HOAPS climatology with a number of other widely used data sets. However only very few datasets provide consistent estimates of both evaporation and precipitation. The variability of the monthly mean global average values differs strongly among all considered data sets, making trend analysis problematic for the comparatively short satellite time series. All compared precipitation fields (HOAPS, ERA-Interim, GPCP V2, TRMM 3B43) exhibit large differences in many regions of the global oceans. To date validation of these differences is difficult because of the lack of extensive and reliable in situ precipitation data over oceans. Neither HOAPS nor GPCP indicate significant trends for the global ocean monthly mean precipitation time series 1987-2005. Freshwater flux estimates in HOAPS, ERA-Interim, and in a combined IFREMER-GPCP product, exhibit distinct differences in terms of global averages as well as regional biases. The mean globally averaged net ocean surface freshwater flux for the 1992–2005 period is 0.73 mm/d for HOAPS, 0.77 mm/d for IFREMER–GPCP and 0.50 mm/d for ERA-Interim. These values differ substantially from the long-term mean global river runoff of about 0.35 mm/d, but are within the uncertainty limits of the precipitation and evaporation estimates of about 10–15%. [Stephan Bakan, Germany]	partly accepted The suggestion of the reviewer is appreciated but as the conclusion drawn is similar to that already reached, it is not necessary to include the lengthy additional text that has is suggested here.
3-532	3	17	55			I suggest deletion of the word "current" in this line. One of the reanalyses was ERA-40, which was completed nine years ago in 2003, and is not regarded by ECMWF, its producer, as current, even if it runs for ten years longer (1958-2001) than its replacement ERA-Interim (1979-present). The NCEP/NCAR reanalysis is still run, though as Trenberth et al. point out its vintage is 1995. The following sentence is appropriate, however, as it quotes results only for two of the more recent reanalyses. [Adrian Simmons, United Kingdom]	Accepted. 'Current' deleted.
3-533	3	17	56			If it is precipitating more (3.7) but not evaporating more (3.6) then the atmosphere must be losing water vapor, which is not observed. So is this what Trenberth et al point out? [Terrence Joyce, United States of America]	noted. It is not yet possible to establish whether there are significant multi-decadal trends in global mean E-P (See 3.4.3 last para). Hence, the conclusion advanced by the reviewer cannot be reached.
3-534	3	18	9	18	15	There may be value to comment on the regional distribution of precipitation changes, and in particular with their coherence with the salinity changes. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	rejected The flux datasets cannot yet be reliably used to identify regional changes in precipitation on the multidecadal timescale of the observed salinity changes so a comment on this cannot be included.
3-535	3	18	14	18	15	Was there any significant trend in this time series? [Terrence Joyce, United States of America]	noted. The text referred to is no longer present in the revised version.
3-536	3	18	15	18	15	Units of Sv provided here cannot easily be compared with units of mm/day provided on p.17 [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted. The text referred to is no longer present in the revised version.
3-537	3	18	17	19	2	Section 3.4.4 – This section was difficult to read because of the dense jargon and acronyms. Suggest streamlining and simplifying the language [Government of United States of America]	accepted Acronyms for reanalyses now clearly defined at one place
3-538	3	18	17			Section 3.4.4: It's not apparent why this section only addresses the Southern Ocean, teleconnections, and the Tropics with respect to wind stress. Are there no changes in other areas? No data? [Government of United States of America]	partly accepted. Text modified to clarify :The section addresses the areas where major changes have been reported in the literature : these are the North Atlantic, Tropics and the Southern Ocean.
3-539	3	18	19	18	21	Relevant section in Ch2 to be referred to is 2.7.2. [Thomas Stocker/ WGI TSU, Switzerland]	*Editorial - To be changed for consistency
3-540	3	18	26	18	26	Section is now 2.7.8 [Peter Thorne, United States of America]	*Editorial - To be changed for consistency
3-541	3	18	26	18	29	The explanation of Figure 3.8 is not adequate. First, trend lines for each dataset should be plotted. Then clearly, there is decadal change of the trend. E.g. all the datasets except ERA40 (no data after 2003) show increasing trends after 2003. Hard to tell whether there is a increasing trend for all the datasets before 2003. [Dongxiao Wang, China]	partly accepted Fig 3.8 replaced with new S Ocean wind stress trend figure adapted from Swart and Fyfe (GRL, 2012,doi:10.1029/2012GL052810) and revised accompanying text.
3-542	3	18	31	18	33	This section needs to link to the wide range of studies that identified a trend in the Southern Annular Mode from trends in atmospheric pressure differences and with the Young et al 2011 study already cited in the chapter. Although the wind stress data may not be sufficient to identify a trend with high confidence on their	accepted Comment linking wind and wave results now added to section 3.4.5 where Young et al. (2011) is cited .

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						own, taken together with the SAM trend the confidence statement may become stronger. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	
3-543	3	18	36	18	36	Figure 3.8: What is a "1-year running mean"? Should it be "12-month" running mean? [Janice Lough, Australia]	Noted, the figure has been completely revised in the final draft.
3-544	3	18	40	18	52	20CRv2 is a model? Some explanation would help the non-specialist reader. [Leticia Cotrim da Cunha, Brazil]	accepted A paragraph summarising the various atmospheric reanalyses (including 20CRv2) has now been added to sec 3.4.1 for clarity and to ensure the various acronyms are detailed in one place.
3-545	3	18	43	18	43	"positive trend" - what does this mean, for example, for NAO? [Janice Lough, Australia]	accepted Text corrected - more explicit description of the NAO/EAP behaviour during 1960s-1990s is provided
3-546	3	18	44			Wu et al. also related the changes in wind curl to those in ocean temperature at latitudes of separated western boundary currents, suggesting poleward shifts of mid-latitude ocean fronts. This should be mentioned. [Terrence Joyce, United States of America]	noted. However, the focus of the text here is changes in wind stress.
3-547	3	18	46	18	46	Add in the reference for 20CRv2, namely, replace "20CRv2" with "20CRv2 (Compo et al., 2010)". Then add in the reference on page 48 (see my comment on page 3-48 below). [Xiaolan Wang, Canada]	See comment 3-544
3-548	3	18	46			Define 20CRv2 [Government of France]	See comment 3-544
3-549	3	18	49	18	52	Please use IPCC confidence language to qualify the evidence rather than a caution note. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted Wording is changed
3-550	3	18	56	18	57	Please quantify the amplitude of the variability in wind stress. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted. The text referred to is no longer present in the final draft
3-551	3	19	1	19	1	Chapter 2 also reports on a recent strengthening. [Government of Australia]	Reference to the Chapter 2 is provided earlier
3-552	3	19	1	19	1	The careful analysis and trend results of the surface ship winds/wave observations over the oceans by the WASWIND project should be included here. Tokinaga, H., and S.-P. Xie, 2011: Wave and Anemometer-based Sea Surface Wind (WASWind) for climate change analysis. J. Climate, 24, 267-285. [Government of Australia]	rejected Tokinaga and Xie (2011) used wind waves from ICOADS to produce a corrected wind speed dataset and their analysis of wind trends from this dataset is already covered elsewhere in the report (wind speed section of Chapter 2).
3-553	3	19	4	20	4	Information on changes in surface height have potentially large impact. Can this information be conveyed in a figure, so the regional trends can be highlighted? [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted Regional trends figures are available in the cited papers.
3-554	3	19	6	19	16	What does the term "wind sea" mean? [Janice Lough, Australia]	noted The term is explicitly explained in the brackets right after it is used for the first time
3-555	3	19	6			Perhaps the word "direct" could be dropped here. Swell is the result of wind forcing, but the forcing of the swell can be at a place and time that is far removed from where the swell is observed. So it might be argued that the link between swell where it is observed and the surface wind is geographically and temporally indirect. [Adrian Simmons, United Kingdom]	accepted The word "direct" has been removed.
3-556	3	19	8	19	10	"Significant wave height (SWH) [...] is frequently attributed to the highest one-third of wave heights." This is not precise. The highest 1/3 of wave height was the first historical definition of SWH (by Walther Munk). The modern definition is four times the square root of the zero-order moment of the wave spectrum. I would suggest changing the wording to "is approximately equal to highest one-third of wave heights". [Government of Poland]	noted in the earlier version this definition (sq. root of zeroth moment) was used. Then it was simplified to adopt the text to a general readership. Correction is made as suggested, i.e. "approximately equal....."
3-557	3	19	11	19	14	Suspect that each reference in these lines to "wind sea" should actually be "wind waves" since it refers to the entire wave spectrum - not just the wind-sea component. [Government of Australia]	accepted the changes are adopted, again it was correct in the earlier version
3-558	3	19	15	19	16	I felt this was a little bit of a throwaway sentence that in some sense perhaps ducks the challenge of making	partly accepted. This sentence is now supported by

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						an assessment or at least could be used to suggest that such a charge was avoided by the authors. I have some sympathy with their assessment but as worded it is not sufficiently articulated to be defensible. It need to be expanded and preferably backed up by a reference rather than being a throwaway line. [Peter Thorne, United States of America]	references.
3-559	3	19	18			Here and below are these interpreted as increases in mean winds or increases in storminess? [Terrence Joyce, United States of America]	rejected. here changes in SWH are discussed, so that it can be only interpreted as evidence of growing wave height. "Storminess" is a somewhat uncertain term, at least it cannot be quantified easily. No changes applied.
3-560	3	19	24	19	24	The "1871-2008" should be replaced by "1871-2010", because the study of Wang et al.(2012) covers the period up to 2010. [Xiaolan Wang, Canada]	Agree, correction is applied
3-561	3	19	24	19	31	The discussion in this part is biased and needs substantial revision. There is evidence (see e.g. Krüeger et al. 2012, J. Climate, doi:10.1175/JCLI-D-12-00309.1 or the references and discussion provided in the SOD of WG1 Ch 02) that estimates of wind speed trends derived from 20CR may be biased because of changes over time in the density of the observational network used in 20CR. [Ralf Weisse, Germany]	accepted. The potential problem with 20CRv2 is now highlighted in the wnd stress section with reference to Krueger (2013).
3-562	3	19	36			Suggest considering: Gemmich et al. (2011) point out that the buoy trends in the north Pacific may be suspect because of historical changes in observing practice and analysis procedures. Buoys along the central Atlantic coast have no net trend in the cold season in the past three decades (Komar and Allan, 2008). [Government of United States of America]	accepted Comment added referencing Gemmich et al. (2011) analysis of buoy record homogeneity
3-563	3	19	53	19	53	the trends are described in page 19 as % per year here but in the conclusions (page 20) and in the executive summary (page 4) the rates are cited as % per decade. I suggest using the same units for these rates through the text. [Antonio Bode, Spain]	accepted, estimates are now given in cm per decade throughout.
3-564	3	20	1	20	4	Is the last sentence why these results have been questioned? If so, better link needed. [Janice Lough, Australia]	accepted Fuller discussion with additional reference now included.
3-565	3	20	3			Suggest that the authors consider comparison of the satellite results with the regional results presented above. [Government of United States of America]	noted. However, the satellite results are for a shorter time period than the regional results presented earlier, so a comparison is not possible
3-566	3	20	8	20	8	the extremely small' change in global mean net heat flux' is only small compared to the total heat flux, but 0.5 W/m2 is actually a large number compared to equilibrium. Please rephrase, e.g. 'observed ocean heat content changes are small (about 0.5 W/m2) compared to mean heat fluxes (200-300 W/m2) and beyond the detection...' [Corinne Le Quééré, United Kingdom of Great Britain & Northern Ireland]	accepted Text revised, 'extremely small' no longer used.
3-567	3	20	8	20	8	I would replace "extremely" with "relatively". If you say "extremely" then you have to say "compared to what". [Sydney Levitus, U.S.A.]	Text now revised, 'extremely' no longer used.
3-568	3	20	8	20	9	Again, the heat fluxes in W/m^2 no not specify the area over which the normalization is defined. [Stephen Griffies, United States of America]	partly accepted The heat fluxes quoted are typical values from available climatological datasets, for example OAFflux, and these products have a grid cell area of order 1x1degree. We now make this point at the location in the text where we first discuss OAFflux.
3-569	3	20	8	20	9	State the error margin associated with this figure so that the readers can determine its reliability. [John McLean, Australia]	noted. The text has been revised to make it clearer that the figure of 0.5 W/m2 is required for consistency with the observed ocean heat content increase. The ocean heat content increase is discussed fully in Sec 3.2 together with its uncertainties and there is no need to repeat that discussion here.
3-570	3	20	11	20	13	The sentence that spans these lines states that it is not yet possible to establish global trends of [oceanic] E-P, and refers to the evidence from salinity as relating to regional trends. Yet the earlier discussion of the salinity	noted. Text revised, now focuses on lack of direct evidence for changes in E and P. Discussion of

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						observations draws inferences about the global water cycle. A bit more discussion of this apparent discrepancy would be welcome here. [Adrian Simmons, United Kingdom]	indirect evidence for changes in E and P from salinity changes is now in Sec 3.3 and not repeated here in order to avoid any possible confusion.
3-571	3	20	18	20	19	"...mid-latitude Northern Hemisphere with typical winter season trends of 8 to 20 cm decade-1 in the midlatitude North Atlantic" probably expresses Wang et al. (2009)'s results better because the Pacific trends were smaller.. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Noted, statement has been rewritten.
3-572	3	20	19	20	20	the trends are described in page 19 as % per year here but in the conclusions (page 20) and in the executive summary (page 4) the rates are cited as % per decade. I suggest using the same units for these rates through the text. [Antonio Bode, Spain]	accepted, estimates are now given in cm per decade throughout.
3-573	3	20	22	20	22	What about changes in upper ocean water properties? [Government of Australia]	Accepted. A brief statement is included.
3-574	3	20	22	20	22	When reading this section, it is useful to have a schematic of the water mass and ventilation pathways, as Figure 3.9 is not informative on different water masses, although the figure is highly referred to. [Government of Australia]	Accepted. The section is reorganized, and references are made to figure 1 of FAQ 3.1
3-575	3	20	22	22	26	Please explain your choice of regions as not all possible water mass are covered, and it is not clear why some are covered in detail while others are not. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. A better introduction is now included
3-576	3	20	22			Section 3.5: Suggest a brief discussion of the observing systems. [Government of United States of America]	rejected. It is the same observing systems as for temperature and salinity, thus covered in the earlier sections.
3-577	3	20	24	20	43	Suggest to refer to a figure visualizing the different water masses here, i.e., either add schematic panels to Figure 3.9 or prominently refer to FAQ 3.1, Figure 1. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted, FAQ3.1 Figure 1 is referenced
3-578	3	20	26	20	34	While it is true that in the long run, "water mass analysis may be a useful tool to assess rates of climate change", this introductory statement builds an expectation that is then deflated in the rest of the section because short term term cycles dominate the patterns. Suggest adding that the complexity of the formation process (and the fact that it is not continuous in time and space) makes it more challenging to establish a link to the gradual global trend using such a short period of observations because some important drivers of the formation process operate on shorter scales. [Government of United States of America]	Accepted. A short introduction to these complexities is now included.
3-579	3	20	26	20	34	For the benefit of non-experts, it would be good to start the section with a definition of water mass. Moreover, it should be clarified that the formation process tends to be quite "local" and can be quite different among water masses. Suggest adding a suggestion of how the masses discussed here were selected from the many water masses that exist. [Government of United States of America]	Accepted. The section has been reorganized and an introduction is added to explain some of the complexities of water mass analysis.
3-580	3	20	26	20	43	Some discussion on the effects of high-latitude freshening on the large scale circulation would help emphasize the importance of the observed trend at high latitudes (The Freshening of Surface Waters in High Latitudes: Effects on the Thermohaline and Wind-Driven Circulations, JPO, Fedorov et al. 2007) [Andrew Shao, United States of America]	rejected. Beyond the scope of the chapter.
3-581	3	20	31	20	31	If you mean carbon dioxide then say so. Carbon and oxygen, as separate elements, have little to do with climate. [John McLean, Australia]	rejected. Carbon directly affects climate; oxygen provides an indicator of changes in circulation relevant to climate
3-582	3	20	36	20	36	The data source for Figure 3.9 is not mentioned in the chapter. It should be part of the main text as well as the figure caption. [Stephen Griffies, United States of America]	Accepted. Numerous references are added.
3-583	3	20	36	20	43	This paragraph repeats information in the other sections, and in that sense is rather superfluous and seems out of place. It would be worth retaining if specific water masses that are affected or drive the zonal changes are mentioned. This would help transition to the presentation of specific water masses, in the context of the other sections. [Government of United States of America]	Accepted. Introduction is altered.
3-584	3	20	36			Fig. 3.9, Fix caption: columns 1 and 3 are clearly Salinity and Potential Temperature, respectively, not what is	Accepted. Caption changed

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						written here. [Terrence Joyce, United States of America]	
3-585	3	20	45	20	57	although much maligned the Bryden et al. (2005) Nature paper showed that there were changes in the deep water mass properties at 26N in the LNADW (3000-5000m)... see more recently Atkinson et al. (2012) Ocean Sci. Disc. [Meric Srokosz, United Kingdom of Great Britain & Northern Ireland]	Rejected. Owing to limited space Ch3 refrained from adding an assessment of remote changes of NADW. The literature base is very large.
3-586	3	20	45			Section 3.5.2: This section has a lot of detail but very little assessment of change (vs. 3.5.3, which is concise and yet clear about observed changes). [Government of United States of America]	Accepted. The explanation why should be much more evident with the present rewrite.
3-587	3	21	1	21	2	"it is the strong , freshening signal... that dominates the trend" -- suggest to specify more explicitly where this is evident in the Atlantic Panel of Figure 3.9. Is this referring to the far north regions in the upper Atlantic panel? [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Done.
3-588	3	21	2			Insert after word "trend": in the northern N. Atl. at depths of 1500-2000m [Terrence Joyce, United States of America]	Accepted. A much more specific reference to where in Fig 3.9 to look is added (several places actually, through the rewrite)
3-589	3	21	7	21	8	If you refer to AR4 then at least briefly state where in AR4 and say what was stated there in enough detail to satisfy the casual reader rather than force them to find that report, then find the section in which it was discussed, then read the section to see what was claimed. [John McLean, Australia]	Accepted. Made reference to Chapter 5 specifically.
3-590	3	21	18	21	18	From the same reasons I would suggest changing "NAO-dominated" to less arguable "NAO-influenced". This statement about Atlantic upper subpolar waters is too strong, much stronger than the (correct) one dealing only with LSW on page 22 (lines 37-38). [Government of Poland]	Accepted. Section changed.
3-591	3	21	24	21	31	This section attempts to identify all plausible causes but earlier sections don't. This is inconsistent. I'd like all sections to describe all plausible causes. [John McLean, Australia]	Accepted. The various sections are now much more streamlined. Do note, though, that some watermasses are more studied than others.
3-592	3	21	24			Again, some explanatory text about where and what depths this is found in the Fig would be helpful to the non-specialist. [Terrence Joyce, United States of America]	Accepted. Done.
3-593	3	21	30	21	30	Does "not so much" mean "very little" or should it be "less"? [Melissa Bowen, New Zealand]	accepted text reworded
3-594	3	21	38	21	38	"Figure 3.10" should be Figure 3.9 [Paul Durack, United States]	Accepted. Fixed.
3-595	3	21	38	21	38	The reference to Figure 3.10 should be Figure 3.9. [Government of Australia]	Accepted. Fixed.
3-596	3	21	38	21	38	The figure referenced in this line should be Figure 3.9 rather than 3.10. [Stephen Griffies, United States of America]	Accepted. Fixed.
3-597	3	21	38	21	38	Figure 3.10 - is this the correct figure to be cited here? [Janice Lough, Australia]	Accepted. Fixed.
3-598	3	21	38			the reference to figure 3.10 is not correct. [Government of France]	Accepted. Fixed.
3-599	3	21	38			Fig. 10 has nothing to do with water mass changes, only SSH. Do you mean 3.9 again? [Terrence Joyce, United States of America]	Accepted. Fixed.
3-600	3	21	42	21	42	Page 21, Line 42. Helm takes into account isopycnal movement in the water column, but not the surface salinity gradient. Consequently this analysis accounts for the southward displacement of isopycnals within the water column, but not the southward displacement of the surface outcrops. These papers are closer than perhaps understood. I suggest that the sentence change isopycnals outcrops to surface isopycnals outcrops. Final point at the salinity minimum, it is hard to freshen the salinity minimum, unless the surface sub ducted water is fresher. So the freshening of AABW, can be straightforwardly interpreted as freshening of the surface water. [Nathaniel Lee Bindoff, Australia]	partly accepted the text is rewritten to clarify the statements
3-601	3	21	54	21	55	This statement contradicts a statement made on p.11 lines 55-56 [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	rejected.. Just because salinity becomes more important, a temperature change can still be important

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							(if it is big enough).
3-602	3	22	1	22	1	No reference is made here to the Rintoul 2012 paper [Government of Australia]	rejected this paper is not yet published
3-603	3	22	6	22	6	You cannot say "global warming of AABW" because you are talking of a very small region of the Earth's surface. [John McLean, Australia]	Accepted. The word "global" is removed.
3-604	3	22	7			Does that reduction in volume take into account the thermal expansion resulting from the warming? [Government of United States of America]	rejected The thermal expansion is microscopical in comparison.
3-605	3	22	8	22	8	Are the numbers in square brackets the lower and upper bounds of the estimate of AABW formation? It's not clear. Some words should be inserted to say what the numbers are. [Melissa Bowen, New Zealand]	noted. uncertainty bounds in brackets follows the IPCC guidelines, as now explained at start of chapter
3-606	3	22	17	22	18	Only surface water and near-surface water is wind driven. The applicability of this comment depends on the depth of AABW but you've failed to mention it. [John McLean, Australia]	rejected. The cited paper shows how changes in wind-driven circulation, which extend throughout the water column in the poorly-stratified Southern Ocean, contribute to changes in water masses of the Weddell Sea
3-607	3	22	23	22	27	Please add the source of the data in the caption of Figure 3.9 [Hugues Goosse, Belgium]	Accepted. Done.
3-608	3	22	23			Figure 3.9: suggest to better link this figure to the text. The amount of information conveyed in this figure is substantial and it seems important to provide more guidance to the reader. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. Done
3-609	3	22	24	22	24	The actual layout of Figure 3.9 is salinity (column 1), neutral density (column 2), and potential temperature (column 3). [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted. Fixed.
3-610	3	22	29			Section 3.5.5: How important is the fact that changes in temperature and salinity may compensate for each other, in some cases. Does this contribute to obscuring the signal? [Government of United States of America]	Accepted. Discussion on this topic is included.
3-611	3	22	31	22	31	Nothing is said about the increased salinity of subtropical high salinity waters in all 5 basins, especially when analysed on density surfaces. This is a remarkable and globally consistent signal that many studies have confirmed and that deserves discussion and attention. [Government of Australia]	Accepted. Included.
3-612	3	22	31	22	34	The opening statement needs to refer to a time interval. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. Included.
3-613	3	22	37			This is a strong conclusion given the link between NAO and changes in Labrador Sea Water was not well established in section 3.5.2. [Government of United States of America]	Accepted. Statement is removed from the exec summary and reworded in the chapter body.
3-614	3	22	41	22	42	But be careful about attributing this to global warming - the base data set is too limited in time. Perhaps worth stating this here. [Terrence Joyce, United States of America]	rejected attribution is not part of Ch3.
3-615	3	22	44	22	44	The ocean circulation section should be strengthened. Ocean boundary currents and tropical ocean circulation are hardly mentioned in the review. Less attention is paid on the ocean circulation in the Indian Ocean. [Government of Australia]	Taken into account. This is an assessment, not a review as suggested here. It should be focused on issues of importance to climate and for which there is substantial observational evidence of long-term variability and change. There is certainly less such evidence for the Indian Ocean than for the Pacific and the Atlantic.
3-616	3	22	44			Section 3.6: The longest and most detailed descriptions of circulation are for those that have no strong signal. Suggest shortening those sections. [Government of United States of America]	Taken into account. The longest and most detailed descriptions are of the Pacific wind-driven circulation and the Atlantic Meridional Overturning Circulation. Their importance to global ocean circulation variability is now discussed; there are relevant multi-decadal datasets; the presence of interannual to decadal variability is noted with the difficulty this presents for

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							detecting long-term trends.
3-617	3	22	46	23	12	The difficulties to observe the integrated ocean circulation, and the potential advantages of data assimilation could be (shortly) mentioned here. [European Union]	Taken into account. The sense of this comment is contained in the new final two sentences of Section 3.6.3. See comment and response 3-665.
3-618	3	22	46			Section 3.6.1: Suggest including the period covered by the Drifter and Argo programs. [Government of United States of America]	Accepted. Sentence added: ". The drifter program achieved its target of 1250 drifters in 2005, and Argo its target of 3000 floats in 2007. "
3-619	3	22	46			The discussion of observation systems is incomplete, and not particularly well structured. It has no reference to the use of moored instrumentation, which play a key role in the TAO/TRITTON, RAPID/MOCHA and Greenland-Scotland Ridge arrays (to name a few). It also ignores the very important contribution from surface drifters. It doesn't read particularly well and would be better structured if it had an historical theme. [Toby Sherwin, United Kingdom]	Taken into account. The first 2 paragraphs of 3.6.1 are a very brief description of global (not regional) observations of ocean circulation. That is emphasized by inserting "global" as the 2nd word of paragraph 2. The regional observations such as RAPID are discussed in the appropriate sub-sections.
3-620	3	22	48	22	52	Need to mention here that it also includes altimeter data, which when combined with subsurface ocean data or with surface wind stress and drifter data can provide an estimate of the absolute geostrophic velocity at the ocean surface. [Terrence Joyce, United States of America]	Taken into account. The first paragraph of the sub-chapter deals only with the most recently implemented additions to global observations of circulation, i.e. drifters and Argo. The second paragraph discusses the 20-year timescale observations such as altimetry and WOCE/CLIVAR.
3-621	3	22	54			In the Chapter 3.6 you say that historical measurements of ocean circulations are sparse. On the basis of proxy data (sea sediments + tree rings) we have found connection between circulation and past climate changes (Medieval Warm Period and Little Ice Age). The variations were longer than only few decades. For more information see Helama, S., Timonen, M., Holopainen, J., Ogurtsov, M. G., Mielikainen, K., Eronen, M., Lindholm, M. and Meriläinen, J. 2009. Summer temperature variations in Lapland during the Medieval Warm Period and the Little Ice Age relative to natural instability of thermohaline circulation on multi-decadal and multi-centennial scales. J. Quaternary Sci., Vol. 24 pp. 450–456. ISSN 0267-8179. [Kari Mielikainen, Finland]	Rejected: Proxy measures of ocean circulation are beyond the scope of Ch3
3-622	3	23	4	23	12	The paragraph is mostly in introduction to the AMOC, and at best motivates the structure of the subsequent subchapters. I would expect to find a general account on the difficulties (and importance) of observing integrated oceanic quantities under the subsections title 'observing ocean circulation variability'. I suggest moving the present paragraph to section 3.6.2. (and possibly delete some of the duplication). [European Union]	Taken into account. The title of the sub-section is modified and the final two paragraphs are shortened and combined. It should be clearer now that this opening sub-section is intended both to document the recent global observations and to introduce the remainder of the section which takes regional perspectives.
3-623	3	23	14	23	19	The paragraph is mostly a motivation for the subsequent section (3.6.2.). I recommend to shorten it here or rewrite with a focus on the observational improvement (with giving the conclusions which should come in the next subsection). Another option would be to combine it with the preceding paragraph, merely introducing the structure of the subchapters. (If the present and preceding paragraphs are thought to summarize the subsequent subsections, the title of the present section should be changed.) [European Union]	Taken into account together with previous comment 3-622.
3-624	3	23	28			Section 3.6.2 – Consider turning some of this (individual information specific to particular bodies of water) into a table, making it easier to compare and see trends. [Government of United States of America]	Taken into account. This is a stylistic suggestion. We believe the content is better communicated as text.
3-625	3	23	28			The authors use a lot of words here to ultimately say that there are no multi-decadal trends. Consider condensing this discussion. [Government of United States of America]	Taken into account. The final paragraph of section 3.6.2 expands on the multi-decadal and global aspects of the variability.
3-626	3	23	30	23	31	I suggest skipping the reference to the Atlantic - analysing the Pacific's variability is justified by its own right. The relation to the Atlantic might make sense at the end of the subchapter (when the Atlantic chapter follows immediately afterwards) or the reverse statement at the beginning of the Atlantic subsection. But at the given location, the reader has hardly thought about the Atlantic overturning circulation, and does not need an	Accepted. The Atlantic reference is dropped, and this paragraph is rewritten.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						introduction to the Pacific through the Atlantic. [European Union]	
3-627	3	23	30	23	31	The invocation of the Atlantic MOC before its later description is out of place. The comparison between the two oceans should come in the introduction to the Atlantic. [Toby Sherwin, United Kingdom]	Accepted. The Atlantic reference is dropped, and this paragraph is rewritten.
3-628	3	23	32	23	33	Clarify what "last two decades" means. Is it 1991-2000 and 2001-10? Is it 1993-2002, 2003-2012? [John McLean, Australia]	Accepted, wording is now "since the early 1990s to the present"
3-629	3	23	35	23	44	It is very likely that a reader will infer that the 2-decade trends in the Alaska gyre and NPC are indications of a longer-term secular trend. Actually, the observations are an expression of the decadal mode of variability in the northern Pacific SSH. See (1) Lagerloef, et al, Twenty Years Of Satellite Altimetry: Understanding The Decadal Variability In The North Pacific Ocean – Overview, Proceedings of the 20 Years of Progress in Radar Altimetry Symposium, Sept. 2012, In Press; (2) Zhang, X. and J. Church, Sea level trends, interannual and decadal variability in the Pacific Ocean, Geophys. Res. Lett., 39, L21701, doi:10.1029/2012GL053240, 2012 (3) Cummins, P., Lagerloef, G. & Mitchum, G. (2005). A regional index of northeast Pacific variability based on satellite altimeter data. Geophys. Res. Lett., 32, L17607, doi:10.1029/2005GL023642. [Gary Lagerloef, United States of America]	Taken into account. Wording in this paragraph ("a similar trend" changed to "a similar 20-year trend", and the new sentences at the end of the 3rd paragraph of section 3.6.2, should clarify the difficulty of distinguishing secular trends in the presence of multi-decadal variability.
3-630	3	23	37	23	38	Shrinking of the Alaska gyre is stated to be "due to" strengthening and northward expansion of the NPC. This suggests cause and effect. Is this clearly the case? Could the shrinkage of the Atlantic gyre be forced by something else, and the northward expansion of the NPC merely a consequence? Do the two changes have a common cause? If cause and effect are understood, fine. Otherwise "associated with" might be better than "due to". [Adrian Simmons, United Kingdom]	Accepted. The sentence is reworded for better clarity.
3-631	3	23	37			The phrase "strengthened while shrinking in size" seems like a contradiction in terms. Perhaps you could say "Over te last two decades the circulation in the cyclonic Alaska Gyre has strengthened whilst its size has shrunk" [Toby Sherwin, United Kingdom]	Accepted. The wording is changed to "the cyclonic Alaska Gyre has intensified while decreasing in size"
3-632	3	23	46	23	56	It is worth mentioning the research on the strengthening trend of the subtropical cells in the Pacific (Feng, McPhaden, Lee, 2010, Geophysical Research Letters). [Government of Australia]	Accepted. Added at the end of the 3rd paragraph of 3.6.2: "This sea level increase in the western tropical Pacific also indicates a strengthening of the equatorward geostrophic limb of the subtropical cells. However, the 20-year increase reversed a longer term weakening of the subtropical cells (Feng et al., 2010), illustrating the difficulty of separating secular trends from multi-decadal variability."
3-633	3	23	51	23	52	In page 19 (lines 1-2) we had "weakening of the tropical Walker circulation" in the a 1854-2005 period. Here we have "a strengthening of the Walker circulation" in an unspecified period but based on papers from 2004 and 2005. The reader has every right to be confused. The reason of the confusion is using different time scales as clearly follows from reading section 2.7.5. which states "Since the 1980s or 1990s, however, trends in the Pacific Walker circulation have reversed". I suggest changing the test adding "recent" before "strenthening" or "over the last two decades" after it. [Government of Poland]	Accepted. As suggested, the word "recent" is added.
3-634	3	23	51	23	52	This is inconsistent with chapter 2 in which we are told that Walker Circulation has weakened. [John McLean, Australia]	Taken into account. The two new sentences noted above in response 3-632 (3rd paragraph of 3.6.2), clarify that the trend is different for different time periods.
3-635	3	23	52	23	54	This sentence needs correction. The period over which the 10mm/yr was calculated needs to be specified. If it is the last few years then the change is entirely consistent with the strong La Nina conditions, which have also meant that warm water moves into the region as the Pacific thermocline dips in the west, and that in turn means thermal expansion of the ocean water, which will mean a rise in sea level. [John McLean, Australia]	Taken into account. The period is specified in the caption to Fig 3.10. "The SSH trend (cm per decade, color shading) for the period 1993–2011"
3-636	3	23	54	23	56	Despite the recent trend over the past 20 years, there are arguments that this is part of a multidecadal variability, instead of climate change driven long term trend. [Government of Australia]	Accepted. The two new sentences noted above in response 3-632 (3rd paragraph of 3.6.2), clarify that there is multi-decadal variability with the trend being different in different time periods.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-637	3	23	54	23	56	I agree, but be careful about whether this is global warming or natural variability - the record is short! [Terrence Joyce, United States of America]	Accepted. This caution is expressed in the two new sentences noted above in response 3-632.
3-638	3	24	5	24	7	The time period for this study needs to be stated. The demise of El Nino conditions will cause a large pool of warm water to move from the central Pacific towards Australia. In some seasons that pool will pass north of Australia and enter the Timor Sea where it causes elevated sea temperatures and sea levels. In other seasons the water mass will strike the NE coast of Australia, with a substantial part added to the East Australian current and then into the Tasman Sea. [John McLean, Australia]	Taken into account. The time period is stated "over the past two decades" in the paragraph's opening sentence.
3-639	3	24	12	24	22	All this is consistent with the ENSO being dominated by conditions on the El Nino side of absolutely neutral (ie. SOI = 0), which has been the case from 1977, so the ENSO should be mentioned briefly in this section. [John McLean, Australia]	Taken into account. Due to space constraints, discussion of the relationship between Nino/Nina-dominant periods and STC weakness/strength strength, though correct is not added.
3-640	3	24	12	24	22	Why is the Antarctic Ocean being discussed in a section apparently devoted to the Pacific Ocean? [Peter Thorne, United States of America]	Taken into account. The Pacific sector of the southern ocean is nonetheless a part of the Pacific Ocean.
3-641	3	24	12			Mention here that this supports a continuation of recent (20) year trends over a longer period (40 years). [Terrence Joyce, United States of America]	Accepted. The longer time periods for sea level pressure trends (60 years) and for southern ocean warming (50-70 years) are stated.
3-642	3	24	15	24	15	Is this statement about 1 deg/40 years just an inference or is there a reference? [Catia Domingues, Australia]	Accepted. The reference (Gille, 2008) is shifted to the end of the sentence. See Gille (2008) Fig 8 caption.
3-643	3	24	18	24	18	Southward shift is also evident in Figure 3.9C, F and I with deep warming along axis of ACC in all 3 oceans. [Government of Australia]	Accepted. Figure 3.9 I is mentioned.
3-644	3	24	24	26	5	this section contains no discussion othe importance of S. Atlantic freshwater flux due to AMOC and its importance for the potential mono / bi- stability of the AMOC. It should at least mention the recent measurements of Bryden et al. (2011) J. Mar. Res. [Meric Srokosz, United Kingdom of Great Britain & Northern Ireland]	Rejected. Beyond the scope of this section. It's agreed that salinity/freshwater flux are important factors in AMOC amplitude. The specific relationship is model-depedant and the oceanic estimates of freshwater flux in the Atlantic (Bryden et al, 2011) remain highly uncertain.
3-645	3	24	24			Section 3.6.3: This section could be condensed and focus more on the observed changes. [Government of United States of America]	Noted.
3-646	3	24	26	24	27	For the non-expert, it would be helpful to highlight the significance of AMOC for the climate system, heat transport, etc... [Government of United States of America]	Accepted. Discussion of the significance of the AMOC is added in the 2nd paragraph of 3.6.3
3-647	3	24	26	24	30	This is wrong. It is correct if the zonal integration is in density space. I doubt this is worth more discussion, but at least re-word to make it more 'correct'. [Terrence Joyce, United States of America]	Accepted. The paragraph is rewritten to clarify that the AMOC amplitude is conceptually in density space. At some latitudes such as 26N (RAPID), depth gives a reasonable approximation, and most estimates of AMOC amplitude are given in depth coordinates (e.g. McCarthy et al., 2012). See also comment and response 3-649
3-648	3	24	26	24	30	I found this very long, single sentence explanation somewhat confusing and impenetrable. Can the sentence be broken up into a number of shorter sentences and clarified? [Peter Thorne, United States of America]	Taken into account. The paragraph is reworded for clarity, though not broken into shorter sentences.
3-649	3	24	26	24	36	in this section, the definition of the thermohaline circulation and therefore the definition of AMOC follow an old vision, don't taking into account the crisis of the THC paradigm due to the eddy contribution, the most important example being the Gulf Stream. A number of studies conducted over the past few years have challenged this new paradigm by revealing the vital role of the ocean's eddy and wind fields in establishing the structure and variability of the ocean's overturning, undermining that the overturning transport is continuous in space and time, yet susceptible in the main to change in deep-water production in the North Atlantic (Lozier, Science, 2010). [VINCENZO ARTALE, ITALY]	Accepted. The rewording of the first paragraph of Section 3.6.3 clarifies that conceptually the AMOC is in density (rather than depth) space. At some latitudes such as 26N (RAPID), depth gives a reasonable approximation. See also comment and response 3-647.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-650	3	24	26	24	36	This is a dull introduction to the AMOC. It fails to state its zonal range, give any indication of its meridional variations, hint at its forcing or say what its role is in controlling climate. [Toby Sherwin, United Kingdom]	Accepted. These two paragraphs are rewritten and expanded to discuss briefly the AMOC's roles in climate.
3-651	3	24	28			Insert after "upper":, lower density [Terrence Joyce, United States of America]	Accepted. Rewritten as "an upper limb of less dense waters"
3-652	3	24	29			Insert after "lower":, higher density [Terrence Joyce, United States of America]	Accepted. Rewritten as "a mass-balancing denser lower limb"
3-653	3	24	50			Mention that the sign difference in the 16N result is consistent with a positive flow in the upper layer. [Terrence Joyce, United States of America]	Accepted. Addition to the caption is (negative indicating the southward lower limb)
3-654	3	24	54			Is the reference here to MOC correct? If so, the acronym should be defined. [Government of United States of America]	Accepted. "MOC" changed to "AMOC"
3-655	3	25	5	25	14	This discussion of time series in the north Atlantic fails to include the current meter measurements since 1995 of the very important inflow of warm salty water to the Nordic Seas across the Greenland-Scotland Ridge. See e.g. Østerhus S., W. R. Turrell, S. Jonsson and B. Hansen (2005) Measured volume, heat, and salt fluxes from the Atlantic to the Arctic Mediterranean, Geophysical Research Letters, 32, L07603, doi:10.1029/2004GL022188 or a manuscript that has just been submitted to Deep-Sea Research by Berx, B, Hansen, Osterhus, S., Laresen, K.M., Sherwin, T. and Jochumsen, K., 'Combining in-situ measurements and altimetry to estimate volume, heat and salt transport variability through the Faroe-Shetland Channel'. [Toby Sherwin, United Kingdom]	Taken into account. These observations are discussed in 3.6.5.3.
3-656	3	25	16	25	17	Are these numbers representing McCarthy et al., 2012? This first sentence should be updated to use numbers from McCarthy et al, and also cite this study right here. [European Union]	Accepted. Updated with numbers from published version of McCarthy et al (2012), and citation moved as suggested.
3-657	3	25	19	25	19	The reference is not appropriate here. Kanzow et al., 2010 was not the first to point this out - it could already be seen in the first year of observations. I suggest deleting the reference. [European Union]	Accepted. Reference deleted.
3-658	3	25	32	25	38	There is no special need for this paragraph (or all the AMOC components listed in lines 5 to 14 deserve a paragraph each). [European Union]	Taken into account. The paragraph is shortened, but retained because the array is a measure of the AMOC lower-limb amplitude, though subject to assumptions of constant reference level and no transport in the eastern basin.
3-659	3	25	34	25	36	This sentence is very weak for IPCC report standard. It would be nice if the authors can rewrite or delete it, so that it strengthen the document. [Government of Australia]	Accepted. The sentence is rewritten.
3-660	3	25	36	25	38	Please see general comment 37 that was made on Chapter 2, but which applies to this sentence also. Arguably the word trend really should not be used for periods of a decade or so. [Adrian Simmons, United Kingdom]	Taken into account. The wording of the sentence is changed. A 10-year time-series can have a linear trend, but the length of the time-series and the confidence level of the linear trend should be stated.
3-661	3	25	40	25	44	I see no justification to present the (important and robust) geostrophic transport estimate at 16N as an AMOC estimate (as done in line 40), and also figure 3.11b. Line 34 specifically states that the 16N estimate 'may be representative of overall AMOC transport and variability...', but no reference is given. With similar justification, the line W deep western boundary estimate (Toole et al., 2010) or the 53N transport estimate (Fischer et al. 2010) could be presented as an estimate of an important component of the AMOC cf. lines 5 to 14). Hence, 16N should be taken out of the list of AMOC estimates, both in line 40, but also in figure 3.11b. [European Union]	Taken into account. This is essentially the same comment as this reviewer's comment 3-658.
3-662	3	25	40	25	44	Nice discussion! [Terrence Joyce, United States of America]	Noted.
3-663	3	25	46	25	52	This is a separate paragraph (on the indirect AMOC estimates). To represent the diversity of AMOC estimates, e.g., Grist et al., 2009 could be included in figure 3.11 [European Union]	Taken into account. The sub-chapter addresses ocean circulation, with focus on direct measures. The indirect AMOC estimates were included for

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							comparison, but their inclusion in Figure 3.11 did not seem necessary.
3-664	3	25	47			A new paragraph starts here. [European Union]	This comment is not understood. Incorrect line number?
3-665	3	26	5	26	5	Something like this could be added here: Due to the relatively sparse observational record, oceanic state estimates have been used as a surrogate for observations. While each state estimate represents an estimate of the AMOC in agreement with the considered observations, the different models and assimilation strategies employed result in a range of estimates of the AMOC and its variability (Munoz et al., 2011). [European Union]	Accepted. Text inserted with minor changes.
3-666	3	26	11			But probably not indicative of a long-term change (due to shortness of record). [Terrence Joyce, United States of America]	Noted.
3-667	3	26	19	27	13	this section is very interesting, but the MOC is characterized by a complex system of current that facilitate the inter-basin exchange within and between the global oceans and the Agulhas current system in the south of Africa is an important component of the THC, thus I would like to suggest to introduce one sub section on this matter as well as an other one on the Mediterranean Sea - Atlantic interaction [VINCENZO ARTALE, ITALY]	Taken into account. It's agreed that the Agulhas is an important contributor to inter-basin exchange, but there are not sufficient observations to justify an additional sub-section.
3-668	3	26	25	26	29	ITF transport measurements are discussed and Wunsch (2010) is referenced but only the value from Sprintall is quoted. It would be worth adding that the Wunsch (2010) 30-year estimate is consistent with the Sprintall estimate. [Melissa Bowen, New Zealand]	Accepted. The text is changed accordingly.
3-669	3	26	34	26	34	Add the statement for changes in transport in the Makassar Strait in 2008-2009 shown in Gordon et al. (2012, GJI 11602) and Susanto et al. (2012 J Geophys Res 117, C09013) [Jae Hak Lee, Republic of Korea]	Accepted. Susanto et al. (JGR, 2012) reference is added. The Gordon et al (GRL, 2012) reference is not appropriate.
3-670	3	26	34	26	35	Before the summary sentence, it is worth adding that "Due to the strengthening trend of trade winds in the Pacific and the rising trend of sea level in the equatorial Pacific in the past 20 years, the weakening trend of the Indonesian Through flow transport in the early decades has reversed, as inferred from sea level observations and reanalysis wind products (Feng, Boning, et al. 2011, Geophysical Research Letters)." [Government of Australia]	Taken into account. The suggested addition is appropriate, but is not inserted since it is lengthy, not based on ocean circulation observations, and does not alter the conclusion.
3-671	3	26	34	26	35	While it might be technically correct to state that something can't be excluded, the fact that it has never been observed is no reason to mention it. You seem to be implying that if no meteorite disrupts the traffic flow when I drive to work tomorrow I should mention this fact to anyone who asks what the traffic was like. [John McLean, Australia]	Accepted. The deleted wording is "or can be excluded".
3-672	3	26	39	26	40	Please link this statement with the conclusions of Section 3.4.4 [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. The statement is linked with 3.4.4 and 3.6.3
3-673	3	26	42	26	46	What is the time frame here, is the data too short to detect a trend in ACC or is there so much variability that a trend could not be detected even with a long (say 30 year) time series? [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. It's a 30-year time-series but very sparsely sampled in the time domain.
3-674	3	26	54	26	55	Need to consider Swart et al. 2012 findings. And what about Morrow et al. 's citation? [Catia Domingues, Australia]	Accepted (partially) The Morrow et al (Progress in Oceanography, 2008) reference is added. Swart and Fyfe (Nature Climate Change 2012) addresses model biases in the position and strength of the westerlies, and does not seem appropriate to cite here.
3-675	3	27	17	27	18	The word "some" should be inserted in this sentence because the text clearly shows that the statement does NOT apply to all ocean circulation systems. For complete clarity you should state something like "have been reported for 5 of 8 ocean circulation systems" (or whatever the correct figures are). [John McLean, Australia]	Taken into account. The first sentence of the paragraph is reworded for better clarity.
3-676	3	27	17	27	25	Consistent with the previous comment, I recommend adding to this conclusion that "It is likely (or very likely) that changes since 1993 in the NPC and Alaska Gyre surface circulation are signatures of regional decadal variability." [Gary Lagerloef, United States of America]	Taken into account. See this reviewers comment 3-629, and response. The new sentences at the end of the 3rd paragraph of section 3.6.2 are meant to clarify the difficulty of distinguishing secular trends in the

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							presence of multi-decadal variability. While it appears the reviewer attributes all of the changes since 1993 in the NPC and Gulf of Alaska to decadal variability, in fact one cannot separate the decadal variability and possible secular trends.
3-677	3	27	21	27	22	Please state the time frame. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted
3-678	3	27	23	27	25	Please state the time frame in the statement, so it cannot be used to undermine the assessment of trends in other ocean data. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted. However, there is no reason to think that the lack of trends in velocity/transport time-series, which are intrinsically noisier than the red-spectrum temperature data, would compromise the finding of trends in ocean heat content.
3-679	3	27	27	28	27	3.7.1. This introduction section has poor paragraph structure. It should either be re-written or replaced with the related sections from chapter 13. 3.7.1 should explicitly mention: The role of geophysical corrections, especially for tides. The role of ITRF and orbit determination. Role of pelagic tide gauges. Additional altimeters: cryosat-2 [Government of Australia]	Taken into account - A reference to comparable sections in Chapter 13 has been added. Adding more details on all corrections to altimetry measurements would take far too much room, and will not change assessment. A note has been added to Table 3.1 (note (a)) that the uncertainty in altimetry trend estimates includes measurable drifts from the instrument, orbit errors, the reference frame. Errors in tide models will have no effect on measurements of long-term changes, as periods (even aliased periods) are much shorter than 1-year. Pelagic tide gauges are bottom pressure recorders that are deployed for short periods and are not suitable for climate assessments. Cryosat-2 was designed for measuring ice sheet topography, and there is no peer reviewed literature using it to quantify sea level change. Moreover, the record is far too short (less than 2 years).
3-680	3	27	27	34	10	It is difficult to understand why this section on sea level change would reside here rather than in the chapter specifically focusing on this issue (chapter 13). While SL change is clearly an important ocean observation, I would have thought that this consideration would be trumped by the presence of Chapter 13. I suppose the rationale is that Ch 13 deals with processes and attribution, whereas Ch 3 (and Chs 4 and 5, which also contribute to Ch 13) deal with observations. I would still have preferred to see all of the evidence in one chapter when it is as important as this one. In part, this is because I regard SLR as a first-order impact of climate change rather than a component of climate change, and underpins many critical higher-order impacts described in Working Group II. That said, the authors of both chapters appear to have tried to co-ordinate their sections, though it is odd that I noted only two names that are in common between the two chapters: Mark Hemer (CA on both) and Philip Woodworth (RE for Ch 13 and CA on Ch 3). Moreover, in spite of the efforts of the authors, some statements emanating from the two chapters are still not fully consistent (see my ES comments). [Timothy Carter, Finland]	rejected- the structure of the WG1 report was approved by the plenary of the Venice Scoping meeting in 2009. Ch3 includes all relevant observed ocean parameters, including sea level. Ch13 takes the sea level results from Ch3 and deals with processes, attribution, and prediction of sea level change.
3-681	3	27	27	34	10	It is a bit confusing to have this text here, while Chapter 13 is completely on Sea level change. Omit here? [Hans Visser, The Netherlands]	rejected- the structure of the WG1 report was approved by the plenary of the Venice Scoping meeting in 2009. Ch3 includes all relevant observed ocean parameters, including sea level. Ch13 takes the sea level results from Ch3 and deals with processes, attribution, and prediction of sea level change.
3-682	3	27	27			Section 3.7: There appears to be considerable overlap in this discussion with Chapter 13. While Chapter 13 makes reference Chapter 3 when discussing overlapping areas, the same is not true for Chapter 3. [Government of United States of America]	rejected- the structure of the WG1 report was approved by the plenary of the Venice Scoping meeting in 2009. Ch3 includes all relevant observed

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							ocean parameters, including sea level. Ch13 takes the sea level results from Ch3 and deals with processes, attribution, and prediction of sea level change.. More references to overlapping sections in Chapter 13 have been added.
3-683	3	27	31	27	33	I think the wording here is overly obscure. For the sentence to be comprehensive, the phrase referring to transfers between land and ocean has to include exchanges involving glaciers, ice sheets, reservoirs, groundwater, soil moisture, and presumably, in vegetation. It seems to me that the phrasing just has to be made more explicit to indicate the various factors that are leading to the exchanges, especially as this is, at least in the future, going to include the largest term affecting sea level (i.e., ice sheets, etc.). [Michael MacCracken, United States of America]	Change to: Rejected – it is not necessary to list every water reservoir on land that can interact with sea level. Such a list is very long and make the sentence long and awkward. It is perfectly correct to state: "...as water is transferred between the ocean and continents, between the ocean and ice sheets..."
3-684	3	27	32	27	32	"...changes in tides.."? Is this suggesting that the tidal regime will change? If so, it needs some references. If not, I suggest that the reference to tides should be removed as they are not really relevant to what this sentence is about. [Neil White, Australia]	Taken into account - "water is redistributed within the ocean due to changes in the tides and ocean and atmospheric circulation" should have been "water is redistributed within the ocean due to tides and changes in the the oceanic and atmospheric circulation." Text will be changed.
3-685	3	27	33	27	33	Suggest revising the sentence beginning on this line as follows "SEA LEVEL CHANGE CAN BE MEASURED ON TIME SCALES RANGING from days to centuries," [Government of United States of America]	Noted - sentence will be rephrased as: " Sea level can rise or fall on time scales ranging from hours to centuries..." since the change occurs on these scales, not just the measurement of the change.
3-686	3	27	34	27	34	This sentence could be clarified by splitting into two as follows: "Thus, sea level...and dynamical signal. Importantly, measurements of sea level are..." [Government of United States of America]	Accepted - the sentences will be split.
3-687	3	27	37	27	39	Page 27. Line 37 to 39. Chapter 10 assess aspects of the detection and attribution human influences on sea level. This is section 10.4.3. A reference to this section in Chapter 10 would help. [Nathaniel Lee Bindoff, Australia]	Accepted - the reference will be added.
3-688	3	27	45	27	46	The statement that there were no island tide gauges before 1950 is WRONG. What about Ireland Island (Bermuda) in the early 19th century? This station has a PSMSL id of 4. Also Port Louis (Indian Ocean) started before 1950. I suspect that there are a number of others. There were a number of gauges installed in the tropical pacific in the late 1940s and the 1950s, and I also find the statement about the majority not being placed until the 1980s surprising. I suggest that this be re-researched and rewritten. In short, there were a number of tide gauges on islands before 1950. Some only just (a few years), but at least one long before. [Neil White, Australia]	Accepted - these sentences will be rephrased. It was meant that no records exist with records 100 years or longer on islands. This will be clarified. In addition, figures showing the gauges available for decades since 1880 have been included in a new Appendix.
3-689	3	27	50	27	50	There must be a better and more modern reference for GIA than Peltier (2001). There have been a number of papers since then by authors such as Kendal, Mitrovica, Tamisiea and Paulson. [Neil White, Australia]	Taken into account - although more recent references exist, they deal with details of GIA modeling and ice sheet models, and do not specifically comment on application of GIA to tide gauge records. Peltier (2001) is still a very good reference for this.
3-690	3	27	50	27	57	That there appear to be regional and large-scale biases (when compared to GPS VLM) in the best global GIA models is missing here. See King et al., 2012 and references therein. Alternative is to discuss under P28L54. King, M.A., M. Keshin, P.L. Whitehouse, I.D. Thomas, G.A. Milne and R.E.M. Riva 2012. Regional biases in absolute sea level estimates from tide gauge data due to residual unmodeled vertical land movement. Geophysical Research Letters, 39: L14604 doi:10.1029/2012GL052348. [Matt King, Australia]	Accepted - it will be discussed near P28L54 in terms of VLM corrections, and residual biases unrelated to GIA.
3-691	3	27	51	27	52	I don't think Holgate (2007) is the right reference here - one of Bruce Douglas' papers would be better here as they give more detail on this. [Neil White, Australia]	Accepted - will be substituted with Douglas (2001).
3-692	3	27	52	27	54	The Spada and Galassi approach only addresses GIA issues - it does nothing about other causes of VLM. [Neil White, Australia]	Taken into account - it will be made more clear that this only addressed GIA.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-693	3	27		34		No suggestion. Text is well and cautiously written, accurate and up to date with consideration of a broad range of representative authors and publications. [Government of Brazil]	Noted.
3-694	3	28	5	28	5	The T/P and Jason satellites go to (and just beyond) 66 degrees, not 65. [Neil White, Australia]	Taken into account - Text will be changed to state latitude extends slightly poleward of 66°.
3-695	3	28	9			The text in parentheses will likely not help a non-expert understand the meaning of a fixed geodetic reference. Suggest using a simpler explanation. [Government of United States of America]	Taken into account - there is not enough space to go into more details in Chapter 3. A reference to Chapter 13.1.2, which discusses this in more detail, will be added.
3-696	3	28	15	28	15	Please explain briefly where the 'mass change' comes from [Corinne Le Quééré, United Kingdom of Great Britain & Northern Ireland]	Taken into account - a parenthetical listing of major sources, and will be given, and a reference to Chapter 13.1.2 will be added.
3-697	3	28	23	28	23	Willis et al 2004 is only for the upper 750 m, so the residual is a combination of mass + thermal below 700 m. Domingues et al 2008 do include a deep ocean thermal component (with large uncertainties). [Catia Domingues, Australia]	Taken into account - however, the point of this statement is to comment on the general method of differencing altimetry and thermosteric data to estimate mass, which both studies do.
3-698	3	28	29	30	25	Query: should references to GPS be replaced with more general term GNSS? [Government of Australia]	Taken into account - all studies use Global Positioning Satellite (GPS) receivers and not another type of GNSS system, so use of GPS is appropriate.
3-699	3	28	29	30	25	Clarify the distinction between multi-decadal and seasonal cycles and long period tides. [Government of Australia]	Rejected - tide signals (including long period tides) have been removed, and seasonal cycles are not discussed.
3-700	3	28	31			You mention continuous records and cite figure 3.12, but not all of these records are continuous [Government of United States of America]	Taken into account - text will be revised to state records are nearly continuous, with only relatively short gaps.
3-701	3	28	32	28	33	Explain why there are "significant interannual and decadal fluctuations". [John McLean, Australia]	Taken into account - comments and references on relationship with ENSO already exist - p39L7-9, and in Sections 3.1 and 3.6.
3-702	3	28	38	28	39	You write: "computing regional sea level for specific basins then averaging based on the ocean area covered". In contrast, Jevrejeva et al. (2006) describe their method thus: "Global sea level rate is calculated by simply averaging the regional rates (the Baltic being excluded, as discussed later). Global sea level is found from the mean of the regional sea level curves." There is no talk of area-weighting the average. Already the peculiar weighting scheme employed to derive mean sea level for each region has very little to do with area-averaging. As far as I can tell, neighbouring tide gauges can differ in weighting factor by a factor of 2 or 4 for no good reason, unrelated to their quality. The "virtual station approach" of Jevrejeva is certainly not an attempt at deriving a properly area-weighted global mean, unlike the Church&White method. It is the task of the IPCC assessment to critically discuss such fundamental differences because this is critical to the interpretation of data sets! [Stefan Rahmstorf, Germany]	Accepted - It is correct that Jevrejeva does not use an area-weighted average, but a simple average. However, Merrifield et al do use an area-weighted average. The sentence will be revised to clarify this.
3-703	3	28	39	28	39	Consider including Woppelmann et al 2009 in the list of citations of work that computes averages of regional rates as representative of studies that have looked at GPS to correct VLM. [Matt King, Australia]	Accepted - will be added.
3-704	3	28	42	28	43	Is there now a citation for this? [Catia Domingues, Australia]	Taken into account - It is unclear what "this" responds to. The sentence in question is: "The time-series from different approaches show very similar long-term trends, but noticeably different interannual and decadal-scale variability (Figure 3.13a)." The statement is justified by the Figure and no citation is

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							needed.
3-705	3	28	44	28	44	Have updated time series been requested for the other sea level estimates? [Catia Domingues, Australia]	Taken into account - updated time series beyond 2010 were requested from all authors, only Jevrejeva provided a longer record to 2009, but it was not published in time to meet the IPCC deadline and can not be used
3-706	3	28	50	28	52	Figure 3.12 is too small. Readers will wonder whether you are trying to hide something. [John McLean, Australia]	Taken into account - Figure 3.12 is large enough to see all variations in sea level recorded at the tide gauges.
3-707	3	28	55	28	57	Include a comment that the project formerly known as SEAFRAME, run by Australia's Bureau of Meteorology and measuring sea level change in the south Pacific shows ENSO driven fluctuations but no discernible trend in sea level after the tidal gauge station heights have been corrected according to GPS measurement. (website http://www.bom.gov.au/oceanography/projects/spslcmp/country_report.shtml) [John McLean, Australia]	Rejected. Sea level plots in report are anomalies after removing a linear trend (e.g., Figure 10 in http://www.bom.gov.au/ntc/IDO60102/IDO60102.2006_1.pdf), not after removing VLM. Thus, they will not show a trend, only interannual variations. Figure 10 in the same report clearly shows linear trends after removal of VLM that are consistent with altimetry. . Moreover, the time period of the records only go back to 1993, and so provide little more information than can be determined from satellite altimetry.
3-708	3	29	4	29	5	The statement has limited credibility in light of the SEAFRAME data mentioned above (previous page lines 55-57). [John McLean, Australia]	Rejected - see reply to previous comment.
3-709	3	29	7	29	20	Somewhere in this paragraph you need to account for the difference between global average sea level as measured by tidal gauge and global average sea level measured by satellite. [John McLean, Australia]	Taken into account - Figure 3.13b shows the actual time-series over a common time-period, and differences (other than that caused by the difference in temporal averaging, which is discussed) are not significant outside the uncertainty bounds.
3-710	3	29	10	29	12	Explain why none of the six tidal stations shown in Figure 3-12 show any sign of any acceleration post 1993. Better yet, show a map of where this increase in sea level has occurred and then, assuming your claimed acceleration is correct, please account for the seemingly regional nature of that acceleration . [John McLean, Australia]	Rejected - no acceleration has been claimed since 1993, only a higher trend that may reflect multidecadal variations (discussed later in Section 3.7). The high, regionally incoherent interannual variations (like ENSO) with amplitudes exceeding 50 mm over periods of 5-10 years mask any visual detection of a trend change of less ~1 mm/year. This is why averages of tide gauges are used to detect variations in GMSL.
3-711	3	29	13	29	13	The "high confidence" should be highlighted in italic - this is an important conclusion. [Leticia Cotrim da Cunha, Brazil]	Accepted - Will be changed.
3-712	3	29	13	29	13	Need to consider Zhang and Church GRL 2012 citation. [Catia Domingues, Australia]	Rejected - this line refers to altimetric measurements of GMSL. The Zhang and Church reference is for Pacific average sea level from tide gauges. It is referenced in the regional sea level section.
3-713	3	29	13	29	13	The' high confidence should be in italic to be consistent [Government of Australia]	Accepted - Will be changed.
3-714	3	29	15	29	16	It should be considered here, that the higher rate of SLR as observed since 1993 could at least partly result from the increasing inflow of water/ice from melting/calving polar ice-sheets and inland-glaciers as well (see Fig. 3.13 ©; chapter 4.3, chapter 4.4) [Government of Germany]	Rejected - Chapter 3 is tasked with assessing observations of sea level. Chapter 13 is tasked with attributing mechanisms.
3-715	3	29	15	29	16	The statement about the high rate being due to a multi-decadal oscillation is unsupported by the evidence provided in section 3.7.4, see my comment to that section. And, of course, it is claimed here that it "is likely the	Taken into account - The statement has been removed from this section and is only discussed in

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						result of a multi-decadal oscillation" whilst 3.7.4. only says that it "likely reflects, in part, a multidecadal oscillation" - quite a different matter, but also this latter, much weaker statement is unsupported by any robust evidence. [Stefan Rahmstorf, Germany]	Section 3.7.4 when supporting evidence is presented.
3-716	3	29	15	29	19	Is this the best you can do - assert that maybe the very sudden acceleration was due to an unexplained oscillation somewhere that just happened to simultaneously impact a sufficient area of sea to make a difference to the global average sea level? This statement needs to either have supporting argument or be removed. [John McLean, Australia]	Taken into account - The statement has been removed from this section and is only discussed in Section 3.7.4 when supporting evidence is presented.
3-717	3	29	15	29	20	I don't understand the logic here at all--still to read section 3.7.4--but given that the Greenland and Antarctic ice sheets are now contributing to sea level rise and were not before, how can the acceleration be a fluctuation instead of an acceleration? Given what this chapter is supposed to cover--so say what is happening and it is leaving the reconciling about sea level rise to a later chapter, why is this comment on why it is happening even in this chapter. Did I miss something? Suggesting that fluctuations are longer than a decade or two seems to me very strange. [Michael MacCracken, United States of America]	Taken into account - Figure 3.12 clearly shows multi-decadal fluctuations at individual tide gauges, e.g., the drop in the 30s in Sydney, and the high in the 1950s at Mumbai (as commented on in Section 3.7.4), Figure 3.14 also shows multidecadal variations in GMSL reconstructions, and Chambers et al., 2012 have quantified the magnitude and phase of multidecadal variations. Chapter 3 is not tasked with discussing ice sheet observations, only ocean observations.
3-718	3	29	15	39	15	"likely" here conflicts with P32L15 "likely reflects, in part" - in part or not? [Matt King, Australia]	Taken into account - text will be revised to be consistent.
3-719	3	29	16	29	16	Is oscillation the most appropriate description? This could be interpreted to mean something like ENSO. [Government of Australia]	Taken into account - "Oscillation" will be changed to "variation" to reflect the fact the change may or may not be regular.
3-720	3	29	16	29	19	Page 29 Line 16 to 19. This is a valuable and important contextualisation of the lower rate in terms of earlier part of the record. [Nathaniel Lee Bindoff, Australia]	Noted.
3-721	3	29	16	29	19	It is not sufficient to say that maybe the reduced rate of rise is lower since 2005; the physical mechanism needs to be explained. [John McLean, Australia]	Taken into account - A new box is being added to Chapter 9 discussing the lower warming over the last decade. That will be referenced here.
3-722	3	29	18	29	18	Is the decreased insolation in the last solar cycle large enough to explain the observed change? [Government of Australia]	Taken into account - A new box is being added to Chapter 9 discussing the lower warming over the last decade. That will be referenced here.
3-723	3	29	18	29	19	Is this decrease in insolation discussed/supported elsewhere in AR-5 WG1? If so, provide reference to relevant section. [Janice Lough, Australia]	Taken into account - A new box is being added to Chapter 9 discussing the lower warming over the last decade. That will be referenced here.
3-724	3	29	23			this is already said before [Karina von Schuckmann, France]	Taken into account - "although for the latter it is only possible since the start of the GRACE measurements in 2002" will be deleted, as this is redundant with an earlier discussion.
3-725	3	29	25	29	25	50% higher rate was for the 1961-2003, same multi-decadal period in AR4. [Catia Domingues, Australia]	Taken into account - however, the new data over the same period 1961-2003 are still 50% higher, so the statement is correct.
3-726	3	29	25			"50% higher than estimates used for AR4" is a major change. Suggest highlighting this change and its implications. [Government of United States of America]	Taken into account - implications of this change are discussed in Chapter 13, in the synthesis and projections.
3-727	3	29	27	29	38	We wonder if the fact that the numbers reported here for the trends in thermosteric sea level rise for the top 700m and two different time periods are identical is real or might be a mistake? On line 28, the trend from 1971-2010 is reported as 0.6 [0.4 to 0.8] mm yr-1, on line 38 the trend from 2005-2010 is also reported as 0.6	Taken into account - a minor mistake has been found and the rate from 1993-2010 is 0.1 mm/year too low. This will be corrected. The trend from 2005 to 2010 will

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						[0.4 to 0.8] mm yr-1. Is this correct? [Thomas Stocker/ WGI TSU, Switzerland]	not be discussed, as it is too short a period to accurately determine trends not influenced by interannual variability.
3-728	3	29	31	29	31	For scientific accuracy I would replace "2000 m" with "2000 db". This might apply to other places in the manuscript also. [Sydney Levitus, U.S.A.]	rejected - most papers on the topic express the pressure level in terms of m of depth, not dbar, so we use this convention.
3-729	3	29	37	29	38	That is not entirely correct. There are many more "most recent" estimates for 2005-2010 in the upper 700 m: Levitus et al. 2012; vonSchuckman and Le Traon 2011; Domingues et al., 2008; Palmer et al. 2007,2009; Johnson et al. 2012. They may vary in rates but the differences appear to be within uncertainties. [Catia Domingues, Australia]	Noted - The period from 2005 to 2010 will no longer be discussed, as it is too short a period to accurately determine trends not influenced by interannual variability.
3-730	3	29	54	29	13	I find all these rates quoted from GRACE since 2002 very confusing. There are numbers in this chapter, others in chapter 13 (and some in chapter 4?). Sometimes it is not clear if models or measurements are being discussed. I think the message from these numbers has to be clearer. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Taken into account - Chapter 3 uses numbers from direct measurements of the mass over the ocean. Chapter 13 uses these estimates along with estimates of mass loss from ice sheets (from Chapter 4), which are expressed in terms of sea level rise. These can differ due to short term hydrological storage/loss of water from land. We will revise text to make sure that it is more clear that these represent only direct measurements over the ocean.
3-731	3	30	15	30	15	Figure 3.13 Y-axis labels should differ between axes to reflect different reference datum. [Government of Australia]	Rejected - the datum is the 5-year average over different period as clearly described in the caption.
3-732	3	30	15	30	25	Figure 3.13 is too small. Readers will wonder whether you are trying to hide something. [John McLean, Australia]	Rejected - Figure 3.12 is large enough to see all variations in sea level recorded at the tide gauges.
3-733	3	30	25	30	25	Figure 3.13, last sentence of caption - unclear what this means. [Janice Lough, Australia]	Taken into account - original GMSL time series are expressed as anomalies, relative to different time means (often decades apart). It is common practice to recompute anomalies relative to a common time average. The last sentence explains the periods the average is computed over, but will be moved to first line.
3-734	3	30	25	30	25	Interchange "1970" and "1993" in caption of Figure 3.13. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Accepted - the typo will be corrected.
3-735	3	30	27	31	6	global patterns replace with "spatial distribution" or similar. [Government of Australia]	Accepted - will change to "Large-scale patterns"
3-736	3	30	27	31	6	One of the major points being made regarding future sea level rise is that there will be large regional differences as the ice sheets melt, a major factor being changes in gravitational pull of the ice sheets plus accounting for Earth rotation, etc. It seems to me that it will be important to lay the foundation here for the projections of sea level rise, which have rather significant regional variations, and an indication of whether those types of changes have or have not started to become evident, what the factors might be that will determine this (i.e., the level of variability, other processes, etc.), but certainly the baseline for what will come later. [Michael MacCracken, United States of America]	Taken into account - this is discussed in the projections Chapter (Chapter 13), as noted in 3.7.1.
3-737	3	30	29	30	46	Despite all the caveats about where sea level has risen and by how much, it must have risen somewhere if the data from satellites is telling us so, so please show a map indicating for each location how much sea level has changed since the start of satellite monitoring [John McLean, Australia]	Taken into account - this is shown in Figure 3.10 and is referenced on Line 42 of this page.
3-738	3	30	29			The use of the expression "global patterns" is a little loose. Why not use "large-scale" or "basin-scale patterns" for actual non-uniform distributions over a large area and reserve "global" for global-mean values that average out any pattern by definition. [Government of France]	Accepted - will be changed.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-739	3	30	32	30	34	We seem to contradict here page 26 line 32. Trade winds either increase or not unless we are talking about different study periods. The cited paper, Merrifield and Maltrud 2012 deal only with the recent two decades when Walker circulation changed its decreasing trend (see section 2.7.5). There is also no reason to be not sure that this reversal comes from PDO variability. I suggest changing to "and may be explained by an intensification of the trade winds in the western tropical Pacific in the latest two decades (Merrifield and Maltrud, 2011), possibly related to the PDO (Merrifield et al., 2012; Zhang and Church, 2012)" [Government of Poland]	Accepted - page 26, line 32 deals with century scale winds. Here, we discuss only with results since 1993. We will clarify that we are only discussing the last 2 decades.
3-740	3	30	32	30	35	This statement is contradicted by by other data. If trade winds had increased in the Pacific Warm Pool we would have seen more La Nina events or at least an ENSO/SOI pattern that was dominated by conditions on the La Nina side of neutral, which according to observations is the opposite to what's occurred since 1976. Also try looking at the growth of sea mounts and at the the number of active subsea volcanoes near Samoa and Tonga because the water they displace has to go somewhere. [John McLean, Australia]	Rejected - there is no peer reviewed literature that contradicts the multiple studies that are referenced.
3-741	3	30	33	30	34	'is caused by an intensification of the trade winds' - however page 89 states that there has been a weakening of trade winds. Please clarify. [Government of Australia]	Taken into account - there is no page 89 in Chapter 3. It is assumed this refers to Chapter 2, which looks at a much longer time period. This discussion is only for winds in the last 2 decades, and is supported by numerous references. We will add a comment that this is only for the last 20 years.
3-742	3	30	34	30	34	The PDO is not defined until later on page 34, line 48. "PDO" appears in a few other lines between here and there. [Gary Lagerloef, United States of America]	Accepted - PDO acronym will be defined here.
3-743	3	30	34	30	34	Link to other part of AR5-WG1 report that covers intensification of Pacific trade winds [Janice Lough, Australia]	Taken into account - Chapter 2 does not currently discuss the intensification over the last 20 years. They have been asked to do so. When Chapter 2 is updated, the reference will be added.
3-744	3	30	34			There is a previous reference from Feng et al. [Catia Domingues, Australia]	Noted - While the Feng et al., 2010 does discuss correlation between western pacific and SE Indian Ocean sea level and winds on decadal scales, it does not link the changes over the last 30-years to the lower frequency portion, but focuses on El Nino-scale variations.
3-745	3	30	37	30	37	Add the description for sea level change in the northwestern Pacific Ocean given by Marcos et al (2012, Prog in Oceanogr 105 4-21). Regional differences of SL change and its possible dynamics are discussed. [Jae Hak Lee, Republic of Korea]	accepted results and citation (Marcos et al 2012) added
3-746	3	30	43)and (typo) [Catia Domingues, Australia]	Editorial - will be corrected
3-747	3	30	49	30	51	This sentence is not necessary here. [Xianyao Chen, China]	Rejected - sentence is quite important, as it clearly states the assessment that regional sea level variations can be significantly larger than the global mean.
3-748	3	31	1			Did the authors intent to refer to Neah Bay, WASHINGTON? [Government of United States of America]	Noted - Washington was meant and text has been corrected.
3-749	3	31	4	31	6	It logically follows from your statement that irregular sea level variation cannot be due to CO2 because that gas is relatively evenly distributed around the world. Please add this to your paragraph. [John McLean, Australia]	Taken into account - Chapter 3 only deals with observations of sea level. Chapter 10 and 13 are responsible for attribution and predictions.
3-750	3	31	5	31	6	If by "this assessment" one means AR5 as a whole, not the WG1 report, it might be questioned whether sea-level change due to factors not related to climate change are really out of scope of the assessment. Adaptation will have to be to the net change of sea-level, comprising both climate-change and other components. So is not the non-climate-change component of sea-level rise within the scope of what WG2 has	Taken into account - the comment was meant to refer to the WG1 report. This will be clarified.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						to be concerned with? [Adrian Simmons, United Kingdom]	
3-751	3	31	8	32	15	Query: should references to GPS be replaced with more general term GNSS? Clarify the distinction between multi-decadal and seasonal cycles and long period tides. [Government of Australia]	Rejected - tide signals (including long period tides) have been removed, and seasonal cycles are not discussed. All studies have used GPS data, so this is an appropriate term.
3-752	3	31	8			The section is headed "Observations of Accelerations in GMSL" but then wildly mixes up global and local records (e.g. Watson 2011 for Australia). Keeping those separate is critical, because there are many mechanisms that can make ocean waters move around (e.g. wind forcing) which physically cannot change total ocean volume, i.e. GMSL. In fact, most of the internal variability in "global" tide gauge reconstructions is very likely due to such regional variability projecting onto the global average because of the sampling problem of the limited numbers of tide gauges, and is not variability in true GMSL - see later comment. [Stefan Rahmstorf, Germany]	Taken into account - title will be changed to "Assessment of Evidence for Accelerations in Sea Level Rise " to reflect that many of the studies focus only on a specific region.
3-753	3	31	10	31	12	This sentence is further support for the contention that sea level change has little if anything to do with thermosteric expansion driven by CO2 because if it was, sea level would change synchronously with atmospheric CO2, which it isn't. Sea level is also not changing synchronously with temperature because temperatures were flat from 1945 to 1977, generally rose from 1977 to 1997 and have been flat since but sea level has consistently risen. Please revise this entire section. [John McLean, Australia]	Rejected- Chapter 3 deals exclusively with assessing observations of sea level. Chapters 10 and 13 deal with explaining processes of sea level change.
3-754	3	31	10	32	15	Whilst I think this is a very informative and much improved section from the First Order Draft (FOD), I would be making the point at the end of this section that sea level acceleration is a more difficult analytical task and that acceleration has been demonstrated to be particularly temporally sensitive due to the significant inflexions particularly in the 20th century MSL time series. This ideology is inferred, but, not explicitly stated. [Phil Watson, Australia]	Taken into account - paragraph will be modified to better reflect this explicitly.
3-755	3	31	12	31	14	Has there not also been progress in understanding and clearing up various problems and biases with various of the data records, thus removing some decadal/multidecadal features in the record that were previously thought to indicate variability? [Michael MacCracken, United States of America]	Taken into account - all records used (and studies cited) use the most recent, corrected data sets, and multidecadal variations remain. As discussed throughout Chapter 3, these are caused by climatic variations in winds and ocean currents, and do not reflect errors or biases in the data.
3-756	3	31	18	31	18	Fitting a quadratic term is not very informative, except when a quadratic is indeed a good model for the observed time evolution (which it rarely is). See detailed explanation in the article "Don't estimate acceleration by fitting a quadratic..." at http://www.realclimate.org/index.php/archives/2012/11/dont-estimate-acceleration-by-fitting-a-quadratic/ [Stefan Rahmstorf, Germany]	Taken into account - the chapter can only refer to peer reviewed literature, not blogs. Quadratic terms have been commonly fit to tide gauge records and have been discussed in the literature, so must be assessed here. Some discussion about the issue of estimating quadratic terms will be added, but quadratic terms can be discussed if appropriate uncertainty estimates are presented. Uncertainty estimates are inflated to account for correlations caused by unmodeled variations in the residuals to the model, and the 60-year sinusoid removes most of the infections.
3-757	3	31	19	31	21	One would expect here a critical assessment of the claims of "start time of acceleration", which to me appear highly subjective and not entirely scientific. In a complex time evolution there is not one meaningful "start time of acceleration". I suggest to not uncritically repeat the poorly founded claims of these papers, unless it is shown they are statistically robust and meaningful. [Stefan Rahmstorf, Germany]	Noted -There is no claim of a "start time of acceleration", only a reporting of the results in the peer reviewed literature of attempts to quantify whether a mean acceleration exists or not.
3-758	3	31	21	31	22	"increase in the rate" - over what time period and increase relative to what? [Janice Lough, Australia]	Taken into account - the time periods are stated at the end of the sentence.
3-759	3	31	21	31	22	"the increase in the rate of sea level rise at Amsterdam ... is xx mm yr-1" -- shouldn't this be in units of mm yr-2 if it represents a change in the rate of increase? [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - this is a change in trends, so mm/year is correct. However, we will add "per century" to show it is a change in trends per 100

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							years.
3-760	3	31	22			It this data is acceleration, the units should be in mm yr-2 [Government of United States of America]	Taken into account - this is a change in trends, so mm/year is correct. However, we will add "per century" to show it is a change in trends per 100 years.
3-761	3	31	23			Please check to make sure the years are correct: "based on differencing 100-year trends from 1874-1884 and 1885-1985" [Government of United States of America]	Accepted - 1874 is a typo. It should be 1774.
3-762	3	31	26			Salt marsh data should also be mentioned here - see Gehrels and Woodworth, GPC, in press. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Taken into account - this is discussed in Chapter 5, and referred to Page 27, line 38. A small comment stating salt marsh records linking the geological record and modern records will be added there.
3-763	3	31	28			This entire section is thoroughly misguided. First, attempts to determine "acceleration" by fitting a quadratic are discussed, but fitting a quadratic term is not very informative, except when a quadratic (+ random noise) is indeed a good model for the observed time evolution (which it rarely is). See detailed explanation in the article "Don't estimate acceleration by fitting a quadratic..." at http://www.realclimate.org/index.php/archives/2012/11/dont-estimate-acceleration-by-fitting-a-quadratic/ How misguided this is has specifically been shown for the Houston and Dean (2011) paper in the comment to that paper by Rahmstorf and Vermeer (2011 - cited nearby), and for Watson (2011) by statistician Grant Foster in his article "How Not to Analyze Tide Gauge Data" (http://tamino.wordpress.com/2011/07/22/how-not-to-analyze-tide-gauge-data/). I am thus very surprised that IPCC discusses this quadratic fitting exercise as if it were a valid approach, completely ignoring the published fundamental methodological criticisms. Subsequently it is stated that there are three inflection points, which already makes it clear that the data cannot be fitted by a quadratic, since a quadratic curve has no inflection points. That is why the discussion of quadratic terms leads to contradictory results and is ultimately meaningless. This discussion based on quadratics should be cut altogether. [Stefan Rahmstorf, Germany]	Taken into account - text has been revised to: "The trend in GMSL observed since 1993, however, is not significantly larger than the estimate of 18-year trends in previous decades (e.g., 1920-1950)."
3-764	3	31	31			as well as in [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Editorial - will be corrected
3-765	3	31	32			"Sallenger" misspelled as "Sallengar" [Robert Dean, United States of America]	Editorial - will be corrected
3-766	3	31	35	31	43	I am very surprised by your handling of the papers of Houston and Dean (2011) and the reply by Rahmstorf and Vermeer (2011). These are two peer-reviewed papers in the same journal. The second largely invalidates the first. However this does not appear at all in your text, it looks like Houston and Dean is as valid as other publications on the topic, and you even suggest that it is more ("more numerous... records") in spite of its regional nature, and ignoring the fact that Rahmstorf and Vermeer explained those results (by methodological flaws and a link to temperature that reduces the impression of randomness). I have the strong impression that this text needs a complete revision, aiming at what the IPCC needs to provide - a solid and balanced assessment of all the available data. [Philippe Marbaix, Belgium]	Noted - The calculations from Houston and Dean (and others) are correct for what they are: results of a quadratic fit over a certain time period. We agree they are not correct in terms of their interpretation for evaluating a long-term average acceleration. In the context of an assessment, they should be mentioned, and various pieces of evidence (including Rahmstorf and Vermeer) discussed as to why the interpretation is incorrect. This section is written in that context.
3-767	3	31	37			Sallenger misspelled as "Sallengar" [Robert Dean, United States of America]	Editorial - will be corrected
3-768	3	31	39	31	41	The explanation of Figure 3.12 here seems not quite right. E.g, Increase trend in New York started from 1930, not 1920-1940; downward trend after 1960 is also not found for New York. [Dongxiao Wang, China]	Taken into account - on lines 39 to 40, the effect of upward downward trends on top of a long term trend are quantified (i.e., a leveling of sea level starting around 1960 if a long term trend is not removed).
3-769	3	31	40	31	41	Regards the Fremantle record, this from Church et al (2006), "Sea-level rise around the Australian coastline and the changing frequency of extreme sea-level events", Aust Met Mag. 55 (2005), 253-260 - "There are suggestions in both the Australian mean time-series and in a number of the individual records (e.g. Fremantle) that the rate of sea-level rise was at a minimum from the mid-1970s to the mid-1990s. This minimum occurs during the period of more frequent, persistent and intense ENSO events, as evidenced by the SOI since the mid-1970s (Folland et al. 2001). ENSO events significantly affect sea level along the west Australian coast. The results are also consistent with global analyses for the period 1950 to 2000 which show a global minimum in sea-level rise in the western equatorial Pacific and in the eastern Indian Ocean (Church et al. 2004) over	Taken into account - El Nino variations have a much shorter period than the multidecadal variations discussed here. The discussion here is on changes over periods longer than 30 years, not the 5 to 7 year period typical of El Nino. A brief comment on El Nino variations in sea level records such as Fremantle and San Francisco will be added in Section 3.7.3.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						this period." (The SOI data indicates that when Church says "ENSO events" he means El Nino events, which is incorrect because he has said elsewhere that La Nina events bring higher sea level to the west Australian coast.) Please summarise this and include it in the text. [John McLean, Australia]	
3-770	3	31	42			Change "terms" to "term" [Robert Dean, United States of America]	Editorial - will be corrected
3-771	3	31	47			Show that an acceleration of 0.01 mm/yr ² would amount to 5 cm in a century [Robert Dean, United States of America]	Rejected - stating this would be confusing, since the mean trend of 1.7 mm/year is responsible for the majority of the sea level rise over the last century (17 cm).
3-772	3	31	55	32	9	Is it not also possible that this pattern was caused by the change in forcing such that the period since the 19th century into mid 20th century was affected by increasing solar, CO2 and CH4 as well as lessening volcanic activity and period from mid-century to 1980s by declining solar, increasing sulfate exceeding increasing CO2 and CH4 contributions to forcing, then since 1980s due to rising CO2, CH4 and declining sulfate forcing? What is basis for thinking the variations are natural internal fluctuations (for which no mechanism is really identified) rather than responses to changes in external forcing (and there have been periodic changes in forcings earlier as well, given Tambora and Krakatoa)? Do model simulations without any external forcings show the 60-year or so cycle? It seems to me that the external forcing hypothesis merits mention as well. [Michael MacCracken, United States of America]	Taken into account - this is discussed in Chapter 10. A statement will be added at introduction to 3.7 directing reader to the appropriate section in Chapter 10 regarding attribution tests.
3-773	3	31	55	32	15	Is oscillation the most appropriate description? This could be interpreted to mean something like ENSO. [Government of Australia]	Taken into account - "oscillation" will be changed to "variation"
3-774	3	31	55	32	15	I had a look at papers discussing SLR acceleration, and I have the impression that - the data suggest that there is an acceleration in the recent decades, - this acceleration is small and there is substantial variability, thus, if looking only at the recent decades, the degree of confidence may be relatively limited - and should be assessed (also in connection with ch 13). Thus I am not convinced by your analysis as it stands : to be valid, it would need that there is a 60 year global oscillation (not just the AMO), and that this oscillation is entirely natural. However, the ups and downs in temperature trends for specific periods in the 20th century are not entirely natural. There is most probably a link between sea level rise and global mean atmospheric temperatures, as suggested e.g. by Rhamstorf and Vermeer (2011). Climate models can explain most of those non-linear changes during the 20th century, largely due to anthropogenic forcings. Thus a part of the observed fluctuations may look like natural oscillations, but we know from the radiative forcing data and climate models that a substantial part of those fluctuations is not natural, and has no reason to repeat itself. If you remove such fluctuations from the SLR data, you are thus likely to remove a part of the anthropogenic signal, and given that you are removing a supposed 60 year oscillation out of a ~100 year time period, you may actually remove the acceleration that you are trying to identify and attribute. It seems that the computations from line 10-12 of page 32 (removal of 60 year oscillation) are not from published sources. Given the problems that I explained above, I am not convinced that this is valid. More justifications would be needed. How did you come to the "likely" in the last sentence (line 15, page 32)? I would feel it more logical to have a sentence that just says that to date, the acceleration is relatively small and that part of it may be due to natural variability, although correlation with temperature suggests that there is a contribution from anthropogenic causes. Finally, I could not find strong support for this 60-Y oscillation concept in chapter 2 (in contradiction with the link that you included in that paragraph). [Philippe Marbaix, Belgium]	Taken into account - section has been modified, clarifying the extent of multidecadal variations, and clarifying the danger of interpreting accelerations in shorter records because of this. While technically correct that the recent sea level has accelerated, there is evidence it has decelerated in the recent past, and accelerated before that. In light of this, one cannot be certain the acceleration will continue. Chapter 3 only discusses observations of sea level, and cannot consider observations from outer areas (ice sheets, atmosphere, etc). These are covered in later synthesis (Chapter 13) and attribution (Chapter 10) chapters.
3-775	3	31	55			Previous references were to "late 1980s" as opposed to 1990 here [Government of United States of America]	Taken into account - Paragraph has been revised to be consistent with dates
3-776	3	31	55			This paragraph is very biased and very poorly founded in evidence. It starts off by saying that it is "possible" that inflections are caused by a multi-decadal climate oscillation, provides no further evidence for this actually being the case, and then miraculously ends with the conclusion that the recent high rate of SLR "likely reflects, in part, a multidecadal oscillation." (The "in part" is then conveniently forgotten on page 29, without further evidence, just referring to this paragraph for evidence.) What is stated, namely that accounting for a 60-year cycle makes agreement between data sets worse, does	Taken into account - text has been revised to state that there are multidecadal variations in the sea level records, for which there is strong evidence. Using a 60-year period does not change the acceleration outside of the 90% confidence range: note [0.006 to 0.012] mm/year ² for Church and White (2006) fitting

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						not exactly speak in favor of the hypothesis that the authors are trying to push here. [Stefan Rahmstorf, Germany]	only a trend+quadratic versus [0.009 to 0.015] mm/year ² when fitting a trend+quadratic+60-year sinusoid. The value for the previous estimate from Church and White (2006) has been moved closer to the new estimate. The difference between time series is not a function of using a 60-year sinusoid in the model.
3-777	3	31				1874-1884 should probably read 1774-1884. [Government of France]	Editorial - will be corrected
3-778	3	32	4			The high rates around 1930 are commented on also in Gehrels and Woodworth re. changes in glaciers and Greenland and also other papers (I think Gregory et al submitted to J Climate). [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Taken into account - numerous studies discussing high rates have already been utilized.
3-779	3	32	8	32	8	should be. Is this advice or a statement of applied processing? [Government of Australia]	Taken into account - text has been modified to describe what was done, not to prescribe what should be done.
3-780	3	32	11	32	11	Is the (0.007 to 0.019) from Jevrejeva also a 90% confidence range. If so, please indicate, or else clarify. [Thomas Stocker/ WGI TSU, Switzerland]	Taken into account - 90% confidence has been explicitly stated.
3-781	3	32	12	32	12	Page 32, Line 12 to 14. could a likelihood value be assigned to result for twentieth century acceleration, and also could the sentence be added that makes clear the long term 20th century acceleration for the northern hemisphere and its likelihood. [Nathaniel Lee Bindoff, Australia]	Taken into account - Confidence is given in conclusions of section 3.7, but will also be added in Section 3.7.4.
3-782	3	32	12	32	14	Page 32, Line 12 to 14. could a likelihood value be assigned to result for twentieth century acceleration, and also could the sentence be added that makes clear the long term 20th century acceleration for the northern hemisphere and its likelihood. [Nathaniel Lee Bindoff, Australia]	Taken into account - Confidence is given in conclusions of section 3.7, but will also be added in Section 3.7.4.
3-783	3	32	12	32	15	Pls see the previous comment on the possible contribution by increasingly melting/calving glaciers and ice-sheets [Government of Germany]	Taken into account - assessment of contributions to sea level change are treated in Chapter 13, and pointed to in the introduction to Section 3.7.
3-784	3	32	14	32	15	Because this statement has no substantial supporting argument it is mere speculation and therefore should not appear in this report. [John McLean, Australia]	Taken into account - text has been revised to: "The much higher trend in GMSL observed since 1993, however, is not different than the best estimate of 18-year trends in previous decades (e.g., 1920-1950)."
3-785	3	32	14	32	15	<p>There are two possibilities to explain the recent, relatively high rate of SLR measured by satellites. It could be (A) a response to global warming or (B) a result of natural internal variability of sea level. Let us look soberly at the evidence with respect to both:</p> <p>(A) Global warming.</p> <p>1. According to the best tide gauge data compilation we have (that of Church and White), the rate of SLR during the past twenty years is unprecedented since records began. This is supported further by Ray and Douglas (2011).</p> <p>2. There is good physical reasons to expect that global warming will increase the rate of sea-level rise. Warmer global surface temperatures mean that continental ice melts faster and that heat penetrates at a faster rate into the ocean.</p> <p>3. Quantitatively, the present rate of SLR of ~3 mm/year is predicted by process models, as chapter 13 finds: "The sum of model-based contributions shows an increase in rate from 1990, as also observed ..." Note this is predicted by models that run free and thus do not predict the phase of any internal oscillation like AMO or PDO; in the models the high rate is due to climate forcing, not internal variability. Note that in the models, as in the data, the most recent rates are unprecedented in a hundred years (Fig. 13.4).</p> <p>4. Also, the present rate of SLR around 3 mm/year exactly fits in the observed correlation between sea-level and global temperature that has held well since 1870, the data period covered by the Church & White data.</p>	noted - assessment of contributions to sea level change are treated in Chapter 13, and pointed to in the introduction to Section 3.7. Chapter 3 is tasked with assessing the measurements of sea level change.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						<p>The correlation is well-documented in the peer-reviewed literature (Rahmstorf 2007a, Vermeer & Rahmstorf 2009) and highly statistically significant ($P = 0.002$, Rahmstorf 2007b). It is, frankly, very surprising that an IPCC assessment of observed sea-level rise in the context of global warming does not even discuss the observed correlation between the observed rate of rise and observed global warming, despite the relevant Science paper (Rahmstorf 2007a) being the most-cited paper on sea level since the AR4, with over 300 citations. If IPCC has reasons to dismiss the observed correlation, then those should be discussed, but simply ignoring prominent published evidence is highly inappropriate.</p> <p>(B) Natural variability.</p> <ol style="list-style-type: none"> 1. Different tide gauge data show inconsistent decadal variability in the rates of SLR. This is not surprising, given the uncertainties in estimating global sea-level rise from sparse tide gauge data and the fact that the time derivative of any “noisy” time series will be much more noisy still. 2. The current evidence strongly suggests that most of the internal variability in many tide gauge data sets reflects a spatial undersampling problem – i.e. it describes variability at the locations of the tide gauges, but not in the true global mean sea level – see detailed discussion in Rahmstorf et al. 2012 and references cited therein. Mechanisms like wind forcing can strongly alter sea-levels at coastlines without being able to change global-mean sea level. The methodologically by far best, and least noisy, attempt at reconstructing global mean sea level from limited tide gauge data is that of Church and White, which does *not* show rates similar to the altimeter rate during 1930-1950 despite the contrary claim in the draft IPCC paragraph. The uncertainty ranges shown in Fig. 3.14 of the altimeter and of the C&W data during 1930-1950 do not overlap. 3. The nature and causes of any sea-level variability unrelated to global temperature changes are not well understood. The paragraph here only provides hand-waiving arguments mentioning the AMO and PDO, but provides no statistical evidence for a significant correlation between GMSL and either of these modes. The AMO has its largest positive peak between 1950-1960 when rates of SLR in Fig. 3.14 are especially low, so the AMO does not fit the sea-level signal. Even if an effect of AMO and PDO on sea level reconstructions exists, it is most likely because these regional (!) modes affect regional sea level, which projects on the global mean of tide gauge reconstructions as an artifact of poor sampling. Note that the AMO shows little if any projection on global mean temperature (Knight et al. 2006, Delworth et al. 2000, Mann and Emanuel 2005) so it is unclear how it would affect global sea level. Likewise, the PDO index by its definition has global mean SST subtracted so is not a global variability pattern. 4. In particular, since we have neither a statistical correlation nor a physical mechanism for what might drive multidecadal GMSL variability, it is not understood what phase it might be in and whether natural variability would have worked to enhance or reduce the rate of sea-level rise since 1993. It is thus pure speculation whether natural variability might have added to the recent high rates, or in fact prevented even higher rates that we would have observed without internal variability. No scientific basis whatsoever is presented for the view that natural variability has enhanced the recent rates. <p>In summary: the claim that the global rate of sea-level rise was likely similarly high in 1930-1950 as it is today is ill-founded (the best sea-level data show the contrary), and the claim that recent high rates are (partly) due to natural decadal variability are completely unfounded. One cannot help but wonder whether double standards are at play here – if there were no statistical correlation of global warming and sea level rise, no physical understanding of a mechanism for how global warming might affect GMSL, and no knowledge whether it might currently enhance or reduce SLR, would the IPCC authors have concluded that the “recent high rates of SLR are likely due to global warming”? I doubt it. So how could this be concluded for decadal variability?</p> <p>Based on a sober look at the evidence, in contrast we must come to the following conclusions which I recommend as SPM conclusions:</p> <ol style="list-style-type: none"> 1. Recent high rates of sea-level rise since 1993 are likely unprecedented since at least the 19th Century. (That at least seems to be the case in Ray and Douglas 2011 and Church and White 2011 according to Fig. 3.14; I have not had a chance to look at Jevrejeva 2012 but assume it is based on the questionable “virtual station method” of their earlier papers, which produces lots of spurious variability. These unprecedented high 	

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						<p>rates since 1993 are also supported by both process-based and semi-empirical models.)</p> <p>2. Consideration of the physical mechanisms, the fact that they are correctly modelled as well as the strong and highly statistically significant observed correlation of global temperature with global sea-level rise since 1880 makes it very likely that recent high rates of sea-level rise since 1993 are largely a result of global warming.</p> <p>References</p> <p>Rahmstorf, S., 2007a. A semi-empirical approach to projecting future sea-level rise. <i>Science</i>. 315, 368-370. Rahmstorf, S., 2007b. Response to comments on "A semi-empirical approach to projecting future sea-level rise". <i>Science</i>. 317. Rahmstorf, S., et al., 2012. Testing the Robustness of Semi-Empirical Sea Level Projections. <i>Climate Dynamics</i>. 39, 861-875. Vermeer, M., Rahmstorf, S., 2009. Global Sea Level Linked to Global Temperature. <i>Proceedings of the National Academy of Science of the USA</i>. 106, 21527-21532. [Stefan Rahmstorf, Germany]</p>	
3-786	3	32	15			This is a very subtle distinction! [Terrence Joyce, United States of America]	Taken into account - text has been revised to: "The trend in GMSL observed since 1993, however, is not significantly larger than the estimate of 18-year trends in previous decades (e.g., 1920-1950)."
3-787	3	32	17	32	42	The substance of this section is unclear. What means "we consider evidence of changes in extreme sea level independent of changes in storminess but related to changes in mean sea level" ? How can "maximum surge" (storm surges ?) be related to mean sea level rise, as it relates to surface wind and pressure ? How does this relate to increased storminess, intensity of tropical and extrat-tropical storms in the Atlantic ? Does this just mean that the only coherent signal over time (multi-decadal trend) in a given place is due to mean sea level rise ? Is this conclusion relevant to policy makers or misleading (the devastating highest extremes may NOT be related to climate change, but to phasing of high tidal waters and storm: see Xynthia) ? Is it worth keeping this in the SPM ? Is it consistent/not duplicating chapter 13. [Government of France]	Taken into account - Section 3.7.5 has been revised to make it clear only changes in the amplitude of extreme sea level have been measured to be increasing, and also to review conclusions from Chapter 2 that there is little evidence for changes in storminess. Moreover, a new Figure has been added to quantify measured changes and to show where (and by how much) the results are reduced to when MSL change is accounted for.
3-788	3	32	19	32	42	This section is dissapointingly short. Please expand. [Aslak Grinsted, Denmark]	Noted - section has been expanded slightly, a review of conclusions in Chapter 2 on changes in storminess has been added, and a new Figure has been added.
3-789	3	32	19			Are tsunamis really a non-climatic event? Ice-ages due to variations in solar forcing are regarded as climate events, as are temperature variations due to volcanic eruptions. So why should a long-term change in frequency or intensity of tsunamis due to a long-term change in forcing from tectonic activity not be regarded as a climatic change? The frequency and intensity of tsunamis can be regarded as part of the climate of those coastal regions affected by the phenomena. And even if the frequency and intensity of tsunamis does not change, their impacts may change as sea-level rises, if adaptive action is not taken. This of course assumes that "climate" is defined in a broad sense so that it does not refer only to some average of the state of the atmosphere, but also to some average of the state of the ocean and terrestrial conditions such as soil moisture, snow cover and lake temperatures. [Adrian Simmons, United Kingdom]	Noted. We know of no peer reviewed literature that has discussed changes in frequency of tsunamis
3-790	3	32	22	32	23	" We consider evidence of changes in extreme sea level independent of the changes in storminess but related to changes in mean sea level. ". This sentence is not very clear or not relevant here. This Chapter does not deal with attribution or modelling, but what is considered are the " observed changes in extremes ". The cause of these changes has been discussed in Chapter 13. [Government of India]	Accepted - sentence has been removed, and paragraph has been revised to be clear that only measured changes in extremes are discussed. A brief reviewwith conclusions from Chapter 2 on strominess has also been added.
3-791	3	32	24	32	25	" Since extreme sea level events are often perceived as a regional problem, global analyses of the changes in extremes are limited, and most reports are based on analysis of regional data ". This sentence is not clear. Extreme sea levels are "perceived" ? The phenomenon is basically regional. [Government of India]	Noted - sentence has been revised to read: "Global analyses of the changes in extremes are limited, and most reports are based on analysis of regional data ".

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-792	3	32	27	32	41	Please clarify what has been found in terms of extreme sea level trends. The text mentions that trends have been found, but it is not clear what the trends represent, how large they are, and what the level of confidence is. [European Union]	Accepted - text has been revised to quantify magnitudes and to include a figure of observed changes in height of 50-year flood events.
3-793	3	32	27	32	41	Please clarify what has been found in terms of extreme seal level trends. The text mentions that trends have been found, but it is not clear what the trends represent, how large they are, and what is the level of confidence. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted - text has been revised to quantify magnitudes and to include a figure of observed changes in height of 50-year flood events.
3-794	3	32	28	32	28	"increasing" - since when? 20th century? Last half of twentieth century? [Janice Lough, Australia]	Noted - specific dates will be added.
3-795	3	32	29	32	29	Please add this paper to that list: Grinsted, A., J. C. Moore, and S. Jevrejeva (2012), Homogeneous record of Atlantic hurricane surge threat since 1923, PNAS, doi:10.1073/pnas.1209542109 In it we observe that surges have become more frequent (after removing effects of tides, sea level rise and seasonal cycle) for the US gulf and east coasts. [Aslak Grinsted, Denmark]	Noted - reference has been added in terms of evaluations of extreme events.
3-796	3	32	31	32	32	Intention of language is unclear and appears to discredit observing system. [Government of Australia]	Taken into account - 'global' has been changed to global without quotation marks.
3-797	3	32	32			delete "reasonably" in this line [Government of United States of America]	Accepted.
3-798	3	32	33			Menendez should have an acute accent over the second 'e'. Her name appears several times in the chapter. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Editotial - accent has been dropped in conversion from Word document to PDF. Will correct.
3-799	3	32	46	32	47	Saying that the rate of sea level is between X and 50% more than X is extremely vague for a scientific report, besides which you give no indication of the basis for your claim of "virtually certain". [John McLean, Australia]	Noted - Text has been changed to reflect it is virtually certain sea level has risen, and rates are very likely.
3-800	3	32	47	32	47	Unless impossible to rectify the Cis should be recalculated and quoted as 90% Cis for consistency with TSU stipulated guidance and within and cross-chapter consistency in the treatment of Cis. [Peter Thorne, United States of America]	Accepted - Confidence intervals will be changed to 90% here and in the Executive Summary.
3-801	3	32	47	32	49	No, the SEAFRAME GPS-corrected data shows no such increase, just ENSO-driven and transient variability (see website http://www.bom.gov.au/oceanography/projects/spslcmp/country_report.shtml). Please correct your sentence. [John McLean, Australia]	Rejected. Sea level plots in report are anomalies after removing a linear trend (e.g., Figure 10 in http://www.bom.gov.au/ntc/IDO60102/IDO60102.2006_1.pdf), not after removing VLM. Thus, they will not show a trend, only interannual variations. Figure 10 in the same report clearly shows linear trends after removal of VLM that are consistent with altimetry. . Moreover, the time period of the records only go back to 1993, and so provide little more information than can be determined from satellite altimetry.
3-802	3	32	47			Why use 99% confidence here and 90% in section 3.7.2 (p. 28 ln. 45)? [Government of United States of America]	Taken into account - Confidence intervals will be changed to 90% here and in the Executive Summary.
3-803	3	32	49	32	52	The conclusion that rates of sea level rise were LIKELY comparable during the 1930-50 period to that since 1993 seems unjustified given the differences between the three data sets. It would be more justifiable to simply point to this period as raising an interesting question about the possible role of multi-decadal oscillations on the sea level record. [Michael Oppenheimer, United States of America]	Taken into account - text will be changed to reflect the conclusion that rate of sea level rise has more likely than not varied at multidecadal periods in the 20th century and that the higher trend in GMSL observed since 1993 is not significantly different than the best estimate of 18-year trends in previous decades (e.g., 1920-1950).
3-804	3	32	49	32	54	Unfounded conclusions, as detailed in my earlier comments. [Stefan Rahmstorf, Germany]	Taken into account - text will be changed to reflect the conclusion that rate of sea level rise has more likely than not varied at multidecadal periods in the 20th century and that the higher trend in GMSL observed since 1993 is not significantly different than the best

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
							estimate of 18-year trends in previous decades (e.g., 1920-1950).
3-805	3	32	50	32	52	Again, is it not roughly as plausible that it was changes in external forcings that caused the observed fluctuations as that it was caused by an unexplained internal fluctuation, the period of record for which includes a number of external forcings that could have given a suggestion of such a periodicity? [Michael MacCracken, United States of America]	Noted - the certainty language of likely (66% confidence) allows for this possibility. Attribution of variations is treated in Chapter 10, as noted in Section 3.7.1.
3-806	3	32	56			The conclusion regarding "large-scale winds and ocean circulation" is not supported with discussion in this section [Government of United States of America]	Taken into account - it is discussed in Section 3.7.3, Paragraph 1.
3-807	3	32	57			"higher or lower rates ..." suggest insert "of sea level rise" to reduce ambiguity and emphasize the point [Government of United States of America]	Accepted.
3-808	3	32		33		It is said that extreme sea levels are related to increasing mean sea level. This is a disputable conclusion. The sea level extreme events have scale of meters while the mean sea level rise has scale of cm. This is a hundredfold difference. The cause in extreme sea level rise is due instead to increased storminess which occurs along with mean sea level rise. So we disagree with the way this whole paragraph is written. [Government of France]	Noted - While extreme sea level events are on the order of several meters, the change in the amplitude of the extreme events is much smaller, of order cm per decade (mm per year). Thus the rate of the change of the amplitude is nearly the same size as rates of MSL rise. A new figure showing the observed rates with and without removing local MSL variations has been added.
3-809	3	33	1	33	5	Downwelling radiation from CO2 penetrates just a few microns into the ocean and ocean heat will rise to the surface, transfer to the air via evaporation and rise by convection. Given all that, please explain the processes by which the ocean supposedly warmed to 700m and even to 2000m. [John McLean, Australia]	rejected the reviewer is referred to FAQ 3.1 "is the ocean warming?" and to oceanographic textbooks explaining how ocean currents transfer heat and other properties from the surface ocean to the interior of the ocean
3-810	3	33	7	33	9	Presumably, the rate of water addition to the ocean did not suddenly jump to this range in 2005 from no contribution before that year. Is there any basis for suggesting a rate earlier, or at least a time when there was no significant contribution occurring? It is also not at all clear how this first sentence relates to the rest of the paragraph? Is the rest of the paragraph referring to changes possibly due to water addition to the ocean? I would not think so, but making the addition sentence the first one in the paragraph makes one think so, and this should be clarified. [Michael MacCracken, United States of America]	Taken into account - text has been changed and trends of ocean mass will not be discussed in this Chapter.
3-811	3	33	7			The units do not match the text: "ocean mass has increased at a rate between 0.8 to 1.6 mm yr-1". The use of mm yr-1 for mass does not make sense. [Government of United States of America]	Taken into account - text has been changed and trends of ocean mass (in equivalent water thickness) will not be discussed in this Chapter.
3-812	3	33	14	33	16	Acceleration discussion is based on inappropriate statistical method (fitting quadratic), as detailed before. [Stefan Rahmstorf, Germany]	Noted - however, fitting quadratic parameters has been commonly used as a method to infer long-term average acceleration in numerous studies.
3-813	3	33	16	33	16	Page 33 Line 16. Given the high levels of likelihood for global mean sea level change I wonder if the lower level of extremes makes sense, given that is due to the shift of the mean. Is if mean change is virtual certain, why isn't the extremes changes virtually certain at global scale? [Nathaniel Lee Bindoff, Australia]	Noted -Confidence is only likely as there is a single recent global study. Moreover, a small fraction of the gauges have extremes that are falling, even with MSL removed.
3-814	3	33	16	33	16	range is 0.0 to 0.019mmyr-2 this range does not include the confidence intervals and is thus not entirely consistent with pp3-32 one 10 and Figure 3.14. [Government of Australia]	Taken into account - will be corrected to include full confidence interval.
3-815	3	33	16	33	16	missing italic on "I" in likely [Matt King, Australia]	Editorial - will be corrected
3-816	3	33	17	33	17	Please quantify the amplitude of changes in sea level extremes and where they occur. [European Union]	Accepted - A new figure showing the observed rates with and without removing local MSL variations has been added.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-817	3	33	17	33	17	It is said that extreme sea levels are related to increasing mean sea level. This is a disputable conclusion. The sea level extreme events have scale of meters while the mean sea level rise has scale of cm. This is a hundredfold difference. I believe that the cause in extreme sea level rise is due instead to increased storminess which occurs along with mean sea level rise. So we disagree with the way this whole paragraph is written. [Government of France]	Noted - While extreme sea level events are on the order of several meters, the change in the amplitude is much smaller of order cm per decade (mm per year). Thus the rate of the change of the amplitude is nearly the same size as rates of MSL rise. A new figure showing the observed rates with and without removing local MSL variations has been added.
3-818	3	33	17	33	17	Please quantify the amplitude of changes in sea level extremes and where they occur. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Accepted - A new figure showing the observed rates with and without removing local MSL variations has been added.
3-819	3	33	20	34	10	Nice summary. It might be useful to also summarize the relative contribution of thermosteric warming and mass addition to the GMSL rise rate since GRACE (eg. 60%:40%, mass:heat). [Terrence Joyce, United States of America]	Noted. This is, however, more relevant to Chapter 13.
3-820	3	34	1			For an example of my comment above, see last line of the Table 3.1. The 1.2 is the same number as the 1.7+/-0.5 on p4 and p98 of chapter 4? [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Taken into account - mass trends from GRACE expressed in Chapter 3 are based on measurements over the ocean only. Chapter 4 deals exclusively with ice mass loss converted into equivalent sea level change. Text has been modified to clarify that Chapter 3 deals only with the ocean observations.
3-821	3	34	12	41	17	Section 3.8. - Delete this section entirely. No subjects discussed in this section either influence the climate or are influenced by it, so this section has no logical place in a report focussed on climate. Sections 3.8.1 and 3.8.2 belong in a UNEP report that discusses physical changes to the environment that are associated with increased atmospheric carbon dioxide. [John McLean, Australia]	rejected. The structure and scope of the WG1 report was approved by the plenary at the Venice scoping meeting in 2009. Both Changes in oceanic carbon and pH are part of the physical science basis of climate change, since carbon dioxide is a primary driver of climate and the ocean uptake of carbon dioxide has direct influence on atmospheric CO2 levels, this section is relevant.
3-822	3	34	12			This section on ocean carbon is lacking information on estuaries and coastal zones [European Union]	rejected estuaries and coastal zones are discussed in WG II
3-823	3	34	12			Section 3.8 :Suggest adopting a standard terminology. The alternating use of acidification as an increase in acidity and then as a decrease in pH is confusing. [Government of United States of America]	noted These terms are now defined in Section 3.8
3-824	3	34	14	34	23	Suggest that C-ant be defined the first time it is used. [Government of United States of America]	accepted Cant defined when first used
3-825	3	34	18			Section 3.8.1: Suggest the authors explain why the C storage rate is high in the N Atlantic and in the southern high latitudes (Fig. 3.16), but the inventory is very high only in the N Atlantic (Fig. 3.15). [Government of United States of America]	Rejected. Both the storage rate and inventory are higher in the North Atlantic. Text of caption revised to highlight change in colour scale in each basin in figure of storage rates.
3-826	3	34	20	36	16	This section needs to provide an assessment on the trends in total ocean CO2 uptake. Did the ocean CO2 sink weaken or not in the past few decades, as has been inferred in some regions (N Atlantic, EqPac and Southern Ocean)? [European Union]	noted This issue was and is addressed in section 3.8.2 in summary, ocean observations are insufficient to assess whether there was a change in the rate of carbon uptake. It is also discussed in Chapter 6, including model results
3-827	3	34	20	36	16	This section needs to provide an assessment on the trends in total ocean CO2 uptake. Did the ocean CO2 sink weaken or not in the past few decades, as has been inferred in some regions (N Atlantic, EqPac and Southern Ocean)? [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted This issue was and is addressed in section 3.8.2 in summary, ocean observations are insufficient to assess whether there was a change in the rate of carbon uptake. It is also discussed in Chapter 6, including model results
3-828	3	34	20			Suggest mention of the long residence time of C once it gets into the interior and that C removal mechanisms	noted This is addressed in Chapter 6.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						exist (sedimentation) but cannot keep up. [Government of United States of America]	
3-829	3	34	21	34	21	"may have a significant impact"? Define what you mean by "significant" and then there's no "may" about it, either it does or it doesn't, so say which. [John McLean, Australia]	accepted Text corrected as suggested by the reviewer
3-830	3	34	21	34	22	Page 34 line 21 to 22. This sentence is only relevant if the ocean gave up carbon dioxide, where as the ocean is still a sink of anthropogenic carbon, and so I would have expressed the importance of the oceans role in taking up anthropogenic carbon. [Nathaniel Lee Bindoff, Australia]	Partially accepted. Introductory paragraph revised to state importance of the ocean for uptake of anthropogenic CO2
3-831	3	34	21	34	22	Given this ratio of loadings, this phrasing gives an implicit message that then any change in the atmospheric loading should have a small effect on the ocean--and yet this is not at all the case for acidification. I'd suggest a bit of clarification so as not to be indicating the converse--namely that a change in the atmosphere would not be able to have an effect on the ocean. [Michael MacCracken, United States of America]	accept Text revised. The chapter makes clear the connection between ocean uptake of carbon and ocean acidification.
3-832	3	34	21			The connection implied by the word "thus" would benefit from an explanation for the non-expert. [Government of United States of America]	accept Text corrected.
3-833	3	34	22	34	24	Page 41, robustness of these methods needs to be discussed, and also the completeness of the profile data needed to make these estimates should also be discussed. I would advocate an appendix that covers the sampling distributions for various data sets, including anthropogenic carbon. I am surprised at the high likelihood attributed to these estimates, virtually certain, while ocean heat content is in some ways less reliable, although the estimation process for ocean heat content is more simple and direct. A statement of why these data are more reliable than OHC estimates would help in this section. [Nathaniel Lee Bindoff, Australia]	accept Section 3.8.1.2 addresses this issue directly, and an appendix is added where the data coverage is presented
3-834	3	34	26	34	26	Section 3.8.1.1: A recent manuscript submitted before the July 31 deadline may be relevant to this section: Jiang, Gille, Sprintall and Sweeney, Southern Ocean pCO2: Evaluating CMIP5 Coupled Carbon/Climate Models using in-situ observations, J. Clim. In Revision. [Gary Lagerloef, United States of America]	rejected Ch3 does not deal with validation of climate models
3-835	3	34	26	35	2	Section 3.8.1.1. This section needs to mention that the global air-sea CO2 flux computed from Delta pCO2 is smaller than the actual carbon accumulation in the ocean because of the additional input of carbon from rivers. [Ralph Keeling, United States of America]	accept Text corrected as suggested by the reviewer
3-836	3	34	26			Suggest the title be revised to "Observed Long-Term Trends and Variability in the Ocean Uptake of Carbon" [Government of United States of America]	noted. Ch 3.8 now consists of 3.8.1.1 ocean uptake of carbon, 3.8.1.2 changes in the oceanic inventory of anthropogenic carbon dioxide, 3.8.2 ocean acidification, 3.8.3 Oxygen, and 3.8.4 Nutrients
3-837	3	34	28	34	30	I suggest adding Ballantyne et al. 2012 (doi:10.1038/nature11299) to the citations supporting the percentage of anthropogenic carbon absorbed by the oceans as the most recent paper in a high-impact journal dealing with the subject. [Government of Poland]	accepted Ballantyne et al. 2012 added
3-838	3	34	28	34	30	This sentence is biased towards anthropogenic emissions when they contribute only a very small proportion of the CO2 pool. Surely we are not expected to believe that the oceans ignore all naturally produced CO2? [John McLean, Australia]	rejected Many of the studies include natural CO2 cycles. See Quay et al (2003).
3-839	3	34	28	35	2	This section only focus on CO2 inventories. No mention is made to observed changes in ocean productivity (biological carbon uptake and sequestration). Although the report adds new information on the AR4 it fails to point out the present knowledge about the variability observed in the biological pump. Perhaps the composition of the expert pannel remains still biased towards experts in physics and ocean chemistry, while there are few biologists among the authors. [Antonio Bode, Spain]	rejected These changes are small as described in Chavez et al (2011) which is cited in the text.
3-840	3	34	28	35	29	The conclusion at page 35 line 28-29 should be presented earlier in the text, preferably at the start of section 3.8.1.1 (line 28 page 34). Right now the conclusion seems to be a bit hidden. There is a lot of information and the main conclusions should be easier to find. [Government of NORWAY]	accepted. Conclusion is now highlighted in a separate paragraph at the end of this section
3-841	3	34	30	34	30	Provide link to Chapter 6 where these numbers are discussed in more detail [European Union]	accepted Reference to Ch6 added
3-842	3	34	32	34	33	This statement needs to be clarified. If the dpCO2 remains unchanged, the ocean CO2 sink also does not change, which is probably not true. This statement is contradicted by the numbers presented in Table 3.2,	accepted the text is reworded. There is no observational evidence that the ocean uptake

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						where two of the four updates trends in ocean pCO ₂ (the longest time series) do not follow atmospheric CO ₂ . [European Union]	changed in the global mean
3-843	3	34	32	34	33	This statement needs to be clarified. If the dpCO ₂ remains unchanged, the ocean CO ₂ sink also does not change, which is probably not true. This statement is contradicted by the numbers presented in Table 3.2, where two of the four updates trends in ocean pCO ₂ (the longest time series) do not follow atmospheric CO ₂ . [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted the text is reworded. There is no observational evidence that the ocean uptake changed in the global mean
3-844	3	34	39	34	40	Please explain where this uncertainty comes from, and what it relates to (50% of what?) [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Reference provided
3-845	3	34	40	34	40	Insert bolded words - "...uncertainand thus are not suitable for detection of trends in DELpCO ₂ ...". This is better language to clarify lack of ability to detect vs lack of evidence for what is expected to be a small (relative to the observation errors) trend. [Government of Australia]	accepted statement reworded and enhanced
3-846	3	34	45	34	47	Providing only the differences in pCO ₂ as absolute value (i.e., the reference to 100 microatm for delta pCO ₂) might be confusing. Would it be useful to provide absolute quantities for pCO ₂ atm and pCO ₂ oc, as well? Or refer to Ch06 for more details? [Thomas Stocker/ WGI TSU, Switzerland]	accepted Reference to Chapter 6 added
3-847	3	34	49	34	50	please state how much CO ₂ uptake has increased since 2004 [Government of Iceland]	noted The inventory increase from 1994 to 2010 is given in section 3.8.1.2.
3-848	3	34	49	34	50	Can uncertainty be added to the reported decrease in North Atlantic CO ₂ uptake? [Thomas Stocker/ WGI TSU, Switzerland]	accepted Uncertainty Added.
3-849	3	34	55	35	2	The sentence should include Lenton et al (2012) reference, which shows a decline in CO ₂ uptake over the last twenty years for the subtropical North Pacific and the Pacific and Indian Ocean basins of the Southern Ocean, but an increase in the uptake for the Atlantic sector of the Southern Ocean due to changes in stratification. Ref: Lenton, A., Metzl, N., Takahashi, T., Kuchinke, M., Matear, R. J., Roy, T., Sutherland, S. C., Sweeney, C., and Tilbrook, B.: The observed evolution of oceanic pCO ₂ and its drivers over the last two decades, Global Biogeochemical Cycles, 26, Artn Gb2021, Doi 10.1029/2011gb004095, 2012. [Government of Australia]	accepted reference added.
3-850	3	35	9			what is "the Green's function approach"? [Government of United States of America]	accepted Text gives short explanation what that is
3-851	3	35	10	35	12	The statement needs clarification. The assumption of steady state in ocean circulation does not assess the natural variability (i.e. it does not underestimate it, it just does not include it). Please also explain the basis for the assessment that neglecting changes in circulation and much smaller than method uncertainty. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted text reworded
3-852	3	35	14	35	20	Different techniques in estimating Cant can have similar inventories, but depth distributions that are very different. This can in turn affect the depth of lysocline because of anthropogenic ocean acidification (Anthropogenic carbon distribution in the eastern South Pacific Ocean, Biogeosciences, Goyet et al. 2009). This would be good to mention as a segue into the next segment on ocean acidification. To me this also suggests that there exists good confidence, at least regionally, in Cant inventories, but perhaps less confidence in being able to globally state where the lysocline has shoaled [Andrew Shao, United States of America]	rejected The Goyet et al (2009) paper does not show time series measurements of changes in pH which is the subject of that paragraph.
3-853	3	35	14	35	29	These paragraphs are confusing. Is the objective to show how the carbon inventory changed from 107 (or even 118) in 1994 to 155 in 2011?. 50 Pg C in 7 years? That is considerably more than 2 Pg C/yr. Why 1994 – particularly when Figure 3.15 is for 2010? [Government of United States of America]	taken into account. For comparison with other estimates the uptake rate for 2000-2010 is given in the text. The difference between 1994 and 2010 is emphasized, because the first global estimate from observations was done for 1994
3-854	3	35	18	35	18	150 PgC in Figure 3.15 not 155. [Janice Lough, Australia]	accepted the number has been corrected in the figure caption
3-855	3	35	23	35	25	An update to Manning and Keeling (2006) is currently in press (and submitted before the July 2012 deadline) in the Treatise on Geochemistry which gives an estimates of 2.2 +/- 0.6 Pg C yr for the decade 1990-2000 and 2.5 +/- 0.6 for the decade 2000-2010. The latter is in good agreement with Ishidoiya. I can provide a copy of	accepted the new reference is added

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the new manuscript on request. It's possible that the NIES group under Tohjima also has relevant recent data that could be cited by IPCC. [Ralph Keeling, United States of America]	
3-856	3	35	24	35	29	There is a range of methods that support an ocean sink around 2 PgC/yr. A full list needs to be provided here, as it is the bases for the high confidence in the mean ocean CO2 sink estimate. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	partially accepted. We now emphasise that different methods give a number around 2 PgC/yr, but do not include an exhaustive table of all results due to lack of space
3-857	3	35	26	35	29	No mention of the river correction that has been applied to surface pCO2 measurements. This may be considered misleading given the text states the estimates were from surface pCO2 measurements [Government of Australia]	accepted Text corrected as suggested by the reviewer
3-858	3	35	27	35	28	Why hasn't Wanninkhof et al (2012) estimate been included, or is this just considered an increment of the Takahashi estimate? [Government of Australia]	rejected Wanninkhof et al not accepted for publication yet.
3-859	3	35	34	35	35	The name "Sea of Japan" must be used instead of "the East Sea (Sea of Japan)," because "Sea of Japan" is the only internationally established name for the sea area concerned. [Government of Japan]	accepted Sea of Japan used in the figure caption
3-860	3	35	38	35	39	In practice, computing the rate of uptake of CO2 by looking at differences in Canth between two time frames also requires steady state assumptions in order to separate natural variability in DIC from the anthropogenic component. The clause about this approach being less sensitive to steady state is therefore not necessarily correct. This clause should be deleted. [Ralph Keeling, United States of America]	accepted Text corrected as suggested by the reviewer
3-861	3	35	39	35	40	Do not use the term "broad agreement" because it is highly subjective. State the correlation coefficient instead. [John McLean, Australia]	accepted. do agree within their uncertainties is inserted
3-862	3	35	49	35	49	Typo: "found a the" [Stephen Griffies, United States of America]	Text corrected as suggested by the reviewer
3-863	3	35	51	35	56	reference to Sarmiento et al., 2010 is given for both sides, the studies that report a reduced CO2 uptake and those studies that conclude that the decline is rather unlikely...is this correct? Note that the "unlikely" will either need to be italicized if it's the result of a formal assessment of the uncertainty or replaced. [Thomas Stocker/ WGI TSU, Switzerland]	accepted reference removed
3-864	3	35	53	35	54	Page line 53 to 54. This interpretation of LeQuere isn't quite right, the assertion was that the southern ocean rate of uptake had declined, not that the uptake had declined. So the southern ocean uptake had slowed rather declined. It is an important distinction. [Nathaniel Lee Bindoff, Australia]	accepted text reworded
3-865	3	35	55	35	56	Sarmiento et al 2010 is listed as supporting a decline in ocean uptake of total CO2 and later in the sentence is also referenced as indicating a decline is unlikely. Please clarify [Government of Australia]	accepted reference removed
3-866	3	35	55	35	56	Section 3.8, subsection 3.8.1.2: Add the most recent publication of Ballantyne et al., 2012 (Article in Nature titled "Increase in observed net carbon dioxide uptake by land and oceans during past 50 years). This study based on mass balance showed that net global carbon uptake has increased significantly by about 0.05 billion tonnes of carbon per year and global carbon uptake doubled between 1960 and 2010. [Government of India]	accepted reference added
3-867	3	35	55			The same reference "Sarmiento et al., 2010" is used for both sides of the argument in lines 55 and 56. Was this intended? [Government of United States of America]	accepted first reference to Sarmiento et al 2010 deleted
3-868	3	35	56	35	57	This is where the change in ocean CO2 sink needs to be assessed, yet the statement is very brief and inaccurate. The references to Glorr and Knorr concern the sum of the ocean+land CO2 sink and cannot be used to comment on the trends in ocean CO2 sink alone. The reference to Sarmiento et al. 2010 is used both to support a decline in efficiency and to oppose it. [European Union]	accepted first reference to Sarmiento et al 2010 deleted
3-869	3	35	56	35	57	This is where the change in ocean CO2 sink needs to be assessed, yet the statement is very brief and inaccurate. The references to Glorr and Knorr concern the sum of the ocean+land CO2 sink and cannot be used to comment on the trends in ocean CO2 sink alone. The reference to Sarmiento et al. 2010 is used both to support a decline in efficiency and to oppose it. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted first reference to Sarmiento et al 2010 deleted

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-870	3	36	2	36	12	Suggest use same colour scale bars for top figures & same y axes scales for lower figures. [Janice Lough, Australia]	Rejected, but change in colour scale for each basin is highlighted in the caption.
3-871	3	36	14	36	15	Line 14 to 15. I wondered from reading this section, whether this level of likelihood conclusion was justified on the basis of the evidence. Virtually certain is a one percent chance of being wrong, and the mixed statements of certainty on regional studies, and no trends in surface fluxes, and whether there are enough profiles to make estimates of anthropogenic carbon with this level of confidence. Perhaps very likely [Nathaniel Lee Bindoff, Australia]	Rejected. The assessment is based on multiple lines of evidence (deltaCstar, transit time distributions, atmospheric O2/N2, deltaC13)
3-872	3	36	14	36	16	This statement needs to differentiate between the mean ocean CO2 sink, which is well established with high confidence, and the trend in ocean sink, which is not. [European Union]	this statement does not include trends in ocean sinks. As mentioned in the text above, there are not sufficient measurements to claim a global change in oceanic uptake rate.
3-873	3	36	14	36	16	Please consider to include descriptive text based on this sentence in the executive summary. Especially the part "It is virtually certain that the ocean is sequestering anthropogenic carbon dioxide" is a strong conclusion that you should consider to link with the paragraph on page 5 line 1-5 [Government of NORWAY]	taken into account. There is no doubt that the ocean is sequestering Cant, and this is stated in 3.8 and in the synthesis. It is indirectly stated in the exec summary.
3-874	3	36	14	36	16	Suggest that the authors consider including a summary statement about the effects of temperature on ocean CO2 uptake. [Government of United States of America]	This is addressed in FAQ 3.2: How Does Anthropogenic Ocean Acidification Relate to Climate Change?
3-875	3	36	14	36	16	This statement needs to differentiate between the mean ocean CO2 sink, which is well established with high confidence, and the trend in ocean sink, which is not. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted, text revised
3-876	3	36	18	36	18	The report addresses ocean acidification. However, nowhere in this chapter there is a section about the impact of a lower pH on sound. A lower pH causes decrease of absorption of underwater sound (the absorption coefficient is dependent on pH). The result of this lower absorption could be that low-frequency underwater sound (from both natural as anthropogenic sound sources) potentially travels further, and there are 'alarming' publications about possible increase of noise levels (see Ilyina, 2010). However, the actual scale of effect of this decreased absorption is uncertain, there are suggestions that it may be negligible (see Joseph 2010). The effect may be also overridden by the increased temperature in the shallow layers, leading to large changes in propagation of sound; and likely measures to reduce sound input from anthropogenic sources (shipping noise). References: Ilyina (2010) Future ocean increasingly transparent to low-frequency sound owing to carbon dioxide emissions, Tatiana Ilyina, Richard E. Zeebe, Peter G. Brewer, Nature Geoscience 3, 18-22. Joseph (2010) A computational assessment of the sensitivity of ambient noise level to ocean acidification, John E. Joseph, Ching-Sang Chiu, JASA Express Letters. 128 (3). [Government of Netherlands]	rejection This is beyond the scope of this assessment. Changes in sound adsorption is not a climate change issue.
3-877	3	36	18	37	11	Page 36 line 18 page 37 line 11 In section 3.8.2 I did not see a clear summary sentence that stated the likelihood of the decrease in acidity trends, and the basis of this conclusion, and robustness of the evidence. Please add a sentence that states the conclusion in the ES in the text here. In the detection and attribution chapter we rely on attribution here to rising carbon dioxide ie (anthropogenic)for table 10.1 [Nathaniel Lee Bindoff, Australia]	accepted Text added
3-878	3	36	18			3.8.2: Anthropogenic Ocean Acidification: this section, combined with Box 3.2, is an important element of the chapter. We suggest summary findings on ocean acidification should also be included in the appropriate section of the SPM. [Government of United Kingdom of Great Britain & Northern Ireland]	noted The SPM will contain statement and a figure about ocean acidification
3-879	3	36	20	36	20	"carbon dioxide" and CO2 should be consistent through texts. [Dongxiao Wang, China]	accepted text made consistent
3-880	3	36	20	36	29	It is hard to understand from this paragraph what acidification we have seen to date . A clear upfront statement about how much acidification has occurred to date is needed. [Government of Australia]	accepted text modified
3-881	3	36	23	36	23	It is unclear what the asterisk immediately after 'Ksp' is meant to signify. [Government of United Kingdom of	accepted Ksp" is the apparent solubility product of

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Great Britain & Northern Ireland]	calcium carbonate in solution. The asterisk provides a reference to the apparent solubility for the total concentrations of calcium and carbonate concentrations in seawater. This is now added in the text
3-882	3	36	26			As stated this downplays pH changes. Alternatively the authors could express the change as a percent. A pH change of 0.1 corresponds to a 30% decrease. (this comment was made by multiple reviewers) [Government of United States of America]	accepted pH changes are now also given as percent of change of hydrogen ions
3-883	3	36	27	36	27	change H+concentration to hydrogen ion concentration, [H+], to be in accordance with line 22 [Government of Iceland]	accept Text corrected as suggested by the reviewer
3-884	3	36	28	36	29	The consequences of ocean acidification on marine organisms and ecosystems are actually addressed in Chapters 5, 6 and 30 of the WG2 report. Note also that there will be a cross-chapter box on ocean acidification. I will send the draft when it will become available. [Jean-Pierre Gattuso, France]	accept reference to Ch6 and WG II added.
3-885	3	36	31	36	42	No Southern Hemisphere examples are provided here - they are all Northern Hemisphere [Government of Australia]	rejected The Southern Ocean work is cited in the work of Takahashi et al (2009) as cited in the text.
3-886	3	36	32	36	32	Please clarify what is computed and what is measured in the ocean acidification section. [European Union]	accepted text clarified
3-887	3	36	32	36	32	Please clarify what is computed and what is measured in the ocean acidification section. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted Text clarified
3-888	3	36	32	36	32	"computed partial pressure", why is the pCO2 confined to "computed"? [Akihiko Murata, Japan]	accept text modified
3-889	3	36	33	36	36	Not clear if the pH values reported are calculated from carbonate chemistry or directly measured. If calculated this should be pointed out. [Government of Australia]	accept information added as suggested by the reviewer
3-890	3	36	36	36	36	Not clear where the "Iceland Sea" data comes from. [Janice Lough, Australia]	noted. the Iceland Sea data are from Olafsson et al., 2009, as cited in the text.
3-891	3	36	36	36	37	Please consider to give absolute numbers to the statement about the greatest changes. [Government of NORWAY]	accept Text corrected as suggested by the reviewer
3-892	3	36	36	36	37	I can't see any justification for this statement in the material thus far presented. [Terrence Joyce, United States of America]	Accepted, text removed.
3-893	3	36	37	36	39	What does it mean when it says "it is in agreement with"? Right now it is slightly unclear, does it mean that these areas are more vulnerable or simply that the magnitude of change is similar in these areas? [Government of NORWAY]	accepted, text clarified: The pH changes from the section are in agreement with the ph changes from the time -series site.
3-894	3	36	45	36	45	Are pH values calculated? [Government of Australia]	accept information added as suggested by the reviewer
3-895	3	36	45	36	53	Figure 3.17 only shows trends in the Northern Hemisphere sub-tropics. Can you add at least the data from the IS station cited on Table 3.2, and if possible data from the Southern Hemisphere. Also please clarify what is directly observed and what is calculated from carbonate chemistry. [European Union]	reject The rate of change for the Iceland Sea data is given in Table 3.2. The trends for Figure 3.17 are for those stations with the longest data records. The additional data records are given in Table 3.2.
3-896	3	36	45	36	53	Figure 3.17 only shows trends in the Northern Hemisphere sub-tropics. Can you add at least the data from the IS station cited on Table 3.2, and if possible data from the Southern Hemisphere. Also please clarify what is directly observed and what is calculated from carbonate chemistry. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	reject The rate of change for the Iceland Sea data is given in Table 3.2. The trends for Figure 3.17 are for those stations with the longest data records. The additional data records are given in Table 3.2.
3-897	3	36	45	36	53	Figure 3.17: Need to be exact as to what is shown in Figure, i.e. "monthly" values with linear trend lines added. [Janice Lough, Australia]	accept The trend lines are explained in the text of Table 3.2.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-898	3	36	51	36	52	Atmospheric pCO ₂ from Hawaii should be Atmospheric CO ₂ from Mauna Loa Observatory, Hawaii, if that is the source. [Government of Australia]	accepted Text corrected
3-899	3	36	55	36	57	This line is slightly unclear: what is causing what? The changes in seawater chemistry result from uptake of anthropogenic CO ₂ . This line is understandable. But: the changes also include other changes? It should be: The changes in seawater chemistry result from A, but also B and C. Please consider to include some specific examples of the changes imparted by local physical and biological variability. You should also consider replacing "imparted by" with an easier to understand term, maybe something like "related to" or "associated with" could work. [Government of NORWAY]	accept Text corrected as suggested by the reviewer
3-900	3	36	55			Here one says that the changes result from uptake of anthropogenic CO ₂ . In the same sentence and on page 37 13, it is mentioned that observations include both anthropogenic and natural variations. This will seem inconsistent to the non-expert reader. Perhaps say "mostly results". This would be consistent with Chapter 6 p. 58 l.45. [Jean-Pierre Gattuso, France]	accepted. Statement clarified
3-901	3	36	57	36	59	This sentence is not clear. The non-expert reader will not know what the depth of the mixed layer is in the North Pacific. [Jean-Pierre Gattuso, France]	rejected the exact depth of the mixed layer is not necessary here
3-902	3	36	58	36	58	It is not a decline but a decrease. [John McLean, Australia]	noted
3-903	3	36	58	36	58	Please quantify what that decrease was and how it compares to diurnal and seasonal variability in pH. [John McLean, Australia]	rejected the long time series show that these changes are smaller than the trends, and in some cases, trends for winter and summer are already presented in the table
3-904	3	36				Section 5.5.5: Zheng et al(2006) conclusion contains not only on floods but on droughts, that is "The severity of floods during the 20th century was comparable in intensity to historical times, but the droughts were usually less severe than earlier times. [Jingyun Zheng, China]	This comment is not appropriate for this section 3.8
3-905	3	37	4	37	4	Please clarify if the range represents the spread among studies, the regional distribution or the error [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accept information added as suggested by the reviewer
3-906	3	37	4			I suggest to use the same unit as in the table for consistency: per year rather than per decade [Jean-Pierre Gattuso, France]	accept numbers now reported per year as in table
3-907	3	37	5	37	6	Please consider to present this finding in the executive summary of the chapter (page 5, line 1-5), and maybe rewrite it slightly. Please consider replacing "imparted by" with an easier to understand term, maybe something like "related to" or "associated with" could work. [Government of NORWAY]	accept statements in the exec summary rewritten
3-908	3	37	5	37	6	The likelihood assessment of the importance of natural variability versus trends for pH needs clarification. How large are the natural changes in pH, what time frame and CO ₂ levels is this compared to? [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	taken into account. Text clarified
3-909	3	37	5	37	6	This sentence is speculation with little if any basis. If the atmosphere does warm, as some models predict, then the ocean's absorption of CO ₂ will reduce and hence the rate of change in pH will slow. [John McLean, Australia]	rejected. The reviewer is referred to FAQ 3.2, where the explanation is given.
3-910	3	37	6	37	9	Sentence could be misconstrued as pH changes were calculated by Sabine et al 2004, which is not the case. Suggest change to " pH change was calculated based on anthropogenic CO ₂ storage (Sabine et al., 2004) [Government of Australia]	Text reworded
3-911	3	37	6	37	11	The smallest reduction in the subtropical South Pacific is described here. But this is not consistent with impression from Sabine et al. (2004) figure and Figure 3.16 in this report, which show distributions of water column inventories changes of anthropogenic CO ₂ . This is probably due to the difference between surface seawater and water column. But for surface seawater, it is difficult to estimate anthropogenic CO ₂ . At least, references should be cited here, which evaluate the regional differences of pH. [Akihiko Murata, Japan]	rejected owing to lack of space, regional differences are not discussed in detail

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-912	3	37	7			Is a time period ending in 1994 the most up to date analysis? [Government of United States of America]	noted. The inventory estimates are based on inventories calculated for 1994 and 2010.
3-913	3	37	13	38	18	Moving this box up to where you first mention OA (p. 34, ln. 12) would be helpful as it provides good background. [Government of United States of America]	rejected the box fits best where it is.
3-914	3	37	15	38	18	Should this discussion include aragonite? It is mentioned in the caption text for table 3.2 (page 3-38 line 26) but not in the text. Only CaCO ₃ is mentioned. [Government of United States of America]	notedThe discussion is broad and includes all polymorphs of CaCO ₃ including aragonite which is described in Figure 2 of Box 3.2.
3-915	3	37	15			Consider adding a figure that depicts the chemical reactions seen with ocean acidification. Consider other world maps for pH (similar to those used for DO). Also consider a global look to go along with the depth figure. [Government of United States of America]	noted. Figure 1 in Box 3.2 provides the pH maps. Chemical reactions included in FAQ 3.2
3-916	3	37	15			Box 3.2 Suggest the authors describe what part is natural and what part is anthropogenic. In the absence of anthropogenic CO ₂ emissions, would the change in pCO ₂ still be 2 PgC/yr. [Government of United States of America]	noted All the changes in surface water pH are mostly due to anthropogenic CO ₂ in the open-ocean.
3-917	3	37	15			Suggest the authors include information from the recent study by Honisch et al, 2012 (http://www.sciencemag.org/content/335/6072/1058.short)? [Government of United States of America]	rejected, this paper by Hönisch et al refers to geological time scales
3-918	3	37	15			Box 3.2.: Figure 2 presents a timeseries of changes in carbonate ion concentration. Suggest to expand on why CO ₃ ²⁻ is relevant in this context. This is only briefly mentioned in the text. [Thomas Stocker/ WGI TSU, Switzerland]	noted the information is given in Figure caption of Box 3.2 Figure 2.
3-919	3	37	17	38	2	Box 3.2. Calcite and aragonite are not mentioned in the text only in figure caption [Government of Iceland]	Text corrected as suggested by the reviewer. The saturation state for aragonite and calcite is described in the text.
3-920	3	37	17	38	18	Box 3.2: It should be considered to mention the potential effect of pH reduction on macro- and micronutrient speciation and cycling, although the knowledge about this is weak at the moment. Two studies that handles this is Beman et al., 2010. Global declines in oceanic nitrification rates as a consequence of ocean acidification. PNAS 2011 108 (1), page 208-213 and Breitbarth et al., 2010. Ocean acidification affects iron speciation during a coastal seawater mesocosm experiment. Biogeosciences, 7, page 1065–1073. [Government of NORWAY]	noted. This issue is outside the scope of this chapter and is being handled in WGII.
3-921	3	37	17	38	18	The statements in this text box need a context so please (a) make it clear that the impact on marine life varies from beneficial to detrimental, with most experiments to date showing results on the beneficial side when experiments use variations in pH that are within the bounds of reality i.e. a shift of 1 unit (for summary see http://www.co2science.org/data/acidification/results.php), (b) point out that the pH scale is not linear but based on logn and (c) state exactly how much CO ₂ would be required to force a pH shift of 0.5 (assuming the unlikely event that none falls out of solution). [John McLean, Australia]	partially accepted. Text in box has been revised, and readers directed to WGII for a discussion of impacts.
3-922	3	37	17			Box 3.2.: suggest to delete the first sentence/the question "What is ocean acidification" and simply start with the second sentence "Ocean acidification refers to....". Given that the box title is "Ocean acidification", the opening question seems unnecessary. [Thomas Stocker/ WGI TSU, Switzerland]	accept FAQ 3.2 significantly modified
3-923	3	37	21	37	22	referencing here to Caldeira 2011 for the IPCC Expert Meeting report from the "Expert Meeting on Impacts of Ocean Acidification on Marine Biology and Ecosystems" is not correct. The proper reference is: 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. Alternatively, the WGI AR5 Glossary (Annex III WGI AR5) could be referred to. [Thomas Stocker/ WGI TSU, Switzerland]	editorial copyedit to be completed prior to publication
3-924	3	37	24	37	28	suggest to refer to the relevant WGI AR5 chapters where these changes in atmospheric CO ₂ over time are being assessed, i.e., chapters 2 and 6. [Thomas Stocker/ WGI TSU, Switzerland]	accept references added when appropriate

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-925	3	37	27	37	28	This statement concerning past and future concentrations of CO2 should be supported with links to Chapters 5 and 6. [Thomas Stocker/ WGI TSU, Switzerland]	accept references added when appropriate
3-926	3	37	31	37	31	Do not use subjective terms like "significant". Quantifying is required. [John McLean, Australia]	rejected This statement is correct as it stands. It is supported by the next few sentences.
3-927	3	37	32	37	34	Put this change in pH into context by stating the range of pH values found across the oceans and how they vary diurnally and seasonally. (I note that in line 32 of 38 you mention that seasonal variation in pH was removed, which means that you are already admitting to a seasonal variation.) [John McLean, Australia]	rejected The range of pH values in surface seawater has been given in this text.
3-928	3	37	34	37	36	It is fanciful to suggest that we have accurate pH data for all the oceans for the last 200 million years. [John McLean, Australia]	rejected The proxies for pH in seawater are recorded in marine sediments (see the references cited here)
3-929	3	37	36	37	36	I'd change "than it has been for more than 20 million years (Caldeira and Wickett, 2003)." for "than it has been for more than 40 million years (Pelejero et al., 2010), using the reference: Pelejero et al., 2010 reference: Pelejero, C., Calvo, E., Hoegh-Guldberg, O., 2010. Paleo-perspectives on ocean acidification. Trends in Ecology & Evolution 25, 332-344. [Carles Pelejero, Spain]	accepted text changed
3-930	3	37	36			20 million years is an underestimate. Looking at the pH reconstruction of Zeebe and Ridgwell (2011; Fig. 2.3) shows that one can use 55 million years and still be on the safe side. [Zeebe R. E. & Ridgwell A., 2011. Past changes of ocean carbonate chemistry. In: Gattuso J.-P. & Hansson L. (Eds.), Ocean acidification, pp. 21-40. Oxford: Oxford University Press.] [Jean-Pierre Gattuso, France]	accepted text changed
3-931	3	37	44	37	44	"carbonic acid"? According to Gattuso and Hansson "Ocean acidification: background and history" book chapter in ("Ocean acidification", 2011, Oxford University Press), it is "A minor form is true carbonic acid (H2CO3) with a concentration of less than 0.3% of [CO2(aq)]". On page 36 (lines 21-23), the authors painfully go around this term and here, in the box, it is used anyway. Is it a feature or a bug? [Government of Poland]	accepted text now clarified
3-932	3	37	45			You could cite, in addition, the ocean acidification book which is both more recent and also much more comprehensive. [Gattuso J.-P. & Hansson L. (Eds.), 2011. Ocean acidification, 326 p. Oxford: Oxford University Press.] Fish were not discussed in the previous sentence. How about "Other shellfish and fish"? In fact this sentence is awkward because it tries to derive a general statement from a very intricate situation. Perhaps it is better to refer to the chapter of WG2 which look at this in detail (that is chapters 5,6 and 30). Another option is to cite the cross-chapter ocean acidification box of WG2.] [Jean-Pierre Gattuso, France]	accepted reference added
3-933	3	37	49	37	49	I'd say 'have' instead of 'produce'. [Carles Pelejero, Spain]	Text corrected as suggested by the reviewer
3-934	3	37	49	38	2	The impact of ocean acidification of ecosystems should be left for WGII, where it can be treated exhaustively. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	A reference to WGII Chapters, 5, 6 and 30 is provided
3-935	3	37	49	38	3	This final paragraph drifts into WGII territory, and should be avoided. [Thomas Stocker/ WGI TSU, Switzerland]	A reference to WGII Chapters, 5, 6 and 30 is provided
3-936	3	37	53	37	53	That seagrasses will be 'winners' of ocean acidification is not totally accepted, see for instance Arnold, T., Mealey, C., Leahey, H., Miller, A.W., Hall-Spencer, J.M., Milazzo, M., Maers, K., 2012. Ocean acidification and the loss of phenolic substances in marine plants. PLoS ONE 7, e35107. [Carles Pelejero, Spain]	Text corrected as suggested by the reviewer
3-937	3	37	55			Fish were not discussed in the previous sentence. How about "Other shellfish and fish"? In fact this sentence is awkward because it tries to derive a general statement from a very intricate situation. Perhaps it is better to refer to the chapter of WG2 which look at this in detail (that is chapters 5,6 and 30). Another option is to cite the cross-chapter ocean acidification box of WG2. [Jean-Pierre Gattuso, France]	A reference to WGII Chapters, 5, 6 and 30 is provided

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-938	3	38	11	38	16	need to distinguish in caption between red and black solid lines [Janice Lough, Australia]	accept caption changed
3-939	3	38	21	38	22	Figure 3.18: I found it very hard to determine what this graphic was showing. [Janice Lough, Australia]	accept figure and figure caption modified to clarify
3-940	3	38	25			Table 3.2: We suggest that the empty cells in table be swapped for minus/dashes if there is no data; the approach should be consistent and appropriately described in the caption [Thomas Stocker/ WGI TSU, Switzerland]	accept Added dashes to Table 3.2
3-941	3	38				Table 3.2. Gain an extra year's data by using ESTOC back to its start in 1994? [David Parker, United Kingdom of Great Britain & Northern Ireland]	rejected. The data presented has been given to us directly by the authors.
3-942	3	39	18	39	22	Figure 3.19: Caption unclear; surely b) and c) show the changes? [Janice Lough, Australia]	accept caption improved
3-943	3	39	30	40	37	the oxygen section seems very isolated in the general context of the chapter 3, I suggest to increase the link with the ventilation of the water column and its stratification etc.. as partially discussed at the end of pages 41 [VINCENZO ARTALE, ITALY]	accept text added
3-944	3	39	30			During the SOD review period, a major paper on oxygen trends was published with several new key results (Stramma, L.; Oschlies, A. & Schmidtko, S. Mismatch between observed and modeled trends in dissolved upper-ocean oxygen over the last 50 yr. Biogeosciences, 2012, 9, 4045-4057). The authors make a detailed comparison between modeled and observed oxygen trends in the global ocean. At 300 m, the global mean trend between 50N and 50S is -0.066 $\mu\text{mol per kg per year}$. Much larger trends (both increasing and decreasing) are observed in various regions and are likely due to regional changes of circulation and/or ventilation. Interestingly and as a challenge for future research, the model does not correctly reproduce the observed regional oxygen changes over the past 50 years. [Denis Gilbert, Canada]	accept, results from Stramma et al included in text
3-945	3	39	30			Section 3.8.3: Suggest including a discussion of the growing, extremely hypoxic area in the Gulf of Mexico. [Government of United States of America]	This suggestion is outside the scope of this assessment.
3-946	3	39	32	39	33	The comment on data quality issues and sparseness of data needs to be either explained or deleted. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	Text corrected as suggested by the reviewer
3-947	3	39	32	39	40	I believe the pioneering work of Garcia et al. (2005) should be included here. Garcia, H.E., T. P. Boyer, S. Levitus, R. Locarnini, and J. I. Antonov, 2005: On the variability of dissolved oxygen and apparent oxygen Utilization content for the upper world ocean: 1955 to 1998. Geophys. Res. Lett., 32, L09604, doi:1029/GL022286. [Sydney Levitus, U.S.A.]	noted. Ch3 focus on literature published after AR4
3-948	3	39	34	39	34	"Nearshore areas" - is this globally or just specific regions? [Janice Lough, Australia]	accept the specific regions are mentioned
3-949	3	39	40	39	40	change mol per year to mol year ⁻¹ [Government of Iceland]	Text corrected as suggested by the reviewer
3-950	3	39	40	39	40	change to mol y ⁻¹ [Government of Iceland]	Text corrected as suggested by the reviewer
3-951	3	39	44	39	44	Is there evidence that increased stratification reduces ventilation in the main thermocline? If so, provide reference. [Government of Australia]	references cited in section 3.8.3
3-952	3	39	45	39	46	The Duce paper doesn't actually address the issue of deoxygenation from nitrogen deposition. Manning and I touch briefly on the related issue of air-sea O ₂ fluxes induced by nitrogen deposition in our review article that is in press (see note 13 above), but I'm not aware of the effect of N deposition on ocean O ₂ having otherwise been broached in the literature. The science here is probably not far enough along to be picked up by IPCC, and the N effects on O ₂ are probably not very important anyway. [Ralph Keeling, United States of America]	accept reference deleted as suggest by the reviewer.
3-953	3	39	45	39	46	The paper by Duce does not provide any data evidence of changes in oxygen, it only infers that they would occur with increased nitrogen deposition. This statement needs to be clarified. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted text clarified
3-954	3	39	45	39	47	Reference for last sentence? [Janice Lough, Australia]	accept reference added
3-955	3	39	46	39	46	change to nmol O ₂ joule ⁻¹ [Government of Iceland]	Text corrected as suggested by the reviewer

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-956	3	39	46			Models? What about data? You should be leading with what the data say. And in the discussion, combine the observed oceanic heat gain and oxygen loss. What does this say about solubility effects vs stratification? [Terrence Joyce, United States of America]	partially accepted. It is not possible to calculate a global ratio of oxygen loss to heat uptake from observations, given the limitations of the oxygen data set. We have cited observational studies where possible (e.g. Helm et al. 2011).
3-957	3	39	47	39	47	"The" should be "This" at the end of the line. [Stephen Griffies, United States of America]	Text corrected as suggested by the reviewer
3-958	3	39	47	39	48	Replace "The means" with "This means" [Denis Gilbert, Canada]	Text corrected as suggested by the reviewer
3-959	3	39	47	39	51	The models suggest that the solubility accounts for about 1/4 of the total ocean signal (see Table 6.14), and thus that the process attribution of oxygen trends from observations are consistent with the model results. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted, information added
3-960	3	39	51			Only one citation available for this big issue??? It is clear from what follows that this is not the case, so why write in this curious style? In fact, there are problems with the writing of this section - perhaps one of the lead authors can fix them? [Terrence Joyce, United States of America]	accept text rewritten and clarified
3-961	3	40	1	40	3	Modify as follows: "Detailed analysis of historical data in the tropical ocean reveals negative trends for the last 50 years in all ocean basins (Helm et al., 2011; Keeling et al., 2010; Stramma et al., 2008), resulting in a substantial expansion of the dissolved oxygen minimum zones there." The sentence in the original manuscript looks curious from the view of the manner for scientific papers. Keeling's is a review of the issue (summarizing forerunning studies with various methods which may include the studies on "time-series records from a few selected spots with sufficient data coverage in the tropical ocean"), Helm's discussed global features based on (all) available historical data, and Ono's showed an example in the subarctic North Pacific. The modification suggested above does not change the meaning of the original sentence and corrects scientific inconsistency, I believe. [Taiyo Kobayashi, Japan]	Text corrected as suggested by the reviewer
3-962	3	40	3	40	3	Please quantify the size of the expansion in oxygen minimum zones. [European Union]	noted. Because the data are limited, section 3.8.3 addresses this issue to the extent possible.
3-963	3	40	3	40	3	Please quantify the size of the expansion in oxygen minimum zones. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	noted. Because the data are limited, section 3.8.3 addresses this issue to the extent possible.
3-964	3	40	8	40	11	Modify as follows: "Observations from the longest time-series sites in the subpolar North Pacific reveal a persistent declining trend in the thermocline for the last 50 years (Ono et al., 2001; Whitney et al., 2007), although this trend is superimposed on oscillations with time-scales of a few years to two decades". Such features have been reported in the east and west of the subpolar North Pacific. It has another advantage not to remove Ono et al. (2001), which should be removed reflecting the above change, from the reference list. [Taiyo Kobayashi, Japan]	noted. Text is reworded
3-965	3	40	12			Add the following sentence. "In the subtropical South Indian Ocean, Kobayashi et al. (2012) reported the deoxygenating in subsurface and intermediate water masses is concentrated in its eastern part." Because there are no studies discussing specific features of oxygen trends in the subtropical nor in the Indian Ocean in the original. Kobayashi et al. also concluded the changes of dissolved oxygen there are "caused by changes in solubility as well as changes in ventilation and circulation over time", as in the original. Kobayashi, T., K. Mizuno, and T. Suga, 2012: Long-term variations of surface and intermediate waters in the southern Indian Ocean along 32°S, Journal of Oceanography, 68, 243-265, DOI: 10.1007/s10872-011-0093-5. [Taiyo Kobayashi, Japan]	accepted reference added
3-966	3	40	14	40	14	Change "intermediate" to "lower" [David Parker, United Kingdom of Great Britain & Northern Ireland]	Rejected. Intermediate waters are defined earlier in the chapter
3-967	3	40	14	40	15	The issue of O2 decrease due to the 2 identified processes should be summarized here, much like heat change is assessed quantitatively in terms of sea level change (vs mass increases). [Terrence Joyce, United States of America]	Helm et al (2011) is cited in this regard.
3-968	3	40	18			I am struck by the similarity between a) and b) or c) in this Figure with similar plots of surface salinity change	Partially accepted. Revised text discusses the factors

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						consistent with an increase in the hydrologic cycle. What would you call this for oxygen? Increase in the biologic cycle? Add some discussion please... [Terrence Joyce, United States of America]	contributing to oxygen changes in more detail. Most studies suggest the oxygen changes can be explained by changes in ventilation and circulation, rather than changes in biological cycling.
3-969	3	40	20	40	20	IS ΔDO the dissolved oxygen change for the last 50 years? It doesn't say in the figure caption. [Dongxiao Wang, China]	accepted caption is corrected
3-970	3	40	24	40	32	Please provide balanced information for regions outside the US. The text includes 7 lines of information on US trends, and 2 lines for the rest of the world. Yet there are ample studies that show deoxygenation around Europe and Japan in particular (see references in Gilbert et al 2010 already cited) [European Union]	accepted information on other coastal areas added
3-971	3	40	24	40	32	Please provide balanced information for regions outside the US. The text includes 7 lines of information on US trends, and 2 lines for the rest of the world. Yet there are ample studies that show deoxygenation around Europe and Japan in particular (see references in Gilbert et al 2010 already cited) [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accepted information on other coastal areas added
3-972	3	40	31	40	32	An important finding from Gilbert et al. (2010) is missing here. I suggest adding this sentence at the end of line 32: "They found that the odds of finding declining oxygen trends increased from the 1951–1975 period to the 1976–2000 period, indicating a recent worsening of hypoxia." [Denis Gilbert, Canada]	partly accepted the discussion on regional coasts are enhanced
3-973	3	40	34	40	37	And hypoxia caused by increased nutrient loading is of increasing concern in coastal waters - this is a third process responsible for de-oxygenization isn't it? If so then why not say so in a summary of this section or in section 3.85? [Terrence Joyce, United States of America]	partly accepted the impact of nutrient changes on the coastal oceans are mentioned in 3.8.4
3-974	3	40	35	40	35	Suggest to explain what is meant by the "hypoxic zone" [Thomas Stocker/ WGI TSU, Switzerland]	taken into account term deleted
3-975	3	40	35	40	37	References should be cited to support this statement concerning the effects of fertilizers and burning of fossil fuels. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Text corrected as suggested by the reviewer Add Reference
3-976	3	40	36	40	37	The attribution of the 'strong increase in the number of hypoxic zones' needs to be substantiated by more information and supported by references. Do we know the contribution of each process to this trend (including climate change). If not please clarify the statement. [European Union]	taken into account. Sentence removed. Coastal zones are subject to WG II
3-977	3	40	36	40	37	The attribution of the 'strong increase in the number of hypoxic zones' needs to be substantiated by more information and supported by references. Do we know the contribution of each process to this trend (including climate change). If not please clarify the statement. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	see comment 3.977
3-978	3	40	37			Suggest including additional information on coastal eutrophication to include an explanation of the factors driving this process including chemical reactions and algal growth issues. Alternatively, the authors could refer to the discussion in Chapter 6 (p. 61) if this seems sufficient. [Government of United States of America]	see comment 3-977
3-979	3	40	39		55	This section on nutrients trends is insufficient [European Union]	there is little published literature that allows a more comprehensive assessment
3-980	3	40	39			Suggest additional reference to recent work on the importance of long-range-transport or iron-rich dust and impacts on primary productivity. Is this covered sufficiently in Chapter 6? [Government of United States of America]	noted
3-981	3	40	39			Section 3.8.4: This treatment is brief and may overlap with that in Chapter 6. Is this adequate? [Government of United States of America]	noted
3-982	3	40	44	40	44	Increased stratification hampers nutrient upwelling, but it should be mentioned that in coastal waters nutrient loads have increased tremendously, causing hypoxia etc. I miss the discussion of this point in the oxygen section [European Union]	noted. Coastal zones are subject of WG II, see comment 3.977
3-983	3	40	45	40	47	b [Akihiko Murata, Japan]	Missing information from Reviewer.
3-984	3	40	53	40	53	As a likely consequence', the word likely is used here outside the normal IPCC guidelines, and makes the	accept likely removed

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						sentence difficult to understand. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	
3-985	3	40	57	41	17	The summary contains no information on nutrient changes, yet long term trends in nutrients were discussed in the previous section. [Government of Australia]	noted. There are no published studies quantifying long-term trends in open ocean nutrient concentrations.
3-986	3	41	1	41	1	It isn't clear how nutrient data have been used to estimate the anthropogenic carbon inventory. [David Parker, United Kingdom of Great Britain & Northern Ireland]	taken into account. The individual methods are not described in detail, the reviewer is referred to the papers cited
3-987	3	41	2	41	5	This statement needs to be clarified. It states 'very high confidence' for an increase in carbon uptake between 1994 and 2010, but the two numbers stated for the carbon inventory change overlap and in fact show no significant difference between the two time periods. The reason why there is high confidence that carbon uptake continued is not because of the inventory surveys, but because there is a wide range of data (pCO ₂ , atmospheric O ₂ /N ₂ , atmospheric d ¹³ C, tracer-based ocean inversions, patterns of change of atmospheric CO ₂ concentration) which support a positive CO ₂ at least since 1990, with an amplitude of around 2 PgC/y. [European Union]	accepted the text is modified and the discussion enhanced, specific statements are reformulated
3-988	3	41	2	41	5	This statement needs to be clarified. It states 'very high confidence' for an increase in carbon uptake between 1994 and 2010, but the two numbers stated for the carbon inventory change overlap and in fact show no significant difference between the two time periods. The reason why there is high confidence that carbon uptake continued is not because of the inventory surveys, but because there is a wide range of data (pCO ₂ , atmospheric O ₂ /N ₂ , atmospheric d ¹³ C, tracer-based ocean inversions, patterns of change of atmospheric CO ₂ concentration) which support a positive CO ₂ at least since 1990, with an amplitude of around 2 PgC/y. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	see 3-987
3-989	3	41	3			Please check if this sentence for consistence. The record described on page 3-35 is from 1994 - 2011 (not 2010). Also, 93-137 Pg C in 1994 doesn't correspond to the values listed on page 3-35 lines 14-20 [Government of United States of America]	accepted, text revised.
3-990	3	41	9	41	10	Is it possible to have a sentence in this paragraph that describes the impacts of these observations with regard to marine life? [Government of United States of America]	rejected this is not a topic of Ch3, but of WG II
3-991	3	41	12	41	12	Please consider if 'high agreement' is representative of the information available. There is only on study that has quantified global changes in oxygen (Stramma et al 2012) since 1960, and the trends at high latitudes do not clearly match those of Helm et al estimated over a short time period. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	partly accepted the results from both papers are discussed and the uncertainties given.
3-992	3	41	14	41	16	Line 14 to 16 page 41 The evidence for oxygen minimum changes in equatorial zone is higher than just likely... I think. The observations show a pretty clear signal.... What is the Likelihood assessment for the open ocean or for the global change over some large depth interval. High confidence.....likely decrease? There is no clear statement on global change, or the coastal regions discussed in the oxygen section. [Nathaniel Lee Bindoff, Australia]	accepted text modified and the issues adressed
3-993	3	41	19			I think this section needs to clearly state that there is substantial evidence indicating that the climate change being forced by human activities is affecting the ocean as well as the atmosphere and that for many aspects of the ocean, the patterns and timings of changes in the ocean suggest that most of the significant changes in the ocean are very likely a result of human activities, namely GHG and aerosol emissions. [Michael MacCracken, United States of America]	accepted. text has been revised, but stops short of attributing the changes to human activities, as Detection and Attribution appears in Chapter 10 and is not part of Ch3
3-994	3	41	22	41	22	Page 41 Line 22 Not only greater confidence, but can place more climate variables with explicit likelihood and for some with higher levels of Likelihood. The progress in the oceans is impressive since AR4. [Nathaniel Lee Bindoff, Australia]	accepted, text revised

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-995	3	41	29			The discussion in Section 3.8 does not make this conclusion explicit with respect to 1950. [Government of United States of America]	accepted, text now consistent in 3.9 and 3.8
3-996	3	41	30	41	30	Line 30 page 41 compare the likely with D and A chapter. The D and A chapter says it is likely the signal is anthropogenic ally forced. Given the evidence and number of new studies around surface salinity change, why isn't the salinity signal given a very likely level? [Nathaniel Lee Bindoff, Australia]	Accept. After careful checking of confidence intervals and compatibility with statements elsewhere in Ch.3, we've changed to "very likely".
3-997	3	41	32	41	34	"The amplification of the contrast between regions of high and low sea surface salinity is consistent with expectations of an intensification of the global water cycle in a warming climate." Given my previous comments, I could equally write: Any correlation between the regions of increased salinity extremes and the pattern of precipitation and evaporation is possibly (is likely to be) just luck. [David Webb, United Kingdom]	Taken into account. We have made the case in section 3.3 that the similarity in the patterns is not due to stratification change, and cannot be due to advection because of its large scale. We are confident that the similarities are due to changes in P-E.
3-998	3	41	33			I have no problem with this statement at all; but it remains a fact that an intensification of the global water cycle has not yet been proven on the basis of observations [Terrence Joyce, United States of America]	Taken into account. See response to 3-997 (immediately preceding comment)
3-999	3	41	33			Please see earlier comments on the use of the word "global" when it comes to the link between salinity measurements and the water cycle. However, the wording in this particular sentence "is consistent with expectations of" enables the word "global" to appear legitimately here, even if I would prefer it to be absent for reasons stated earlier. [Adrian Simmons, United Kingdom]	accepted, text revised
3-1000	3	41	36	41	36	Figure 3.20 is excellent. [Government of Australia]	Noted. Thank you
3-1001	3	41	37	41	41	I like the simplicity and clarity of Figure 3.20. It speaks volumes. [Gary Lagerloef, United States of America]	Noted. Thank you
3-1002	3	41	43	41	56	Maybe here also consider highlighting some of the main conclusions in the summary: high confidence in the rate and pattern of sea level rise, for instance. [Leticia Cotrim da Cunha, Brazil]	noted. Text revised and enhanced
3-1003	3	41	46	41	46	Figure 3.21 not 3.20. [David Parker, United Kingdom of Great Britain & Northern Ireland]	Reject. This really is in reference to Fig. 3.21
3-1004	3	41	46			GMSL change is shown in the 2nd panel, not the "top panel" [Government of United States of America]	accepted and fixed
3-1005	3	41	48	41	52	Page 41, line 48 to 52 See my earlier comment. I would say the evidence is more robust than indicated here, and a Likelihood statement of likely would seem appropriate given the number of papers, the consistency across the papers and the agreement with climatology, and hence understanding from meteorology. There is a dramatic change in understanding since AR4 in terms of surface salinity field. [Nathaniel Lee Bindoff, Australia]	Accept. Based on confidence limits, the statement now reads "very likely", and is consistent with statement in executive summary and section 3.3
3-1006	3	41	54	41	54	This is a strong statement, but it is here substantiated by very little evidence where the observed decline seem to refer to 3 time-series stations in northern sub-tropics and calculated CO3 concentration. This statement needs to be backed up a much wider set of observations, or alternatively to explain the very strong theoretical and empirical basis for it. [European Union]	accept the relevant subsections were modified and enhanced
3-1007	3	41	54	41	54	CO3 2- is written here as CO3= [Government of Iceland]	noted text changed
3-1008	3	41	54	41	54	This is a strong statement, but it is here substantiated by very little evidence where the observed decline seem to refer to 3 time-series stations in northern sub-tropics and calculated CO3 concentration. This statement needs to be backed up a much wider set of observations, or alternatively to explain the very strong theoretical and empirical basis for it. [Corinne Le Quéré, United Kingdom of Great Britain & Northern Ireland]	accept the relevant subsections were modified and enhanced
3-1009	3	41		41		Page 41 synthesis section. The introduction of the confidence language in this section, felt to me a dilution of	text, including confidence language, is now consistent

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the likelihood language that had been used earlier. So the synthesis felt weaker as a result, when really, it is stronger across such a wide array of ocean variables that all show changes ranging from virtually certain to lower levels. I like the figures in this section. [Nathaniel Lee Bindoff, Australia]	throughout chapter
3-1010	3	41		42		The link between increasing stratification and reduced ventilation is not at all obvious to me and I am not aware of a study demonstrating this. Ventilation is a fully 3 dimensional process driven by wind convergence and mixed layer depth characteristics (depth and slope), far different from a simple 1D turbulence/stratification balance. I think this statement is misleading and confusing unless you have a study attributing reduced ventilation/oxygen levels to increased stratification. [Government of Australia]	rejected there is for instance evidence for reduced ventilation from the change in oxygen (see 3.8.3)
3-1011	3	42	26	42	32	Would you maintain that there are enough data to say there is a decrease in AABW production? [I hope not]. [Terrence Joyce, United States of America]	accepted there is not enough evidence from observations for a decline in AABW formation. AABW is warming and contracting , but as recent papers show, other processes might be responsible for that.
3-1012	3	42	27	42	32	I suggest also to include the AMO index between the major mode of variability that can induce some influence on the THC variability, in fact the two main hypotheses about the origin of this multidecadal cycle take into account the THC, the first one is relates to the internal variability of the thermohaline circulation (THC) and to the intrinsic nonlinear behavior of the climate system (Huck et al. 1999;Delworth andMann 2000; teRaa andDijkstra 2002;Knight et al. 2005), while the second, based on experimental and numerical evidence, brings into play the role of the free oscillation motion of the atmosphere–ocean–ice coupled system (Jungclaus et al. 2005; Dong and Sutton 2005; Dima and Lohmann 2007). [VINCENZO ARTALE, ITALY]	accepted, text revised
3-1013	3	42	30	42	32	It seems to me that since the radiative forcing (so the combined effects of natural and anthropogenic contributions) comes from many factors and has not, at least over some areas like the North Atlantic, not been uniformly changing in time, that some of the fluctuations could be due to, at least in part, to anthropogenic factors. Starting the sentence with "Much" allows for this, but I think it would be worth including explicitly instead of having it be just implicit. Indeed, with GHGs and aerosols changing the atmospheric conditions, including the dynamics, etc., one would be expecting a human influence, and the anthropogenic forcings have changed and could, also or even instead of, be the cause of all or part of thedecadal/multi-decadal fluctuations being suggested here as natural. [Michael MacCracken, United States of America]	attribution is addressed in Chapter 10, but text has been revised to avoid suggestion that all low frequency variability is necessarily natural
3-1014	3	42	34	42	34	Line 34 I would have thought a likelihood statement could be placed here rather than a confidence statement, which I interpret as a weaker level of evidence than an explicit likelihood statement. [Nathaniel Lee Bindoff, Australia]	for such a summary statement, we believe it is difficult to assign a single quantitative likelihood statement, as the level of certainty in the evidence for change varies with the property concerned
3-1015	3	42	34	42	37	Same comment as above: highlight the very high confidence that the physical and biogeochemical state of the ocean has changed. [Leticia Cotrim da Cunha, Brazil]	as the statement says there is very high confidence, we are not quite sure what the reviewer would like changed
3-1016	3	42	34	42	38	This is a a final sum-up of all before but it is very vague. From a concise layout of evidence and a label of certainty applied to each ocean variable, the final word simply states that there is 'high confidence that the physical and biogeochemical state of the ocean has changed.' Heraclitus's quote 'No man ever steps in the same river twice' can also apply to the ocean, which is constantly changing. Some mention of time period involve and what 'state' means would help to make this paragraph more concise. [Tim Boyer, United States of America]	The paragraph has been revised. Given the different levels of certainty associated with observed changes in different variables, it is difficult to sum up the chapter in a sentence and be much more specific
3-1017	3	42	38			See my previous comments aabout delta Os vs O2 as mirroring delta S vs S. What does this say about the system? Is it consistent with an enhancement of an ecological state? [Terrence Joyce, United States of America]	there is no published literature that allows such an assessment
3-1018	3	42	39	42	47	People will look for guidance on the adequacy of the observing system to capture changes in oceanography in	As some comments asked for more on observing

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						the global ocean. Thus, this paragraph needs to be extensive. The oceanographic community also needs endorsement from this high level document to support various global and regional observing systems. [Government of Australia]	systems, and others asked for less, we believe we may have the balance about right. While we agree on the importance of sustained support for ocean observing systems, the IPCC assessment is probably not the right place for this.
3-1019	3	42	45	42	47	I suggest to be more precise in the begining of this sentence in order to more insist on the importance of biogeochemical observations, that now begins to be routinely acquired (eg O2 and nutrients from Argo profilers), and suggest "Sustained global-scale multiparameter (temperature, salinity but also biogeochemical parameters) observations, will increase...". [BERNARD BOURLES, France]	accepted
3-1020	3	42	45	42	47	This final sentence beginning "Sustained global-scale observations..." reads like a push for funding of ongoing observation programs. Suggest removal. [Thomas Stocker/ WGI TSU, Switzerland]	accepted. Sentence removed
3-1021	3	42	49	44	19	This FAQ felt incredibly long and detailed compared to all other FAQs which I have reviewed. I think that it should be cut by roughly 50% and some details may be unnecessary to answer the question posed. I think some re-evaluation of what is critical in this FAQ is required and then a redraft should look to reduce the word count by somewhere in the range a third to a half. [Peter Thorne, United States of America]	Partly accepted. Other reviewers have felt differently. Some material has been trimmed, but not as much as requested here.
3-1022	3	42	51	42	51	I think the authors provide a good responses to three FAQs in this chapter, all of the responses could be made more accessible to lay readers by reducing the amount of technical jargon that is used. [Francis Zwiers, Canada]	Noted. Specific responses are given to more specific comments from this reviewer made below.
3-1023	3	42	51	42	51	I suggest coordinating with the authors of Chapter 10 to make sure that the response given here is consistent with the treatment of ocean heat content (OHC) in that chapter. For example, they consider OHC changes from 1950 onwards, which the response to this FAQ suggests that pre-1970 data may not be reliable. [Francis Zwiers, Canada]	Accepted. Chapter 10 has been made aware of Chapter 3's assessment.
3-1024	3	42	51	42	51	Perhaps the question could be elaborated a bit so that it is clear to the lay reader that "ocean" means the interior of the ocean as well as the surface. Perhaps "Is there warming throughout the depths of the ocean?". [Francis Zwiers, Canada]	Partly accepted. The first sentnce is modified to emphasize this point.
3-1025	3	42	51			The FAQs do a good job of explaining these concepts. Suggest additional questions to address policy options, mitigation efforts, how to move forward given uncertainty in the data, and opportunities for international collaboration. Similar questions could be used to help the reader understand the modeling process including the use of ensembles, dealing with uncertainty, parameters used in the analysis, temporal and spatial limitations, and new technologies. [Government of United States of America]	Rejected. That sort of thing is beyond the FAQs, which are supposed to be based on assessments within the report.
3-1026	3	42	53	42	53	First sentence in the italic text of FAQ 3.1 is very vague and not helpful to the average reader. Suggest that the second part of this sentence should be a little more specific, such as "...but the warming varies depending on the depth, geographic location and timescale being considered") [Government of Canada]	Accepted. The first sentence now notes that the ocean is warming in many regions, over many depth rangds, and over many time periods, although neither everywhere, nor constantly.
3-1027	3	42	53	42	54	In that the question for the IPCC as a whole is if human activities are affecting the climate, which is considered the average conditions over more than a decade (typically three decades), this phrasing seems very strange to me. It seems to be focused, in my view around short-term changes--so yes, the ocean changes in a lot of different ways season to season and year to year. Then the second sentence--so not the primary sentence--gets to the climate change issue in a sort of flippant way, not talking about the significant changes over the century, etc. It really seems to me that the answer should be indicating that the changes being seen are most consistent with the projections of change from human activities, citing earlier assessments if need be as this chapter is ahead of later reporting on expected ocean repsonses. [Michael MacCracken, United States of America]	Partly accepted. This is an FAQ, and it seems wise to note that there are the equivalents of weather and climate in the ocean. The first sentence has been modified to make this point more explicitly.
3-1028	3	42	54	42	54	I would add "and basin" after "global. This has been very well-documented by Levitus et al. (2012) and their previous papers. It is only relatively small regions of the upper part of the world ocean that are not characterized by a warming trend. I feel that the authors of this chapter are not making as compelling a case	Accepted.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						for global warming as can be made. This is one example. [Sydney Levitus, U.S.A.]	
3-1029	3	42	55	42	55	FAQ3.1: Suggest adding a few lines of introduction here to set the stage for the discussion of ocean warming (i.e. enhanced GHE leads to excess energy in the climate system, this heat will propagate down into the ocean...). These FAQs should establish at the beginning of each of them, why the question addressed is important. [Government of Canada]	Rejected. A FAQ for the ocean observations chapter should be focussed on observations, not attribution.
3-1030	3	42	56	42	57	Define "upper ocean". [Francis Zwiers, Canada]	Accepted.
3-1031	3	43	2	43	4	FAQ3.1: These lines stress the variability in ocean temperature which is useful for understanding the need for good measurements in time and space (info that follows) but it does give a different message than presented earlier in the introduction to this chapter (Page 6 lines 15-18) where the main message is that the ocean is a good integrator of natural variability. What is the more important message to stress in this FAQ (or is it both)? [Government of Canada]	Noted. The FAQ stresses both.
3-1032	3	43	2	43	4	Again, the impression being given here is that if the change is not as smooth as the CO2 concentration curve, then the deviations from this must be natural fluctuations. Quite clearly, the change in human-induced radiative forcing is not uniformly changing over time, particularly over the North Atlantic, and so there can certainly be anthropogenic aspects of changes in the ocean that are not uniformly/monotonically changing (in the future, we will hopefully better understand the land cover change implications as well, so I think the suggestions that the fluctuations are mainly natural needs to be a good bit more moderated, at least indicating that external forcing changes (natural and anthropogenic) are likely playing a role, even if not yet fully understood. [Michael MacCracken, United States of America]	Rejected. These sentences say nothing about attribution, and they are discussing local changes, not global.
3-1033	3	43	2	43	57	FAQ3.1: The information flow on this page is awkward and jumps back and forth between subjects. Suggest keeping together paragraphs that discuss warming of the upper ocean, and likewise paragraphs that address deep ocean warming. [Government of Canada]	Accepted. The material has been rearranged to start with the upper ocean changes and then move to the deep ocean.
3-1034	3	43	2			As written, this sentence appears to negate the previous sentence. Revise sentence to read: "Despite the overall warming trend, ocean temperature at any given location can vary greatly with season." [Government of United States of America]	Accepted. The material has been rearranged and rewritten to remove this appearance of negation.
3-1035	3	43	8	43	8	Is there a simplified term for "profiling floats" that lay readers will find easier to comprehend - "automated floats" or "robotic floats"? [Francis Zwiers, Canada]	Partly Accepted. The adjective "temperature/salinity" has been added. The word "robotic" was added, but the consulting editor suggested removing it again.
3-1036	3	43	9			for global ocean estimations such as global ocean heat content, one should start with Argo only in the year 2005. So I would say here in this context, that Argo achieved global coverage in 2005, not in 2004. [Karina von Schuckmann, France]	Rejected – refers to Chapter 2
3-1037	3	43	11	43	11	FAQ2.2 Figure 2: I found the "Except in these regions" heading in this figure misleading: When I first read it I thought you meant the events referred to (e.g. heatwaves) were NOT happening in the indicated regions. However mostly it is a difference in the likelihood of the changes which is being referred to, rather than a difference in the direction. I suggest you change the wording to something like "But there are some differences in likelihood in these regions" to go above the icons for North America and Europe, and "except there are differences in sign" above the Australia icon. [David Wratt, New Zealand]	???? Is this comment for Chapter 2?
3-1038	3	43	14	43	14	How do you say "Temperature anomalies enter the subsurface ocean by paths in addition to mixing from above" to lay readers? [Francis Zwiers, Canada]	Rejected. A good question. If we could think of a simpler, but precise wording we would use it. Suggestions are welcome.
3-1039	3	43	14	43	15	I would eliminate the figure associated with FAQ3.1. and the relevant text. I think this figure and text are too pedagogical in nature. This is an assessment, not a textbook. [Sydney Levitus, U.S.A.]	Rejected. This is a frequently asked question, and FAQs should be more pedagogical than the rest of the assessment.
3-1040	3	43	15	43	15	Colder, and thus denser, waters can sink from the surface in high latitudes, then... [BERNARD BOURLES, France]	Accepted.
3-1041	3	43	16	43	17	You do a very poor job of explaining how warm water defies normal physics by heat not convecting to the	Partly accepted. Reading an introductory textbook or

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						surface, especially seeing how the subsequent paragraph describes a process that is consistent with normal physics. Your comments look far more like speculation than based on solid science. Ocean warming is very substantially driven by insolation and while it penetrates the surface and warms down to about 120 m in the tropics, this does not explain the claimed warming below 700m, let alone 2000m. [John McLean, Australia]	taking an introductory class on physical oceanography would probably help your confusion. The text in the FAQ has been modified in an attempt to better make the point about how ocean circulation can communicate ocean warming at the surface to great depths, even to the bottom of the ocean.
3-1042	3	43	20	43	26	This paragraph is partially repetitive of the previous paragraph. [Francis Zwiers, Canada]	Accepted. These two paragraphs have been combined.
3-1043	3	43	24	43	24	I suggest the words "appear to" be removed - either bottom water has been warming detectably, or it has not been warming detectably. [David Wratt, New Zealand]	Accepted.
3-1044	3	43	28	43	40	I would eliminate the figure associated with FAQ3.1. and the relevant text. I think this figure and text are too pedagogical in nature. This is an assessment, not a textbook. [Sydney Levitus, U.S.A.]	Rejected. This is a frequently asked question, and those should be more pedagogical than the rest of the assessment.
3-1045	3	43	45	43	46	Can "spurious instrumental biases" and "biases ameliorated" be simplified? [Francis Zwiers, Canada]	Rejected. If we knew how to simplify these terms, we would. Concrete suggestions are welcomed.
3-1046	3	43	48	43	49	To avoid ambiguity, suggest revising this sentence to: "In some years, the ocean appears to warm faster than average; in others the rate of warming seems to slow." [Government of United States of America]	Accepted.
3-1047	3	44	4	44	5	Isn't the heat content of the ocean 3300 times that of an equal volume of air at sea level? [John McLean, Australia]	Rejected. The ocean has about 260 times the mass of the atmosphere, and the heat capacity (per unit mass) of seawater is about 4 times the heat capacity of air.
3-1048	3	44	4	44	8	The oceans only occupy 71% of the Earth's surface, they are principally warmed by insolation, cannot be warmed by downwelling radiation from CO2, and are not absorbing insolation that falls on land, ergo to say the oceans are absorbing "nearly all this excess heat" is just silly if you are claiming that the excess heat is due to CO2 downwelling radiation. [John McLean, Australia]	Rejected. First the ocean heat gain occurs as a result of an imbalance in the net exchange of heat across the ocean surface. This could mean less heat is leaving the ocean because of a warmer atmosphere above. Second, longwave, sensible, and latent heat fluxes are all mediated by the ocean mixed layer, where turbulence keeps ocean properties vertically homogenous, usually to depths of at least 20 m, but sometimes (owing to convection) to depths of 2000 m or more in winter. Around Antarctica and in the Northern N. Atlantic, cold water sinks to the bottom in dense plumes. Third, in the ocean currents carry colder water from the surface high latitudes poles to deeper in the ocean at lower latitudes. As the ocean surface warms, these sinking waters at high latitudes become warmer, and transmit this signal to the subsurface ocean.
3-1049	3	44	8	44	9	Add to the paragraph the sentence "A warmer ocean will release heat to the atmosphere and therefore cause the atmospheric temperature to increase." [John McLean, Australia]	Rejected. Again, this suggestion adds extraneous material. Also, this will only happen if the atmosphere is not also warmer.
3-1050	3	44	10	44	10	Suggest replacing "the long time scales of ocean circulation" with "its slow circulation". [Francis Zwiers, Canada]	Accepted.
3-1051	3	44	10	44	17	Should add citations? Meehl et al 2012 Nat. Clim. Change [Catia Domingues, Australia]	Rejected. Citations are not placed in FAQs.
3-1052	3	44	10	44	17	FAQ 3.1: this paragraph refers to the future and possible long-term commitments which are not assessed in Chapter 3. Suggest to refer here to Chapters 12 and 13 and in particular to their commitment sections 12.5.2, 12.5.4 and 13.5.2 [Thomas Stocker/ WGI TSU, Switzerland]	Accepted.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1053	3	44	12	44	17	I think this information can be presented a bit more directly. Here's a suggestion, beginning after the sentence that ends with "changes in greenhouse gas concentrations". "Thus, if greenhouse gas concentrations could be held at present levels into the future, the Earth's surface temperature would begin to stabilize within about a decade. However, it would take centuries to millennia for deep ocean temperature to warm and stabilize, and thus sea-levels would continue to rise for centuries to millennia as well." [Francis Zwiers, Canada]	Accepted.
3-1054	3	44	15			Thus use of "slightly more" is confusing here. Do the authors mean slightly more than substantially or slightly more than zero? [Government of United States of America]	Accepted. This portion of the FAQ has been reworded along the lines suggested in comment 3-1053.
3-1055	3	44	21	45	4	This FAQ does not answer the question that it asks. Even if CO2 is causing a change in temperature, the ocean pH is still completely independent to climate (ie. it neither impacts climate nor is impacted by it), which of course implies that none of the discussion of ocean pH should appear in this report, which is focussed on climate matters. The correct place for a discussion of ocean pH is in a report about the environmental consequences of CO2. [John McLean, Australia]	rejected the change in pH is a consequence of rising CO2.
3-1056	3	44	21	45	4	You seem to be claiming that CO2 absorption will increase at higher temperatures, which would defy known chemistry and physics besides being contrary to empirical observations. If the oceans warm as you are claiming, then CO2 absorption will reduce and consequently the pH will increase rather than decrease. Please correct your statement. [John McLean, Australia]	rejected a higher CO2 level in the atmosphere will lead to a decreasing pH in the ocean
3-1057	3	44	21	45	4	FAQ 3.2: in our view needs significant further work. Material should be expanded upon and the basic terms and concepts defined. The introduction to this FAQ should provide upfront the context for why we are interested in Ocean Acidification, with appropriate links to the WGII chapters where these impacts are assessed. FAQs need to stand alone, so it should not be assumed that the reader has already obtained this background information from Box 3.2. Some material from the comprehensive Box should also be included here to strengthen the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	noted the FAQ has been substantially revised.
3-1058	3	44	23	44	23	Perhaps the response to this question would be a bit more accessible, albeit longer, if chemical species were identified by name rather than (or in addition to) being identified by chemical formula. [Francis Zwiers, Canada]	accepted
3-1059	3	44	23	44	51	FAQ3.2: FAQs are aimed at general readers and in that regard, this FAQ falls short in that it is quite technical and doesn't address key questions many readers will have on this topic. Namely, why are we concerned about ocean acidification, and can it be stopped (similar to the question "can GW be stopped?"). Almost missing from this first paragraph is an indication of why anyone is concerned about ocean acidification (with only the last sentence acknowledging a concern). For an FAQ, this contextual information is essential. Suggest the first paragraph begin by setting an appropriate context and elaborating a bit about the reasons for concern. This is required even though the FAQ is directed to answer a question about linkages between two subjects. Choosing to phrase the FAQ that way doesn't negate the need for context for why we care about the question anyway. Information about timescales of the perturbation could be added elsewhere in the FAQ. Overall there is a need to simplify the text. [Government of Canada]	partly accepted the FAQ was rewritten
3-1060	3	44	23			It is not clear why the term "anthropogenic ocean acidification" is used, but "anthropogenic climate change" is not. In the interest of simplicity, is it possible to use just "ocean acidification" and "climate change" throughout this FAQ? [Government of Canada]	accepted text adapted when possible
3-1061	3	44	23			FAQ 3.2: There is currently no mention in this FAQ to the non-anthropogenic effects on Ocean pH, as is included in the glossary definition. [Thomas Stocker/ WGI TSU, Switzerland]	partly accepted the FAQ was rewritten
3-1062	3	44	23			FAQ 3.2: Table 1: This table is much too complex, and this level of technical detail is not required in an FAQ. We recommend removing this table. [Thomas Stocker/ WGI TSU, Switzerland]	We discussed this with Thomas Stocker at the Lead Authors meeting in Hobart and agreed to leave it in. It answers directly one of the major questions about ocean acidification.
3-1063	3	44	23			FAQ 3.2: Figure 1: We would recommend simplifying this figure as much as possible, e.g., removing the equations and any other non-essential details from the figure. See for example, SPM figure 2. In addition, consider aspects of Box 3.2 Fig 1 which links nicely to the global scale of the phenomenon, and potential impacts by including reef locations. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted and the figure was revised.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1064	3	44	25	44	25	Please replace "are caused by" with "are influenced by". AR5 definition of climate change includes both anthropogenic and naturally forced change. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. The FAQ was revised.
3-1065	3	44	25	44	26	Remove comma after "Both" and add "(CO2)" after first use of carbon dioxide in this sentence. [Government of Canada]	Accepted. The FAQ was revised.
3-1066	3	44	27	44	27	FAQ3.2: Suggest 'emitted' replace the word "reflected" here to be consistent with how the GHE is described elsewhere in this report (e.g. Chapter 1, page 5, line 25). [Government of Canada]	Accepted. The FAQ was revised.
3-1067	3	44	33			This is the first mention of cement production in this chapter. Suggest adding some mention in the preceding text. [Government of United States of America]	Accepted. The FAQ was revised.
3-1068	3	44	36	34	38	There are various technical terms and formulae here and in FAQ 3.2 Table 1 that might not be known to the non-technical reader. Since the FAQs are supposed to be stand-alone they should not require reference to the Report Glossary. How about providing a brief explanation of the technical terms at the end of the FAQ, e.g. pCO ₂ : The partial pressure of CO ₂ ; CO ₂ (aq) ...; H ₂ CO ₃ : carbonic acid; HCO ₃ ⁻ : Bicarbonate ion; CO ₃ ²⁻ : carbonate ion; CT:" [David Wratt, New Zealand]	Accepted. The FAQ was revised.
3-1069	3	44	37	44	37	The term "pCO ₂ " is not explained. [Government of Canada]	Accepted. The FAQ was revised.
3-1070	3	44	37	44	37	We suggest to explain/define pCO ₂ in the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	Accepted. The FAQ was revised.
3-1071	3	44	44	44	46	Figure caption would be clearer if the words "on the island of Hawaii" were to be moved so as to occur after the words Mauna Loa Observatory (top). "and the" would then be inserted after the word "Hawaii" and the word "and" that occurs before the words "Station Aloha" would be changed to "at". The present caption refers to "surface ocean pH (bottom) on the island of Hawaii", which forced me to pause when I read it. [Adrian Simmons, United Kingdom]	Accepted. The FAQ was revised.
3-1072	3	44	48	44	55	This paragraph needs rewording. People may question how reduction of the solubility of carbon dioxide would still affect the ocean acidification. [Government of Australia]	Rejected. This point is explained in the following sentence.
3-1073	3	44	48	44	55	FAQ3.2: There is no mention of whether the impacts of ocean acidification on marine ecosystems would affect CO ₂ uptake. Is this worth mentioning? [Government of Canada]	Rejected. This question is outside the scope of this FAQ.
3-1074	3	44	49	44	50	FAQ3.2: This sentence "The CO ₂ taken up by the ocean does not contribute to greenhouse warming" is potentially confusing. Suggest replacing with "By removing some of the CO ₂ emitted to the atmosphere from human activity, the oceans reduce the enhancement of the greenhouse effect." [Government of Canada]	Accepted. The FAQ was revised.
3-1075	3	44	50	44	50	Accessibility could also be improved by explaining key concepts in lay language. For example, one could replace "the solubility of carbon dioxide in seawater" with "the amount of carbon dioxide that will dissolved in seawater" [Francis Zwiers, Canada]	Accepted. The FAQ was revised.
3-1076	3	44	53	44	54	Suggest the authors explain why the decrease in pH remains unchanged. [Government of United States of America]	Accepted. The FAQ was revised.
3-1077	3	44	53	45	2	FAQ3.2: This text is difficult to understand and no explanation is given for why the decrease in pH remains unchanged even as less CO ₂ is taken up. This text requires additional explanation especially as it is part of an FAQ. The associated table is too detailed for inclusion in an FAQ and is not properly described in the text except for a brief reference. [Government of Canada]	Accepted. The FAQ was partially revised as suggested in 1076 above.
3-1078	3	44	54	44	54	I don't think it is necessary to explain pH decrease a second time. [Francis Zwiers, Canada]	Rejected. The FAQ was revised as suggested in 3-1076.
3-1079	3	45	6	46	21	There is no compelling reason for including a discussion of the water cycle in Oceanic Observations (Chap.3), even under the guise of Frequently Asked Question. The regional focus that is natural to an oceanographic perspective leads to a serious misrepresentation of global water cycle dynamics. The view that salinity data may constitute an important proxy rain gauge is erroneous, except over very short time periods after a rain event before oceanic diffusion intervenes. Once again, salinity data (while essential for ocean dynamics) determine neither precipitation nor evaporation but only regional P-E differentials that do not imply changes in	Rejected. FAQs are outside the chapters, were agreed on at the WG1 level, and will be published separately. They are not tied to one chapter or another. This FAQ, while proposed and led by Chapter 3, involved writers from Chapters 2 and 9.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						globally averaged P or E. FAQ 3.3 Delete altogether. [Government of France]	
3-1080	3	45	8	45	8	FAQ3.3: Suggest changing the title of this FAQ to "Is there evidence of an intensification of the Earth's water cycle". The phrase "intensification of the water cycle" is the phrase that is always used in reference to future changes in the water cycle. Often it is unclear exactly what this phrase means. This FAQ is an opportunity to address that. [Government of Canada]	Rejected. Title of the FAQ is general and does not imply the answer. It was agreed on at many levels and many iterations within WG1.
3-1081	3	45	16	45	17	Replace "because warmer air can be moister: the atmosphere can..." by "because air can...". Reason: keep it as simple as possible. [Urs Neu, Switzerland]	Rejected. This is just a stylistic choice and several including an editor have agreed that this language is fine.
3-1082	3	45	16	45	18	In the absence of a referenced argument, I find it hard to justify the first sentence going directly from warming to an intensified water cycle. I would replace the first sentence with something like: "The water cycle will inevitably change as climate changes, because surface-to-atmosphere energy and water fluxes are strongly linked. Atmospheric water content is expected (and observed) to increase in the warming climate, because air can hold about 7% more water vapor for each °C of warming. Moreover, because water surface temperatures are expected and observed to increase more than air temperatures, the water cycle is expected to intensify in a warmer climate, with global increase in evaporation and precipitation." (cf. lines 38-39 on the same page.) [Robert Kandel, France]	Taken into account. Last sentence has been inserted. Supporting text is found within Chapter 3 and Chapter 2, and the TSU has agreed to allow us to provide a general statement about which chapters include information that is synthesized in this FAQ.
3-1083	3	45	16	45	19	This statement is incorrect insofar as surface and atmospheric boundary layer water vapour over land have not increased consistent with atmospheric temperature rise over the last decade. Please see section 2.5.5 and a number of comments above on Chapter 2. [Adrian Simmons, United Kingdom]	Noted. Communicating with chapter 2 about this comment.
3-1084	3	45	16	46	11	FAQ3.3: The text for this FAQ jumps back and forth between topics which distracts from understanding. Lines 38-43 would seem to belong with lines 16-19 and help explain what "intensification of the water cycle means" and why this FAQ will focus on observations of water vapour and salinity (rather than precipitation or evaporation). This information belongs up near the top. Lines 26-28, which explain salinity, should come before lines 21-24 which describe observed change in salinity. Then we jump back to salinity again at the end of the FAQ in lines 4-8 on page 46. This information should be brought together with the other text about salinity changes. [Government of Canada]	Taken into account. We have reorganized the material. See also comment 3-1088.
3-1085	3	45	21	45	24	"Changes in ocean salinity in the last 50 years also provide strong evidence that the global water cycle is increasing in intensity as the earth warms." The justification for this statement is suspect. See my comment on chapter 3, page 14, line 38, Section 3.3.4. [David Webb, United Kingdom]	Reject. See responses above to previous comments. We stand by our conclusion, and are grateful to D Webb for highlighting the importance of looking at whether changes in stratification due to warming could themselves have been the main source for surface salinity change. Since we find that freshwater content has also changed, coincident with surface salinity, and since this has happened over too large a scale as to be due to advection changes, we conclude that the surface forcing changed. We do not say that it changes point by point - even the mean salinity pattern has very important advective differences from the mean E-P regionally.
3-1086	3	45	21			Insert the word "circumstantial" between "strong" and "evidence": we need to relate delta(E-P) to (E-P), NOT delta S to S to prove this, which has not been done. So be circumspect. [Terrence Joyce, United States of America]	Noted.
3-1087	3	45	21			As discussed in several earlier comments, the salinity observations do not provide strong evidence of intensification of the "global" water cycle. The salinity observations provide strong evidence of an increase in the intensity of the water cycle over the oceans, but this does not necessarily mean that this will carry over into continental interiors. Please also note comments 158 and 161, referring to statements that seem at odds with those deduced from the salinity measurements. Do these sentences cast doubt on the strength of the evidence from the salinity measurements? [Adrian Simmons, United Kingdom]	Taken into account. We have added "intensity over the oceans". We (chapter lead authors) do not have access to the numbering used in this comment, so we have not been able to cross-reference with the referenced comments (158 and 161).

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1088	3	45	26	45	28	These 3 sentences should be moved up and inserted at the end of the first sentence on line 22, i.e., introduce the concept of 'salinity' before you go on to say that there have been changes in salinity. [Thomas Stocker/ WGI TSU, Switzerland]	Accept. We also reorganized the material around this - with the definition of salinity we have placed the description of the basic salinity field relative to E-P and water vapor transport. We then describe the trends. (See also comment 3-1084.)
3-1089	3	45	26			It would seem to be valuable to define salinity when it is first mentioned in the main text (or provide a pointer to the definition here). [Government of United States of America]	Accept. Salinity is now defined in the main text, and it is also defined here. We cannot use pointers to the text within an FAQ; they are meant to be self-contained.
3-1090	3	45	26			Salinity is the amount of dissolved salts in a unit mass of seawater, not just a "kilogram." Use either "weight" or "kilogram" or mass consistently. [Government of United States of America]	Accept.
3-1091	3	45	27	45	28	Maybe add something like "on human time scales" after "does not change" on line 27, and add "quickly" after "changed"? [Francis Zwiers, Canada]	Accept.
3-1092	3	45	30	45	31	Lay readers might be confused by the word "distribution" (e.g., if they have had some exposure to statistics, then they will think of distribution in a different way that intended here, or they might not understand at all). Here and elsewhere (eg, line 12) if it is necessary to refer directly to the spatial distribution, perhaps an easier word would be "pattern". But perhaps it would be sufficient for the spatial pattern to be implicit. Line 30 could say "The salinity of the ocean surface largely reflects the difference between evaporation and precipitation, and regionally is also affected by runoff from land and sea ice freezing and thawing." [Francis Zwiers, Canada]	Taken into account. Changed "distribution" to "spatial pattern".
3-1093	3	45	35	45	36	Here is another place where the language could be made accessible - this sentence could be rewritten as "The atmosphere connects the ocean's regions of net fresh water loss to those of fresh water gain by moving evaporated water vapour from one place to another." [Francis Zwiers, Canada]	Accept.
3-1094	3	45	38	45	38	Your statement assumes that the atmosphere will warm. You've presented no credible reason why it should, so your comments are only supposition, and based on the absence of warming over the last 16 years it is hardly a credible supposition. [John McLean, Australia]	Reject. All of the evidence is that the atmosphere is warming and will continue to warm, backed up by many different chapters, both observational, attributional, and predictive, in the IPCC WG1 report.
3-1095	3	45	38	45	38	"A warmer atmosphere is projected to ...". This needs a good reference. [David Webb, United Kingdom]	Reject. References are not used in FAQs.
3-1096	3	45	46	45	46	The use of the formal uncertainty language 'likely' should be avoided in the FAQ. [Thomas Stocker/ WGI TSU, Switzerland]	Accept.
3-1097	3	45	47	46	2	I suggest the addition of "northern" here, ie "... more flooding associated with earlier snow melt at high NORTHERN latitudes ..." [David Wratt, New Zealand]	Accept.
3-1098	3	46	1	46	1	Replace comma by point and start new sentence (Land-based observations are....). Otherwise there is a mixture of floods and droughts in the same sentence. [Urs Neu, Switzerland]	Accept.
3-1099	3	46	1	46	2	Check this sentence to ensure it is consistent with other discussions of drought. [Government of United States of America]	Taken into account. This FAQ was co-authored with Chapter 2, where drought is treated.
3-1100	3	46	4	46	4	"Ocean salinity, on the other hand, acts as a sensitive and effective rain gauge ... ". Yet again. Salinity may act in this way but only if one can allow for the effects of changes in ocean stratification, diffusion and advection. So far nobody has sensitively and effectively shown that, as far as climate changes is concerned, the statement is anything more than a wish. [David Webb, United Kingdom]	Reject. Because changes are observed in upper ocean freshwater content, well below the mixed layer, we are confident that we are not just seeing a response of a more highly stratified upper ocean to unchanging E-P. Because the freshwater content changes are of very large spatial scale, we are equally confident that they are not the result of anomalous advection or diffusion.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1101	3	46	4	46	5	Suggest simplifying the language here by writing "Ocean salinity, on the other hand, acts as a sensitive and effective rain gauge because it naturally reflects the difference between water gained by the ocean from precipitation and water lost by the ocean through evaporation." [Francis Zwiers, Canada]	Accept, although others wanted more words (see comment 3-1102).
3-1102	3	46	4	46	11	It is not evident for everyone, why salinity can be measured more accurately than precipitation and evaporation, respectively. Is it spatially more homogeneous (and therefore needs less point measurements)? Is it easier to measure? This should be explained. [Urs Neu, Switzerland]	Accept. We thought we were explaining it well, as an integrator, but apparently it wasn't clear enough, so we've added some more words.
3-1103	3	46	4			In view of earlier comments on the implications of ocean salinity measurements for the "global" water cycle, perhaps the words "over the ocean" should be added after "rain gauge" for avoidance of doubt. [Adrian Simmons, United Kingdom]	Accept.
3-1104	3	46	10	46	11	When I read this I just thought Ugh! Its incredibly techy, it's a single sentence paragraph which is poor English and it adds absolutely nothing of value beyond the preceding text and the figure. I would strongly recommend its deletion or rewriting. [Peter Thorne, United States of America]	Accept. See comment 3-1105
3-1105	3	46	10	46	11	I wonder if this could be said without referring to statistical significance. Perhaps replace this with "Oceanographers have high confidence [is this the correct assessment?] in ocean salinity data that has been collected over the past 50-years. These data indicate widespread changes in the salinity of the ocean surface that are indicative of systematic changes in precipitation, as is illustrated in FAQ 3.3, Figure 1." [Francis Zwiers, Canada]	Taken into account. We have changed the sentence as suggested, but not used the confidence language (see comment 3-1096 from T. Stocker which indicates that we should not use formal uncertainty language in the FAQ).
3-1106	3	46	11			Should it be "over about 50% of the global ocean surface". [Catia Domingues, Australia]	Reject. Number in Durack and Wijffels is 43.8%, which we've rounded down to "over 40%" for simplicity in the FAQ
3-1107	3	46	15	46	15	Explain (or simplify) "total precipitable water". [Francis Zwiers, Canada]	Accept. Definition added
3-1108	3	46	15	46	16	Replace "in total precipitable water from Special Sensor Microwave Imager (after Wentz et al., 2007)" by "in total precipitable water from satellite observations (Special Sensor Microwave Imager; after Wentz et al., 2007)" [Urs Neu, Switzerland]	Accept.
3-1109	3	46	17	46	17	Replace "NCEP" by "meteorological measurements reanalysis (NCEP)". Reason: NCEP is not a widely known acronym. [Urs Neu, Switzerland]	Accept. NCEP explained
3-1110	3	46	17	46	17	Explain "NCEP" [Francis Zwiers, Canada]	Accept.
3-1111	3	46	17	46	18	"Trend (1950 to 2010)" For consistency this should be "Trend (1950 to 2000)" [Paul Durack, United States]	Accept.
3-1112	3	46	21			Suggestion for another FAQ - "Is sea level rising"? [Government of United States of America]	rejected. FAQ 13.1 from Ch13 "why does local sea level change differ from global average?" includes the sea level rise question
3-1113	3	47	1	61	1	all geographic names must begin with capital letter (e.g. Atlantic, British Columbia, etc.) [Antonio Bode, Spain]	Editorial - will be corrected
3-1114	3	47	47	47	47	Josey is not a coauthor of this paper. Also most of the acronyms that should be capitalized in the Reference Section are not capitalized as they should be. [Sydney Levitus, U.S.A.]	rejected Josey is a coauthor of this publication
3-1115	3	47	53	47	53	I am not sure what this reference refers to but it could be: T. P. Boyer, J. I. Antonov, O. K. Baranova, H. E. Garcia, D. R. Johnson, R. A. Locarnini, A. V. Mishonov, T. D. O'Brien, D. Seidov, I. V. Smolyar, M. M. Zweng, 2009: World Ocean Database 2009, Chapter 1: Introduction, NOAA Atlas NESDIS 66, Ed. S. Levitus, U.S. Gov. Printing Office, Wash., D.C., 216 pp., DVD. [Sydney Levitus, U.S.A.]	accepted this is the correct reference
3-1116	3	48	45	48	46	Add in the following reference: Compo, G.P. and co-authors (2011), The Twentieth Century Reanalysis Project. Q. J. R. Meteor. Soc., 137, 1-28. DOI:10.1002/qj.776. [Xiaolan Wang, Canada]	noted references are only added when cited in the text
3-1117	3	50	46			There are many words in references that have lost their capitals: North Atlantic, English Channel etc. [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Editorial - will be corrected
3-1118	3	53	18	53	18	This paper has been published and the citation is: Levitus, S., J. I. Antonov, T. P. Boyer, O. K. Baranova, H.	editorial copyedit to be completed prior to publication

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						E. Garcia, R. A. Locarnini, A. V. Mishonov, J. R. Reagan, D. Seidov, E. S. Yarosh, M. M. Zweng, 2012: World Ocean heat content and thermosteric sea level change (0-2000 m) 1955-2010. Geophys. Res. Lett., 39, L10603, doi:10.1029/2012GL051106. [Sydney Levitus, U.S.A.]	
3-1119	3	56	1	56	1	Last author of the ref is Rios, A.F. [Carles Pelejero, Spain]	editorial copyedit to be completed prior to publication
3-1120	3	57	37	57	40	These two are the same reference: Schuster U., Watson A.J. 2007. A variable and decreasing sink for atmospheric CO2 in the North Atlantic. J. Geophys. Res. 112: C11006, doi: 10.1029/2006jc003941. [Carles Pelejero, Spain]	editorial copyedit to be completed prior to publication
3-1121	3	59	31	59	31	Update this reference as follows: Wang, X.L., Y. Feng, and V. R. Swail, 2012: North Atlantic wave height trends as reconstructed from the twentieth century reanalysis. Geophys. Res. Lett., 39, L 18705, doi:10.1029/2012GL053381. [Xiaolan Wang, Canada]	editorial copyedit to be completed prior to publication
3-1122	3	60	39	60	40	May update the reference: Xue, Y., et al., 2012: A comparative analysis of upper ocean heat content variability from an ensemble of operational 40 ocean reanalyses. J. Climate, 25, 6905-6929. doi:10.1175/JCLI-D-11-00542.1 [Zeng-Zhen Hu, United States of America]	editorial copyedit to be completed prior to publication
3-1123	3	63	5			Why not showing a hovmoller for 700-2000 m although the same Levitus estimates were quoted /used in the text to estimate OHC? [Catia Domingues, Australia]	Partly accepted. The Levitus estimate from 700–2000 m is now included in Fig. 3.3
3-1124	3	63	5			Should an extra panel be added (below hovmoller) to show the changes in ocean coverage for 0-700 m and 0-2000 m? [Catia Domingues, Australia]	Partly accepted. The figure is already too large. There is now an appendix discussing ocean coverage.
3-1125	3	63	5			Figure 3.1: Panel C is the anomaly relative to 1971-2010, but the data extends to 2011 (or maybe 2012?) Need to note in the caption the beginning and end time point of the x-axis. [Government of United States of America]	Rejected. The reader can assess this without lengthening the caption.
3-1126	3	63				Fig 3.1: To improve the illustration of global SST-trends it could be useful to turn Fig. 3.1(b) by 90° and to place it right of Fig. 3.1(a); Pls complete further the graph by adding (b), (c) and (d) [Government of Germany]	Partly accepted. The figure maximizes use of the page as configured. The panel labels have been moved, enlarged, and made bold in the revision.
3-1127	3	63				Figure 3.1: Labels missing for panels b, c, and d [Andrew Shao, United States of America]	Partly accepted. The panel labels have been moved, enlarged, and made bold in the revision.
3-1128	3	64	4			This figure is terrible. You can barely evaluate the different curves because of the aspect ratio and also because some colours have little contrast (cyan/green/orange). [Catia Domingues, Australia]	Accepted. The colors are the IPCC suggested pallet and not changed in the revision. However, the aspect ratio has been changed and the shadings for the uncertainty envelopes have been made much lighter.
3-1129	3	64	4			Fig 3.2: Please add text to the caption that explains the x-axis (e.g.: "between 1950 and 2011"). [Government of United States of America]	Rejected. The reader can discern that information from the axis label.
3-1130	3	64	4			Fig 3.2: Please state which studies assume no change in areas with sparse data records? [Government of United States of America]	Rejected. This topic is discussed in the body of the text.
3-1131	3	64	8			Fig 3.2: What is meant by "overlap" (line 8)? It may help to shade this portion of the graph. [Government of United States of America]	Partly accepted. "of overlap" is changed to "that are". The regions is not shaded (others felt there was already too much shading on this figure).
3-1132	3	64		64		Fig. 3.2 is very hard to decipher. Try lighter colors or use colored lines with vertical bars for the confidence intervals or plot in "landscape" mode as opposed to "portrait" mode. [Sydney Levitus, U.S.A.]	Accepted.
3-1133	3	64		64		Figure 3.2. I think this figure should be made clearer and I have a number of suggestions on how this might be achieved: (i) choose a different colour palette – there are many similar shades of purple/blue which I think makes the figure hard to read; (ii) Increase the transparency of the shaded regions and remove the outline from these regions; (iii) Change the aspect ratio of the figure so that x- and y- extent are similar. [Matthew	Partly accepted. The colors are the IPCC suggested pallet and not changed in the revision. However, the aspect ratio has been changed and the shadings for the uncertainty envelopes have been made much

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Palmer, United Kingdom of Great Britain & Northern Ireland]	lighter.
3-1134	3	64		64		Figure 3.2. The authors should consider including, and commenting on, the major volcanic eruptions. These are strong radiative forcings of the climate system that we can see in ocean heat storage changes. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Accepted. A sentence has been added to section 3.2.3
3-1135	3	64				Figure 3.2 Description is very complicated and picture is poorly readable. However the convergence of results in the last 10 years is easy to understand [Government of Poland]	Accepted. The figure aspect ratio has been changed and the shadings for the uncertainty envelopes have been made much lighter to allow for better reading.
3-1136	3	64				Figure 3.2: This figure is hard to see. Suggest making it wider to help distinguish the lines? Are these lines in a particular order- is there a reason that some are graphed on top of others? [Government of United States of America]	Accepted. The figure aspect ratio has been changed and the shadings for the uncertainty envelopes have been made much lighter to allow for better reading.
3-1137	3	64				Suggest auxiliary vertical axis in units W yr m-2 and lines of constant slope corresponding to different heating rates in W m-2, to guide the eye. I find this figure confusing and hard to read. Compare the figure at http://www.ncdc.noaa.gov/bams-state-of-the-climate/2009-time-series/ohc . Now if you could just put some indication of uncertainty on that figure, it would be much better than the existing figure; then add the second axis and lines of constant slope and you would have a great figure. [Stephen E Schwartz, United States of America]	Accepted. The figure aspect ratio has been changed and the shadings for the uncertainty envelopes have been made much lighter to allow for better reading.
3-1138	3	64				Figure 3.2: Hard to distinguish the 5 results from each other due to overlap. Showing only the outermost uncertainties would make the figure slightly easier to read at the cost of artificially portraying the level of uncertainty. However, this would be a much more conservative estimate of the total uncertainty while still showing that five separate studies all clearly show a warming trend beginning in the 1990s. [Andrew Shao, United States of America]	Partly accepted. The figure aspect ratio has been changed and the shadings for the uncertainty envelopes have been made much lighter to allow for better reading.
3-1139	3	64				Figure should use 90% Cis [Peter Thorne, United States of America]	Rejected. The published uncertainties are used here.
3-1140	3	65	4			Fig. 3.3 What is the trend period in the figure? [Government of United States of America]	Accepted. The caption now notes the trend is "centred on 1992–2005"
3-1141	3	65	4			Fig. 3.3: Please note in a title or caption that these values are for 1992-2005. What happens to the uncertainty level at >5000m in "a"? The numbers inside the map (b) are hard to read. [Government of United States of America]	Partly accepted. In the deepest regions of the ocean there are fewer measurements so the uncertainties increase. However, there is not room to discuss this in the text.
3-1142	3	65				Figure should use 90% Cis [Peter Thorne, United States of America]	Accepted.
3-1143	3	66	9			Box 3.1 Figure 1: Revise line 9 to read "Uncertainty in ocean measurements" rather than "the ocean uncertainty". [Government of United States of America]	Partly accepted. The text is changes to read "The uncertainty in the ocean estimate . . .".
3-1144	3	66		66		Box 3.1, Figure 1. I find the figure caption a little confusing, regarding "with below 2000 m starting from 1992 ". My suggestion is that the authors split the ocean heat content into 3 layers: 0-700m, 700-2000m and 2000m-bottom. I think this would be much clearer. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Rejected. There are enough colors without adding another one, and 700 m is a reasonable dividing point given the historical emphasis on the upper ocean heat content.
3-1145	3	66				Box 3.1 Figure 1. Very good and informative figure representing the role of the global ocean as the main heat excess sink [Government of Poland]	noted thank you
3-1146	3	67	4	67	8	Figure 3.4 a) to d). Please identify the meaning of colors such as red, pink, blue and sky-blue in the figure. [Government of Chile]	Accept. Caption expanded to include this information
3-1147	3	67	4	67	8	Figure 3.4 (c) and (d): Please include a colour scale. [Government of India]	Accept. Caption expanded to include this information
3-1148	3	67	4			Fig. 3.4 lacks color scales for the four panels. The legend should also mention salinity units for panels (a), (c) and (d). [Government of France]	Accept. Caption expanded to include this information
3-1149	3	67	4			Figure 3.4: Suggest revising line 4 to read "sea surface salinity (SSS)". [Government of United States of America]	Accept.

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1150	3	67		67		Fig. 3.4 is difficult to read. Try lighter colors. "World Ocean Atlas 2009" should be replaced by Antonov et al. (2009). The citation is: Antonov, J. I., D. Seidov, T. P. Boyer, R. A. Locarnini, A.V. Mishonov, H. E. Garcia, O. K. Baranova, M. M. Zweng, and D. R. Johnson, 2010: World Ocean Atlas 2009, Volume 2: Salinity. S. Levitus, Ed. NOAA Atlas NESDIS 68, U.S. Gov. Printing Office, Wash., D.C., 184 pp. [Sydney Levitus, U.S.A.]	Accept.
3-1151	3	67				Figure 3.4d is hardly mentioned in the text, and the comparison with the trend map in Figure 3.4c should be discussed. [Government of Australia]	Accept text modified to include a discussion
3-1152	3	67				fig. 3.4 The units for the color contours should be stated (salinity in ppt?). A legend for the color scale would also be helpful. [Government of United States of America]	Accept. Caption expanded to include this information
3-1153	3	67				Figure 3.4: Why does the period of record end in 2005? [Government of United States of America]	Taken into account. 2005 was mid-year and this was not precise. Rewritten.
3-1154	3	67				Figure 3.4: Figure C and D appear to be inconsistent. They agree on the region of reduced salinity in the Western Equatorial Pacific warm pool but in the South Atlantic and Central Equatorial Pacific Hosoda et al. seem to show essentially no change (or noise) whereas Durack et al show a strong positive precipitation signal. If I was a good devil's advocate I would also be tempted to point out that the Durack 1950 data will include bucket and engine intake samples measured using titration whereas the Hosoda 1975 data should all be based on more accurate CTD data. The latter is "very likely" to be the most accurate. If the present IPCC report had concentrated on the Hosoda data instead of the Durack data then some of the conclusions might have been very different. [David Webb, United Kingdom]	Accept. Text added for the comparison between the two. The data set used here for Durack had some erroneous points and now the problem is fixed. The agreements between Durack and Hosoda are hence much better now, and the general conclusions reached in the report are unchanged.
3-1155	3	68	5			fig. 3.5 What is the "one degree zonal belt of the Atlantic, Pacific, Indian, and World Oceans"? [Government of United States of America]	Taken into account. Zonal average along the latitude, which has one-degree length. Explanation modified.
3-1156	3	68		68		It is nearly impossible for me to discern the lines and even the colors in this figure 3.5. [Sydney Levitus, U.S.A.]	Taken into account. Color changed.
3-1157	3	68				Could red be used instead of blue? Probably more contrast. [Catia Domingues, Australia]	Accept.
3-1158	3	69				In the lower panel of Figure 3.6, ERA40 is only up to 2002. [Government of Australia]	Accepted, figure modified.
3-1159	3	69				Fig. 3.6: Evaporation E (positive upward) and the latent heat flux $H = LE$ ought to be strictly proportional. Why is the latter pictured in the other direction? [Government of France]	noted. This is simply due to the sign convention adopted for the latent heat flux.
3-1160	3	69				Fig. 3.6 Alternative approaches to clarify this presentation could include a linear fit to see what level of significance there is. Would a map of the different quantities or their long term rate of change also reveal some pattern? [Government of United States of America]	noted. Heat flux variability at regional scales is more heavily influenced by regional processes making identification of a spatial pattern of change difficult. Hence, the focus on global averages. Note: Regions of the ocean that are ice covered for part of the year were omitted from the global mean calculations presented in the Second Order Draft version of this figure. They have been included in the calculation for the revised figure and this reduces the magnitude of the evaporation and latent heat flux values by a small amount.
3-1161	3	70	4			Fig. 3.7: Revise first sentence (line 4) to "Long-term reconstruction of change in ocean precipitation..." [Government of United States of America]	accepted
3-1162	3	70				Fig. 3.7: Revise title for y-axis to "precipitation anomaly". [Government of United States of America]	accepted

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1163	3	71				Fig. 3.8 The text corresponding to this figure discusses other data types. The figure would be stronger if it also included the satellite and island station data in some way thereby demonstrating that most sources show increases in wind stress. [Government of United States of America]	Figure replaced by one showing wider range of reanalysis based wind stress time series adapted from Swart and Fyfe (2012). The satellite and island station data from Yang et al. (2007) is wind speed not stress as is now made clear in the revised text.
3-1164	3	71				Figure caption should cross-reference Box 2.3 that outlines reanalyses. [Peter Thorne, United States of America]	accepted. A summary of the reanalyses is now included in Sec 3.4.1; and box 2.3 is cross-referenced in caption
3-1165	3	72	5	72	5	"potential temperature Column 1" should be salinity (column 1) and "salinity (column 3)" should be potential temperature (column 3) [Paul Durack, United States]	accepted figure caption checked
3-1166	3	72	5			Fig. 3.9: The order of the columns and the caption description (line 5) seem to disagree. Units are needed for temperature and density. [Government of United States of America]	accepted figure caption checked
3-1167	3	72		72		The caption of Fig. 3.9 is incorrect. Column 1 is not "potential temperature" but is "salinity". Theta and S columns are reversed. [Sydney Levitus, U.S.A.]	accepted figure caption checked
3-1168	3	72				Figure 3.9: Figure caption has an error where temperature and salinity need to be reversed. [Government of Australia]	accepted figure caption checked
3-1169	3	72				Fig 3.9: To bring in line the graph and the attached explanation of Fig. 3.9 : pls exchange column 1 (salinity) vs. column 3 (temperature); furthermore add the scale-units of temperature, density and salinity in the legend. [Government of Germany]	accepted figure caption checked
3-1170	3	73	1	73	1	Figure 3.10. The values of the SSH contours are most probable too small to be legible in the print version [Government of Poland]	Noted. The figure is revised. The contour interval for SSH contours is noted in the caption.
3-1171	3	73	4	73	4	Argo era should be given exactly. [Xianyao Chen, China]	Accepted. The caption is revised to note the time span.
3-1172	3	73	4	73	4	Explain abbreviation SSH [European Union]	Accepted. Added: sea surface height. This acronym was also introduced on page 3-22 line 56.
3-1173	3	73	4	73	8	For the scientist scanning the chapter and reading the captions, I'd suggest defining SSH in the figure caption (basically have abbreviations/mnemonics explained in the captions. These chapters can get looked at by many from outside the field. [Michael MacCracken, United States of America]	Accepted. Added: sea surface height. This acronym was also introduced on page 3-22 line 56.
3-1174	3	73	7	73	8	It is neither the direct result of the Figure 3.10, nor the conclusion from some references. [Xianyao Chen, China]	Noted. It is assumed readers of this subchapter will be familiar with geostrophic velocity, as found in any basic physical oceanography textbook. Length constraints do not permit an explanation to be provided.
3-1175	3	73				In the last sentence of the Figure 3.10 caption, '...SSH trends are proportional to changes in surface geostrophic velocity'. Still variations in Coriolis terms should be taken into account, so that same amount of change in low latitude is different from that in high latitude. [Government of Australia]	Accepted. The caption is revised to include the Coriolis parameter.
3-1176	3	74	4			Fig 3.11: Note the time covered (x-axis) by saying something like "between 1965 and 2012". [Government of United States of America]	Accepted. Figure revised.
3-1177	3	74	4			Fig 3.11: Move the definition of a Sverdrup to the first sentence: "Volume transport of the Florida Current between 1965 and 2012 (Sv...) from dropsonde measurements..." [Government of United States of America]	Accepted. Caption revised.
3-1178	3	74	10			Fig 3.11: It would be helpful to spell out MOVE as was done for RAPID/MOCHA [Government of United States of America]	Accepted. Meridional Overturning Experiment (MOVE) spelled out in the caption.
3-1179	3	74	13	74	13	It would be nice to have both 26N and 41N shown at the same temporal resolution, even if that means that the	Taken into account. The 3 time-series: 26N, 41N, 16N

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						26N data have to be smoothed. It might make sense to show both original and smoothed version (maybe the latter in a light colour?), but since both 41N and 26N timeseries are shown on the same plot, they should be shown in a directly comparable way. [European Union]	are all plotted equivalently at 3 month smoothing. As the moored time-series at 26N and 16N have very high temporal resolution, the unsmoothed versions are not shown.
3-1180	3	74				Figure 3.11: in panel b, 16N should either be taken out or replaced by an indirect AMOC estimate. [European Union]	Taken into account. The sub-chapter is focused on direct measurements of ocean circulation. The indirect estimates are relevant and constitute another line of independent evidence but are not considered appropriate for display together with the direct measurements.
3-1181	3	74				Figure 3.11: A map of this current would aid in understanding the data presented. [Government of United States of America]	Taken into account. The caption is modified, "the Florida Current between Florida and the Bahamas"
3-1182	3	74				Fig. 3.11a. Given the spread of observations at each time step this figure is very difficult to read and the multidecadal trend referred to in the text is not apparent. The data need to be suitably smoothed or averaged before plotting. [Toby Sherwin, United Kingdom]	Taken into account. The text says there is no apparent multidecadal trend. The dropsonde observations of Florida Current transport are at irregular intervals, so smoothing is problematic, and it is thought better to display the original data.
3-1183	3	75	4	75	4	Suggest replace "representing" with "in different", since these records cannot represent the sea level change in whole basin due to the strong spatial variability of sea level rising. [Xianyao Chen, China]	accepted, text revised
3-1184	3	75	4	75	6	Figure 3.12: Is it possible to provide standard error associated with the mean sea level variation? [Government of India]	Accepted - a 90% confidence interval has been added to plots.
3-1185	3	76	1	76	1	Fig 3.13: Please include the long tide-gauge reconstruction from Jevrejeva et al. GRL 2008. (Btw. The differences to earlier Jevrejeva reconstructions are fully understood. The earlier reconstruction is sensitive to errors in Peltier's GIA in the early part of the record. The more careful manual early reconstruction in Jevrejeva et al. 2008 supercedes the earlier reconstruction. [Aslak Grinsted, Denmark]	Noted - the tide gauge reconstruction of Jevrejeva et al., 2012 plotted was updated from Jevrejeva et al., 2008. Because Jevrejeva et al., 2012 has not been accepted by the deadline, Jevrejeva et al., 2008. will be used instead.
3-1186	3	76	4			Fig. 3.13: Line 4 should say "Global mean sea level rise" Consider explaining the baseline here. [Government of United States of America]	Noted - text will be changed to state "global mean sea level anomalies" since rise is not correct. The baselines are given in the final sentence, but this will be moved to the first sentence.
3-1187	3	76	9	76	9	Figure 3.13(d) is referred to Leuliette and Willis (2011), but (d) does not look same as the original Figure 3 in Leuliette and Willis (2011). Any different ways to generate the new data should be mentioned. [Xianyao Chen, China]	Noted - the data in Figure 3.13d was produced by Eric Leuliette and includes updated values beyond the end of the record published in Leuliette and Willis. Vertical and horizontal scales are different between this plot and the one is published in Leuliette and Willis, which makes plots difficult to compare visually.
3-1188	3	76	10			Eric Leuliette has numbers to 2012 now [Philip Woodworth, United Kingdom of Great Britain & Northern Ireland]	Noted - will try to update for final draft.
3-1189	3	76	13			Maybe move the last sentence (line 13) higher up in the caption. [Government of United States of America]	Accepted.
3-1190	3	76				Should not mix red and green to consider colour-blinded readers. [Catia Domingues, Australia]	Noted - color is actually "warm mustard" from the IPCC color palette.
3-1191	3	76				Figure 3.13 - caption should specify if thermosteric is full depth or just to 700m. [Government of Australia]	Accepted.
3-1192	3	76				Fig. 3.13: It is difficult to understand four graphs as each has different x and y axes but are the same colors (c is especially hard to understand). [Government of United States of America]	Noted.
3-1193	3	76				I have significant reservations about the presentation and interpretation of the uncertainty estimates here.	Noted - Authors only report a standard error on the

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Firstly, and most easy to fix, they should be 90% Cis not 1-sigma ranges. More substantively these error estimates take into account different sources of uncertainty and in fundamentally distinct ways (see e.g. Box 2.1). They will not be 'comparable' estimates. The trouble is that nobody will read the small print and therefore the potential for significant mis-interpretation is rife here. Personally I would not show such parametreic uncertainty estimates unless you are comfortable that they really are comparable. I am far from convinced that they are. [Peter Thorne, United States of America]	results. We would have to make certain assumptions in order to convert to 90% confidence, which we are reluctant to do.
3-1194	3	77	4	77	6	In the figure, it really seems strange that the uncertainty in the estimate does not become larger as one goes back in time, especially given how the networks, etc. change. [Michael MacCracken, United States of America]	Noted - uncertainty is based on the internal variability of residuals about the fit, which does not change much from the early part of the reconstruction to the latter. The internal variability is also higher than the estimated error bars, so give a more conservative estimate of uncertainty.
3-1195	3	77		77		Figure 3.14. Similar to Figure 3.2, to improve clarity, I suggest that the shading in this figure is made more transparent and the outline of the shaded region removed. [Matthew Palmer, United Kingdom of Great Britain & Northern Ireland]	Reject – there are not many choices in the approved IPCC color palette, and this figure has 4 colors. One curve has to be green.
3-1196	3	77				replace green colour. [Catia Domingues, Australia]	noted. Figure has been revised
3-1197	3	77				Fig. 3.14 – Note that the 60-years period natural oscillation is easy to recognize in this figure. It suggests a deceleration of sea level rise in the next 30 years. This 60 years-period sinusoid is also seen in Box 2.2 Fig. 1 Bottom in this SOD, and is also observed in various proxies e.g. length of the day, AMO, PDO, ENSO, JISAO indices, fishing productivity (Klyashtorin and Lyubushin 2007), Swanson and Tsonis (2009), Schlesinger and Ramankutty (1994), Loehle (2004), Zhen Shan and Sun Xian (2007). [François Gervais, France]	Taken into account - already discussed in section 3.7.4 when Figure 3.14 is referenced.
3-1198	3	78	6	78	7	The name "Sea of Japan" must be used instead of "the East Sea (Sea of Japan)," because "Sea of Japan" is the only internationally established name for the sea area concerned. [Government of Japan]	accepted text changed
3-1199	3	79		79		Figure 3.16. The colors used for the lines in the lower three panels are too faint. Some curves can barely be distinguished from the underlying grid (which perhaps should be removed anyway). [Ralph Keeling, United States of America]	accepted figure redrawn
3-1200	3	80	1	80	1	Figure 3.17. The black line for atmospheric Mauna Loa CO2 data seems offset almost 10 ppm downwards. For example the average value for 2010 should be 388 ppm while the the one in Fig. 3.17 looks lower than 380 ppm. [Government of Poland]	accepted figure redrawn
3-1201	3	80		80		Figure 3.17. This figure would almost certainly be visually more compelling if the y axis were reduced in scale so that the seasonal swings are less overwhelming allowing the eye to more easily trace the curves. [Ralph Keeling, United States of America]	accepted figure modified
3-1202	3	81	4			Box 3.2 Fig. 1: Is 1995 the most recent data available? [Government of United States of America]	noted. the figure shows the most recent data
3-1203	3	81				Box 3.2 Fig. 1: Consider adding a simple scale along the pH scale that shows which direction is more acidic and more basic. [Government of United States of America]	accept scale added to the figure
3-1204	3	81				What are models doing in an observations chapter? This seems like a bona fide case of Chapter over-reach. If we need to make recourse to pure-models then we should stay such a discussion to more appropriate Chapters such as Chapters 9 or 10 instead. [Peter Thorne, United States of America]	rejected this is a FAQ and if necessary model results can be added.
3-1205	3	82	7	82	9	Box 3.2. Fig 2. Calcite and aragonite are expressed as concentration here but as saturation state in table 3.2 [Government of Iceland]	accept text modified
3-1206	3	82				Box 3.2, figure 2: It may be more interesting with a figure that shows the shallowing of the saturation horizon during the last decade(s). Please consider to insert information about this development in a separate panel c) if such data from these three oceans are available. [Government of NORWAY]	noted.
3-1207	3	84	5			Fig. 3.19: Line 5, Does the statement "between 1960 and 1974 and 1990 and 2008" mean that b= 1960-1974 and c=1990-2008? [Government of United States of America]	accepted text improved to clarify this issue

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
3-1208	3	84	5			Fig. 3.19: Why is one period 14 years and the other 18 years? Or is it [1990-2008] MINUS [1960-1974]. Please clarify. [Government of United States of America]	accepted text improved to clarify this issue
3-1209	3	84				Fig. 3.19: The <95% and >95% text to the right of figure 3.19b is hard to understand. Consider putting that information in the caption. [Government of United States of America]	partly accepted the figure caption is expanded to include this information
3-1210	3	84				Fig. 3.19: When is this climatological mean measured? [Government of United States of America]	this comment seems not appropriate for that figure
3-1211	3	85				Fig 3.20: Pls cite the graphs in the order as they are shown here (change the position of comments as regarded to GMSL and UOHC) [Government of Germany]	accepted
3-1212	3	85				Fig. 3.20: The panels should be labelled a to d, and matched to the corresponding description in the caption, which are not labelled a-d either, and seem to be out of sequence with the graphic. [Government of United States of America]	accepted
3-1213	3	85				Fig. 3.20: The top plot (C) should be labelled "Anthropogenic Carbon" rather than "Carbon". [Government of United States of America]	accepted
3-1214	3	85				Fig. 3.20: The curve looks very smooth and it is unclear how many data points contribute to it. The others look like annual values, possibly smoothed? This should be clarified in the caption. [Government of United States of America]	accepted
3-1215	3	85				Please clarify in the caption what the shading denotes [Peter Thorne, United States of America]	accepted
3-1216	3	86	4	86	16	Figure 3.21 check that the sea level rise anomaly shown is consistent with other chapters in shape, and with other figures in this chapter. The greater sea level rise in the southern ocean is perhaps a different message from other chapters, and is different from the thermosteric sea level rise as a function of latitudes. Make clear what sea level means here, ie is it contributions of thermosteric, ice mass loss and GIA. Using a common reference period might help here. [Nathaniel Lee Bindoff, Australia]	sea level curve has been removed from figure, as it covers a different time interval and may therefore be confusing, and other Figures in chapter indicate sea level change
3-1217	3	86	12			Although it is quite obvious, the minus signs (for example ---) are not explained in the legend. [Jean-Pierre Gattuso, France]	accepted and fixed
3-1218	3	86	13	86	13	Figure 3.21. carbonate ion is noted as CO3= [Government of Iceland]	accepted carbonate has been changed in figure
3-1219	3	86	14	86	15	I assume that S- should be the character S with an horizontal line on top of it, the symbol for "mean". [Jean-Pierre Gattuso, France]	accepted, caption revised
3-1220	3	87	4			FAQ 3.1, Fig. 1: The caption would be more accurate if it said that in a stratified ocean, the "densest" instead of "coldest" is in the deep portion. [Government of United States of America]	Accepted.
3-1221	3	87	7	87	7	Here and elsewhere in the caption, replace "most recently-ventilated" with wording that a lay person would understand. [Francis Zwiers, Canada]	Accepted.
3-1222	3	87				Also the effort to be qualitative is a little confusing: "slightly warmer" and "still warmer" and "even warmer". Consider, "cool, less cool, slightly warm, much warmer" [Government of United States of America]	Accepted.
3-1223	3	87				It would clarify to note that squiggly arrows represent a much slower vertical mixing. [Government of United States of America]	Rejected. "mixes slowly downward (sub-surface squiggly red arrows)" is sufficient.
3-1224	3	88	1	88	1	I suggest removing the text giving the details of regressions that presumably have been fitted to these three curves, since they are not discussed in the response or in the caption. [Francis Zwiers, Canada]	The text has been changed to include a comment about the regressions.
3-1225	3	88	4			FAQ 3.2 Figure 1: add caption text to explain "between 1990 and 2012". It is curious that the long-term Mauna Loa record is concatenated. [Government of United States of America]	The text has been changed to make this clear.
3-1226	3	88	5			The units should be given for CO2 (ppm), pCO2 (uatm) and pHT (total scale) [Jean-Pierre Gattuso, France]	TSU - Can you make the requested changes to this Figure?
3-1227	3	88				Figure "insitu" should be "in situ" [Jean-Pierre	TSU - Can you make the requested changes to this

Expert and Government Review Comments on the IPCC WGI AR5 Second Order Draft – Chapter 3

Comment No	Chapter	From Page	From Line	To Page	To Line	Comment	Response
						Gattuso, France]	Figure?
3-1228	3	88				Figure: the Y axis should be "pHT" (T is a subscript) rather than "pH" [Jean-Pierre Gattuso, France]	TSU - Can you make the requested changes to this Figure?
3-1229	3	88				Figure: the left Y axis should be "CO2 and pCO2" rather than "CO2" [Jean-Pierre Gattuso, France]	TSU - Can you make the requested changes to this Figure?
3-1230	3	88				Figure FAQ3.2 Fig 1: The technical information alongside the timeseries curves should not be within the graph, but included in the caption and explained to readers. [Government of Canada]	TSU - Can you make the requested changes to this Figure?
3-1231	3	88				FAQ 3.2 Figure 1: Please add a sentence to the figure caption that explains the equations written near the plotted lines, e.g. "The equations shown next to the plotted lines are the best-fit linear trends for 1990 to 2012". Also, I think the "y" in each of the equations is redundant, but suggest adding "x" in parenthesis after the "Year" label on the bottom axis, (ie "Year (x)") [David Wratt, New Zealand]	TSU - Can you make the requested changes to this Figure?
3-1232	3	89	1	89	1	FAQ 3.3. Figure 1 is highly confusing with no color scales. Even as some description of the colors is contained in the caption, it is not easy for the reader to connect the colors to change signs. In addition to that giving the unit of "climatological mean net E-P" in the caption has no point without a color scale (it is not important whether it is "cm yr-1" or "mm per decade" or even "inches per lunar month" if no scale is given. I suggest adding vertical color scales with at least two actual values (with units) next to each map. [Government of Poland]	Accept. Figure has been reworked with color scales.
3-1233	3	89	1	89	1	I'm not sure how convinced the non-specialist will be by this figure. The argument put forward in the question is that surface salinity trends should reflect trends in E-P, but it is not clear (at least to me) how they would be able to relate the upper two panels (trend in precipitable water and the climatological E-P) to the third panel (trend in surface salinity). I *think* a more direct illustration will be required. Also, it would be good to use consistent time periods and colour schemes across all parts of the figure, and to provide colour scales for the quantities that are displayed. [Francis Zwiers, Canada]	Taken into account. We are shifting the panels around. The somewhat tenuous correspondence between water vapor and E-P is discussed in the text and FAQ, but since both of these are related to hydrological cycle, we have chosen to retain them, since this FAQ spans several chapters and is not just "oceans". Conflicting comments on color palettes - comment 3-1234 would prefer that they be different for every panel. We'll try several different approaches.
3-1234	3	89	4			FAQ 3.3, Fig. 1: This figure would be much easier to understand if the scale were beside each map, instead of embedded in the caption. Having the same color palette, but different scales on the bottom two maps is confusing. [Government of United States of America]	Accept. Figure has been reworked with color scales.