#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
1	36294	30	0	0	0	0	The Marine Region divided here are somewhat different from my (probably old) recoginition. For example, Areas of the Kuroshio passway along the Japanese Islands is out of the Western Boundary System and belongs to Subtropical Gyre or High Latitude Spring Bloom System. Same for the Gulf Stream in North America. Is there universal agreement or rational on this regional divisionm e.g. based on the physical-chemical properties? (Sanae Chiba, JAMSTEC)	We recognise that the broad sub-regions are not perfect. In addition, they have been developed along broad biological lines which do not match perfectly over the physical-chemical properties or geographical boundaries. We have added a sentence to explain this. "Dividing the ocean into these sub-regions takes into account the diversity in the structure and functioning of marine ecosystems, although it is recognised any sub division will not perfectly over physical-chemical patterns or specific geographies."
2	37113	30	0	0	0	0	Both Chapters on coastal and oceans should benefit from the overall global marine "assessment of assessments" report recently published by UNEP and IOC - See : http://www.unga-regular-process.org/ including analysis of several regional seas and oceans linked at time to thematic climate change assessment. (Salif Diop, UNEP - SAB - DEWA)	We have downloaded and considered these comprehensive review documents. We have not cited these articles directly because of the following three issues: 1. We are instructed to use a primary literature, and grey literature like this is a second resort. 2. We are expected to do our own assessment and not depend on that of others. 3. We are focused on climate change and its impacts - these assessments of assessments involve consideration of a multitude of other factors.
3	37265	30	0	0	0	0	This Chapter was fairly well written and covered the topics in a reasonably balanced way. (Erica Head, Fisheries and Oceans Canada)	We are grateful to the reviewer for his/her comments.
4	38286	30	0	0	0	0	Looking at figures and tables made for the different chapters, there are similarities (e.g. magnitude of temperature and rainfall changes, impacts on ecosystems...) between chapters because they have they deliver similar information, but for different regions. (Guillaume Simioni, INRA)	We are grateful to the reviewer for his/her comments. In the SOD draft - we have significantly rewritten the text and have reduced overlaps and enhanced synergies.
5	38287	30	0	0	0	0	Having a similar layouts (i.e. same styles and legends, symbols, columns, colors, ...) accross the chapters, would help the comparison between regions. Not sure it is important, especially if the readership is different from one chapter to another. It's just a suggestion. (Guillaume Simioni, INRA)	We are grateful to the reviewer for his/her comments and are working with the IPCC WGII TSU for comments on this layout issue between chapters.
6	38569	30	0	0	0	0	This chapter does a wonderful job of describing patterns of temperature change and the subsequent biological responses. However, the chapter almost completely ignores ocean acidification, with only brief mentions of OA as a risk in a few subsections (e.g., deep sea) and a paltry four sentences on OA on pp 43-44. Halpern et al 2008 (Science) appropriately identifiy OA as a threat on par with warming, and it should be treated as such in this chapter. I realize that there is much less information available with regards to historical trends, but there are some long term time series (e.g., the one just off Hawaii, Gretchen Hofmann et al.'s data from the Antarctic, Wootton et al. 2008 (PNAS), etc., see also AR5 Ch. 6) indicating abiotic change. There are also good projections for future conditions, such as Gruber et al.'s recent paper for the California current system (Nicolas Gruber, Claudine Hauri, Zouhair Lachkar, Damian Loher, Thomas L. Frölicher, Gian-Kasper Plattner. Rapid Progression of Ocean Acidification in the California Current System. Sciencexpress 14 June 2012 / Page 1 / 10.1126/science.1216773). Furthermore, there are important aspects of the biological responses to OA that deserve mention. For example, there are numerous studies on OA effects on coccolithophores, and the response of these plankters will influence marine primary production, food web dynamics, and carbon export to deep sea sediments. There is also decent data for pteropods (aka fish food). Perhaps even more importantly, elevated carbon dioxide may stimulate phytoplankton and macroalgal growth and actually be a boon to productivity (at least for some species, potentially including harmal algal blooms). Finally, there are specific areas, like the coral reef case study, where the effects of OA need to be included. Yes, bleaching due to warming will impact corals in the warmer portions of their current ranges, but OA will impact corals everywhere (including their future ranges), both directly through reduced calcification and indirectly through the reduction of the crustose coralline algae that act as cement to hold coral reefs together. There is an extensive physiological and ecological literature here as well, including work done by Fabricius and colleagues on CO2 vent systems in the Indo-Pacific. It is true that we lack a beautiful landmark OA study summarizing ongoing OA effects like what Poloczanska et al. 2012 have shown for temperature, but the magnitude of the predicted impacts of OA suggests that much more attention to this stressor, and the currently very large uncertainties that surround its ecological impacts, is warranted. (Christopher Harley, University of British Columbia)	Much of the reviewers concerns are covered by chapter 5 and chapter 6 which capture the growing information on how organisms to ecosystems are likely to change. Clear and compelling information,however, at the regional scale is missing. The IPCC also held a special workshop in Okinawa in 2011 on ocean acidification and has produced workshop report (https://ipcc-wg2.gov/publications/SupportingMaterials/index.html). One of the challenges with respect to ocean acidification is that we are seeking studies which indicate impacts/changes at a regional scale as opposed to individual studies speculating on the impacts of ocean acidification on the organisms and ecosystems. We have, however, taken these comments on board and have created greater linkages to discussions concerning ocean acidification within chapters 5 and 6.
7	38678	30	0	0	0	0	It is very good to see this new IPCC Chapter on "Open Oceans" included in AR-5. It is, obviously, an enormous topic and I think that the authors have captured the major elements in this FOD. The key challenge for the next draft is to bring this information together in a consistent and easily digestible manner - at present, I found it hard to come away with a clear picture of the evidence for changes and impacts for the seven ocean regions from the recent scientific literature (which I understand to be the charge for this chapter) and the associated levels of confidence and agreement. Close attention to consistency and overlap with relevant chapters (e.g. 5, 6 and 29) is also required. (Janice Lough, Australian Institute of Marine Science)	We thank the reviewer for encouraging comments. We have improved our text with regard to confidence and agreement. We have also built in linkages to other chapters in WGI and WGII, and checked for consistency among chapters.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
8	39024	30	0	0	0	0	GENERAL COMMENTS. This chapter is outstanding in the organization and content of its writing and the transparency with which its messages get across. As the authors mention in the introductory section (which is an outstanding setting-of-the-stage for the whole chapter), the oceans did not receive a highly focused analysis in the previous Report. In the current Report there almost seems to be an overcompensation for this past neglect: topics related to the oceans are treated in several different chapters, and this is likely to lead to considerable overlap and to raise the possibility of inconsistencies in conclusions, e.g., about certainty of data or conclusions. I just finished reviewing Chapter 6 and found many of the topics treated there to be covered as well in this chapter. However, it's my sense that Chapter 30 does the better job, I found the writing to strike just the right compromise between detail and synthesis. Cross-referencing to other chapters is very good; this helps to tie the whole report together. I found almost nothing to criticize in this chapter. It represents what I think is a near-perfect job of doing the required analyses. (George Somero , Stanford University)	We are grateful to the reviewer for his comments. We will work with chapter 6 to perfect the cross-referencing, and to try and minimise the overlaps between chapter 6 and chapter 30.
9	39293	30	0	0	0	0	GENERAL COMMENTS TO CHAP. 30 - I consider very important the region analysis presented in chapter 30, although much information overlaps with that presented in chap. 6. I suggest to condense the information in every chap. Because of region analysis, chap. 30 could be the natural extension of chap. 6, while the attribution and detection analysis based on a not yet published paper (Poloczanska et al., 2012 – to date, it is probably still under revision) may be reduced considering that chap. 18 has been specifically thought on this topic. (Gianluca SARA, University of Palermo)	We are grateful to the reviewer for his comments. We will work with chapter 6 to perfect the cross-referencing, and to try and minimise the overlaps between chapter 6 and chapter 30.
10	39294	30	0	0	0	0	Also this chapter, like the others previously, is a bit convoluted and difficult to read, although the use of different heading fonts partially helps. I understand this is the FOD. However, the order of references in the body text should be always the same. Here, Authors used the alphabetical order which is not the same when checked against other chapters. (Gianluca SARA, University of Palermo)	We are grateful to the reviewer for his/her comments and are working with the IPCC WGII TSU for comments on this layout issue between chapters.
11	41255	30	0	0	0	0	The draft mostly covered the "Open Ocean" characteristics and was well written. However, the facts already emerged and those projected are mixtured in some sections. More clear separation of thoes items are helpful for readers. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We are grateful to the reviewer for his/her comments. We have worked solidly to reduce the potential confusion between our discussions on presentation of past versus projected changes.
12	41256	30	0	0	0	0	Nonlinearlity of the biological response is partly described. Linear regression analysis is very dangerous for biological data. This fact must be expressed in the beggining of the discussion. Please add more discussion on the nonlinearlity; e.g. critical temperature, distinction of spawning area, match/mismatch between prey and predator, migration pattern change, etc. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	The use of linear regression analysis represents a conservative approach to describing rates of change. Naturally, we are not suggesting that changes over longer term periods are necessary in linear nature. We have added a sentence to explain the use of linear regression and its conservative nature. "It should be noted that the use of linear regression here does not imply (as elsewhere) that the underlying mechanisms are necessarily linear in nature." (Page 3.32)
13	42275	30	0	0	0	0	I suggest to include some information on the effect of increasing temperatures of the seawater on the oxygen content of seawater. Higher temperatures mean a decreasing oxygen solubility and more microbial activity. For example the Gulf of Mexico (page 2397) with only one major source of N and P and a high degree of oxygen depletion will not benefit from warmer seawater temperatures. (Klaas Van der Hoek, RIVM)	We agree and we have added text to page 3.22 and to Exec summary: “Increasing temperatures will also reduce the solubility of oxygen, and increasing oxygen stress is highly likely.”
14	43240	30	0	0	0	0	This chapter is a great step forward from the previous version, however, it seems that major issues of scoping across marine chapters remain unresolved. Naturally, some balancing between chapters is needed, especially since the scoping process did not differentiate sufficiently between the roles of sectoral and regional chapters, especially for the marine realm. Readers would be surprised to find independent, but similar (i.e. at the same level) treatments of the same issues, some building on different materials, in the sectoral and regional chapters. However, an impression of unwanted overlap or even competition between chapters should be avoided. For any regional chapter like chapter 30, an ideal regional treatment would, according to my understanding, lead to synthetic text across working group material for each relevant region, with reference to relevant chapters in WGI, WGII sectoral chapters, i.e. chapters 5 and 6, and to those of WGIII. This strategy to develop synthetic text would help avoid simple overlap and support the development of new insight. The division into ocean regions should follow human and political (including economical) interest and associated geographical criteria more than criteria building on ecosystem aspects which would be handled by sectoral chapters. (However, alternative strategies of differentiation between chapters might be more useful for the marine chapters and should be discussed.) As a consequence, especially chapters 5, 6, and 30 should be cross-calibrated so that it becomes clear that regional treatments in chapter 30 build on the sectoral treatments in chapters 5 and 6. The chapter teams should make sure that all the sectoral aspects important to them show up in chapters 5 and 6 and are briefly referred to and integrated in chapter 30. Last not least, page numbers might decrease in due course. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	The CLAs from chapter 6 and chapter 30 met with Chris Field, Vincente Barros and Katie Mach and resolved this issue to the mutal satisfaction of all.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
15	43249	30	0	0	0	0	While regional aspects are now much stronger in chapter 30 than in ZOD my impression is that this could still be improved further by refraining from a merely sectoral (ecosystem) treatment (as for the North Atlantic) and converting to synthetic writing. Several chapter 30 authors are already Contributing Authors to chapter 6 but may not have considered that chapter as their sectoral basis. Such problem of insufficient differentiation persists throughout the chapter. It would be unfortunate if chapters were simply redundant in certain sections. Also, each chapter should be comprehensive with respect to its specific role. Presently, chapters 5 or 6 might cover regional ecosystem aspects not covered in chapter 30 and, vice versa, chapter 30 might cover global ecosystem aspects not covered in chapter 5 or 6. This needs to be cross-checked. An ideal solution for the SOD regional chapters would be to start synthetic regional writing early, after some clear definition of the regions according to geography and countries affected, then integrating WGI, II and III aspects for each region, balancing the sectoral aspects with the respective chapters in WG I, II, and III. Much of the rich material presented in this chapter could be used or developed accordingly with purely sectoral treatments moving to sectoral chapters. For each region the human interest should be made very clear. The present separation of physical, chemical and biological aspects prevents the regional, synthetic focus intended for these regional chapters. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	The CLAs from chapter 6 and chapter 30 met with Chris Field, Vincente Barros and Katie Mach and resolved this misunderstanding.
16	43293	30	0	0	0	0	The figure collection is impressive. Graphs reflecting synthetic analyses across working groups I to III would match the role of a regional chapter more than the depiction of material which could equally be seen in the global views discussed in the sectoral chapters (see below, for p. 76 onward). (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Our focus in Chapter 30 is on the region known the "Ocean". As discussed with the TSU, we have defined the ocean as ranging from high tide mark to open sea. We recognise the overlap in terms of coastal systems and marine ecosystems in general. However, our treatment necessarily has to start with global maps given our region, the ocean, stretches across the globe. The only way to discuss it sensibly is to start with the global views which illustrate the interactions between the sub regions which we have defined an evolved as part of the chapter.
17	44570	30	0	0	0	0	Section 30.3.1.3: Observed changes in wind and waves – Chapter 3 of the SREX includes an assessment of changes in the extremes of waves, and reference to this chapter could be made here. (Thomas Stocker, IPCC WGI TSU)	Have ensured consistency and have added text to Exec summary: "Storm impact on coastal areas increases with sea level rise through greater storm surge impacts (WGI, 3.7.4)." Have also added text to 30.3.1.5 - "Coastal areas will experience greater storm surge impacts as sea levels increase [WGI, 3.7.4]."
18	44571	30	0	0	0	0	Section 30.3.1.5: Observed changes in Storms – Please update relevant statements to ensure consistency and cross-referencing with the relevant WGI AR5 chapters, and the SREX Chapter 3. (Thomas Stocker, IPCC WGI TSU)	Have ensured consistency and have added text: "Other aspects of storms are likely to change under the influence of warming oceans. Wind speed velocity of tropical cyclones is likely to increase although the frequency will either decreased or remained essentially unchanged (WGI, 3.4.4). There is medium confidence that extratropical storm tracks will shift polewards (WGI, 3.4.5). "
19	44572	30	0	0	0	0	Section 30.3.3: Projected SST – Please update relevant statements to ensure consistency and cross-referencing with the relevant WGI AR5 chapters. (Thomas Stocker, IPCC WGI TSU)	Have added several sentences linking to CH 3 in WGI
20	45579	30	0	0	0	0	Chapter 30 complements Chapter 6 well in that Chapter 30 gives a detailed analysis of detection and attribution of climate change impacts on marine organisms and ecosystems, while Chapter 6 focuses more on underlying mechanisms and projections of future impacts on the biota. Conversely, Chapter 30 presents detailed information on projected temperature changes according to RCP scenarios, which is currently lacking in Chapter 6. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We thank the reviewer for his comment. The authors from chapter 30 also feel that it is a nice fit with chapter 6.
21	45582	30	0	0	0	0	Inconsistency between Chapter 30 and Chapter 6: Whereas the authors of Chapter 30 state that "Longhurst, 1998 identified over 75 distinct ecological provinces..." (p. 5, l. 17), the authors of Chapter 6 state that the 4 biomes were subdivided "into a total of 51 provinces" (p. 8, l. 25 and Figure 6-1). (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	Reviewer is correct: We have changed to "Longhurst [1998] identified over 50 distinct ecological provinces in the ocean, ..."
22	45583	30	0	0	0	0	Figure 6-1 in Chapter 6 (p. 8) is almost identical to Figure 30-1 in Chapter 30 (p. 5). (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We are using the figure for different reasons. We have been told this is not a problem by the TSU secretariat. We have explicitly linked to CH6's use of the figure.
23	45594	30	0	0	0	0	Chapter 30 and Chapter 6 overlap considerably with respect to changes in phenology, abundance and distribution of plankton in the North Atlantic, impacts and projections on fisheries, eastern boundy upwelling systems and coral reefs. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have discussed potential areas of overlap with the TSU and Chapter 6 (and other relevant chapters) - our approach has been generally agreed to with the proviso that appropriate linkages and made back to relevant chapters. We will work to reduce overlaps and to promote synergies.
24	46582	30	0	0	0	0	HadISST 1.1 is used often through the Chapter; it should be referenced to "Rayner, N. A., D. E. Parker, E. B. Horton, C. K. Folland, L. V. Alexander, D. P. Rowell, E. C. Kent, and A. Kaplan, Global analyses of sea surface emperature, sea ice, and night marine air temperature since the late nineteenth century, J. Geophys. Res., 108(D14), 4407, doi:10.1029/2002JD002670, 2003." (Neville Smith, Bureau of Meteorology)	Reference has been added to mention of HadISST 1.1.

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25	46583	30	0	0	0	0	The authors of Chapter 30 have faced a very difficult task with both WG 1 Chapters 3 and 13 dedicated to oceans, as well as Chapters 5 and 6 of WG II, and others like WG II Chp 29 with significant overlaps. In general terms they have done an excellent job negotiating their way through this and working from the basis provided by other chapters whenever possible. (Neville Smith, Bureau of Meteorology)	We thank the reviewer for his supportive comments.
26	46584	30	0	0	0	0	The results of an as yet unpublished study [Poloczanska et 2012] are central to many of the findings of this Chapter. The initiative in seeking a deeper understanding/evidence base on the link between climate change and marine organisms led by one of the LAs is admirable. However in many parts of this chapter it is the only cited evidence and must be seen as a high-risk strategy. Without access to the Poloczanska et al study it is hard to judge the robustness of the analyses therein and, in particular the methodology for determining whether changtes in a particular dataset were consistent with climate change. The Chapter will need to undergo significant change if this study is not published by the cut-off. (Neville Smith, Bureau of Meteorology)	We thank the review for their encouraging comment. We understand that the manuscript (Poloczanska et al) needs to be published by the cut off date (end August), and will ensure that this in the case. This synthesis is imprtant for detection and attribution of biological responses to climate change in the ocean
27	46585	30	0	0	0	0	The chapter is long (not unique for WG II) and probably devotes too much time to assertions/commentary around general issues, and at times emotive language is used. I assume this will be tightened, along with considerable editing in the SOD. (Neville Smith, Bureau of Meteorology)	Will work with authorship team to improve and reduce these problems.
28	46767	30	0	0	0	0	All three chapters mention ocean acidification and coral reefs in varying details. It will be good to compare the relevant subsections (as for example, 5.3.1.6, 5.4.1.6.) with those of Chapter 6 and 30. (Venugopalan Ittekkot, University of Bremen (retired))	We will look at this potential problem and discuss it with the three chapters involved. Cross chapter boxes for ocean acidification and for other issues such as coral reefs are being prepared as result.
29	47322	30	0	0	0	0	This is a chapter ostensibly about open ocean systems, but the majority of the text deals with coastal oceans and marginal seas. Maybe this is a problem of definition but I find it hard to understand how coral reefs constitute an open ocean ecosystem. For oceanographers, “open ocean” usually signifies waters beyond the continental shelves (e.g., > 200 m depth). This does not a priori exclude marginal seas, some of which contain substantial areas of deep water, but others such as the North Sea are entirely epicontinental. The chapter also focuses heavily on benthic organisms and pelagic macrofauna (e.g., fish). Primary production is often referred to but otherwise there is very little reference to impacts on microplankton or biogeochemical cycles. (James Christian, Government of Canada)	This is a problem which we solved in the early meetings of chapter 30. While the chapter is entitled open oceans, the definition is used in the broadest sense - and that is that changes in the open ocean have huge ramifications for coastal and benthic ecosystems as well is regional weather patterns. We were also given permision from the TSU to change our chapter to "The Ocean". Chapter 30 should be seen as an integrating chapter for the 71% of the earth planet that is covered by water. that is the intention of these regional chapters. Otherwise, there is not an integrating chapter for coastal and other ocean systems.
30	47323	30	0	0	0	0	Modes of climate variability such as the PDO, AMO etc are consistently referred to as cycles. They are not. Other than ENSO none of these have been demonstrated to be cyclic phenomena. The term “oscillation” implies cyclicity for some people, but it is simply an unfortunate historical usage. The term “Atlantic Multidecadal Oscillation” is falling out of favour and being replaced by the more accurate “Atlantic Multidecadal Variability”. An excellent 800 year long palaeodata record of AMV was recently published by Winter et al (2011, Earth and Planetary Science Letters 308: 23-28): it shows that the amplitude of the changes is large and the temporal pattern is in no way cyclic. (James Christian, Government of Canada)	We have replaced the word cyclic with variability where appropriate (numerous places throughout the manuscript).
31	47324	30	0	0	0	0	More generally, in my view this draft does a poor job of clearly delineating anthropogenic climate change, climate variability and other human impacts. Isn’t the point of these reports to identify, as far as is possible, the effects that can be clearly associated with AGW, not to warn people that ecosystems are vulnerable to range of human impacts including AGW (e.g. p. 23 lines 32 and 46)?. This is no way implies that complex synergistic impacts of multiple stressors should not be discussed, or that particular observations should be excluded simply because of the difficulty of isolating the AGW component. But it is important to be clear in the text about what is what. In particular, short term trends of the same sign as expected under AGW but of too short duration to clearly associate with it need to be clearly stated to be natural variability with a possible AGW component of unknown magnitude. Any time series shorter than about 30 years falls in this category. In addition, the text frequently refers to “climate change” when it means anthropogenic climate change; this definition needs to be stated up front or the word anthropogenic should be inserted in every case. For example the caption to Figure 8 refers to “consistency of observed responses to climate change” when what is clearly meant is consistency with the expectation of a warming climate. But preindustrial climate was stationary for millennia: it warmed and cooled, but with no overall trend. Figure 9 caption says “consistent with climate change predictions" which are more accurately referred to as projections. (James Christian, Government of Canada)	This is invariably going to be challenging and consequently we have added the paragraph on page 3.13: "Anthropogenic climate change (e.g. ocean warming and acidification) and other human driven changes (e.g. pollution, introduced species, overexploitation of fisheries, and disease) are co-mingled {Halpern, 2008 #8} and hence identifying a clear signal due to climate change is not easy in many circumstances. Long-term records are important for this task but are rare. Stressors not related to climate change may interact synergistically or antagonistically with each other and climate change, making it difficult to distinguish changes due to climate change as opposed to non-climate change factors." We also beg to differ with the reviewer on several points here. The first is that any discussion of open ocean impacts without reference to the other issues that ocean systems face would present a fairly poor picture of how rapid anthropogenic climate change is and is likely to affect oceans and the multitude of physical, chemical and biological processes important to the planet. The second is that we have to discuss natural and anthropogenic climate change within the one context - some is clearly delineated and others less so. The third is that we refer to the expert literature from expert groups and other parts of the IPCC who are looking at the physical, chemical and biological impacts of climate change and addressing issues such as timeframes, variability and the climate signal.

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32	47325	30	0	0	0	0	I am also unconvinced that the authors are using terms like “likely”, “very likely”, “high confidence” etc in a consistent way. Aren’t these terms supposed to refer to confidence in attribution of anthropogenic impacts, and not simply confidence in existence of a trend that may or may not have a significant anthropogenic component (e.g., 31/4-5)? I wouldn’t express “high confidence” in the attribution of an anthropogenic component to the 1970s Pacific climate shift (29/19-20), although as noted above the authors say “climate” when they clearly mean anthropogenic. A statement like “Changes to the distribution of warm water also influence the intensity of storm systems such as cyclones” is so general as to merit “virtually certain” rather than the “moderate confidence” assigned here (29/10-12), but it is difficult to tell what is actually being asserted here. (James Christian, Government of Canada)	We have completely revised and have carefully calibrated our use of these terms.
33	47326	30	0	0	0	0	The Western Boundary Systems domain makes no sense to me. I simply do not see any unifying biogeophysical characteristics of the systems discussed, and the discussion of the Gulf Stream and Kuroshio (what oceanographers call Western Boundary Currents) is just confusing because none of the systems discussed in any detail are in any meaningful way analogous to these. It lumps systems that arguably belong in the marginal sea domain (e.g., South China Sea), and systems like the Somali Current that are oceanographically more analogous to Eastern Boundary Currents (i.e., characterized by Ekman upwelling normally associated with equatorward wind stress, that happens to be poleward in these anomalous monsoon-driven cases even if occurs on the western boundary of the basin), with WBC ecosystems that are barely mentioned. (James Christian, Government of Canada)	The reviewer has a good point - when one looks at the distribution of Western boundary systems, they are hardly Western. This has been changed to "Coastal boundary systems" or CBS throughout the manuscript.
34	47327	30	0	0	0	0	When impacts of ocean acidification are discussed it somehow needs to be made clear that OA is not a consequence of climate change but an independent and parallel consequence of CO2 accumulation. At present OA impacts are mostly treated as AGW impacts. (James Christian, Government of Canada)	We agree with the reviewer and will reorganise the text accordingly. We have added a sentnece that explains the linkage yet distinction of GW and OA. Page 3.21: "Increases in atmospheric concentrations of CO2 have increased the CO2 flux into the ocean, which has resulted in the acidification of the ocean, which is an independent and parallel impact of CO2 to that of global climate change. "
35	47328	30	0	0	0	0	I have doubts as to whether cloud cover should be discussed at all. Neither the reanalysis data (Figure 5) nor the GCM projections (Figure 13) are very robust in terms of confidence that can be placed in them. The projected changes are small (less than 5% max change by 2080-2100 even under RCP8.5), and they are only tied to impacts in a very hand-waving sort of way. (James Christian, Government of Canada)	The authorship begs to differ. Clouds represent an important part of the ocean atmosphere systems. While the changes may be small and uncertain, there is a need to explore these potential changes for the sake of being complete. Changes in cloud cover has important implications for the biosphere, including the productivity of the spring bloom systems or the extent to which warm water impacts Reef building coral communities. These are not handwaving connections. We have been careful to discuss its limitations, however.
36	47329	30	0	0	0	0	The hypothesis that changes in land-sea temperature gradients will affect upwelling-favourable winds in consistent and predictable ways in coastal upwelling systems remains a hypothesis. This is stated in the Introduction (p 3 lines 24-25) and in section 30.8.1 in what I would call a cautious and responsible manner and also in Chapter 6 (p. 13 lines 5-18), but elsewhere it is implied to be a robust inference (e.g., 4/33-34, Figures 16 and 17). I can not make sense of the next sentence of the Introduction (p. 3 lines 25-28), however. Why do we expect anthropogenic climate change to cause cooling in the high-latitude spring bloom and equatorial upwelling domains? (James Christian, Government of Canada)	We accept the reviewers concerns regarding our treatment of upwelling regions and will ensure all discussion is 'cautious and responsible'. We have restrcutured and rewritten our Executive Summary (appropo page 4/33-34) and been more cautious with regard to upwelling "Understanding how winds and hence up-welling will change under climate change is not clear but should be a priority for research given the important implications (positive and negative) for ecosystems and fisheries ". The restructuring has also removed the confusing text re cooling in high-latitude spring bloom and equatorial upwelling regions.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
37	49755	30	0	0	0	0	The chapter would benefit from a frank and thorough discussion of the problems associated with using relatively short and sparse time series to form conclusions about the response of ecosystems to anthropogenic climate change. The rampant use of the qualifying phase "consistent with..." is unsettling, but I understand the difficulty involved in attribution. This "consistent with..." phrase is scientific jargon for "the sign is the same as we would expect, but we don't want to go on record saying this is due to anthropogenic climate change, because we don't yet have enough evidence to support such a concrete statement." But to the public, "consistent with climate change" means "this is due to climate change." I admit that I'm guilty of this as well. Perhaps this is the opportunity to be a bit more explicit by what is meant when we scientists say "consistent with..." For example: "When we report a trend as one which is 'consistent with climate change,' we mean that the sign of the trend is as one might expect given projections of climate change, but we cannot confidently attribute this shift to anthropogenic factors because we lack the appropriate understanding of the system's dynamics. There is a large degree of natural variability and a myriad of other mechanisms (of which anthropogenic climate change is one) that may be responsible for the change observed." In my opinion, highlighting this uncertainty and the difficulty in understanding the effects of climate change given such short oceanic (and especially biological) time series is more valuable than attempting to draw conclusions from a couple decades of data. Section 30.1.2 may be the place to make a point about why scientists often use the "consistent with..." language as opposed to something more definitive. This section begins with the statement "One of the primary goals of Ch. 30 is to assess the extent to which the recent literature has successfully attributed detected changes in the world oceans to climate change." After reading the chapter, I do not get the impression that this goal was applied in all sections. Two paragraphs in Section 30.1.2 are devoted to highlighting the aliasing that may occur when short time series are used in the presence of low-frequency variability, but this issue is seldom raised in the remainder of the chapter (with some notable exceptions). Rather than imploring that the authors be more definitive when highlighting trends, I would rather they accept the fact that attribution is rarely possible with short, poorly sampled biological time series. I suggest that the "detection and attribution" language be dropped from the chapter, as it is being used in a different manner than used in WGI. Physical climate scientists have a hard enough time with detection and attribution. Do you feel that you can attribute a 19-year, poorly sampled trend to anthropogenic climate change? Probably not. Likewise, do not report a trend in a short, oceanic time series starting in 1998 and assume readers will know that a major ENSO event likely biases the data at the beginning of the time series and thus the trend. (Ryan Rykaczewski, Princeton University)	The reviewer has some important points which we have taken on board by adding statements about the limitations of marine data sets (long-term variability, shortness of time series). We have endeavoured to present problems while at the same time not veering off from our purpose, which is to provide a frank assessment of the evidence that climate change is impacting the Ocean from physical, chemical and biological point.
38	49871	30	0	0	0	0	Are the numbers in Table 30-4 supposed to correspond to those in Figure 30-1, because currently they don't. In both cases, presumably these seven sub-divisions are the seven main regional groups described in the text? Please clarify these regional subdivisions. (Timothy Carter, Finnish Environment Institute)	The following issues have been corrected: Tables 30-2 to 30-4 has to be corrected according to the numbering in Tabel 30-1 and Figure 30-1. And the "Coral Reef Provinces" in Table 30-2 to 30.4 is not defined as one of the biomes. Number 7 here should be deleted, and we should consider to implement the 4 Coral Reef provinces within the defined biomes.
39	49872	30	0	0	0	0	This is an excellent and very informative FOD. The authors are to be congratulated on a very well constructed chapter. Many of the conclusions are sure to find their way into the SPM and the Synthesis Report, in part because the oceans have been somewhat neglected in previous reports, but primarily because some of these findings are compelling and of potentially great concern. These include the rates of change detected in many regions and projected changes inferred from CMIP5 climate projections, as well as the acidification results. The rates of shift of isotherms (up to 20 km per year) are encouragingly similar to maximum shifts projected in Europe over land in some early work I was involved in during the late-1980s (simple geography combined with early transient climate projections), which estimated potential northward shifts of over 300 km per decade over the flattest terrain (eastern Europe and up through Finland). Over land, topography and water can exert significant barriers to species migration. I wonder if there are similar barriers to migration over the oceans that are worthy of comment (e.g. ocean currents, ocean depth, etc.)? (Timothy Carter, Finnish Environment Institute)	We thank the reviewer for their insightful comments and will pick up the issue of potential barriers to range extension (which is highly relevant given that marine organisms can only occupy particular shallow water environments which may be discontinuous ultimately).
40	49873	30	0	0	0	0	The biogeochemical assessment of past, ongoing and projected changes over the oceans in this chapter is excellent. Multiple stresses on ocean systems are revealed, with potentially severe implications for ecosystems and human livelihoods. What is currently weaker is a discussion of the possible human responses to address these problems, presumably through strengthened governance (e.g. national, bi-lateral and multi-lateral agreements covering different sectors) as well as climate policies of adaptation and mitigation. I was surprised not to see any mention of UNCOLS - do climate change related threats to ocean ecosystems suggest that modifications might be needed to that Convention? What other avenues are open to national and international policy makers to promote sustainable management of the oceans under a changing climate? (Timothy Carter, Finnish Environment Institute)	We agree with the reviewer and have modified the text to include mention of UNCLOS (in the section - Global frameworks for decision-making).
41	51968	30	0	0	0	0	1) Overall -- In preparing the 2nd-order draft, the chapter team should prioritize making each section of the chapter a polished, comprehensive treatment of topics considered. From these sections, the chapter team is then encouraged to maximize the utility of its findings, ensuring that they are robust, compelling, and nuanced. Themes to consider informing in constructing findings include decisionmaking under uncertainty, risks of extreme events and disasters, avoided damages, and limits to adaptation. To these ends, the chapter team has prepared an outstanding 1st-order draft. In an effort to inform further chapter development, I provide some general and specific comments below. (Katharine Mach, IPCC WGII TSU)	We will include more on decision-making and its complexities and uncertainty, recent extreme events et cetera.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
42	51969	30	0	0	0	0	2) Highlighting key findings -- In developing the 2nd-order draft, the author team should continue presenting key findings throughout the sections of the chapter, using calibrated uncertainty language to characterize its degree of certainty in these conclusions. In this way, a reader of the chapter will be able to understand how the literature reviews and syntheses in the chapter sections--the traceable accounts--support the conclusions of the chapter, especially those presented in the executive summary. Such identification of key findings from the chapter readily enables findings in the executive summary that are robust, compelling, and nuanced. (Katharine Mach, IPCC WGII TSU)	We have implemented these suggestions.
43	51970	30	0	0	0	0	3) Usage conventions for calibrated uncertainty language -- The chapter team has adopted effective conventions for using calibrated uncertainty language and is encouraged to continue with the following approach: Where used, calibrated uncertainty language, including summary terms for evidence and agreement, levels of confidence, and likelihood terms, should be italicized. In addition to incorporating these terms directly into sentences, the author team may find it particularly effective to present them parenthetically at the end of sentences or clauses. Casual usage of the reserved uncertainty terms should be avoided, as has been flagged in some specific comments throughout the chapter. (Katharine Mach, IPCC WGII TSU)	We have implemented these suggestions.
44	51971	30	0	0	0	0	4) Specificity of described observations and projections -- The author team is very much encouraged to continue presenting observed and projected impacts and trends with the high level of specificity and conciseness already employed: indicating relevant time periods, geographic areas, etc. for observations; indicating relevant time frames, scenarios for climate change or socioeconomic development, geographic regions, or other assumptions for projections; and characterizing key driving factors where ranges of outcomes are presented. (Katharine Mach, IPCC WGII TSU)	Will do - we have done this to some extent we need to have a look and see if we can do it even further.
45	51972	30	0	0	0	0	5) Conditional constructions -- The chapter team has also done a nice job of using conditional constructions that explicitly separate a given physical change from its corresponding conditional impact. The chapter team is encouraged to continue using such constructions, also separately characterizing the degree of certainty for the physical change and conditional impact where appropriate. (Katharine Mach, IPCC WGII TSU)	We have implemented these suggestions.
46	51973	30	0	0	0	0	6) Figures -- The author team has developed a superb series of figures that complement assessment in the chapter text. The chapter team is encouraged to continue prioritizing figure development in subsequent revisions of the chapter. (Katharine Mach, IPCC WGII TSU)	We have implemented these suggestions.
47	51974	30	0	0	0	0	7) Coordination across the Working Group 2 contribution -- In developing the next draft of the chapter, the author team should consider treatment of topics not only in this chapter, but also across the report as a whole. For each topic, the chapter team should ensure that treatment here is reduced to the essence of what is relevant to the chapter, with cross-references made to other chapters as appropriate, also minimizing overlap in this way. In particular, the author team should continue to coordinate with chapter 6 (as well as chapters 5, 28, etc.) to ensure harmonized assessment, with logical handoffs between sectoral and regional treatments, with consistency in findings presented, and with overlap reduced wherever possible. (Katharine Mach, IPCC WGII TSU)	We have implemented these suggestions.
48	51975	30	0	0	0	0	8) Harmonization with the Working Group 1 contribution to the AR5 -- At this stage of chapter drafting, the author team should carefully consider the working group 1 contribution. Wherever climate, climate change, climate variability, and extreme events are discussed, the chapter team should ensure that their treatment is harmonized with the assessment findings of working group 1. (Katharine Mach, IPCC WGII TSU)	We have improved cross-referencing throughout the manuscript.
49	52144	30	0	0	0	0	“Key risks and vulnerabilities have been well defined (Shelley Bhattacharya, Visva Bharati University)	We are grateful to the reviewer for these comments and have implemented them.
50	54587	30	0	0	0	0	GENERAL COMMENTS: I would like to thank the authors for a very interesting and enjoyable FOD. When considering the expert review comments received on your chapter and the next round of revisions, I suggest several overall priorities. (1) Keep in mind that the preparation of the SOD is the time to ensure that each section of the chapter presents a comprehensive treatment of relevant literature, and that the Executive Summary presents findings that capture the key insights that arise from the chapter assessment. (2) This is also the time to focus on distilling the chapter text, not just fine-tuning wording but editing with a critical eye to improving quality by making discussions succinct and synthetic, while still being comprehensive. (3) Cross-chapter coordination is also important at this stage, as it should now be possible to identify topics that overlap with other chapters and to coordinate with other chapter teams to minimize that overlap. (4) Cross-Working Group coordination is important as well, and relevant chapter sections should cross-reference chapters from the other Working Groups, particularly in the case of statements about changes in mean or extreme climate conditions that are assessed in the contribution of Working Group I. (Michael Mastrandrea, IPCC WGII TSU)	We are grateful to the reviewer for these comments and have implemented them.
51	54588	30	0	0	0	0	EXECUTIVE SUMMARY: Thank you for developing a very nice draft of the Executive Summary for the FOD. For the SOD, I would recommend considering ways to modify the existing paragraphs to present an assessment finding in bold with calibrated uncertainty language, followed by additional nonbold sentences providing further explanation and context, as well as line of sight (a draft of which you already have provided) to supporting chapter sections where the traceable account appears. (Michael Mastrandrea, IPCC WGII TSU)	We have implemented the suggestions and have totally rewritten the executive summary based on these and other suggestions.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
52	54589	30	0	0	0	0	TRACEABLE ACCOUNTS: The author team has made a very good start to providing traceable accounts for assessment findings and highlighting the location of those traceable accounts in the Executive Summary. In general, I would recommend the author team continue to strengthen the linkage between support in the chapter text and assessment findings in the Executive Summary. In this context, I suggest providing some explanation of the calibrated uncertainty language used in the Executive Summary in the corresponding chapter section(s) where the traceable account appears for each finding, for cases where this is not done already. For example, in situations where confidence is not high, it would be useful to understand why the author team has made this judgment (e.g., is there a lack of robust evidence?, are there multiple perspectives in the literature?). In situations where confidence is high or likelihood language is employed, what is the evidence that forms the basis for these assignments? Succinct descriptions in the chapter text of this type will both highlight the basis for ES findings and help explain the author team's assessment of the literature. We in the TSU are available to discuss these issues as well if that would be of use. (Michael Mastrandrea, IPCC WGII TSU)	We have rewritten the executive summary along these lines, and have used the highlighted statements to point back at discussions throughout the manuscript along the lines with what is required from the point of view of traceable accounts.
53	54905	30	0	0	0	0	The author team should update the reference list and remove citation inconsistencies between in text citations and full citations given in the reference list. Please see supplementary document named WG2AR5-Chap30_Reference Checks.pdf at https://ipcc-wg2.gov/AR5/author/FOD/SuppMat (Monalisa Chatterjee, IPCC WGII TSU)	We have corrected the inconsistencies between text citations in full references.
54	47063	30	1	12	0	0	My authority to participate in the review is because I am a PICES employee, not because I am from Canada or because Canada has endorsed or supported my participation. (Skip McKinnell, PICES)	Noted. We thank the reviewer for his input and clarification.
55	51976	30	2	49	0	0	Executive Summary -- In subsequent work on the executive summary, there are several aspects of development for the author team to consider further: 1st, it would be preferable to present the paragraphs of the executive summary with a key finding in bold text followed by explanatory non-bold text. 2nd, where calibrated uncertainty language is used to characterize the author team's degree of certainty in these conclusions, the calibrated terms (including summary terms for evidence and agreement and likelihood terms) should be italicized. (Katharine Mach, IPCC WGII TSU)	Some good points. We have rewritten the executive summary with these comments and others in mind.
56	38679	30	2	49	4	46	The Executive summary needs to be simplified with the key points laid out as in earlier chapters, most usefully cascading down the levels of certainty/confidence. Too much detail provided at present for the reader to follow the main points. (Janice Lough, Australian Institute of Marine Science)	Executive summary has been rewritten with these and other comments in mind.
57	45581	30	2	49	4	46	The executive summary extensively addresses physical and chemical changes of the ocean, but falls short in outlining the implications of biological impacts for humans in a regional context. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have completely rewritten the executive summary to correct this shortcoming.
58	49248	30	2	51	2	51	Please consider to make the first sentence boldfaced. It is common practice in the IPCC reports that the most important and general key findings are boldfaced. (Oyvind Christophersen, Climate and Pollution Agency)	We have implemented this in the rewritten executive summary.
59	43241	30	2	51	2	54	Following the characteristics of a regional chapter the division into ocean regions should follow political or geographical criteria more or rather than criteria building on sectoral (ecosystem) aspects. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We respectfully do not agree with this perspective. We explored the various ways we could divide the ocean up and the only way you could do this within a single chapter was to have multi national 'ocean systems'. Any other way is confusing and would not achieve the regional aspect of this chapter which is so important. Note, this is not about nations, it is about regions within the global ocean. subsequently, there is a nice tie-in to chapter 6 which is looking at ocean systems but not a regional scale. Broad agreement was established in BA.
60	41257	30	2	53	3	1	It is easy to understand the location of the regions if the order of the region in the sentence is the same as one of Figure 30-1 and Table 30-1. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We will implement this helpful suggestion.
61	41258	30	2	54	0	0	The word "Western Boundary Systems" may mislead the meaning since "Eastern Boundary Currents" is used. The authors defined the "WBS" as bordered by equatorial regions and high-latitude spring bloom systems. However, for example, the Gulf Stream itself (the East coast of US) is not included. It may suitable to use a term like "Western Boundary Shallow Sea". (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	The reviewer has a good point - when one looks at the distribution of Western boundary systems, they are hardly Western. This has been changed to "Coastal boundary systems" or CBS. We have also included the Gulf stream within the CBS.
62	46756	30	3	1	3	4	"Seas, the Deep Sea…regional scales." Cite either here or elsewhere in the chapter: Glecker et al. 2012. Human-induced global ocean warming on multidecadal timescales. Nature climate change, online: 1-6. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	We have added Gleckler et al. 2012
63	46586	30	3	1	4	46	This Executive Statement is a very different stru cture and style from others I have read which have usually been in the for Statement of finding in bold (confidence level in bold) Summary of supporting evidence. While the bolding is immaterial at this stage, the structure is important since it clarifies the headline finding and each paragraph is an assessment summary. (Neville Smith, Bureau of Meteorology)	We have completely rewritten the executive summary to take on board these comments and others.
64	38680	30	3	3	3	4	rather than "other long-term cycles" would suggest "sources of inter-annual and decadal climate variability such as ENSO etc". ENSO events are not long-term cycles. (Janice Lough, Australian Institute of Marine Science)	The reviewer has a very good point here. We have implemented this suggestion. Reference to cycles has been removed - the term variability has been substituted.
65	38681	30	3	4	3	6	Suggest simplify to just significant warming 1950-2009 in all bar one of the ocean regions examined. That warming in the semi-enclosed seas is higher since 1982 may also be true for the ocean regions in Table 30-2. (Janice Lough, Australian Institute of Marine Science)	We have implemented this suggestion.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
66	46587	30	3	4	3	6	It is strange to be citing warming numbers for semi-enclosed seas which seem are drawn from a source that is not expert in SST and that is at odds with the dataset that isused everywhere else in this Chaper (HadISST, the basis for Fig 30-3A). No explanation is provided why all these basins would have cooled considerably in the period 1950-1982. Note that a change that has persisted over 60 years in T would fall within the definition of climate change, and include any contribution from natural long-term variability (Neville Smith, Bureau of Meteorology)	We have implemented this suggestion.
67	47330	30	3	4	3	6	"warming examined over 60 years is lower" how can this be? numbers cited previously are absolute changes not rates (James Christian, Government of Canada)	We have deleted the sentence: The average rate of warming examined over 60 years is lower [Table 30.2]. This sentence was badly written and confusing.
68	51977	30	3	6	3	6	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	We have implemented this suggestion.
69	49756	30	3	9	3	10	This assumes that marine organisms are responding directly to SST. Alternate hypotheses might be that depth distribution shifts or that SST is not as important as other habitat properties. A change in the SST isopleth should not be assumed to indicate an equivalent change in habitat, especially in the marine environment where vertical temperature gradients are often several orders of magnitude steeper than horizontal temperature gradients. (Ryan Rykaczewski, Princeton University)	We have implemented this suggestion.
70	43243	30	3	9	3	15	...an example for a nice sectoral treatment of WG I material integrated with WGII but not WGIII aspects. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We thank the review of this suggestion and have now added the caveat: "although it is important to note that temperature is not the only variable driving on the distribution and abundance of marine organisms."
71	51978	30	3	10	3	10	Where the author team here says "either move or adapt evolutionarily," is there also a 3rd option of coping, via physiological tolerance or phenotypic plasticity? (Katharine Mach, IPCC WGII TSU)	We have looked at the suggestion. Phenotypic change is unlikely to extend species ranges for a number of reasons.
72	51979	30	3	14	3	14	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	We have clarified uses calibrated language throughout the manuscript.
73	38560	30	3	14	3	15	It will get too hot for some coral systems, but not all of them (including newly invaded areas at higher latitudes). (Christopher Harley, University of British Columbia)	We thank the reviewer for their comment. Wost of the evidence suggest that coral ecosystems will not survive these changes in sea temperature although individual corals may. An important distinction is the fact that a few corals make it to higher latitudes but there is very little evidence to suggest that they will build the calcareous frameworks that typified coral reef ecosystems. That is, our assertion here is correct in that coral reef ecosystems are unlikely to migrate. We have added a sentence to make this important distinction clearer (note - executive summary has been rewritten).
74	43242	30	3	14	3	15	This statement has high confidence for present coral ecosystems and should be specified as such. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Executive summary has been rewritten and these problems have been solved.
75	38682	30	3	17	3	17	Here and in relevant portions of the chapter the recent review paper is relevant: Sen Gupta A and B McNeil (2012) Variability and change in the ocean. Chapter 6 in The Future of the World's Climate, Elsevier, doi: 10.1016/B978-0-12-386917-3.00006-3 (Janice Lough, Australian Institute of Marine Science)	Executive summary has been rewritten and these problems have been solved.
76	49249	30	3	17	3	19	Please boldface the two first sentences (Oyvind Christophersen, Climate and Pollution Agency)	Executive summary has been rewritten and these problems have been solved.
77	43244	30	3	17	3	37	...an example for a nice sectoral treatment of WG I material integrated with WGII but not WGIII aspects. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Executive summary has been rewritten and these problems have been solved.
78	38818	30	3	24	0	29	I aggree with the idea that upwelling are not changing in a unidirectional way. To the contrary asa authors reflect in their paragraph could change in diferent forms. By the same processes I diseagree with the idea of increasing primary production due to intensification; I my opinion and there are several data the increase depends on timing and location of events, as well as with their intensity. I suggest to reflect this aspect. SOme references cited as comments to other chapters could be Llope, M., Anadón, R., Viesca, L., Quevedo, M., González-Quirós, R., Stenseth, N.C. 2006 Hydrography of the Southern Bay of Biscay shelf break region: integrating the multi-scale physical variability over the period 1993-2003. J. Geophys. Res. 111, C0921 (doi:10.1029/2005JC002963). F I Z F. PÉ REZ*, XOSE A . PADÍN, YOLANDA PAZOS, MIGUEL GILCOTO, MANUEL CABANAS, PAULA C. PARDO, Maria DOLORES DOVALand LUIS FARINA-BUSTO. (2010) Plankton response to weakening of the Iberian coastal upwelling. Global Change Biology 16, 1258–1267, doi: 10.1111/j.1365-2486.2009.02125.x ALISON C . ILES, TARIK C. GOUHIER, BRUCE A. MENGE, JULIA S . STEWART, ALISON J . HAUPT and MARGARET C. LYNCH. 2012. Climate-driven trends and ecological implications of event-scale upwelling in the California Current System. Global Change Biology (2012) 18, 783–796, doi: 10.1111/j.1365-2486.2011.02567.x] (Ricardo Anadon, University of Oviedo)	Most of these references are part of the HLSB systems of 30.5 and have been dealt with there.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
79	35990	30	3	24	3	25	Shifts in predominant wind patterns should both strengthen and weaken upwelling/downwelling depending on positions (and intensity) of high and low pressure systems. Present wording suggests only intensification of upwelling is anticipated whereas reduced upwelling and intensified winter downwelling may also occur in some regions (e.g. Polovina, JJ, Dunne, JP, Woodworth, PA and Howell, EA, 2011. Projected expansion of the subtropical biome and contraction of the temperate and equatorial upwelling biomes in the North Pacific under global warming. ICES Journal of Marine Science; doi:10.1093/icesjms/fsq198). (Frank Whitney, Institute of Ocean Sciences)	Executive summary has been rewritten and these problems have been solved - but this comment has now been incorporated into 30.5.4 (equatorial upwelling systems).
80	49250	30	3	31	3	37	Please consider to make these two sentences into a stand-alone paragraph, and boldface the first of them. Rationale the other sentences are linked to physical parameters and salinity, while these two are linked to pH and oxygen levels and should be separated. (Oyvind Christophersen, Climate and Pollution Agency)	Have rewritten executive summary.
81	51980	30	3	32	3	34	Across scenarios of climate change, it would be interesting to indicate when this level of change would be reached, as can be supported by the chapter assessment. For example, is a statement of the following form a possibility?: "It is virtually certain that pH and carbonate ion saturation states, between 20XX and 20YY, will decrease below those seen in the last several million years, based on projections for modest to high increases in atmospheric CO2." (Katharine Mach, IPCC WGII TSU)	Have rewritten executive summary - and have incorporated this comment.
82	35991	30	3	35	0	0	differs between locations... (Frank Whitney, Institute of Ocean Sciences)	Have rewritten executive summary.
83	35992	30	3	36	0	0	...and is influenced by non-climate factors... (Frank Whitney, Institute of Ocean Sciences)	Have rewritten executive summary.
84	51981	30	3	36	3	36	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	Have rewritten executive summary - we have incorporated this idea generally throughout the manuscript..
85	49251	30	3	39	3	41	Please boldface the two first sentences (Oyvind Christophersen, Climate and Pollution Agency)	Have rewritten executive summary with this and other suggestions in mind.
86	36295	30	3	39	3	48	On the observed northward shifts of zooplankton, many cases are likely to be caused by advection associated with change in oceanic currents/circulation rather than merely overall temperature increase, e.g. in Alaskan Gyre, (Batten & Freeland, 2007), Californica Coast (Mackas et al., 2004), Kuroshio-Oyashio Transition (Chiba et al. 2009). In the well-known case of the North Atlantic, too, obesrved northward shift of warm water copepods (Beaugrand et al. 2002) was associated with change in the intensification of counterclockwise ocean circulation, thus opposite influence, increase in southwardshift of coldwater copepods, in the western NA (see Chapter 6.3.2.1.1) was actually observed. This description here may have a potential of misleading understanding on influence of warming for the readers. Reference: Batten, S.D., Freeland, H.J., 2007. Plankton populations at the bifurcation of the North Pacific Current. Fisheries Oceanography 16, 536-546. Beaugrand, G., Reid, P.C., Ibanez, F., Lindley, J.A., Edwards, M., 2002. Reorganization of North Atlantic Marine Copepod Biodiversity and Climate. SCIENCE 296, 1692-1694. Chiba, S., Sugisaki, H., Nonaka, M., Saino, T., 2009. Geographical shift of zooplankton communities and decadal dynamics of the Kuroshio-Oyashio currents in the western North Pacific. Global Change Biology 15, 1846-1858. Mackas, D.L., Peterson, W.T., Zamon, J.E., 2004. Comparisons of interannual biomass anomalies of zooplankton communities along the continental margins of British Columbia and Oregon. Deep-Sea Research II 51, 875-896. (Sanae Chiba, JAMSTEC)	We respectfully disagree and cannot see convincing evidence within the published literature that the northward shift in the Atlantic is associated with increased volume fluxes on a multidecadal scale.
87	43245	30	3	39	3	48	...an example for a nice sectoral treatment of WG II material. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We thank the reviewer for his kind comments.
88	51982	30	3	46	3	47	As possible, it would be helpful to indicate the approximate time frames (for example, in terms of decades... "Since 19XX") for these observed changes, as well as the relevant geographic area (across most open ocean areas?). (Katharine Mach, IPCC WGII TSU)	Have rewritten executive summary with this and other suggestions in mind.
89	51983	30	3	50	3	50	The phrase "robust evidence," as calibrated uncertainty language, should be italicized. Additionally, a summary term for agreement could potentially be assigned as well. (Katharine Mach, IPCC WGII TSU)	Have rewritten executive summary - we have incorporated this idea generally throughout the manuscript..
90	49252	30	3	50	3	52	Please boldface the two first sentences (Oyvind Christophersen, Climate and Pollution Agency)	Have rewritten executive summary with this and other suggestions in mind.
91	43246	30	3	50	4	8	...an example for a nice sectoral treatment of WG II material not integrated with WGIII aspects. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Have rewritten executive summary with this and other suggestions in mind.
92	51984	30	3	51	3	51	It would be preferable to indicate slightly more specifically the nature of the strong impacts described here, to provide an overarching "topic sentence" for the more detailed explanation that follows. (Katharine Mach, IPCC WGII TSU)	We have implemented this suggestion in a rewritten executive summary.
93	51985	30	4	5	4	26	Likelihood terms on these lines, if used per the uncertainties guidance for authors (reflecting a probabilistic basis for their assignment), should be italicized; casual usage should be avoided. The following instances are relevant: "virtually certain" on line 5, and "likely" on lines 17, 19, 34, 26. (Katharine Mach, IPCC WGII TSU)	We have implemented this suggestion in a rewritten executive summary.
94	49253	30	4	10	4	11	Please boldface the first sentence (Oyvind Christophersen, Climate and Pollution Agency)	We have implemented this suggestion in a rewritten executive summary.
95	38819	30	4	13	0	0	The phrase refers to the expansion of subtropical gyre, but probably the circulation has not changed and the subtropical gyre not expand, as suggested by Longhurst (Ecological geography of the sea, 2nd ed. 2007). This author discuss the asignation of the expansion of the oligotrophic low chlorophyll areas descrived by Polovina as increasing subtropical gyre in in any of the ways that can be described, oceanic surface height or otres. I suggest that this idea be qualified in a more general point of view, i.e. only expansion of low chlorophyll areas. (Ricardo Anadon, University of Oviedo)	Have rewritten executive summary with this and other suggestions in mind.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
96	38561	30	4	16	4	18	Does this prediction allow for adaptation or migration? (Christopher Harley, University of British Columbia)	It does. The range migration of corals faces several hurdles including decreasing light levels at higher latitudes, increasing acidity and reduce the carbonate ion concentrations, as well as the problems of competing with already existing organisms. I will check to see that this has been stated within later sections discussing these challenges. One of a clear issues here is that evidence of a coral species arriving at a higher latitudes is not evidence that coral Reef ecosystems with all of their goods and services can migrate to higher latitudes. Again, need to make sure that this distinction has been brought out in the current draft.
97	51986	30	4	16	4	18	Is it possible to indicate the approximate time frame for such "elimination"? Perhaps across climate/socio-economic scenarios? (Katharine Mach, IPCC WGII TSU)	Have rewritten executive summary with this and other suggestions in mind.
98	47331	30	4	22	4	22	"although" makes no sense here (James Christian, Government of Canada)	Have rewritten executive summary with this and other suggestions in mind.
99	49254	30	4	22	4	25	Please boldface the two first sentences (Oyvind Christophersen, Climate and Pollution Agency)	Have rewritten executive summary with this and other suggestions in mind.
100	51987	30	4	25	4	27	For the statement, the author team might consider separately indicating its degree of certainty in the 2 parts of this statement: impacts on ecosystems and then consequences for goods and services. (Katharine Mach, IPCC WGII TSU)	Have rewritten executive summary with this and other suggestions in mind.
101	35993	30	4	27	4	29	Twice rapid warming is stated. This needs to be qualified (are water bodies warming at twice the rate of global warming for example, or at the same rate as Arctic tundra?) (Frank Whitney, Institute of Ocean Sciences)	Have rewritten executive summary with this and other suggestions in mind.
102	51988	30	4	29	4	29	For the rapid increases in sea temperature described here, it would be informative for the reader to indicate their approximate magnitude (for given climate/socio-economic scenarios). (Katharine Mach, IPCC WGII TSU)	We have implemented this suggestion in a new version of the executive summary.
103	35994	30	4	33	4	34	In some regions, an increased nutrient supply through intensified upwelling threatens,,, (Frank Whitney, Institute of Ocean Sciences)	We agree. Text now reads: "In some regions, an increased nutrient supply through intensified upwelling threatens to the upper ocean parts of the ocean (through intensified upwelling) threatens deep sea ecosystems by increasing the rate of metabolism (and hence oxygen use)."
104	35995	30	4	37	4	38	I am not comfortable with the statement "deep-sea ecosystems will experience stress". Is declining productivity stress or just a control on growth rate and reproduction? Loss of habitat due to hypoxia is stress. (Frank Whitney, Institute of Ocean Sciences)	The sentence has been moved to the deep sea section - section 30.5.7 - and changed to: "Since the upper ocean is currently experiencing warming, increased stratification, and changing productivity (WGI Ch3; see other sections of 30.5), there is evidence (medium confidence) of impacts on quite different time scales between the slow warming at depth and the more rapidly changing (decreasing/increasing) food supply to deep-sea animals that falls from above."
105	49758	30	4	37	4	39	The significance of this mismatch between rates of temperature and organic matter change is not at all clear. It comes across as if a match in the rates of change would not be as stressful top the ecosystem, and I don't believe that is the meaning intended. (Ryan Rykaczewski, Princeton University)	Have rewritten executive summary with this and other suggestions in mind.
106	49255	30	4	41	4	42	Please boldface the first sentence (Oyvind Christophersen, Climate and Pollution Agency)	Done
107	43247	30	4	41	4	46	This paragraph lacks specificity and uncertainty language, especially in light of a very recent paper not available to the authors that illustrates the capacity of terrestrial and ocean systems to respond to increasing CO2 levels still remaining unchanged (Nature 488, 70 (2012)). (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Have rewritten executive summary with this and other suggestions in mind.
108	35996	30	4	45	0	0	briefly note (in brackets) what is meant by regulatory services (Frank Whitney, Institute of Ocean Sciences)	Have rewritten executive summary with this and other suggestions in mind.
109	35997	30	4	51	0	0	"dominate Earth" perhaps "exert a dominant influence on Earth climate" ? (Frank Whitney, Institute of Ocean Sciences)	We agree and have changed the first sentence to: "The Ocean exerts a profound influence on Earth, interacting with its atmosphere, cryosphere, land and biosphere to determine planetary conditions. "
110	46588	30	4	52	4	53	I was not sure how the oceans directly provide "economic and social security". "social security" is used elsewhere in a way that was not familiar to me. (Neville Smith, Bureau of Meteorology)	Agreed. Sentence has been reworked.
111	41259	30	4	52	4	54	Another important role of the ocean is "water resorvoir" and regulate water circulation. Please add it to the sentence. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	The reviewer has good point. We have now added this term.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
112	45584	30	5	0	0	0	Figure 30-1: Please explain in the figure legend what the numbers in circles placed on the map refer to. This figure is partly redundant to Figure 30-16 (p. 42). (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	The encircled numbers are indicating where particular ocean regions are located and they match up with table 30.1. I think that is pretty clear to everyone we have talked to since receiving this comment. The figure 30.1 is fundamentally different to that shown in 30.16. In one case, we are defining the ocean regions that lie at the heart of chapter 30. In the other we are trying to summarise some of the impacts that are arising from ocean warming and acidification.
113	51989	30	5	1	5	1	For accuracy, the author team might consider broadening the phrase "atmospheric greenhouse gases" to encompass other anthropogenic alterations that affect radiative forcing. (Katharine Mach, IPCC WGII TSU)	Good point - we have amended. Note that we have also included a sentence to inform the reader that we are also dealing with ocean acidification as a separate unit important consequence of changes to atmospheric carbon dioxide.
114	47064	30	5	3	0	0	“attributed to climate change” Isn't the major policy debate about the anthropogenic (Skip McKinnell, PICES)	Good point. We have added the term anthropogenic.
115	47065	30	5	4	0	0	In the Introduction, and to be true to the name of the chapter, I suggest adding a very clear statement of the authors' definition of open ocean. My innate interpretation is “beyond the continental shelf margins.” (Skip McKinnell, PICES)	After discussions with the TSU - it is clear that our chapter represents the ocean in its entirety - much like the Australasian chapter includes the entire set of resources both land and sea of Australia and New Zealand. This has been adopted after deep discussions and meetings with the TSU and Chris Field.
116	43248	30	5	8	0	0	It should also be said what role would chapter 30 have different to that of chapter 6? (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	This issue has been discussed and resolved in a meeting with Chris Field and the CLAs of chapter 6 and chapter 30.
117	36296	30	5	11	5	33	Presentation of the Major Regions is bit confusing. They set 7 Regions including deep-sea but no information of deep-sea but coral-reef in stead in Table 30-2. The order and number of each Region in Figure 30-1/Table 30-1 differs from that in Table 30-2/30-4. Also, add each region name in the legend of Figure 30-1. (Sanae Chiba, JAMSTEC)	We have rectified these problems in the latest version.
118	38683	30	5	11	5	33	Need to define "primary productivity"; add key to Figure 30-1; Table 30-1 needs further explanation in the caption as to what the Area, Primary Productivity and Fisheries Productivity "%'s" refer to. (Janice Lough, Australian Institute of Marine Science)	We have rectified this. The primary prodcutivity have been calculated from Field et al. (1998) and Fish productivity should be changed to 'Fish Catch'
119	46589	30	5	13	5	17	Not sure any of this is needed. Delete. (Neville Smith, Bureau of Meteorology)	We respectfully disagree. It is important for us to define how we have divided up the ocean into regions as this is the first time this has been done within working group to the IPCC. We will look at the language and explore whether we can make this raison d'être clearer.
120	35998	30	5	16	5	17	delete "especially within the space allocated here." (Frank Whitney, Institute of Ocean Sciences)	Again, we respectfully disagree. We need to remind the reader that this is an enormous and complex topic which requires justification.
121	46590	30	5	22	5	30	not sure why the Gulf Stream to Cape Hatteras has been excluded from the WBS region 4. Region 2 (upwelling)also seems to contain much ocean that is part of convergence zones (eq downwelling). (Neville Smith, Bureau of Meteorology)	We have corrected this in the latest version of 30-1
122	37266	30	5	32	0	0	Table 30-1 Clarification needed Do the primary and fishery production numbers include the contributions from polar regsion? I note that the sum of the primary production numbers comes to 94%, while that fro the fishery production comes to 101%! (Erica Head, Fisheries and Oceans Canada)	We included the polar regions for completeness. We say in the text that we have excluded them beyond this point because they are dealt with in the polar chapters.
123	49757	30	5	33	5	33	Consider augmenting the caption of Table 30-1 to note that "fisheries productivity" is based on landings, not only by the actual productivity of the fish populations. The portion of fisheries landings in each region is likely influenced by fishing effort and distance from shore (as well as by the productivity of the fish populations). Perhaps just change "fishery productivities" to "fisheries landings" or to "fishery productivities (based on commercial landings)." (Ryan Rykaczewski, Princeton University)	We have changed the terminology to "fish landings".
124	46591	30	5	36	0	0	Title seems to miss mark. "Challenges to the Detection of Changes in the Open Ocean and Attribution to Climate Change" perhaps. As Chapter 18 notes, detection is about determining whether a system is changing outside its normal behaviour - for climate change we use the term persistant. Having detected the change, we might then wish to know whether it can in part or wholly be attributed to climate change, and further, in part or wholly to anthropogenic climate change. (Neville Smith, Bureau of Meteorology)	We have rewritten section to take on these and other comments. We thank the reviewer for this important point. The title now reads: "30.1.2 Detection and attribution of climate change with the Ocean"
125	52086	30	5	40	5	42	For the introduction here of attribution of impacts to climate change, the author team might consider cross-referencing the glossary for the report, in addition to the sources provided at the end of the paragraph. (Katharine Mach, IPCC WGII TSU)	We will be linking closely to the glossary and to sections in WG1 and chapter 6 (WGII).
126	46592	30	5	40	5	45	I far prefer the explanation in Chp 18.2.1.1. (Neville Smith, Bureau of Meteorology)	We Agree and have reduced the text and made explicit links to 18.2.1.1.
127	46593	30	5	47	6	2	Here and in other Chapters the terminology is a challenge. Climate change is well defined, as is climate, 'non-climate', 'non-climate change', etc are. In this case the terms could be omitted altogether without loss of message. Note model simulations rather than projections are used for attributions (Neville Smith, Bureau of Meteorology)	We have taken on these comments and have shortened the sentence considerably. We have also provided stronger linkages to chapter 18.
128	45585	30	6	0	0	0	Figure 30-2: Please define all abbreviations (AMO, ENSO,...) in the figure legend. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	The comment is a good one but we have decided to drop this figure and to refer to a similar figure included as part of chapter 6.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
129	49759	30	6	4	6	4	In physical climate science, "fingerprint" is typically used in a specific fashion that is very different than that used here. A fingerprint is a temporal and spatial mode of variability that specifically allows one to distinguish the global warming signal from other types of variability... and so the use here is inappropriate. The assumption that such a signal is unique from other modes of variability is what makes it a fingerprint. I suggest changing this language; the sentence is just as valid without using this jargon. (Ryan Rykaczewski, Princeton University)	We have rewritten the sentence to take on this observation - and will avoid the term 'fingerprint'
130	46594	30	6	4	6	21	As written I am not sure this paragraph adds a lot. Certainly the same messages are repeated several times later. The simple message is that natural variability in space and time can confound our ability to detect or attribute a particular change. Not sure Fig 3-2 adds a lot of value in a space-challenged chapter like this one. (Neville Smith, Bureau of Meteorology)	We have removed this paragraph.
131	36000	30	6	9	0	0	I think this might be a good spot to note that the interior waters of the subarctic Pacific are warming at rates of ~0.1 C/decade over the past 50 years (26.8 to 27.0 isopycnal surfaces at least, 150 to 400 m depth range at Ocean Station P; 9. Nakanowatari, T., Ohshima, K. I. & Wakatsuchi M. Warming and oxygen decrease of intermediate water in the northwestern North Pacific, originating from the Sea of Okhotsk, 1955–2004. Geophys. Res. Lett. 34, L04602 (2007); Whitney, F. A., Freeland, H. J. & Robert, M. Persistently declining oxygen levels in the interior waters of the eastern subarctic Pacific. Prog. Oceanogr. 75, 179–199 (2007)). This is important to note because it coincides with the region of maximum oxygen loss in these waters and helps explain the cause (reduced dense water formation in Okhotsk Sea) (Frank Whitney, Institute of Ocean Sciences)	We have removed this paragraph.
132	51990	30	6	12	6	13	As appropriate, it may be beneficial to more specifically cite the findings from this chapter here. (Katharine Mach, IPCC WGII TSU)	We have removed this paragraph.
133	38684	30	6	23	6	26	I did not find Figure 30-2 very informative; it lacks a key for the acronyms; it is unclear to me how "Shelf seas" and "sub-grid scales" are "climate-related phenomena". (Janice Lough, Australian Institute of Marine Science)	We have dropped this figure from chapter 30 and referred to an earlier figure in the chapter 6
134	47068	30	6	24	0	0	You've added the PDO to the figure without adjusting the caption. How did you determine that the PDO has a smaller spatial scale than the AMO? (Skip McKinnell, PICES)	We have dropped this figure from chapter 30 and referred to an earlier figure in the chapter 7
135	36515	30	6	29	0	0	Just because the information on ocean impacts was spread over several chapters in AR4 seems an inadequate reason for ignoring AR4. As things stand, information on ocean impacts will be spread over several chapters in AR5 too. A lot of the material in AR5 is already covered in AR4 and this section does an inadequate job of summarising it. Why not produce a table that points to all the ocean impacts material in AR4 and AR5, so that people can locate it more easily? It would also help with reducing duplication and would allow for a proper update, so that people can see what we have learned since AR4. Have the polar regions been located somewhere else in AR5? There is a fairly large section on the ocean impacts inthe Arctic in chapter 15 of AR4 (Keith Brander, DTU)	The polar oceans are included (as explained in the text) in the polar chapter. CH30 integrates and pulls together a large amount of information on the oceans as a region. This is unique and important contribution of CH30. we have gone through AR4 and provided much more comprehensive linkages to between AR4 and AR5.
136	51991	30	6	36	6	41	It would be preferable to cite the specific relevant chapter for these findings. (Katharine Mach, IPCC WGII TSU)	We have done this.
137	51992	30	6	46	6	46	The phrase "very likely," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have adopted this convention.
138	37121	30	6	50	6	52	Regarding the sentence 'The influence of rising atmospheric CO2 varies geographically, with the lowest decrease (DpH = -0.06) in the tropics and subtropics, and the highest (DpH = -0.12) at high latitudes, which is consistent with the reduced buffering capacity of the high latitudes compared to the low latitudes', I have not had the chance to read AR4, 5.4.2.3, which is the reference for this, but I guess other numbers could be given based on Feely R.A., Doney S.C., Cooley S.R. 2009. Ocean acidification: Present conditions and future changes in a high-CO2 world. Oceanography 22: 36-47, see Table 2 of this paper. It would be worth to mention that, regarding carbonate ion concentration and saturation state, latitudinal relative changes are different than absolute changes. For example, in absolute terms, carbonate ion concentration will decrease more in the tropics than in the high latitudes. However, in relative terms, these concentrations will decrease more in the high latitudes. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	This part of the report is drawing on major conclusions from previous assessments. This is not about describing the latitudinal variability in pH at this point.
139	47332	30	6	51	6	51	DpH not defined: should be Delta? (James Christian, Government of Canada)	Fixed in current version - need to make sure that the symbol is transferred in the production of the TSU version.
140	49762	30	7	27	0	0	Section 30.3. I don't understand the format of this section. Historical changes in individual factors are highlighted in the "Physical Changes" and the "Chemical Changes" sections (30.3.1 and 30.3.2). In some cases, comments are also made on the modeled, future projected changes. Then in section 30.3.3 there is a paragraph on projected SST changes. Perhaps "historical" and "future" would be more appropriate divisions than "physical, chemical, and projected SST." Also not that there are some important changes that are not directly related to SST (e.g., pH, ventilation). (Ryan Rykaczewski, Princeton University)	We have reorganised to chapter to discuss each variable in terms of past, present and future changes. This partially accommodates the reviewers concern.
141	38685	30	7	27	11	38	Need throughout this section to make clear what is evidence from recent scientific literature and what new illustrative analyses have been undertaken (e.g. Table 30-2; all or parts of Figures 30-3 to 30-6). My understanding is that the IPCC assessments "review" the scientific literature rather than undertake new analyses. If the latter is considered acceptable then Table 30-2 should also include asessments of trends for variables other than SSTs for the seven ocean regions to illustrate the observed changes. This section should also focus on the observed changes and not, at this stage, introduce discussion of observed impacts (e.g. Page 8, lines 20-23; Page 9, lines 15-17). (Janice Lough, Australian Institute of Marine Science)	Advice from the TSU is that depicting HADsst or CMIP5 datasets does not require them to be republished. These have been already published as reference datasets.
142	51993	30	7	29	7	29	Assuming "thermal expansion" is meant here where the term "expansion" is used, it might be clearest to use the full phrase. (Katharine Mach, IPCC WGII TSU)	Sentence has been reworded to make it clearer.
143	47333	30	7	30	7	30	"WG1 Ch. 3" and 6? (James Christian, Government of Canada)	We have gone through the manuscript have clarified these linkages to working group 1 and other chapters in working group 2 and 3.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
144	51994	30	7	32	7	32	It would be preferable to specify the relevant chapter from working group 1 within this parenthetical reference. Additionally, the phrase "changes were examined" could be further clarified--presumably the author team means that changes were examined by the author team of this chapter, rather than within Chapter 3 of the working group 1 contribution. (Katharine Mach, IPCC WGII TSU)	We have gone through the manuscript have clarified these linkages to working group 1 and other chapters in working group 2 and 3.
145	43250	30	7	39	12	11	The present separation of physical, chemical and biological aspects prevents the regional, synthetic focus intended for these regional chapters. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We respectfully disagree. It is very difficult to discuss these issues at a regional scale all in one. The chemical, physical and biological aspects are described on a regional basis first before they are integrated later in the chapter. This issue has been discussed the TSU who support our position.
146	46757	30	7	41	9	51	Section 30.3.1.1 needs to cite: Glecker et al. 2012. Human-induced global ocean warming on multidecadal timescales. Nature climate change, online: 1-6. (Maria Caffrey, National Park Service and University of Colorado, Boulder)	Reference added. Text now reads: "The heat added to the ocean has resulted in an increase in ocean temperature, which is largest in the upper 75 m of the ocean (> 0.1°C per decade, (WGI, CH3, Box 3.1) and which has a significant ($p < 0.01$) anthropogenic fingerprint {Gleckler et al. 2012}."
147	38820	30	7	43	0	54	At least in many areas of the surface waters of the Atlantic the rate of increase of temperature was much higher. In our recent paper for the North Atlantic and adyacent seas temperature and stratification change [González-Taboada, F. y Anadón, R. online. Patterns of change in sea surface temperature in the North Atlantic during the last three decades: beyond mean trends. Climatic Change. http://www.springerlink.com/content/n1171w7628217167/?MUD=MP] we observed a great warming in subarctic waters, and not specifically in the Mediterranean or Caribbean Seas. And is clear for us that the rate of temperature rise was higher that reflect in this chapter. I think is important due to use of data of the draft for other comments and references along all the chapter, and probably convey the feeling of change is very weak, inconsistent with those refelected in Chapter 6 (Ricardo Anadon, University of Oviedo)	We have added the reference. Text now reads: "The warming trend shows significant variability, in part due to the difficulties of sampling such vast regions over time. Tome authors have reported significantly higher rates of warming for the North Atlantic (González-Taboada and Anadon 2012)."
148	36297	30	7	54	8	1	"... warming occuring particularly around 1976/77." Which area does it mention? Temperature shift around 1976/77 was caused by AL dynamics that derive SST dipole between east and west NP, and western and central NP experienced rather cooler shift after the year. (Sanae Chiba, JAMSTEC)	We have deleted sentence.
149	38821	30	8	2	0	4	The same coment that two previous comment, related with the difference between subtropical gyre and oligotrophic regions (Ricardo Anadon, University of Oviedo)	We have replaced the term 'oligotrophic gyre' with 'subtropical gyre' through out the manuscript.
150	41753	30	8	4	8	4	Figure reference (Fig. 30-12b) is out of chronological order (Juergen Weichselgartner, University of Kiel)	Correct, "chronologically" this should be Figure 30-4
151	45586	30	8	11	8	23	Please indicate over which time period the migration of isotherms and the timing of spring conditions was calculated. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	This has been indicated - Since 1960 (Burrows et al. 2011). However, it should also be mentioned that this time period is coinciding with the shift from low to high AMO - This is listed in Burrows et al 2011 - ADD (1960 - 2009)
152	49760	30	8	13	8	14	This assumes that marine organisms are responding directly to SST. Alternate hypotheses might be that depth distribution shifts or that SST is not as important as other habitat properties. A change in the SST isopleth should not be assumed to indicate an equivalent change in habitat, especially in the marine environment where vertical temperature gradients are often several orders of magnitude steeper than horizontal temperature gradients. (Ryan Rykaczewski, Princeton University)	This subtlety has been added. Added the Caveat: "although it is important to note that other variables (e.g. Right, food, habitat) can play important roles in determining the distribution and abundance of marine organisms. "
153	51995	30	8	14	8	14	Where the author team here says "either move or adapt genetically," is there also a 3rd option of coping, via physiological tolerance or phenotypic plasticity? (Katharine Mach, IPCC WGII TSU)	Phenotypic change is unlikely to operate significantly in extending habitat range of species.
154	51996	30	8	18	8	18	The phrase "robust evidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	Agreed - this has been adopted
155	51997	30	8	19	8	20	It would be helpful to indicate the overall timeframe over which these changes have been observed. (Katharine Mach, IPCC WGII TSU)	This has been incorporated.
156	51998	30	8	20	8	20	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	Agreed - rectified.
157	37123	30	8	22	8	23	Regarding the ref. 'Poloczanska et al., 2012', I have not seen this work published yet, please check. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	This manuscript is submitted, and will be published by the IPCC deadline (end August 2013)
158	47066	30	8	27	0	0	The Introduction and Fig. 30-1 have 7 regions (Skip McKinnell, PICES)	The 8th region in figure 30-1 is the polar oceans, which are not dealt with in our treatment. We have made sure that this is clearer.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
159	47067	30	8	28	0	0	It is appropriate to represent temperature change with a linear trend IF the data have this underlying property (linearity) and it is inappropriate if they do not. Fitting a linear model to data that are not changing linearly will misrepresent the nature of the temporal change because the rate parameter in a linear model of Y vs time implies a constant rate of change over the period examined. The statistical tests of hypothesis are invalid if the data do not fit certain assumptions. I have a paper in press that shows how average summer coastal temperatures along the British Columbia coast warmed abruptly, on one occasion in late 1976. There is no statistically significant trend up to 1977 and no linear trend thereafter. All of the change occurred in 1 year. To describe this kind of warming as a rate per decade mis-represents the nature of the change. If you mis-represent the nature of the change, how can you possibly discover its cause? The paper mentioned above was trying to understand a sudden, a one time change (in 1977) in the marine growth of juvenile sockeye salmon. It is McKinnell, S.M. And Reichardt, M. In Press. Early marine growth of juvenile Fraser River sockeye salmon in relation to juvenile pink and sockeye salmon abundance. Can. J. Fish. Aquat. Sci. (Skip McKinnell, PICES)	These issues are legitimate but have been resolved in the most recent draft.
160	46595	30	8	33	8	35	Velocity? How does a colour coded plot show speed and direction? What is a seasonal trigger? (Neville Smith, Bureau of Meteorology)	Have changed seasonal trigger … sentences now read: At the same time, seasonal changes that drive natural history events are advancing by 2-5 days.decade-1.
161	46596	30	8	42	0	0	I was unable to figure out what the "highest monthly SST' for the 1985-2000 climatology was (Donner et al was easier to understand). (Neville Smith, Bureau of Meteorology)	Now reads: "The total heat stress accumulated over the period 1981-2010 was calculated using the methodology of {Donner, 2007 #28} and a reference climatology based on 1985-2000 in which the highest monthly sea surface temperature (SST) was used to define the thermal threshold, above which accumulated thermal stress was calculated as 'exposure time multiplied by stress' or Degree Heating Months (DHM). "
162	49761	30	9	3	9	4	The definition of "proportion of years with a thermal anomaly" needs to be changed. What is meant by "thermal anomaly"? As I understand it, an anomaly is a deviation from the mean… and so 100% of individual years will have a thermal anomaly (unless by some wild stroke of luck, a year matches the mean, exactly). Perhaps I am misunderstanding the caption. (Ryan Rykaczewski, Princeton University)	The description of the methodologies has been improved and clarified in the latest version of the manuscript.
163	38198	30	9	14	9	14	given the magnitude of storm surges (in m) it seems unlikely that sea level rises in mm will give significant changes in storm surge penetration. You need to look at water level records over the timescales of hundreds of years to see progressively higher surge levels on a rising sea level baseline (THOMAS SPENCER, University of Cambridge)	We have shortened and clarified this section - with linkages to WG1 and to other discussions of storm surge.
164	51999	30	9	15	9	15	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	This has been adopted.
165	37520	30	9	15	9	17	Perhaps relevant to say here that where this has occurred in the eastern Indian Ocean it has led to a beneficial effect (albeit maybe temporary) on shallow intertidal reefs - see Brown et al (2011) Increased sea level promotes coral cover on shallow reef flats in the Andaman Sea, eastern Indian Ocean Coral Reefs 30:867-878 which supports earlier predictions by Buddemeier RW and Smith SV (1988) Coral Reefs 7:51-56 (Barbara Brown, University of Newcastle)	Important point - hhave added: "While habitat may be lost, some examples of habitat expansion have been reported {Brown, 2011 #560}."
166	38562	30	9	17	9	19	Specify that this is due to tectonic uplift and other geological processes. (Christopher Harley, University of British Columbia)	We disagree with the reviewer - the variability which we refer to concerns differences in the rate of sealevel rise that arise due to long-term variability due to PDO/and so as well is Earth rotational effects.
167	47334	30	9	22	0	0	Section 30.3.1.3 I don't believe that the secular trends in either wind speed or wave height, much less their attribution to AGW, are robust inferences. Gemmrich et al 2011 GRL doi:10.1029/2011GL049518 show that some trends previously attributed to wave buoy records are spurious. If this is discussed in the WG1 report it is more likely Chapter 3 than 6 (Ch. 6 is biogeochemical cycles). (see also 28/48-50) (James Christian, Government of Canada)	We take on board the reviewer's concerns and are now more cautious with respect to attribution et cetera. We have made stronger connections to working group 1 where appropriate.
168	38686	30	9	22	9	39	Should this section on "winds and water movement" also specificalyy mention ocean circulation patterns? (Janice Lough, Australian Institute of Marine Science)	Good point. This has been adopted.
169	41754	30	9	24	9	39	This paragraph discusses changes in wind speed without outlining the changes to air-sea gas exchange. (Juergen Weichselgartner, University of Kiel)	Good point. We have included discussion of air- sea exchange.
170	38822	30	9	26	0	27	The idea about increasing upwelling intensity due to upwelling-favourable winds (Bakun, 1990) can nor be sustained for all coastal areas. In fact in some areas decreased and in many other regions changed time and frecuency with observable impacts on pelagic and benthic communities [Llope, M., Anadón, R., Viesca, L., Quevedo, M., González-Quirós, R., Stenseth, N.C. 2006 Hydrography of the Southern Bay of Biscay shelf break region: integrating the multi-scale physical variability over the period 1993-2003. J. Geophys. Res. 111, C0921 (doi:10.1029/2005JC002963). F I Z F. PÉ REZ*, XOSE A . PADÍN, YOLANDA PAZOS, MIGUEL GILCOTO, MANUEL CABANAS, PAULA C. PARDO, Maria DOLORES DOVALand LUIS FARINA-BUSTO. (2010) Plankton response to weakening of the Iberian coastal upwelling. Global Change Biology 16, 1258–1267, doi: 10.1111/j.1365-2486.2009.02125.x ALISON C . ILES, TARIK C. GOUHIER, BRUCE A. MENGE, JULIA S . STEWART, ALISON J . HAUPT and MARGARET C. LYNCH. 2012. Climate-driven trends and ecological implications of event-scale upwelling in the California Current System. Global Change Biology (2012) 18, 783–796, doi: 10.1111/j.1365-2486.2011.02567.x; Aristegui et al 2011 yet referred in the chapter] The question was that winds change in intensity but also in tempo and location and their effects on upwellings must be regional or local (Ricardo Anadon, University of Oviedo)	We refer the reader to sections 30.5.2 and 30.5.4 where these issues are discussed.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
171	36001	30	9	29	9	32	stronger winter winds can enhance downwelling in areas such as the N BC and Alaska coasts. The relaxation of downwelling in summer draws nutrient rich waters onto continental shelves, enhancing primary productivity (e.g. Whitney, F.A., W.R. Crawford and P.J. Harrison. 2005. Physical processes that enhance nutrient transport and primary productivity in the coastal and open ocean of the subarctic NE Pacific. Deep-Sea Research II. 52:681-706). No need to cite this paper, but I feel the importance of downwelling needs to be recognized since its relaxation supports rich coastal fisheries (Ware and Thomson, 2005). (Frank Whitney, Institute of Ocean Sciences)	We have now included a discussion of the impacts of wins on upwelling - also have referred readers to fail box on the Bakun hypothesis.
172	46597	30	9	33	9	34	Cai's studies were models I think; should it be "latitudes was found during ..." (Neville Smith, Bureau of Meteorology)	We agree and have modified text accordingly. "Observed" has been changed to "found".
173	46598	30	9	37	0	0	Significant wave height? Reference for this result? (Neville Smith, Bureau of Meteorology)	Have inserted reference for North Atlantic: Kushnir, Y., Cardone, V.J., Greenwood, J.G., and Cane, M.A. 1997. The Recent increase in North Atlantic wave height. Journal of Climate, 10: 2107-2113.
174	38563	30	9	37	9	38	Some of the apparent increase in wave heights may be due to changes in instrumentation. Once these biases have been corrected for, both increases and decreases in wave heights are observed in the North Pacific, depending on location. See Gemmrich, J., B. Thomas, and R. Bouchard. 2011. Observational changes and trends in northeast Pacific wave records. Geophysical Research Letters 38, L22601, doi:10.1029/2011GL049518. Projected changes in northern Europe also include both increases and decreases in wave height, depending on location and season (Zacharioudaki, A., S. Q. Pan, D. Simmonds, V. Magar, and D. E. Reeve. 2011. Future wave climate over the west-European shelf seas. Ocean Dynamics 61:807-827.) (Christopher Harley, University of British Columbia)	We have made reference to working group one on this issue.
175	52000	30	9	39	9	39	It would be helpful to clarify the relevant geographic regions for these increases. Additionally, "likely," as a calibrated uncertainty term, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have gone through the manuscript carefully and have adjusted the use of calibration language.
176	37124	30	9	46	9	46	Regarding global salinity changes, the recent reference Durack P.J., Wijffels S.E., Matear R.J. 2012. Ocean Salinities Reveal Strong Global Water Cycle Intensification During 1950 to 2000. Science 336: 455-458 should also be quoted. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	This is an important reference which we have added to the main text under 30.3.2.2
177	52001	30	9	46	9	46	It is not completely clear if this citation is the source of the entire figure or just part D. (Katharine Mach, IPCC WGII TSU)	We have clarified this caption.
178	52002	30	9	54	9	54	The phrase "high confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have adopted this convention.
179	46599	30	10	0	0	0	A lot of space ias being devoted to recounting the results of WG1 AR5 Chp 3. Perhaps this is needed but it would be good to do it with 50% less words. (Neville Smith, Bureau of Meteorology)	We have significantly rewritten section and have referred to working group 1 more substantially - allowing us to reduce the amount of text here. However, we believe that we need to properly describe the extent to which changes have occurred within the physical and chemical conditions within the ocean in order to set the appropriate basis for discussing the biological and ecological impacts.
180	41260	30	10	6	0	0	Section 30.3.1.5 does not include any fact already detected. It is better to add detected phenamena such as Lau, W. K. M., and Y. P. Zhou (2012), Observed recent trends in tropical cyclone rainfall over the North Atlantic and the North Pacific, Journal of Geophysical Research Atmospheres, 117, doi:10.1029/2011JD016510. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We have added reference to detected phenomena in section on storms.
181	52003	30	10	6	0	0	Section 30.3.1.5. The focus of this section could be clarified. Information presented seems to pertain to future-oriented projections, while the reader expects a focus on observations. (Katharine Mach, IPCC WGII TSU)	We have combined past, present and future aspects for each variable.
182	38687	30	10	6	10	12	What is meant by "storms" - need to define. (Janice Lough, Australian Institute of Marine Science)	We have added a sentence to define what we mean by storms.
183	52004	30	10	10	10	10	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	Agreed- adopted.
184	47335	30	10	15	0	0	Section 30.1.3.6 I don't understand why the subarctic Pacific is an exception. Whitney 2011 says there is "evidence that the upper ocean is becoming more stratified". He does not detect a significant trend in nutrient flux over 23 years but there are probably few if any places where such a trend has been demonstrated. If this is what is being asserted more data or literature references should be provided. (James Christian, Government of Canada)	We have qualified the sentence - see also comment 188 - changes made.
185	38688	30	10	15	10	22	See also Ganachaud et al (2011, in reference list) regarding stratification in the tropical Pacific (Janice Lough, Australian Institute of Marine Science)	Have added {Ganachaud, 2011 #374} reference.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
186	38823	30	10	17	0	22	Thermal stratification means different aspects in my opinion. First the depth at which changes were significant increasing the nutrient stress in summer, that in our waters could be 40 m (see references of Llope et al, 2006, 2007)[Llope, M., Anadón, R., Viesca, L., Quevedo, M., González-Quirós, R., Stenseth, N.C. 2006 Hydrography of the Southern Bay of Biscay shelf break region: integrating the multi-scale physical variability over the period 1993-2003. J. Geophys. Res. 111, C0921 (doi:10.1029/2005JC002963); Llope, M., Anadón, R., Sostres, J.A.,Viesca, L. (2007) Nutrients dynamics in the southern Bay of Biscay (1993-2003): winter supply, stoichiometry, long-term trends and their effects on the phytoplankton community. J.Geophys.Res. doi:10.1029/2006JC00357]. The second aspect is the timing of the stratification period. For the North Atlantic our analysis do not shows a clear pattern, but in any caase were increasing their length in northern (spring bloom) and eastern areas [González-Taboada, F. y Anadón, R. online. Patterns of change in sea surface temperature in the North Atlantic during the last three decades: beyond mean trends. Climatic Change. http://www.springerlink.com/content/n1171w7628217167/?MUD=MP]. In my opinion both issues should be considered in this section. (Ricardo Anadon, University of Oviedo)	Have included discussion of these issues in the text.
187	47069	30	10	21	0	22	It is not clear in this sentence that “These trends” applies to the lack of detectable trend in nutrient concentrations. (Skip McKinnell, PICES)	We have expanded the term 'these trends'. Now reads: 'The stratification has therefore variable impacts on nutrient supply and primary production depending on the physical setting in the various sub-regions.'
188	36002	30	10	22	0	0	"These trends may not hold for some areas such as the subarctic North Pacific where enhanced stratification depends largely on a freshening of the surface layer." It's necessary to also recognize the importance of salinity in ocean stratification, and consequently ocean ventilation. (Frank Whitney, Institute of Ocean Sciences)	We agree. Salinity is mainly stratifying the sea in coastal regions, and see comment #184 above on thermal stratification at high latitudes. ACTION: Done
189	38689	30	10	27	10	34	Durack & Wijffels (2010, in reference list) should be cited in the text; see also Durack et al (2012) Ocean salinities reveal strong global water cycle intensification during 1950 20 2000. Science 336: 455-458. (Janice Lough, Australian Institute of Marine Science)	That is correct. Durack et al. 2012 shows increased salinity in subtropical gyres and decreased at high latitudes. CITE ACTION: Cited
190	52005	30	10	30	10	34	Calibrated uncertainty language on these lines ("robust evidence" on line 30, "high confidence" on lines 31 and 34) should be italicized. (Katharine Mach, IPCC WGII TSU)	Agreed and adopted
191	36003	30	10	34	0	0	It may be appropriate to restate that freshening of the subarctic Pacific is one cause of reduced ocean ventilation (e.g. Whitney et al 2007; Watanabe et al 2008. GEOPHYSICAL RESEARCH LETTERS, VOL. 35, L01602, doi:10.1029/2007GL032188). (Frank Whitney, Institute of Ocean Sciences)	Agreed and we have inserted a sentence regarding the freshening of the subantarctic Pacific as a cause of reduced ocean ventilation.
192	38690	30	10	37	11	2	Add appropriate references to relevant scientific literature to this section. (Janice Lough, Australian Institute of Marine Science)	We have an appropriate references
193	41261	30	10	39	0	0	It must be "increased CO2 flux into the ocean". (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Agreed and adopted
194	37125	30	10	39	10	39	Remove 'flux' after CO2. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	Agreed and adopted
195	45587	30	10	45	10	46	Please indicate the time period over which the pH change was determined. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	Since the Industrial Revolution - this has been added. Now reads: Surface ocean pH has declined by approximately 0.1 pH units since the beginning of the Industrial Revolution (WG1, Ch3, Box 3.2), intensifying the already naturally lower pH conditions associated with upwelling sub-regions such as those of the west coasts of equatorial Africa as well as North and South America (Figure 30.6A).
196	38824	30	10	46	0	49	I suggest to incorporate the idea of Aragonite Saturation Horizon, relevant for the future for deep corals. As interesting reference the paper [Guinotte, J. M. Orr, J. Cairns, S. Freiwald, A. Morgan, L. George, R. 2006. Will human-induced changes in seawater chemistry alter the distribution of deep-sea scleractinian corals? Frontiers in Ecology and the Environment 4(3): 141-146] analyse the ASH depth during this century in all oceans in relation to this very interestings benthic communities. Obviously have conections and relevance for other calcifiant benthic and pelagic organisms. (Ricardo Anadon, University of Oviedo)	We agree and have added a sentence on deepwater corals and their exposure to ocean acidification through the shallowing of the aragonite/calcite saturation horizon.
197	47336	30	10	46	10	46	It seems tautological to say "ocean pH has declined as a result of ocean acidification" (James Christian, Government of Canada)	This is correct and has been corrected in the text. Sentence now reads: Surface ocean pH has declined by approximately 0.1 pH units since the beginning of the Industrial Revolution (WG1, Ch3, Box 3.2), intensifying the already naturally lower pH conditions associated with upwelling sub-regions such as those of the west coasts of equatorial Africa as well as North and South America (Figure 30.6A).
198	52006	30	10	46	10	46	It would be preferable to specify the timeframe for this decline. (Katharine Mach, IPCC WGII TSU)	We agree - see changes made above.
199	38564	30	10	48	10	49	Both the regions with the lowest saturation states and the regions with the most quickly changing saturation states should be discussed. For example, the staturation state in the California current system is predicted to change rapidly (Nicolas Gruber, Claudine Hauri, Zouhair Lachkar, Damian Loher, Thomas L. Frölicher, Gian-Kasper Plattner. Rapid Progression of Ocean Acidification in the California Current System. Sciencexpress 14 June 2012 / Page 1 / 10.1126/science.1216773), and available evidence suggests rapid declines in pH in this region (Wootton, J. T., C. A. Pfister, and J. D. Forester. 2008. Dynamic patterns and ecological impacts of declining ocean pH in a high-resolution multi-year dataset. Proc. Nat. Acad. Sci. USA 105:18848-18853). (Christopher Harley, University of British Columbia)	We Agree and have included discussion of the rates of change.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
200	45589	30	10	51	0	0	Figure 30-6 contains information on projected pH changes, it should therefore be moved to the section "Projected changes" (p. 11). (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have combined past, present and future aspects for each variable (and made it very clear that we are talking about future CO2 levels).
201	45588	30	11	5	0	0	Chapter 30 and Chapter 6 should use consistent units to describe hypoxic conditions in text and figures (Ch30, p. 11, l. 5 and Ch6, p. 11, l. 19), unless they find a good way to reduce overlap. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We strongly agree. We should use mass units for O2 of µmol/kg and ask that we convert the mix of mg and mls etc to this form. For the definition of "hypoxia" - the Brewer lab team have written whole papers on this (see Hofmann, A.F., Peltzer, E.T., Walz, P.M., Brewer, P.G. (2011) Hypoxia by degrees: Establishing definitions for a changing ocean. Deep-Sea Res. I. 58, 1212-1226. doi:10.1016/j.dsr.2011.09.004.) - we will cite this paper here and recommend it to others."
202	36004	30	11	7	11	24	It needs to be made clear that climate change is causing some widespread increase in hypoxia. The subarctic Pacific is perhaps the best example of an ocean basin where physical forcings are clear (Siberian warming leads to reduced ice formation, a reduction in dense water formation and a weakening oxygen transport into the interior ocean; Nakanowatari et al 2007 forms the basis for this concept). (Frank Whitney, Institute of Ocean Sciences)	Agreed - the following text has been added: Climate change is also driving part of the widespread increase in hypoxia. The subarctic Pacific is perhaps the best example of an ocean basin where physical forcings are clear, where Siberian warming leads to reduced ice formation, a reduction in dense water formation and a weakening oxygen transport into the interior ocean (Nakanowatari, 2007).
203	43257	30	11	7	11	38	The calculation of hypoxia profiles according to an assumed critical threshold of 60 matm is illustrative but conveys an overly simplistic message, especially in the way it is discussed. The critical PO2 is species specific and shifts during the lifecycle of a species and depending on body size, interactions or with temperature. Species specific changes will thus occur above and below this apparent threshold and shape especially climate induced ecosystem changes. This threshold, while a very rough indicator for the present separation of hypoxia intolerant and tolerant communities, can only vaguely represent climate effects. These constraints should be reflected in the discussion. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Note that the Figure from Hofmann et al that we use states that we give the 61 µmolar map contours for the sake of convention. The actual reference (again we don't list it) has a full discussion of the range. We could improve on this given another 200 words – but this definition is not a regional problem! It should be given in Chapter 6 and we can refer to that.) We have added text to link to ch 6. We have added the following text in this regard: The calculation of hypoxia profiles according to an assumed critical threshold of 60 matm is illustrative but conveys an overly simplistic message. The critical PO2 is species and life history specific with a dependence on body size and/or temperature. These variability in sensitivity shape climate induced ecosystem changes (Ch6).
204	52007	30	11	11	11	11	"High confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We agree - this has been adopted.
205	38565	30	11	12	11	14	To the climate-change related drivers, you could add changes in river run-off which bring nutrients into the marine system. The run-off is determined by precipitation and snow melt, which in turn are driven by climate change. (Christopher Harley, University of British Columbia)	Have changed sentence - now reads: "These areas are rapidly expanding due to a number of regional human drivers not directly related to climate change (i.e. eutrophication of coastal areas) to drivers that are (e.g. increasing sea temperature, increasing stratification, changes in terrestrial run-off and reduced ventilation). "
206	41262	30	11	13	0	0	It must be "(ie. eutrophication of coastal areas) in additon to drivers that are". (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Haved added "in addition to drivers "
207	38825	30	11	16	0	17	I think is interesting as reference of hypoxia the papers of Feely et al, Chan et al and the model for the future of Gruber et al. [N. Gruber, C. Hauri, Z. Lachkar, D. Loher, T. L. Frölicher, G-K. Plattner. 2012. Rapid Progression of Ocean Acidification in the California Current System. http://www.sciencemag.org/content/early/recent / 14 June 2012 / Page 1 / 10.1126/science.1216773 ; R. A. Feely, C. L. Sabine, J. M. Hernandez-Ayon, D. Ianson, B. Hales. 2007. Evidence for Upwelling of Corrosive "Acidified" Water onto the Continental Shelf. Science 320: 1490-1492; F. Chan, J. A. Barth,J. Lubchenco, A. Kirincich, H. Weeks, W. T. Peterson, B. A. Menge. 2008. Emergence of Anoxia in the California Current Large Marine Ecosystem Science 319, 920] (Ricardo Anadon, University of Oviedo)	We agree with the reviewer and have added the Feely and Chan papers you suggest to the end of the sentence "the Arabian Gulf, and the California, Humboldt and Benguela current systems (Figure 30.7), where eruptions of hypoxic and sulfide-laden water have occurred [Weeks et al., 2002]". We have also included the Feely and Gruber papers in section 30.3.2.2 on ocean acidification
208	38826	30	11	17	0	19	I suggest to incorporate the papers of Stramma et al as references of deoxygenation and their potential influence on pelagic fishes[Stramma, L. E.D. Prince, S. Schmidt, J. Luo, J.P. Hoolihan, M. Visbeck, D.W. R.Wallace, P. Brandt and A. Körtzinger.2011. Expansion of oxygen minimum zones may reduce available habitat for tropical pelagic fishes. Nature Climate Change, 2:33-37; Stramma, L. Visbeck, M. Brandt, P. Tanhua,T. Wallace, D. 2009. Deoxygenation in the oxygen minimum zone of the eastern tropical North Atlantic. Geophysical Res.Let. 36 (doi:10.1029/2009GL039593): 1-5] (Ricardo Anadon, University of Oviedo)	These references have already been cited in section 30.5.5.1.1 Pacific Ocean subtropical gyre.
209	52008	30	11	17	11	17	It would be preferable to indicate more specifically what is meant by "critical." (Katharine Mach, IPCC WGII TSU)	Corrected. Now reads: In the Atlantic Ocean, the hypoxic area is centered on 10ºN and 10ºS of the equator.
210	47070	30	11	26	0	0	Exactly how is the Atlantic different? (Skip McKinnell, PICES)	Clarified, now reads: "The Atlantic Ocean differs from the Pacific and Indian Ocean as hypoxic conditions are here largely limited to the regions at and adjacent to the two eastern boundary upwelling ecosystems due to its better ventilation. "

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
211	37126	30	11	30	11	30	I believe 'Hofmann et al 2011', as listed in the references list, is not the proper reference for this figure. The paper listed in the reference list, 'Hofmann, M., B. Worm, S. Rahmstorf, and H. J. Schellnhuber (2011), Declining ocean chlorophyll under unabated anthropogenic CO(2) emissions, Environ Res Lett, 6(3).' focuses on chlorophyll changes, not on oxygen. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	We thank the reviewer and have subsequently rectified this problem.
212	36005	30	11	41	0	0	Projected Changes in SST (or expand section to include other modes of change - hypoxia, ocean acidification, primary productivity, etc. discussed in Ch 6) (Frank Whitney, Institute of Ocean Sciences)	We have restructured the chapter and now combine observed (past, present) and projected variables in each discussion.
213	45590	30	11	41	0	0	This section only contains infomation on projected temperature changes, whereas other changes of physical and chemical conditions are lacking. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	Agreed, see above.
214	38691	30	11	41	12	11	Are these new calculations? Is this information not available from WGI? (Janice Lough, Australian Institute of Marine Science)	These are published as part of CMIP5 and hence not new calculations. We have spoken to the TSU - and have obtained the decision that it is okay to present these numbers as they are not new.
215	46600	30	11	43	11	54	As with the previous comment regarding HadISST analyses, Table 30-4 is more than just presenting the data of others. It requires some interpretation, assumptions and judgment, none of which has been subject to peer review. This is a risk (Neville Smith, Bureau of Meteorology)	These are published as part of CMIP5 and hence not new calculations. We have spoken to the TSU - in their opinion, we are re-presenting published numbers - the risk is minimal. We will be working closely with a number of WGI and other experts on climate modelling to ensure that we have presented the numbers correctly.
216	41755	30	11	46	11	48	It would be useful to illustrate the model and data comparison with a figure. (Juergen Weichselgartner, University of Kiel)	We respectfully disagree. This would lead to over 20 additional figures and the Table allows the reader to more clearly attain the regional averages at a glance.
217	49763	30	12	14	0	0	Section 30.4. I suggest that the "Detection and Attribution" language in this section be changed. This usage differs from that of the physical climate sciences. I think the usage here could, over time, dilute the meaning of the specialty. Instead, it would be worthwhile to discuss why detection and attribution is rarely possible with ecological time series. The problem faced is more complex than that in the physical climate sciences, and time series are much shorter. Instead the best we can do is examine a large number of relatively short time series with reference to corresponding physical data. Take pride in choosing the more difficult discipline in ocean sciences! Do not aspire to replicate detection and attribution when the proper data to do so are unavailable. (Ryan Rykaczewski, Princeton University)	Thank you, we do take pride! We are working closely with WGII Chp 18 to standardise our language use. However, detection and attribution for ecological time series is one of our remits , see Chp 18, and data is growing for such an approach. We find that the ecological datasets in this study have a median 42 yrs median time span of observations = 41 yrs, range = 19 - 343 yrs. The challenges for detection and attribution for ocean biological systems are discussed in section 30.1.2 and also in Chp 18.
218	43251	30	12	14	14	25	This section, based on new meta-analyses, is fully matching the definition of a sectoral treatment and would be better placed in chapter 6 as it nicely complements and integrates with the respective analyses presented there. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	This has been resolved after discussions between CLAs and the TSU. Meeting between Chris Field, Vincente, Katie Mach, Hans Portner, David Karl and Ove Hoegh-Guldberg (Oct 24 2012, 7am) Meeting to discuss how to resolve Hans' issues with chapter 6/30. Key points resolved: 1. Need to have clearer distinction between executive summaries a. talking to David Karl afterwards, I suggested that they forward their two-page executive summary to us to help us craft our executive summary as a clear sequence from Ch6 2. Chris Field clearly saw the distinction between the two chapters as a. Chapter 6 - focused on processes with some reference to the global implications – that any regional implications would be part of chapter 30. b. Chapter 30 draws together from physical, biological to socio-economic impacts c. Both can have global treatments although CH30 does this extensively by comparison to Ch6. 3. One possibility is that we could craft a summary box for the NCEAS stuff - which would point to our analysis of chapter 30. This would be repeating a small amount of information chapter 30.
219	36006	30	12	17	0	0	Variability on many timescales... (Frank Whitney, Institute of Ocean Sciences)	Agreed - now reads: Variability on many timescales, from hours to decades, as well as a multitude of non-climate related influences, confound detection and attribution of observed changes within the world's oceans to climate change [Hegerl et al., 2010].
220	37268	30	12	17	0	0	Suggested change "Variability in the different time scales" (Erica Head, Fisheries and Oceans Canada)	As above

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
221	46601	30	12	17	12	23	Lines 17-20 tell us it is very hard, but then the remainder says there is great opportunity (not sure what the reference to AR4 is about). (Neville Smith, Bureau of Meteorology)	Have corrected - now reads "Variability on many timescales and a multitude of non-climate related influences has confounded detection and attribution of observed changes within the world's oceans to climate change [Hegerl et al., 2010]. This has led to global analyses to date having few marine observations {Parmesan, 2003 #23547}{Rosenzweig, 2007 #40221}. The rapid increase in the number of studies of climate change impacts on the ocean since the last IPCC AR4 ([Hoegh-Guldberg and Bruno, 2010; Poloczanska et al., 2012], however, presents an increasing opportunity to examine and potentially attribute observed changes within the global ocean to climate change. "
222	38692	30	12	25	14	25	This is a long section devoted primarily to the results of a single paper Poloczanska et al (2012 - in press?). Suggest this needs considerable simplication and shortening and avoiding repetition (e.g. Page 13, lines 26-33). (Janice Lough, Australian Institute of Marine Science)	The paper is in the final stages of review and will be in press in time (August 2013 deadline - currently in review)
223	49764	30	12	25	14	25	I am thus far unfamiliar with the Poloczanska et al. (2012) paper. It seems like a significant contribution to the field. However, discussion of the technique which was applied in the paper came across as a bit awkward in a section titled "Global Detection and Attribution…", as the method relies on the "consistent with..." jargon. My experience with (and my personal usage of) this jargon tells me that it is used specifically when attribution to climate change is not possible. Am I wrong? If attribution of the change were possible, then there wouldn't be a need for the "consistent with..." phrase. Is consistency among authors which note that changes are "consistent with climate change" an accepted method of attribution? If we all say that it "could be climate change," then have we unambiguously attributed the observed changes to anthropogenic global warming? I don't think so. My concern is that there may be no attempt here by the individuals (i.e., authors of studies aggregated for this meta analysis) to distinguish secular trends from modes of natural variability. This concern is alleviated a bit by the last two paragraphs of the section when it is stressed that these relationships need to rely on real understanding of the system. (Ryan Rykaczewski, Princeton University)	We understand the reviewers concerns and refer them to Poloczanska is paper which includes a rigourous approach to rating studies for their intention to separate climate change from natural processes - we have have rewritten this section to ensure this is clear.
224	45591	30	12	29	12	31	Please define which factors were considered as "climate change". Does this mainly refer to temperature increase or extremes, or does it include hypoxia and ocean acidification? (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have added the following sentance to clarify "90% of the studies identified temperature as the primary driver of change, with the remainder focusing on sea ice, pH and climate oscillation, where changes in their indices have been explicitly linked to global warming."
225	46602	30	12	32	12	34	While I applaud the intiative in bringing these datasets together, the fact that a number of different authors have made subjective calls on consistent/not consistent does mean any conclusions based on the studies must be treated with caution. For attribution, a judgment has already been made about the possible link to climate change so there is significant potential for overly optimistic conclusions (see Chapter guidance re independence). Figure 30-9, where levels of confidence are assigned, highlights the criticality of these underlying assumptions. (Neville Smith, Bureau of Meteorology)	The theory behind this vote-counting approach is outlined in Parmesan and Yohe (2003) Nature and also in Chp 1, WGII, AR4. We refer the reviewer to these for clarification - text has been added to manuscript.
226	37269	30	12	40	0	0	Bad colour I could hardly see the yellow points on the graphs. Can you change the colour, please? (Erica Head, Fisheries and Oceans Canada)	We have reworked these figures so they are clearer. We have kept the yellow points but removed the blue background which was swamping the colours.
227	47073	30	12	40	0	45	What is the relevance of this figure to the open ocean? Most of the data points are coastal or in marginal seas. (Skip McKinnell, PICES)	Our chapter is focused on Oceans. The original title for this Chapter Open Oceans was misleading, this has been modified to The Ocean. We have discussed our treatment of the oceans with Chapter 5, coastal and low-lying areas, and have differentiated between us. We include coastal and marginal seas but with a focus on the influence of ocean processes, whereas Chapter 5 considers the influence from land
228	52009	30	12	47	12	53	Is this material also from Poloczanska et al., 2012? Additionally, the author team might consider indicating its degree of certainty in the final statement of the paragraph through use of calibrated uncertainty language. (Katharine Mach, IPCC WGII TSU)	We have read in this section and clarified and shortened it. We have added consistent calibration language across the chapter.
229	37270	30	12	48	0	0	For clarity. Fig 30-8 shows some sites where changes are apparently inconsistent with climate change, but Fig 30-9 does not. This is because Fig 30-9 only includes data from sites/studies in Fig 30-8 where the changes were consistent with climate change, but this was not very clearly stated in the text. I suggest inserting a couple of words to clarify this thus “Of the observations that showed a response in either direction, 84% were in a direction that was consistent with climate change (Fig. 30-9). For these, consistency was variable etc.” (Erica Head, Fisheries and Oceans Canada)	We have changed the text. Now reads: "Of the observations that showed a response in either direction, 84% were in a direction that was consistent with climate change "
230	38827	30	12	49	0	0	The observed differences in predictions may be due to mechanistic processes. For instance, unexpected responses to heating, or observations are made at depths where the temperature or other changes are not as important to generate observable impacts. There is no differentiation by depth in the graph 30-9, and you are not be able to discriminate differences. I suggest a review or an analysis of the data in this regard (Ricardo Anadon, University of Oviedo)	Respectively, the authors beg to disagree. Most observations involve shallow water organisms. We are still getting significent results despite not separating observations by depth. 70% of the observations are from coastal or shelf waters, and a further 28%. from the top 200m in open ocean
231	47071	30	13	1	0	0	Why is there a “polar” analysis here when you've taken a position that it is dealt with in another chapter? (Skip McKinnell, PICES)	We have left in the polar points on the graphs for comparison with the regions considered in Chp 30.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
232	45592	30	13	8	13	15	Please indicate the time periods over which the speed of redistribution of biota was determined. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have added to the text - see above.
233	37271	30	13	9	0	0	Talking about Fig. 30-10. Fig 30-10A should be Fig 30-10B and vice versa (Erica Head, Fisheries and Oceans Canada)	We have corrected this.
234	52010	30	13	9	13	11	Given the substantially different result obtained by the "previous analysis," it would be helpful to indicate further the reasons for the difference (in terms of study design, drivers considered, treatment of invasion versus gradual changes, etc.). (Katharine Mach, IPCC WGII TSU)	We have removed reference to this study, it is misleading as the study only considers range-expanding species, regardless of the driver, hence the estimates for range shifts are high. Climate change is only identfied as a driver in 70% of the studies.
235	41756	30	13	9	13	14	Figure 30-10A is referenced first but the authors mean B. Because of this, the points are discussed out of chronological order. The figure referenced as 30-10B is really 30-10A. (Juergen Weichselgartner, University of Kiel)	We have corrected
236	38828	30	13	11	0	13	I suggest to incorporate the recent paper [Sunday, J.M. Bates, A.E. Dulvy, N.K. 2012. Thermal tolerance and the global redistribution of animals. Natre Climate Change, doi:10.1038/nclimate1539. 5 pp] as reference related to range shifts in marine animals, and their comparison with terrestrial organisms. Could clarify the idea (Ricardo Anadon, University of Oviedo)	We have added this reference.
237	41263	30	13	12	13	15	"km dec -1" must be "km decade -1". (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We have altered
238	37272	30	13	17	0	0	Fig 30-10 Mislabeled panels Either the labels should be changed to match the text (Page 13, Paragraph2), or the text should be changed to match the labels in the Fig. (Erica Head, Fisheries and Oceans Canada)	We have corrected
239	46603	30	13	26	14	19	This is very bold. In the light of the confounding factors highlighted on p 5, and the advice provided in Chapter 18, it is challenging to see how confidence could be attached to to 'attributions' based on 19-30 years of data. The authors are clearly familiar with Chapter 18 but I still conclude they are stretching things. Would you love to have multiple lines of evidence? (Neville Smith, Bureau of Meteorology)	The median time span of observations = 41 yrs, range = 19 - 343 yrs . There are multiple lines of evidence, some of which is presented in the regional sections below. Poloczanska et al elaborates further on the diagnostic fingerprints. We agree with the reviewer at one level - however - that the time series is too short to conclude on that it is anthropogenic climate change, but the observed ecosystem responses are examples of how ecosystems will change whether the cause is anthropogenic or natural.
240	41757	30	13	30	13	30	An end parenthesis is missing after the Burrows reference. (Juergen Weichselgartner, University of Kiel)	We have corrected
241	47072	30	13	33	0	0	It is rather amazing that the open ocean part of the Sub-arctic Pacific is highlighted as a region with “correspondingly large ecological changes” when surveys that would identify them are almost never done. (Skip McKinnell, PICES)	Have modified sentence to now read: "We observed rapid velocities (>50km.decade-1) in the North Sea, the sub-Arctic Pacific and Atlantic, and within 15° of the equator, implying a risk of large ecological responses."
242	37273	30	13	35	0	0	Poorly written paragraph The first sentence should be re-written. e.g. “The meta-analysis of Poloczanska et al. (2012) supports attribution of observed biological changes to climate change, because they are mainly consistent with expectations.” BUT – are there any explanations for the cases where responses were in the “wrong” direction? Maybe they should not be completely ignored, even if it’s just a “throwaway line” like “For cases where biological responses were contrary to expectations based on climate change, other effects (e.g. overfishing, pollution etc) were probably involved.” – and, by the way, I made up these effects! Also, a simple explanation should be given of what is meant by “sign-switching”. My suggestion might be. “Further support for attribution was gained, using subsets of the database, to provide additional evidence of “sign-switching” (concurrent reversals in trends in climate and/or biological responses) within studies etc.” - if this is what you mean! (Erica Head, Fisheries and Oceans Canada)	We have rewritten this entire section
243	52011	30	13	37	13	38	"High confidence," as calibrated uncertainty language, should be italicized. Additionally, given the description of the results as unambiguous, the author team might consider if an assignment of "very high confidence" could be appropriate--or alternatively, if the descriptor "unambiguous" might be better if tempered. (Katharine Mach, IPCC WGII TSU)	We have addressed this is part of getting the calibration language consistent across entire chapter.
244	39025	30	13	39	0	0	I suggest giving an explicit definition of "sign switching" when this expression is introduced. (George Somero , Stanford University)	We have removed the sign-switching section, as this is confusing, and instead inserted text which describes the diagnostic responses in ocean systems
245	43252	30	13	43	13	46	sentence is incomplete (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We have rewitten this section
246	37127	30	13	44	13	44	Better 'foraminifera' instead of 'forams' (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	We have removed this paragraph as this level of detail is superfluous and instead refer readers to the original study
247	52012	30	13	46	13	48	The author team might consider indicating its degree of certainty in the statement through use of calibrated uncertainty language, per the guidance for authors. (Katharine Mach, IPCC WGII TSU)	We have addressed this is part of getting the calibration language consistent across entire chapter.
248	37274	30	13	50	0	0	The text here is repeated in the next paragraph on Page 14. (Erica Head, Fisheries and Oceans Canada)	We have rewritten this section to remove duplication
249	39295	30	13	50	0	0	Also, there is a repetition at p. 13 lines 50-54 and p.14 lines 5-19. (Gianluca SARA, University of Palermo)	We have rewritten this section to remove duplication
250	41264	30	13	50	14	19	There are many duplicated sentences. Please correct it. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We have rewritten this section to remove duplication
251	41758	30	13	50	14	19	The two sentences (pg 13, lines 50-53) are exactly repeated on page 14 (lines 5-8). In addition, page 13 line 53 to page 14 line 3 are incorporated repetitively in the next paragraph (page 14, lines 12-19). (Juergen Weichselgartner, University of Kiel)	We have rewritten this section to remove duplication
252	45580	30	13	50	14	19	These two paragraphs are partly identical. Maybe one of the two can be deleted. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have rewritten this section to remove duplication

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253	45593	30	13	50	14	19	Please explain which mechanisms you are refering to. Those outlined in Chapter 6? Then please cross-reference. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	This text has now been removed
254	36298	30	13	53	13	54	"In general.... Figure 30-10)." This sentence is dupulicate of the line 13-14, Page 14 (Sanae Chiba, JAMSTEC)	We have rewritten this section to remove duplication
255	43253	30	14	14	14	16	same sentence as p. 13, 54 to p. 14, l. 3. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We have rewritten this section to remove duplication
256	39026	30	14	23	0	0	Check the species numbers given in this figure legend. There seems to be a disagreement/error. (George Somero , Stanford University)	We have fixed this problem
257	46604	30	14	24	0	0	Could not find the O'Connor reference. (Neville Smith, Bureau of Meteorology)	Figures related to O'Connor have been removed as has the reference, this manuscript might not make the IPCC deadline for inclusion
258	38695	30	14	28	37	25	These sections on regional impacts/vulnerabilities etc need strong editorial control to ensure that each region/system is discussed in the same manner and with the same level of detail - it is somewhat uneven at present. I think it would also be useful for each section to start with a brief description of the key ecosystems within each region. (Janice Lough, Australian Institute of Marine Science)	Ideally, we would like to do this. However, some regions are not only heavily studied but show high impacts and vulnerabilites to climate change (eg North Atlantic) and therefore warrant expanded discussion, while our knowledge is sparse in other regions. We agree that describing key ecosystems in each region in more detail would be useful but we do not think it is necessary
259	47074	30	14	41	0	0	It's hard to imagine that this phenomenon wasn't discovered until Racault 2012. (Skip McKinnell, PICES)	The sentence does not imply this, but rather the Racault paper gives a global description of phytoplankton phenology and so is a useful paper for background information for the reader.
260	52013	30	14	47	14	47	Section 30.5.1.1. For each subsection, the author team should consider ways and opportunities for highlighting key assessment findings, also with use of calibrated uncertainty language to characterize the author team's degree of certainty in the findings. Such highlighting of key findings would provide a nice complement to section 30.5.1.2. (Katharine Mach, IPCC WGII TSU)	We will do this
261	43254	30	14	49	16	36	This is a nice , but purely sectoral treatment of the North Atlantic, no synthesis with WGI or III material is attempted the latter would have to consider a wide range of countries and econimies. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We have added cross-references with WG1 and WGIII in the chapter
262	47075	30	14	51	0	0	“some areas” is not sufficiently informative; provide names after reading my comment on Fig. 30-3. (Skip McKinnell, PICES)	Yes, we agree and have adjusted the text to be more specific
263	38693	30	14	51	14	52	Here, and elsewhere, when referring to changes through time I would strongly recommend being specific about the time period of the study rather than just say "over the past 50 years". (Janice Lough, Australian Institute of Marine Science)	Yes, we agree and have adjusted the text to be more specific
264	52014	30	14	52	14	52	"High confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have done this is part of getting the calibration language correct across the entire chapter.
265	46605	30	14	52	14	53	"Many species in this region are responding to climate change (high confidence)". Should 'climate change' be warming? Where is the evidence for the 'climate-driven change' part?Do the spatial patterns follow the mixed warming/cooling pattern? What are the line of evidence for this confidence level? Just Tasker (a non-peer-reviewed report)? (Neville Smith, Bureau of Meteorology)	Note: check with Keith re the peer-review status of Tasker report "This work has been synthesized, edited, and agreed by scientific representatives of the 20 ICES Member Countries". We have added cross-references to WG1 re physical changes
266	38829	30	15	12	0	13	Our results shows a higher (one order of magnitude) rate of warming in the North Atlantic [González-Taboada, F. y Anadón, R. online. Patterns of change in sea surface temperature in the North Atlantic during the last three decades: beyond mean trends. Climatic Change. http://www.springerlink.com/content/n1171w7628217167/?MUD=MP], and this values are concordant with the results of long term monitoring programme in the North Iberian Peninsula. I suggest to revisite this values.Changes in coastal waters of inner Bay of Biscay could be obtained in [Goikoetxea, N., A. Borja, J. Egaña, A. Fontán, M. González and V. Valencia (2009). "Trends and anomalies in sea surface temperature, observed over the last 60 years, within the southeastern Bay of Biscay " Continental Shelf Research 29: 1060-1069] (Ricardo Anadon, University of Oviedo)	This is because of the positive change in AMO/AMV and is already thoroughly covered in the text. We have added an explanation and explore this issue briefly.
267	46606	30	15	12	15	20	Is this attribution supportewd by WG1 AR5 [perhaps that is what the WG1 reference means]? (Neville Smith, Bureau of Meteorology)	Yes, that is what the WG1 reference and figure shows.
268	36299	30	15	17	0	0	"... areas of the Noreth Atlantic (>50N) hae cooled ..." Does it mean "northern" North Altantic? (Sanae Chiba, JAMSTEC)	Yes, we have clarified where the cooling is
269	38830	30	15	17	0	20	In our analysis the stratification strength (lenght) varies between regions of the North Atlantic, changes more rapidly in the north and east of the region, see reference in the above comment. Also I suggest to consider the changes in the Iberian upwelling as I showed in previous comments because has a great impact on species, communities and fisheries (Ricardo Anadon, University of Oviedo)	We have made some slight alterations to our regionalisation so the Iberian upwelling in now included under section 30.5.2 eastern boundary currents.
270	46607	30	15	22	15	40	Like this paragraph. (Neville Smith, Bureau of Meteorology)	We thank the reviewer for his comments
271	52015	30	15	27	15	44	"High confidence" on lines 27 and 44, as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have done this is part of getting the calibration language correct across the entire chapter.
272	47337	30	15	36	15	50	How can animals be 'conspecifics' if they're not in the same species or even in the same genus (Semibalanus/Chthamalus)? (James Christian, Government of Canada)	We have changed the terminology in both cases using "Warm-water equivalent" and "warm-water competitor"

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273	45595	30	15	46	15	51	I am not convinced that examples from the intertidal zone are well placed in a chapter on open oceans. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We integrate the information from high tide mark out to be centre of the oceans - this definition has been discussed and accepted by the TSU. In line with other regional chapters, we touch on subjects that may also be referred to in the coastal and island's and low-lying areas chapters. As a regional chapter our role is to act to integrate information which may have also been presented in previous chapters - we naturally link to these.
274	38831	30	15	51	0	52	Must of the references to the North Atlantic were devoted to North Sea, Baltic Sea of Northern Areas. I suggest to incorporate some references of changes in the Bay of Biscay and West Iberian Peninsula, in whic some specific hydrographic signatures take place. A winter poleward current, summer upwelling, seasonal stratification from April to October, and some southern limits of boreal species occurs. This features produce changes in especies distribution (The Bay of Biscay shows a sharp latitudinal gradient of several hundred kilometers with biogeographical limits associated to the summer upwelling. Long term studies in the area has show changes in brown seaweds of boreal origin [C. Fernández (2011): The retreat of large brown seaweeds on the north coast of Spain: the case of Saccorhiza polyschides , European Journal of Phycology, 46:4, 352-360; Lamela, C., Fernández, C., Arrontes, J. y Anadón, R. 2012. Fucoids Assemblages on the North Coast of Spain: Past and Present (1977-2007). Botánica Marina, 55: 199–207; Díez, I. Muguerza, N. Santolaria, A. Ganzedo, U. Gorostiaga J.M.. 2012. Seaweed assemblage changes in the eastern Cantabrian Sea a nd their potential relationship to climate change. Estuarine, Coastal and Shelf Science 99: 108-120], apprisal of seaweeds of warmer origin [Lima, F.P. Ribeiro, P.A. Queiroz, N. Hawkins, S. J. Santos, A.M. 2007. Do distributional shifts of northern and southern species of algae match the warming pattern?. Global Change Biology 13, 2592–2604], new mollusc species of warm waters [Guerra, A., A. F. González and F. Rocha (2002). "Appaerance of the common paper nautilus, Argonauta argo related to the increase of the sea surface temperature in the north-eastern Atlantic." Journal of the Marine Biological Association of the United Kingdom 82(5): 855-858;Lima, F.P. Queiroz, N. Ribeiro, P.A. Hawkins, S.I. Santos, A.M. 2006. Recent changes in the distribution of a marine gastropod, Patella rustica L. 1758, and their relationship to unusual climatic events. J. Biogeogr., 33: 812-822], new southern species fishes fishes [Bañon, R. Villegas-Ríos, D. Serrano, A. Mucientes, g. Arronte, J.C. 2010. Marine fishes from Galicia (NW Spain): an updated checklist. Zootaxa, 2667:1-27; G117 and changes in upwelling event frequency and seasonality with impact the primery productivity and pelagic communities [Valdés, L. López-Urrutia, A. Cabal, J.A. Álvarez-Osorio, M. Bode, A. Miranda, A. Cabanas, M. Huskin, I. Anadón, R. Alvarez-Marqués, F. Llope, M. Rodríguez, N. 2007. A decade of sampling in the Bay of Biscay: What are the zooplankton time series telling us? Progress in Oceanography, 74: 98-114; Bode, A. Anadón, R. Morán, X.A.G. Nogueira, E. Teira, E. Varela, M. 2011. Decadal variability in chlorophyll and primary production off NW Spain. Climate Res., 48: 293-305]. Some changes have a great impact on mussel aquaculture with potential economic impact in the Northwest Spain [Álvarez-Salgado, X., M. J. Fernández-Reiriz, U. Labarta, R. Filgueira, L. Peteiro, F. G. Figueiras, S. Piedracoba and G. Rosón (2009). Influencia do cambio climático no cultivo de mexillón das rías galegas. Evidencias e impactos do cambio climático en Galicia. X. d. Galicia. Santiago de Compostela, Xunta de Galicia: 373-389] and fisheries (see reference of Bañon 2009) and [Poulard, J. C., F. Blanchard, J. Boucher and S. Souissi (2003). Variability of the demersal fish assemblages of the bay of Biscay during the 1990s. ICES Marine Science Symposia] (Ricardo Anadon, University of Oviedo)	We agree that the Bay of Biscay is definitely part of the high Latitude spring bloom systems, albeit at the southern edge. We have added some of these references are appropriate.
275	41265	30	16	0	0	0	Not only for Section 30.5.1.1.2, but latitudinal movement by higher temperature will change the daylength available for fish larvae and the growth of the larvae will be affected. At least, in Pacific, it was confirmed by field experiment by Shoji et al. (2011). Shoji, J., S. Toshito, K. Mizuno, Y. Kamimura, M. Hori, and K. Hirakawa (2011), Possible effects of global warming on fish recruitment: shifts in spawning season and latitudinal distribution can alter growth of fish early life stages through changes in daylength., ICES Journal of Marine Science, 68, 1165-1169. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We agree that this is important piece of information and have added to section 30.5.1.1.2
276	46608	30	16	3	0	0	What is thew uncertainty of x degree/decade when there are only two decades in the data set?? (Neville Smith, Bureau of Meteorology)	This warming is part of the much larger change in the AMV. The stated rates of change over the past few decades are a statement of fact and no where in our discussion of this point do we imply that these rates of change should be projected into the future or are anything other than a change over the past few decades. I don't see a problem with what we have done here. In effect this comment/critisism is out of contect, but in any case, this comment will be addressed in our rewording of the front of chapter 30. This rewording will place these sorts of references (ie to short term changes) into the context of an observed change rather than a statement of long term trends. We have added sentences about large-scale and long-term variability and the shortness of records. We have been careful not to overstate what the changes over the past couple of decades actually mean.
277	47338	30	16	4	16	5	"cool-water specialists generally shifted northwards while abundant warm-water species shifted southwards reflecting winter warming of the shallow southern North Sea" Does this make sense? Where are the warm-water species that are moving southward coming from? (James Christian, Government of Canada)	We have rewritten the text to be clearer

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
278	38832	30	16	7	0	8	The same results with sardine, anchovy and red mullet is observed with souther species. New fisheries of Balistes capriscus, Trachurus picturatus and Scomber japonicus, and several species of Carangidae develops early this century in the nortwest Spain (Galician coast) [Bañón, R. (2009). Variacións na diversidadde e abundancia ictiolóxica mariña en galicia por efectos del Cambio climático. Evidencias e impacto do Cambio Climático en Galicia. X. d. Galicia. Santiago Composteñ, Xunta de Galicia: 355-372]. Unfortunately the language of this paper is Galician But graphs and names will be scientiphic easely understandable. (Ricardo Anadon, University of Oviedo)	We have added this reference and added to the text. I have asked the reviewer to supply a copy of this reference
279	38833	30	16	18	0	0	I suggest that this reference (Valiente et al 2011) was eliminated from selected references, due to a serious mistake in the input data of the work. The authors use the day of the first catch of salmon in each river, the Campanu. Unfortunately the rules on the opening of the fishing season has changed since the first days of March (3,4,5, March) in the early 80s to late March (20 March- 1 April) during the last 15 years of the serie. Obviously the result says that delays the first fish caught, but not by the effect of the mean annual temperature (work indicates this temperature) but by a mere administrative matter, not reflected in the paper. The information about decline of Atlantic Salmon captures in Nothern Spain is correct., also a decreasing trend on individuals with 2 or 3 years on sea. With this severe problem I suggest to avoid the reference. (Ricardo Anadon, University of Oviedo)	We have removed this reference
280	52016	30	16	26	16	26	"Medium confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have adopted this.
281	47339	30	16	39	0	0	Section 30.5.1.1.2 I do not know what to make of the assertion that “the dominant source of variability in North Pacific climate is caused by the annual solar cycle”. Firstly, the annual cycle dominates everywhere except in the equatorial Pacific. Secondly, climate variability is by definition what is outside the annual cycle. Third, I have never heard the annual cycle referred to as a “solar cycle”. The text then goes on to make several dubious assertions about ENSO and PDO influences. These issues are discussed in my general chapter comments, but two notes here. First, one can not neatly separate ENSO and PDO into “sub-decadal” and “decadal and longer”; the PDO is largely a statistical construct and the pattern arises in part from ENSO. Secondly, Mantua and Hare may have identified slight enhancements of variance in the frequency bands noted, but it is misleading to refer to these as “periodicities” for the reasons noted above. (James Christian, Government of Canada)	The text has been rewritten, solving this and other problems.
282	43255	30	16	39	17	47	The analysis of the North Pacific has nicely included some WGIII elements which should be expanded considering the specific countries and economies affected. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Good point
283	47076	30	16	41	0	0	As this is about impacts, I suggest changing “climate” to “marine ecosystems”. (Skip McKinnell, PICES)	we have done
284	49765	30	16	41	16	41	This first line "The dominant source..." struck me as awkward given that seasonal variability is certainly the main mode of variability in all of the systems that are classified as "High-Latitude Spring-Bloom Systems," not just in the North Pacific. Maybe a note to this effect could be inserted in the introduction to Section 30.5.1 and removed from this subsection. (Ryan Rykaczewski, Princeton University)	We agree and have removed this sentance
285	38694	30	16	41	16	45	"annual seasonal cycle" rather than "annual solar cycle"; "Victoria pattern" is not a commonly used term - needs explaining. (Janice Lough, Australian Institute of Marine Science)	We have removed the sentance re "annual solar cycle" (see reviewer comment 285).
286	37275	30	16	44	0	0	Unclear statement. Is the “Victoria Pattern” = the second mode of SST, or its SSH equivalent (which is the NPGO)? If so, adding a comma, thus “Victoria Pattern (Bond et al. 2003), the second mode of SST etc.” will do the trick. If not, then please clarify (Erica Head, Fisheries and Oceans Canada)	This has been clarified in the text.
287	47077	30	16	44	0	0	I believe it was Shoshiro Minobe who argued for a 50-70 y period, not Hare and Mantua. Reference is GEOPHYSICAL RESEARCH LETTERS, VOL. 24, NO. 6, P. 683, 1997, doi:10.1029/97GL00504. A 50–70 year climatic oscillation over the North Pacific and North America (Skip McKinnell, PICES)	We have added this reference
288	52017	30	16	45	16	47	It would be helpful to clarify what the acronym SSH stands for. Additionally, for the statement on these line, the author team might consider cross-referencing findings from the working group one contribution to the 5th assessment report. (Katharine Mach, IPCC WGII TSU)	We have added cross-references with WG1 and clarified SSH
289	41759	30	17	2	17	4	Why is the Atlantic Ocean being discussed in the section entitled "North Pacific"? (Juergen Weichselgartner, University of Kiel)	We have removed the North Atlantic discussion
290	37276	30	17	7	0	0	Omit “rapid””in pelagic ecosystems and (rapid) sardine and anchovy” (Erica Head, Fisheries and Oceans Canada)	We have removed the text.
291	36008	30	17	15	17	22	Should note also that warm periods can be highly stressful on more southern stocks of salmon (e.g Fraser River), a consequence of poor survival during spawning in rivers that are too warm (Martins et al 2011 Global Change Biology 17, 99-114 DOI: 10.1111/j.1365 2486.2010.02241.x) (Frank Whitney, Institute of Ocean Sciences)	We are short on room to add more references and this reference does not seem to add significantly to the manuscript.
292	47340	30	17	21	17	22	SLP affects virtually every aspect of atmospheric forcing of the upper ocean. This isn't wrong exactly (although the term "wind mixing" probably wouldn't be used much by physical oceanographers) but it reads like an attempt to make a connection to biological impacts in the ocean by someone not very comfortable with their understanding of the actual physical processes. (James Christian, Government of Canada)	Correct: have changed "wind mixing" to be "turbulent mixing via wind stress"
293	46609	30	17	29	17	31	Can you place 'high confidence' in a result based on "a study"? (Neville Smith, Bureau of Meteorology)	We have done this is part of getting the calibration language correct across the entire chapter.
294	41760	30	17	39	17	40	This study (reference) is controversial. The global trend is thought to be that mean catch per unit effort is decreasing. (Juergen Weichselgartner, University of Kiel)	We respectfully disagree - this is not controversial has written. Warming in the Arctic has resulted in migration of boreal species northwards, and increases in fish catches, also in the Atlantic part. ACTION: No change

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
295	36009	30	17	42	0	0	Oceanic waters that are low in oxygen are pervasive at relatively shallow waters of the eastern and subarctic Pacific Ocean (Frank Whitney, Institute of Ocean Sciences)	Sentence added to section 30.3.2.3 - "Oceanic waters that are low in oxygen are pervasive at relatively shallow waters of the eastern and subarctic Pacific Ocean. "
296	36011	30	17	42	17	47	The role that global warming plays in restricting oxygen transport into the subarctic Pacific is quite well described. Starting with Nakanowatari et al 2007, they describe how Siberian warming in winter reduces ice formation in Okhotsk Sea. This in turn reduces dense water formation and therefore oxygen flux. The 18.6 y tidal mixing signal generated in outflow waters from Okhotsk is strongly detected across the subarctic Pacific (Ono et al 2001; Whitney et al 2007; Keeling et al 2010 Ocean Deoxygenation in a Warming World. Annu. Rev. Mar. Sci. 2010. 2:199–229), confirming the influence Okhotsk has on oxygenation of the broad subarctic Pacific. The impact of oxygen loss should restrict deep habitat across the subarctic Pacific. Whitney et al (Progress in Oceanography 75 (2007) 179–199) described a shoaling of the 60 uM oxypleth from ~400 to 300 m over 50 y at Ocean Station P. McClatchie et al (GEOPHYSICAL RESEARCH LETTERS, VOL. 37, L19602, doi:10.1029/2010GL044497, 2010(and Koslow et al (Mar Ecol Progr Ser Vol. 436: 207–218, 2011) have described the impact oxygen loss has on California demersal fishes, and have projected the impact continued deoxygenation could have on habitat. Warming projections would suggest ice formation will continue to decline, therefore I believe oxygen transport will continue to weaken. (Frank Whitney, Institute of Ocean Sciences)	This point has been addressed. Have added several sentences above (see comment 131)
297	36010	30	17	44	0	0	Chan et al. 2008 Science 319 (Frank Whitney, Institute of Ocean Sciences)	We respectfully disagree and suggest that the reference doesn't add anything to the chapter (and that we already have a very large number of references which will have to be trend in total already)
298	43256	30	17	50	18	30	The analysis of the Southern Hemisphere has also included some limited WGIII elements. The WGI aspects should also have been integrated more. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We have attempted to do this in the final construction of the SOD.
299	36300	30	18	6	18	7	"...calcifying plankton assemblages play a key role.... Open ocean sediments". Which time-scale does it consider? Calcifying plankton release CO2 in shalow water and do not contribute CO2 fixation in ocean in a short time-scale. (Sanae Chiba, JAMSTEC)	We have included this point as much as possible.
300	52018	30	18	21	18	28	"High confidence" on line 21 and "very high confidence" on lines 27-28, as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have done this is part of getting the calibration language correct across the entire chapter.
301	52019	30	18	33	0	0	Section 30.5.1.2. For key findings throughout this section, the author team should consider using calibrated uncertainty language to characterize its degree of certainty in the conclusions. (Katharine Mach, IPCC WGII TSU)	We have done this is part of getting the calibration language correct across the entire chapter.
302	36012	30	18	37	0	0	and loss of subsurface habitat to hypoxia (Frank Whitney, Institute of Ocean Sciences)	We have added the suggested phrase.
303	36301	30	18	53	18	54	"Shifts in the distribution… response warm and cool period…" Again, we should mention many case of the observed warm-cool cycle and northward shift of zooplantkon, either in NP and NA, are likely to be due to advection associated with changes in oceanic currents/circulation rather than merely temperature increase. (Sanae Chiba, JAMSTEC)	We agree the advection distributes plankton but the shifts in distribution are as a result of temperature change
304	41266	30	19	7	19	8	Ito et al. (2010) projected size reduction and egg production enhancement of Pacific saury, Okunishi et al. (2012) projected nothward movement habitat and spawning areas of Japanese sardine and Kaeriyama (2008) prejected distinction of salmon from Japanese areas. Ito S., K. A. Rose, A. J. Miller, K. Drinkwater, K. M. Brander, J. E. Overland, S. Sundby, E. Curchitser, J. W. Hurrell and Y. Yamanaka, 2010, Ocean ecosystem responses to future global change scenarios: A way forward, In: M. Barange, J.G. Field, R.H. Harris, E. Hofmann, R. I. Perry, F. Werner (Eds) Global Change and Marine Ecosystems. Oxford University Press., 287-322, pp440. Okunishi T., S. Ito, T. Hashioka, T. T. Sakamoto, N. Yoshie, H. Sumata, Y. Yara, N. Okada, Y. Yamanaka, 2012, Impacts of climate change on growth, migration and recruitment success of Japanese sardine (Sardinops melanostictus) in the western North Pacific, Climatic Change, accepted. Kaeriyama M. (2008) Scenario on biomass variation of Pacific salmon concerning the climate change, Nippon Suisan Gakkaishi, 74, 876-879. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Thank you, we have incorporated many of these studies
305	46610	30	19	8	0	0	A stray "but" in here? (Neville Smith, Bureau of Meteorology)	Yes, we have removed
306	37277	30	19	13	0	0	Slightly misleading statement. "Decreases in seasonal sea-ice in sub-polar regions are likely to lead to increases in regional primary production and modifications of ecosystem structure (Arrigo et al. 2008)." Primary production can be limited by light or nutrients, and Arrigo et al. are talking mainly about the effect of the loss of sea ice on light. These waters tend (in general) to be highly stratified, due to melting ice, so nutrient limitation is likely to become the norm, except in regions where there is upwelling. Arrigo et al seem to downplay nutrient limitation as a possibility in the future, whereas Li et al. 2009, definitely found it in the Canada Basin in summer. So, can I suggest instead: "Decreases in seasonal sea-ice in sub-polar regions will lead to increases in the growth season and the intensity of the light available to fuel phytoplankton growth and hence to enhanced primary production and modifications of ecosystem structure (Arrigo et al., 2008). In the long-term, however, primary production may become limited by the supply of nutrients to the surface layers." Reference Li, W.K.W., McLaughlin, F.A., Lovejoy, C. and E.C. Carmack (2009) Smallest algae thrive as the Arctic Ocean freshens. Science. 326, 539 (Erica Head, Fisheries and Oceans Canada)	Thank you, we have updated
307	46611	30	19	13	0	0	Variations in place of oscillations. (Neville Smith, Bureau of Meteorology)	Agreed. We have made this change throughout the manuscript.
308	52020	30	19	14	19	19	"likely" on lines 14 and 19 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have made changes to ensure consistency with calibration language throughout the manuscript.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
309	43258	30	19	22	22	49	If not combined with a treatment of the countries and societies dealing with the Eastern boundary currents such discussion remains sectoral. The treatment could include a comparative assessment of the general principles effective in all of them as well as the specific differences distinguishing them. This illustrates how important the inclusion of regional societies and their specfic needs and economies is for a synthetic treatment. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We disagree. Regions are not defined by countries - we have been very clear on this from the beginning.
310	37267	30	19	27	0	0	Section 30.3.2.1. Surface salinityThis seems to be the only spot where the Chapter talks about trends in salinity, so I thought a little more explanation was needed. For example, while it is clear why salinities should generally increase with increasing temperatures, especially at low latitudes, and that they should decrease at high latitudes, there seem to be a few regions where the effects of less clear. In particular, “the East, North and West Pacific Ocean along with parts of the Gulf of Mexico”. Could we (the readers) be given a hint of why there is freshening at these locations? I note that Jury (2011) shows a decrease in salinity in the Caribbean Sea (and hence perhaps parts of the Gulf of Mexico), which he attributes to changes in circulation, and a greater input of S Atlantic water. I also note that Singh and Delcroix (2011) attribute the freshening in the Western tropical Pacific to increased precipitation. Are they right? References Jury, M. R. 2011. Long-term variability and trends in the Caribbean Sea. 2011. Int J. Oceanogr. Doi:10-1155/2011/465810 Singh, A and T. Delcroix. 2011. Estimating the effects of ENSO upon the observed freshening trends of the western tropical Pacific Ocean. Geophys. Res. Let. 38, L21607, doi:10.1029/2011GL0496361 (Erica Head, Fisheries and Oceans Canada)	We have considered these comments in the relevant section - this comment seems to be reference to the wrong pages.
311	52021	30	19	37	0	0	Section 30.5.2.1. For each subsection, the author team should consider ways and opportunities for highlighting key assessment findings, also with use of calibrated uncertainty language to characterize the author team's degree of certainty in the conclusions. Such highlighting of key findings would provide a nice complement to section 30.5.2.2. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
312	46612	30	19	42	0	0	Levitus did not do projections. (Neville Smith, Bureau of Meteorology)	We have found the appropriate reference and have replaced Levitus.
313	38834	30	19	48	0	50	The problemof the upwelling intensification or not for me is not the question, if not the spatial and temporal changes of the upwelling events. In our coast has changed (see the reference of Llope et al, 2006, or Perez et al, 2009,) both the timing and frequency in summer months. Probably the result was a rapid temperature increase and a great impact on many species and groups (see previous comments). A similar analysis with more data has been developed by Iles et al, 2012 [A.C . Iles, T.C. Gourier, B.A. Menge, J.S . Stewart, A.J . Haupt and M.C. Lynch. 2012. Climate-driven trends and ecological implications of event-scale upwelling in the California Current System. Global Change Biology (2012) 18, 783–796, doi: 10.1111/j.1365-2486.2011.02567.x] As these last athors coment in their abstract "changes in coastal upwelling that are consistent with climate change predictions are altering the tempo and the mode of environmental forcing in near-shore ecosystems, with potentially severe and discontinuous ramifications for ecosystem structure and functioning". Probably there are other papers (the paper of Arístegui et al, yet incorporated to the chapter) related with the effects on changes in seasonality, frecuancy and intensity of coastal upwellings; for these reasons I suggest a more affordable writting about upwelings, due to the severe influnece of these observed changes (Ricardo Anadon, University of Oviedo)	We explore the issue of changes to the intensity of upwelling of a number of points including in a special box. We will consider this information and how it might be added to those discussions.
314	52022	30	19	48	19	51	"likely" on lines 48 and 51 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. Additionally, for the statement on lines 49-50, the author team should consider potentially cross-referencing findings from the working group one contribution to the 5th assessment report. (Katharine Mach, IPCC WGII TSU)	As above, we have made changes to ensure consistency with calibration language throughout the manuscript.
315	36890	30	20	12	20	19	The heterogeneity in the trends and responses in this must be acknowledged (see comments to Chapter 6, p. 49-50) There are evidences of differential effects of climate on upwelling intensity in marginal compared to central areas in the Canary-African upwelling, implying a reduced nutrient input in the former (e.g. Pérez et al., 2010) which do not match observed trends in phytoplankton biomass and primary production (e.g. Bode et al., 2011). Additional references: Bode, A., R. Anadón, X. A. G. Morán, E. Nogueira, E. Teira, and M. Varela. 2011. Decadal variability in chlorophyll and primary production off NW Spain. Climate Research 48:293-305. Pérez, F. F., X. A. Padin, Y. Pazos, M. Gilcoto, M. Cabanas, P. C. Pardo, M. D. Doval, and L. Farina-Bustos. 2010. Plankton response to weakening of the Iberian coastal upwelling. Global Change Biology 16:1258-1267. (Antonio Bode, Instituto Espanol de Oceanografia)	We explore the issue of changes to the intensity of upwelling of a number of points including in a special box. We will consider this information and how it might be added to those discussions.
316	38835	30	20	16	0	17	Arístegui et al, 2011 present results about decreasing trends in chlorophyll in many areas of the Canary Current system, associated to a rapid warming bgetween 1998-2007. Also the authors shows a decreasing trend in landings os several commercial fish species. I suggest to revise this reference (Ricardo Anadon, University of Oviedo)	We take these comments on board and have revised the text.
317	36891	30	20	17	20	18	In contrast to the mentioned conclusions of Aristegui et al. (2009) decadal increase in primary production measured in situ has been shown for the northern limit of this upwelling ecosystem (Bode et al., 2011; Chávez et al., 2011). See complete references in comments to Chapter 30, p. 20, l.12-19 and Ch. 6, p. 49-50 (Antonio Bode, Instituto Espanol de Oceanografia)	We have taken these comments on board and have woven into the text. The Bode study is outside the region defined as the Canary Current (is part of the HLSB systems - section 30.5.1
318	46613	30	20	17	20	19	This would seem to be a key finding. (Neville Smith, Bureau of Meteorology)	This result has been retained and is discussed in the context of long term variability plus comments from various reviewers.
319	52023	30	20	28	20	28	It would be helpful to clarify the source of the "present" statistic, given that the citations at the end of the sentence do not span to the present time. (Katharine Mach, IPCC WGII TSU)	We do not really understand the reviewer's comments since Hutchings et al 2009 covers fisheries statistics from 1950 to 2006 - and support the statement statistics.
320	49766	30	21	9	21	9	Minor: This definition of the latitudinal range of the California Current differs from what is noted in Figure 30-1 where the region appears to extend to southern Mexico. (Ryan Rykaczewski, Princeton University)	We have adjusted figure 30 – 1 but point to the fact that the California current LME includes Baja California.
321	45596	30	21	12	21	12	Please define "long-term" in this context. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	Since 1950. We have modified text.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
322	49767	30	21	26	21	27	The Bakun hypothesis is significant and applicable to all Eastern Boundary Current systems. I feel that reference to it, with a brief explanation, should be made with regard to all upwelling systems. That could be done either at the beginning of Section 30.5.2 or saved until Section 30.8.1. The line here in reference to the California Current and ENSO seems out of place. (Ryan Rykaczewski, Princeton University)	Bakun's hypothesis is mentioned in the introduction to the EBC section - and there is a specific box exploring the pros and cons to intensification of upwelling versus the alternative.
323	46614	30	21	42	0	0	Ainley et al is hardly the reference I would go to for La Nina/El Nino.. Also … change IN ECOSYSTREM RESPONSE may be … (Neville Smith, Bureau of Meteorology)	We have replaced Ainley with a more appropriate reference.
324	52024	30	22	24	0	0	Section 30.5.2.2. For key findings throughout this section, the author team should consider using calibrated uncertainty language to characterize its degree of certainty in the conclusions. Where the term "likely" is used (lines 28, 33, 39, 41, 47, 49), if the usage is per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized; casual usage of this reserved likelihood term should be avoided. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
325	38836	30	22	31	0	33	I aggree with the modest rate of temperature increase predicted by GCM indicated in the paragraph. The actual rate of increase detected by Arístegui et al, 2011 and Gonzalez-Taboada and Anadón, 2012 was much higher. Probably in the near future this rate increased faster. With the well know constrains of the AOGCM models, I suggest to incorporate a new phrase to explain better the different result obtained from actual observations and with modelled projections to the future, with the aims to avoid misinterpretation for non specialist peoples (Ricardo Anadon, University of Oviedo)	We have considered this issue and have modified text accordingly.
326	46615	30	22	31	22	35	Shoould comment on the skill (or otherwise) of GCCMs in simulating EBC regions. (Neville Smith, Bureau of Meteorology)	We have discussed the efficacy of the climate models to match observations now within the text. With the comment that high resolution circulation models are needed.
327	49768	30	22	37	22	37	Reference to the Bakun hypothesis seems out of place without a more complete explanation. See my comment on Ch. 30, page 21, line 26. (Ryan Rykaczewski, Princeton University)	Bakun (1990) proposed that upwelling will intensify under the anthropogenic climate warming - we respectfully disagree with the reviewer and feel that this reference is correct.
328	49769	30	22	42	22	42	The consequences here are not limited to oxygen. A decrease in pH is also a consequence of increased respiration of organic material that may occur as a result of increased productivity or decreased ventilation of the water mass. (Ryan Rykaczewski, Princeton University)	Good point - text reads: At the same time, upwelling waters also have high concentrations of CO2 as well as low pH and reduced concentrations of oxygen, trends that are likely to increase as atmospheric CO2 increases {Feely, 2008 #23583}{Gruber, 2011 #22257}.
329	43259	30	23	1	25	53	Similar problems albeit to a lesser degree prevail for the treatment of Western Boundary Systems. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	Correct. We discuss this in the appropriate section.
330	52025	30	23	13	0	0	Section 30.5.3.1. For each subsection, the author team should consider further highlighting key assessment findings, also with use of calibrated uncertainty language to characterize the author team's degree of certainty in the findings. Such highlighting of key findings would provide a nice complement to section 30.5.3.2. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
331	46616	30	23	15	23	22	The use of western boundary systems spanning the equator is awkward in places. (Neville Smith, Bureau of Meteorology)	We respectfully disagree. There is confusion with respect to our sub regions being physical regions, not ecosystem regions
332	52026	30	23	36	23	39	The 1st statement in this paragraph implies that increasing sea temperatures in this region have been formally attributed to anthropogenic climate change. If this is the case, have the changes described on lines 38-39 also been attributed to climate change? Additionally, "high confidence," as calibrated uncertainty language on line 36, should be italicized. (Katharine Mach, IPCC WGII TSU)	Because of global warming (AGW), the meridional differences of land-sea temperature in monsoon area,e.g., Asian monsoon region become smaller, which lead East Asian monsoons weakening.The weakening monsoon is one of main reasons for increasing sea surface temperature in ECS and its adajcent waters. Hence, the changes in SST are closely linked to the weakening EAM. Furthermore,the warmth of the western boundary current, e.g., Kuroshio, is also one of main reasons of increasing SST in the region. The reason of the increasing warmth of Kuroshio might be due to AGW, but it need further exploring in future owing to limited observations. Therefore, the increasing SST in this region could be partly attributed to ACC. We change the "high confidence" to "medium confidence".
333	47341	30	23	44	23	45	How can community structure "decline"? (James Christian, Government of Canada)	We agree and have modified the sentence accordingly. The sentence now reads: "There is robust evidence that primary productivity and biomass yields are declining, and that fish community structure is changing rapidly within the Bohai/Yellow Sea and ECS [Lin et al., 2005; Tang et al., 2003; Tang et al., 2009]. "
334	52027	30	23	44	23	45	"Robust evidence" on line 44, as calibrated uncertainty language, should be italicized; the author team might also consider assigning a summary term for agreement. On line 45, it would be preferable to indicate more specifically how the decline has been "rapid" or to delete the term "rapidly." (Katharine Mach, IPCC WGII TSU)	The change has been made - calibrated language italicised and the word"rapid" had been deleted.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
335	47342	30	23	52	23	53	I was at a recent presentation by Shin-ichi Uye and I thought he said that eutrophication was the primary cause of increases in Nemopilema (which is misspelled), with increasing T and decreasing O2 as secondary factors. It appears that in his published work (Plankton Benthos Res 3 (Suppl.) : 125–131, 2008) he is a bit more agnostic, but that was 5 years ago. Note that the loss of O2 in this context is caused primarily by eutrophication not AGW. (James Christian, Government of Canada)	We have corrected spelling errors and have replaced "attributed to" with "associated with".
336	46617	30	23	53	0	0	Perhaps 'associated with' rather than 'attributed'? (Neville Smith, Bureau of Meteorology)	Agree.As the above mentioned.
337	46618	30	24	5	0	0	This was a good sub-section (Neville Smith, Bureau of Meteorology)	The authors thank the reviewer for this generous comment.
338	46619	30	24	41	24	42	Over what period does the 100 mm/year apply? There has been significant interannual variability, particular in the last few years. (Neville Smith, Bureau of Meteorology)	Correct the sentence to "The sea level rising rate of 10 mm/year applied for 1993-2000".
339	42603	30	24	41	24	51	It would be better if you add the result in China sea by Zuo, J. Ch and Zhang, J. L ,et al. Reference: Zuo, J. Ch, Zhang, J. L., Du, L., Li, P. L. and Li L., 2009. Global Sea Level Change and Thermal Contribute. J. Ocean Univ. China, 8(1), 1-8. (Juncheng Zuo, Hohai university)	Agree.We added the reference: Zuo et al., 2009.
340	47343	30	24	42	24	43	I don't see how a local rate of sea level change that deviates significantly from the global mean can be meaningfully associated with AGW: it's either caused by land subsidence or it's natural variability (in this case probably ENSO, as noted by Green et al) that shows a trend due to short data records. (see also 9/17-19) (James Christian, Government of Canada)	The sea level within this region experenced a rising change for the periods of 1993-2000. We have not concluded that the changes in sea levelt should be absolutely associated with AGW ,becasue detecting the changes is chanllenge due to limited observations.But this is an important phenomena in the past decades and should be pointed out here.
341	37521	30	24	48	24	50	If using the Tanzil et al (2009) reference here then note there was NO evidence that acidity was responsible for the effect observed (Barbara Brown, University of Newcastle)	Agree. The "and acidity" had been deleted.
342	38696	30	24	49	24	51	Reword, likely due to rising SSTs and role of ocean acidification in coral growth trends is inconclusive at present. (Janice Lough, Australian Institute of Marine Science)	Agree. The "and acidity" had been deleted.
343	35920	30	25	8	0	0	Comment to “Salinity of surface waters in the Arabian Gulf has increased by 0.5-1.0% over the past 60 years (Figure 30-5D) due to increased evaporation from warming seas and contributions from the outflows of the saline Red Sea and Arabian Gulf”. Comment: I guess this refers to the Arabian Sea, not the Arabian Gulf. Also the surface salinity of the Arabian Gulf has increased, but this is not only a footprint of climate change, but also of the discharge of hecto-tons of brine from desalination plants (Sheppard et al., 2012) Note: Sheppard and Loughland (2002, Aquat Ecosyst Health Manag 5(4): 395-402) suggest an increased warming of Gulf waters from 0.2 deg C per decade to by 0.45 deg C in the last 20 years based on HadISST data. (Bernhard Riegl, Nova Southeastern University)	Agree. We have changed "Arabian Gulf" to "Arabian Sea".
344	52028	30	25	16	25	35	"likely": lines 16 and 35 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
345	52029	30	25	29	25	29	With the phrase "mass" coral bleaching is used here and elsewhere in the chapter, is very commonly accepted usage standard for defining the magnitude of bleaching associated with "mass" bleaching? (Katharine Mach, IPCC WGII TSU)	The statement refers to large numbers of corals bleaching as opposed to one bleaching here or there - it has a well-established meaning within the scientific literature.
346	52030	30	25	35	25	35	"High confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
347	47344	30	25	47	25	49	Does this make sense? How can aggregating 2 regions reduce the rate in both? (James Christian, Government of Canada)	It requires explanation and is due to the different sizes of the two areas. We have removed this part of the sentence as it is confusing and does not have too much to the discussion.
348	41267	30	26	0	0	0	Section 30.5.3.2 does not included the change of heat transportation of the western boundary currents. The transport of the western boundary currents are very important to climate and ecosystem since the heat transport is very large. Additionally, tropical cyclones will severly affect on the western boudanry region. This issue must also be discussed. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We have talked about the increasing warmth of the Kuroshio Currents, and its associated with the changes in sea surface temperature in ECS and its adjcent seas.(Page 23, line 35-40 of Ch30 FOD). In addtion, tropical cyclones indeed severely affect on the western boundary region. We have also talked a little about the changes of storms (Page 10, line 6-12 of Ch 30 FOD). However, we still don't fully understand the impacts with the changes of storms. It may need much exploring.
349	52031	30	26	7	26	42	"likely" on lines 7, 32, 37, 38; "unlikely" on line 42 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
350	52032	30	26	24	0	0	Section 30.5.3.2. For key findings throughout this section, the author team should consider using calibrated uncertainty language to characterize its degree of certainty in the conclusions. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
351	46620	30	26	30	0	0	increase the vulnerability of coastal communities' The latter would seem out of scope. In any case, I think the vulnerability does not change but the likelihood of hazardous events does rise. (Neville Smith, Bureau of Meteorology)	I.e. think it remains in scope as we are talking about the consequences of changes in the ocean on coastal communities. We link to chapter 29 and other coastal chapters at this point. We'll have a look at to find out if there is a reason to reduce its. [S. Jung] Added chapter 30 as reference, and left a note to check later.
352	45597	30	26	47	26	47	Please define DHM. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	It was already defined in Page 8, Line 44
353	46621	30	27	8	0	0	For clarity, I assume the 'warm pool' refers to the > 28C water of the western Pacific, not to the warmer waters flanking the cold tongue to the north and south? As noted previously, the equatorial upwelling system also embraces the NEC/NECC (a zone of convergence) as well as the SECC/SEC systems (also convergent zones). These are also areas of instability which (by my understanding) are important for productivity. (Neville Smith, Bureau of Meteorology)	We respectfully disagree. We believe this should be clear to most people.
354	47345	30	27	8	27	10	Not sure what this means: there is some elevated productivity associated with the convergence zone but the vast majority of productivity in the equatorial zones is associated with Ekman upwelling especially in the "cold tongue". (James Christian, Government of Canada)	The reviewer is correct. We have resolved this confusion and have modified the text accordingly.
355	46622	30	27	19	0	0	"variability" rather than 'patterns' (Neville Smith, Bureau of Meteorology)	We have changed the text accordingly.
356	52033	30	27	23	0	0	Section 30.5.4.1. It would be helpful to clarify why separate subsections are not provided here--is there less literature available for these systems? (Katharine Mach, IPCC WGII TSU)	The reason for the lack of subsections here is due to the fact that there are really only two equatorial upwelling systems - and our knowledge of them is pretty limited. Hence, try to split them up into two separate systems was not likely to be very useful. we suggest leaving it as the structure as it is. we have added a short sentence to explain this.
357	42604	30	27	25	27	32	It would be better if you add the research of the team of us .The SST rises in west and falls in east in recent 20 years while in recent 50 years the SST rises in east and falls in west. (Juncheng Zuo, Hohai university)	We thank the reviewer for the comment. We believe we have covered this aspect.
358	36013	30	27	25	27	47	I think expansion of equatorial OMZs should be included (Stramma et al Expanding Oxygen-Minimum Zones in the Tropical Oceans SCIENCE VOL 320 2 MAY 2008). Also Stramma et al 2011: 4 DECEMBER 2011 | DOI: 10.1038/NCLIMATE1304) suggest this will reduce habitat for pelagic fishes. See also Prince, E. D. & Goodyear, C. P. Hypoxia-based habitat compression of tropical pelagic fishes. Fish. Oceanogr. 15, 451–464 (2006) (Frank Whitney, Institute of Ocean Sciences)	Note - we have done so. Text reads: Other risks include the expansion of hypoxic conditions and associated dead zones in many parts of CBS. Given the impacts on coastal ecosystems and fisheries, these changes increase the vulnerability of coastal communities throughout the CBS.
359	41268	30	27	27	0	0	The reference of El Nino Modoki is Ashok, K. and S. K. Behera, S. A. Rao, H. Y. Weng and T. Yamagata, 2007, El Nino Modoki and its possible teleconnection, Journal of Geophysical Research Oceans, 112, doi: 10.1029/2006JC003798. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We have added this reference.
360	46623	30	27	47	0	0	AGW? (Neville Smith, Bureau of Meteorology)	This has been removed and replaced with anthropogenic global warming.
361	45598	30	27	47	27	47	Please define AGW. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	This has been removed and replaced with anthropogenic global warming.
362	41269	30	27	50	0	0	Section 30.5.4.2 must include tuna fishing issues although it is argued in the Subtropical Gyres. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Have included a sentence makes this link
363	52034	30	27	50	0	0	Section 30.5.4.2. For key findings in this section, the author team should consider using calibrated uncertainty language to characterize its degree of certainty in the conclusions. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
364	47346	30	27	52	27	53	I find this statement hard to support because we are still struggling to determine whether climate models even simulate ENSO accurately much less whether future projections can be trusted regarding changes in its frequency and amplitude. The idea that ENSO will continue in its present form, superimposed on whatever AGW trend exists, remains a reasonable null hypothesis but does not derive from model projections as stated here. (James Christian, Government of Canada)	We have taken this comment on board and have replaced "unanimously project" with "indicate".
365	46624	30	27	52	27	54	Climate models broadly indicate that ENSO will continue to be a major source of oceanic variability … [DELETE] albeit most probably in a modified form [there is no evidence for that]. Also delete the same thing in (). (Neville Smith, Bureau of Meteorology)	Change adopted.
366	47347	30	28	9	28	11	Extremely vague. There must be a better way to say this. (James Christian, Government of Canada)	We have modified the text become more direct and have reduced the vagueness of the previous text.
367	47348	30	28	16	28	17	"due to the Coriolis effect" Actually practically every ocean circulation process of interest is due to the Coriolis effect: Eastern Boundary Current upwelling, equatorial upwelling, Western Boundary Current meandering etc. (James Christian, Government of Canada)	We make mention of the Coriolis effect to explain the circulation patterns in this particular case. We see no need to change the text.
368	36302	30	28	25	0	0	Figure 30-12 Add description of each plot of B and C (Sanae Chiba, JAMSTEC)	We have done so.
369	45599	30	28	25	0	0	In the figure 30-12 legend there seem to be two panels named B, and B and C seem to be switched in the figure. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have done so.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
370	38837	30	28	26	0	27	Longhurst discuss (Ecological geography of the sea, 2nd ed. 2007) the asignation of the expansion of the oligotrophic low chlorophyll areas descrived by Polovina as increasing subtropical gyre in in any of the ways that can be described, oceanic surface height or others. I suggest that this idea be qualified in a more general point of view, i.e. only expansion of low chlorophyll areas. (Ricardo Anadon, University of Oviedo)	We agree and text has been modified.
371	49770	30	28	32	0	0	Section 30.5.5.1. It would seem prudent to note that changes over a short period of time are likely indicative of changes related to natural modes of variability and not anthropogenic climate change; especially if time series are initiated during the largest El Nino events in the last century. (Ryan Rykaczewski, Princeton University)	We agree and text has been modified.
372	43260	30	28	32	28	50	Here and elsewhere, the certainty in satellite observations of chlorophyll is much less than anticipated in the present writing. These uncertainties have been discussed in chapter 6. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We have modified the text to indicate this and have provided a link to chapter 6.
373	37278	30	28	35	0	0	Fig. 30-12 Mislabeled panels B. Shows changes in SST, but is labeled “Time series of anomalies in chl-a” C. shows changes in chl-a, but is labeled “Sea Surface Temperature SST for subtropical gyres (Erica Head, Fisheries and Oceans Canada)	We thank the reviewer and the text.
374	46625	30	28	39	0	0	A 13 year record is too short to remove interannual variability. (Neville Smith, Bureau of Meteorology)	Agreed - text has been modified to indicate the problems associated with short time records. Text now reads: "Some uncertainties remain associated with satellite detection methods (Ch 6) and the shortness of records relative to longer-term patterns of climate variability. "
375	52035	30	28	46	28	50	"High confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language throughout the manuscript and have addressed these issues.
376	36014	30	28	49	28	50	Some model results suggest ventillation could increase in the subtropics, I believe because warming will be countered by increased evaporation (increased salinity). E.g. Oschilles et al GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 22, GB4008, doi:10.1029/2007GB003147, 2008; Gnanadesikan, A., J. L. Russell, and F. Zang (2007), How does the ocean ventilation change under global warming?, Ocean Sci., 3, 43–53 (Frank Whitney, Institute of Ocean Sciences)	The paper studies how the thermohaline circulation could be enhanced by the salinity changes, but that implyes other processes outside the subtropical gyres and does not impact the increased stratification in the subtropical gyres due to temperature increase of the surface layers. ACTION: No change
377	46626	30	28	50	0	0	Does the "high confidence" derive from WG 1? Need to be clear if it does, otherwise evidence is lacking. (Neville Smith, Bureau of Meteorology)	Agreed and we have added a stronger link to WG1 and have removed "(high confidence)" for the time being.
378	46627	30	29	1	0	0	gyres not gyre (Neville Smith, Bureau of Meteorology)	Corrected
379	52036	30	29	1	0	0	Section 30.5.5.1.1. All calibrated uncertainty language used in this section should be italicized. Please note that the phrase per the uncertainties guidance for authors is "medium confidence," rather than "moderate confidence." Additionally, all usages of "likely" should be checked to ensure they are per the uncertainties guidance for authors (reflecting a probabilistic basis for their assignment); casual usage of this reserved likelihood term should be avoided. Relevant terms to consider include: "likely" on line 6, 15, 47, page 29; "moderate confidence" on line 11, 16, 23, 45, page 29; "high confidence" on line 20, page 29, and on line 9, 11, 15, page 30; and "high agreement" on line 12, page 30. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
380	46628	30	29	6	29	7	Statements of this type have been made numerous times through this assessment. (Neville Smith, Bureau of Meteorology)	Agreed, but it is an important regional characteristic that needs to be included in each relevant section.
381	46629	30	29	12	29	13	TCs do not go to 40S (not yet anyway!). (Neville Smith, Bureau of Meteorology)	We have changed the text to reflect this important point. Text now reads: "Tropical cyclones are prominent in the Pacific (particularly the western Pacific), and CBS sub-regions between 10°-30° north and south of the equator, although the associated storm systems may reach higher latitudes."
382	38697	30	29	27	29	53	Simplify - many of the details regarding the mechanisms of coral bleaching should have already been covered in earlier chapters and the focus here should be on literature that focuses on observed bleaching events in the Pacific subtropical gyre. (Janice Lough, Australian Institute of Marine Science)	We have reduced the text here, and have made explicit linkage to the box on coral reefs and climate change in chapter 5.
383	52037	30	29	32	29	33	It would be helpful to clarify if this statement refers to changes since 1980. (Katharine Mach, IPCC WGII TSU)	We have added a sentence to help here: "There are few if any scientific records of mass coral bleaching and mortality prior to this period {Hoegh-Guldberg, 1999 #24}. "
384	46630	30	29	47	29	53	Seems out of scope here (Ch 5). (Neville Smith, Bureau of Meteorology)	We respectfully disagree and feel that this remains in scope for reasons which we talk about already. As indicated above, we have reduced overlaps and have increased the reference to the cross chapter box on coral Reef ecosystems.
385	47349	30	29	50	29	50	check unit: this doesn't seem very fast for a horizontal migration (James Christian, Government of Canada)	These are the numbers reported by Gilman et al 2007, however.
386	49771	30	30	1	30	16	Dr. Elliott Hazen of the University of Hawaii has recently submitted (which has been accepted) that addresses changes in habitat of top predators in the North Pacific with climate change. My impression is that shifts are generally poleward in response to a shift in the location of the subtropical convergence zone. Consider contacting him at Elliott [-DOT-] hazen [-AT-] noaa [-DOT-] gov if such a note would be applicable here. (Ryan Rykaczewski, Princeton University)	We thank the reviewer .Have obtained references - they are highly relevant and we have modified the text accordingly.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
387	52038	30	30	19	0	0	Sections 30.5.5.1.2, 30.5.5.1.2, and 30.5.5.2. All calibrated uncertainty language used in these sections should be italicized. Please note that the phrase per the uncertainties guidance for authors is "medium confidence," rather than "moderate confidence." Additionally, all usages of "likely" should be checked to ensure that they are per the uncertainties guidance for authors (reflecting a probabilistic basis for their assignment); casual usage of this reserved likelihood term should be avoided. Along these lines, please check page 30, lines 26, 30, 36, and 44; page 31, lines 6, 9, 14, 22, 24, 25, 28, 30, 31, 33, 43, 44. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
388	46631	30	30	24	30	26	Why attach "high confidence" to a finding from a single event and from one study? (Neville Smith, Bureau of Meteorology)	The Western Indian ocean event was documented over a very wide area and across a number of research groups. The mechanism and models behind temperature stress are extremely well-known. Overall, the statement that "Coral reef ecosystems in the Indian Ocean gyre system were heavily impacted by record positive sea temperature anomalies seen in the southern hemisphere February-April 1998 [Ateweberhan et al., 2011] (high confidence)." We have are a few more references to strengthen the statements. Therefore, we are confident that our assessment is correct. However, we are assessing and calibrating our assessment language throughout the chapter - which has resulted in further modifications here.
389	46632	30	30	42	30	44	Similar issue here. This is an IPC climate change assessment; why attach a confidecne level to a particular event? (Neville Smith, Bureau of Meteorology)	See above.
390	46633	30	30	52	0	0	this is more about sensitivity to temperature variability than vulnerability to climate change. (Neville Smith, Bureau of Meteorology)	We respectfully disagree with this statement - yes, corals are sensitive to extreme events today. The combination of rising background sea temperatures together with extreme events means that corals become more vulnerable as anthropogenic climate change increases the risk of larger and larger extreme temperatures in the summer months.
391	46634	30	31	4	31	5	Is salinity included in these studies? In any case quite short to be making conclusions, and problematic whether there is anything to attach a 'virtually certain' to other than the fact we can now measure SST!. (Neville Smith, Bureau of Meteorology)	We have included reference to this issue in the text.
392	36892	30	31	4	31	16	(see also comment to Ch. 6, p. 31, l.26-28). Fixation of atmospheric N in the tropical and subtropical occeans will likely gain more importance in warmer and stratified oceans (e.g. Sohm et al., 2011). Recent field studies point also to a growing fraction of primary production derived from atmospheric N in large ocean basins (e.g. Mouriño-Carballido et al., 2011). Additional references: Mouriño-Carballido, B., R. Graña, A. Fernández, A. Bode, M. Varela, J. F. Domínguez, J. Escánez, D. De Armas, and E. Marañón, 2011: Importance of N2 fixation vs. nitrate eddy diffusion along a latitudinal transect in the Atlantic Ocean. Limnol. Oceanogr., 56, 999-1007. Sohm, J. A., E. A. Webb, and D. G. Capone, 2011: Emerging patterns of marine nitrogen fixation. Nat. Rev. Microbiol., 9, 499-508. (Antonio Bode, Instituto Espanol de Oceanografia)	We have added a couple of sentence to capture this aspect of the subtropical gyres.
393	46635	30	31	5	0	0	strength" rather than velocity. (Neville Smith, Bureau of Meteorology)	Text has been modified and now refers to "strength".
394	46636	30	31	14	31	16	How can this sentence be reconciled with the high-confidence attached to the previous finding? (Neville Smith, Bureau of Meteorology)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
395	41270	30	31	19	0	0	Section 30.5.5.2 does not include the issue of current speed of the gyres. Many of small pelagic fish spawns in the upstream of the subtropical gyres and spread out to the offshores and the current speed change is important issue for their recruitment (e.g. Pacific saury: Ito S., H. Sugisaki, A. Tsuda, O. Yamamura and K. Okuda, 2004, Contributions of the VENFISH program: meso-zooplankton, Pacific saury (Cololabis saira) and walleye pollock (Theragra chalcogramma) in the northwestern Pacific, Fish. Oceanogr., 13. Suppl. 1, 1-9) (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Text has been modified to include discussion of the speed of the gyres.
396	46637	30	31	21	0	0	have been subject to' rather than 'are sensitive to' (Neville Smith, Bureau of Meteorology)	We respectfully disagree - our reference is the responsiveness of the gyre systems. We have changed the word "sensitive to" to "are responsive to".
397	47350	30	31	21	31	22	It isn't universally true that greater stratification leads to lower primary production (e.g., Karl et al 1995 Nature 373: 230; Karl et al 1996 Deep-Sea Res II 43: 539). Chapter 6 has a graphic that shows upward trends at several subtropical stations over the last few decades (their Figure 10). (James Christian, Government of Canada)	I don't think this overturns the observation of the expanding subtropical gyre systems. But we have now included linkages to chapter 6 and its discussion of chlorophyll.
398	46638	30	31	21	31	48	I would have like to see the "adaptation limit" concept (Chp 16) used here. (Neville Smith, Bureau of Meteorology)	Have included reference to adaptation limit concept and chapter 16.
399	47351	30	31	29	31	29	"consistent with the current emission pathway" Aren't all of the RCPs consistent with the current emission pathway? (James Christian, Government of Canada)	This is an important point - we have modified the text: "At the other end of the spectrum, subtropical gyre systems are likely to be 2.18-3.23°C warmer by the end of the century if a reduction in the current growth in emissions does not occur."

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
400	52039	30	31	32	31	33	For this statement, it would be helpful to clarify the phrase "primary driver"--primary driver of what exactly? Also, it would be helpful to further clarify the mechanism through which oligotrophic would increase. (Katharine Mach, IPCC WGII TSU)	Text has been made clearer. Text now reads: "If the primary driver of water column stratification and nutrient loss is the underlying increase in sea temperatures, then it is likely that the world's most oligotrophic ocean sub-regions will continue to expand over coming decades with consequences for important ecosystem services such as gas exchange, fisheries and carbon sequestration. "
401	52040	30	31	36	31	38	It would be helpful to specify the time frame for this projected outcome. (Katharine Mach, IPCC WGII TSU)	See above: Text now reads: "If the primary driver of water column stratification and nutrient loss is the underlying increase in sea temperatures, then it is likely that the world's most oligotrophic ocean sub-regions will continue to expand over coming decades with consequences for important ecosystem services such as gas exchange, fisheries and carbon sequestration."
402	46639	30	31	44	0	0	"rainfall flux" is precipitation. (Neville Smith, Bureau of Meteorology)	Agreed. We have modified the text accordingly.
403	47352	30	31	46	31	48	"While much of the sub-tropical gyres will experience drying conditions, large increases in rainfall are projected around these regions." I can't tell what this means. (James Christian, Government of Canada)	Agreed. We have cleared up the ambiguity in the text.
404	41271	30	32	0	0	0	Section 30.5.6. only argued on five seas. I could not understand the reason why the five seas were selected. There are many other semi-enclosed seas well studied e.g. Labrador Sea, Hudson Bay, Baffin Bay and Sea of Japan. For example, in the Sea of Japan, physical and chemical change was studied by Jenkins (2008) and zooplankton change was studied by Chiba et al. (2005) and fish responses were argued in Masuda (2008), Tian et al (2012) and Seo et al (2011). Jenkins, W.J. (2008) The biogeochemical consequences of changing ventilation in the Japan/East Sea, Marine Chemistry, 108, 137-147. Chiba, S., Y. Hirota, S. Hasegawa, and T. Saino (2005) North-south contrasts in decadal scale variations in lower trophic-level ecosystems in the Japan Sea. Fisheries Oceanography, 14, 401-412. Masuda, R. (2008) Seasonal and interannual variation of subtidal fish assemblages in Wakasa Bay with reference to the warming trend in the Sea of Japan., Environmental Biology of Fishes, 82, 387-399. Tian Y., H. Kidokoro, T. Watanabe, Y. Igeta, H. Sakaji, S. Ino (2012) Response of yellowtail, Seriola quinqueradiata, a key large predatory fish in the Japan Sea, to sea water temperature over the last century and potential effects of global warming., Journal of Marine Systems, 91, 1-10. Seo, H., H. Kudo, and M. Kaeriyama (2011) Long-term climate-related changes in somatic growth and population dynamics of Hokkaido chum salmon, Environmental Bioology of Fishes, 90, 131-142. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	The answer to this is that we use a very strict definition of semi-enclosed seas - as outlined in the text within this chapter. However, these other more connected yet constrained seas are included as part of the other systems. Note that Hudson Bay could meet the demands of our definition, but Hudson Bay is part of the polar regions chapter (Ch28).
405	41272	30	32	0	0	0	Section 30.5.6 does not iclude the Okhotsk Sea which is the southern limit of sea ice area and most vulnerable for the climate change. It must be included. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Unfortunately the Okhotsk Sea does not meet our definition for semieclosed seas. Okhotsk Sea is identical to the Berings Sea, and besides it will be considered also in Ch28.
406	44432	30	32	9	32	9	Should "sorghum" be changed to "SORGHUM". The sentence makes more sense if it is changed in this way. (Tae sung Cheong, National Disaster Management Institute)	We don't understand this comment as we have never had the word "sorghum" in our manuscript.
407	47353	30	32	12	32	13	"Decreasing aragonite saturation states pose serious risks to marine calcifiers such as coccolithophores" coccolithophores are calcitic (James Christian, Government of Canada)	Have modified text: "Decreasing carbonate ion situations and saturation states pose serious risks to marine calcifiers such as coccolithophores"
408	47354	30	32	22	32	23	I don't see how "geography dictates" political boundaries between states. Maybe this is the current reality but it's hardly an inevitable effect of coastline geometry. (James Christian, Government of Canada)	Good point - text has been modified
409	52041	30	32	27	0	0	Section 30.5.6.1. All calibrated uncertainty language used in this section should be italicized. Please note that the phrase per the uncertainties guidance for authors is "medium confidence," rather than "moderate confidence." Additionally, all usages of "likely" should be checked to ensure that they are per the uncertainties guidance for authors (reflecting a probabilistic basis for their assignment); casual usage of this reserved likelihood term should be avoided. Along these lines, please check page 32, lines 32, 39, 41, 52, 54; page 33, lines 2, 21, 26, 29, 48; page 34, lines 3, 11, 17, 18, 36, 49, 51; page 35, lines 1, 8, 12, 13, 14, 19, 27, 42, 45, 47, 48. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
410	47355	30	32	33	32	35	Comparing apples and oranges: a change of 0.65K over 60 years implies a lower rate than recent observations but it isn't "lower" than a rate of 0.6K/10y. (James Christian, Government of Canada)	We are confused by this comment and do not understand what the reviewer is referring to.
411	46640	30	32	34	32	35	Again, the discrepency between the HadISST result over 60 years and the change quoted for a short period needs explanation. (Neville Smith, Bureau of Meteorology)	We have checked these numbers and have made some adjustments and have discussed the discrepancy now.
412	47356	30	32	40	32	40	"invertivores"? I don't think I have ever heard this term before. (James Christian, Government of Canada)	Despite this being well used term, we have replaced it with "invertebrate feeding". Text now reads: "These changes to coral reefs have resulted in a loss of fish abundance and diversity, fish species depending on invertebrates for food decreasing in abundance while herbivore and planktivore abundances have increased {Riegl, 2002 #408}, moderate confidence). "

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
413	52042	30	32	41	32	42	The author team might consider indicating here the role of other drivers, not related to climate or climate change. (Katharine Mach, IPCC WGII TSU)	We have modified the text and have incorporated exist to this by referring to increasing pressures from non-climate related disturbances.
414	46641	30	32	52	0	0	I think we have moved on from attaching confidence to a temperature measurement. (Neville Smith, Bureau of Meteorology)	We have removed "high confidence"
415	46642	30	33	1	33	2	What are "these trends"? (Neville Smith, Bureau of Meteorology)	We have modified text to make what we mean by "these trends" more explicit.
416	52043	30	33	1	33	3	As appropriate, it would be helpful to indicate the relevant climate/socio-economic scenario for the statement. (Katharine Mach, IPCC WGII TSU)	We do not understand what the reviewer is getting at, here.
417	35921	30	33	8	0	0	Insert: "Long-term monitoring of coral community structure and size over 20 years has shown that between 1988 and 2008, average coral size has declined [high confidence] and species latitudinal limits may have changed [moderate confidence]. The decline in average coral size is ascribed to significant heat-mediated bleaching in 1998, and again in 2010 (Riegl et al 2012). The patterns of this decline correlate well with the pattern of heating in the Red Sea (Raitsos et al 2011), i.e. the biggest changes are seen in the southern part of the Red Sea." Source: Riegl BM, Bruckner AW, Rowlands GP, Purkis SJ, Renaud P (2012) Red Sea Coral Reef Trajectories over 2 Decades Suggest Increasing Community Homogenization and Decline in Coral Size. PLoS ONE 7(5): e38396. doi:10.1371/journal.pone.0038396 (Bernhard Riegl, Nova Southeastern University)	We thank the reviewer for this edition. We have included text and reference to this paper and the general conclusions.
418	52044	30	33	8	33	9	It would be helpful to clarify the mechanisms of this study--in situ or other? (Katharine Mach, IPCC WGII TSU)	We have done so in the text.
419	46643	30	33	18	0	0	It is debatable (given the pressure on pages) whether space can be devoted to individual seas. (Neville Smith, Bureau of Meteorology)	We respectfully disagree. These areas are extremely important in terms of the number of countries that are involved. Separating the seas out also matches how we handle different parts of the regions so discussed - we have also been told that our current length is acceptable for chapter 30 - which has an enormous job in terms of integrating many sources of information. Ensuring that it has original context is very important to countries that border on each of our oceanic subregions. Lastly, an individual focus on each of these seas provides a template for understanding other parts of the ocean that may not have got the focus with given these particular seas.
420	47357	30	33	18	0	0	Section 30.5.6.1.3 The assertion that "short-term sea surface temperature variability (week-month) appears to be strongly influenced by interactions with the overlying atmosphere and other aspects of this highly landlocked sea" is so vague as to be almost meaningless. "Freshwater discharge from rivers draining into the Black Sea remained more or less constant" should state over what time period. I have a hard time understanding how environmental changes could have "exacerbated low oxygen conditions in the deeper parts of the Black Sea" since these are already anoxic. (James Christian, Government of Canada)	We have revised the text to make this clearer.
421	43261	30	33	18	34	18	The section on Back Sea and Baltic remains largely sectoral and could be more comprehensive considering recent literature: CIESM, 2010. Workshop monographs 39, Climate forcing and its impacts on the Black Sea marine biota. ... or Baltic Sea: Climate Impacts on the Baltic Sea: From Science to Policy,. Springer Earth System Sciences, DOI 10.1007/978-3-642-25728-5_4, The BACC author team, 2008: Assessment of Climate Change in the Baltic Sea Basin., Springer Verlag Berlin - Heidelberg; ISBN 978-3-540-72785, 473 pp (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We are required to use primary peer-reviewed literature where possible - nonetheless, we have investigated this literature and have cited accordingly.
422	46644	30	33	20	0	0	Yet another unexplained difference with HadISST. "No confidence" might be the better label. The authors are getting a little generous with the use of "high confidence" (here and over the following pages). (Neville Smith, Bureau of Meteorology)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
423	52045	30	33	23	33	32	For the statements on lines 23-24 and on lines 26-29, it would be helpful to specify the relevant timeframes. Additionally, on line 32, it would be beneficial to clarify if the "environmental perturbations" include climate change. (Katharine Mach, IPCC WGII TSU)	We have modified the text accordingly.
424	37279	30	34	1	34	18	. Repetition Paragraphs 1 and 2 show some repetition (Erica Head, Fisheries and Oceans Canada)	The first paragraph deals mainly with non-climate related changes while the second touches on the subject of the combination of climate change induced changes as well as those coming from the dification and overfishing. We respectfully disagree that there is repetition here.
425	46645	30	34	10	0	0	The literature is a little thin for this confidence level. (Neville Smith, Bureau of Meteorology)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
426	46646	30	34	23	34	42	Why are the authors referring to a mish mash of results from shorter periods after having gone to the trouble of using HadISST as a reference? (Neville Smith, Bureau of Meteorology)	The reviewer has a good point which we implementing across our chapter.
427	52046	30	34	26	34	27	It might be helpful to clarify the logic underpinning this statement. (Katharine Mach, IPCC WGII TSU)	A modified text to make this clearer.
428	37280	30	34	37	0	0	Suggested changes - for clarity "abundance of zooplankton, decreases in fish such as anchovies, decreases in the frequency of red tides and increases in mucilage outbreaks." (Erica Head, Fisheries and Oceans Canada)	We thank the reviewer and have changed the text accordingly.
429	46647	30	34	37	34	42	I am lost here. (Neville Smith, Bureau of Meteorology)	Not sure what part that the reviewer is referring to. Will read this section and try to modify for clarity purposes accordingly.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
430	37118	30	34	38	34	39	Jordà et al 2012 paper only refers to the atmospheric component of sea level. The winter decrease in sea level that they observe is only in this component, but this does not translate into an effective decrease in the total sea level change, which also includes an important steric component. The correct quotation here should be: 'If the positive phase of the NAO is more frequent in the future [Kuzmina et al., 2005; Terray et al., 2004], then the future sea level rise may be suppressed slightly due to atmospheric changes [Jordà et al., 2012]' Note also the accent in Jordà. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	Agreed and have changed taxed accordingly.
431	45600	30	34	47	34	47	Please specify to which respect fish decrease. Is this abundance, diversity, catch potential? (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	We have done so and modified text accordingly. Inserted "stock abundance".
432	47358	30	34	50	34	50	"thermophilic species" I don't think this is proper usage of this term. (James Christian, Government of Canada)	We have done so and modified text accordingly. Inserted "stock abundance".
433	52047	30	34	50	34	54	The author team should consider indicating the time frames for these changes. (Katharine Mach, IPCC WGII TSU)	The first sentence states when the changes occurred - around 1980 - we think it is very clear and that trying to specify the time frame of records (which goes back to the 1970s) will lead to confusion for the reader.
434	46648	30	34	52	34	54	This would seem to be a key finding. (Neville Smith, Bureau of Meteorology)	Agreed - and have highlighted it.
435	46649	30	35	8	0	0	What is the "very high confidence" being attached to? The years? (Neville Smith, Bureau of Meteorology)	The linkage between a warmer climate and the tropicalisation of the Mediterranean marine fauna and flora.
436	46650	30	35	16	0	0	> decade is hardly long-term (Neville Smith, Bureau of Meteorology)	Agreed - we have now indicated that records over several decades are desirable but are rare.
437	37119	30	35	17	35	20	Touratier et al., 2012 was submitted to 'Biogeosciences', but has been rejected. Even if it wil be posted in the web as in 'Biogeosciences Discussions', this paper should not be cited in the report. The sentence 'Even the deepest Mediterranean water is contaminated by anthropogenic CO2, which places this region as one of the world's most acidified', however, could be left, as it is also a result of the published paper Tourratier and Goyet, 2011, cited in the sentence above. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	We have removed citation. We have The sentence and have added the reference.
438	52048	30	35	24	0	0	Section 30.5.6.2. For key findings in this section, the author team should consider using calibrated uncertainty language to characterize its degree of certainty in the conclusions. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
439	46651	30	35	30	0	0	Coastal run-off is not a human impact. (Neville Smith, Bureau of Meteorology)	Agreed - it is case of not necessarily so. So we have used the phrase "enhanced run-off from disturbed coastlines."
440	46652	30	35	34	0	0	risk' is more appropriate than 'vulnerability' (Neville Smith, Bureau of Meteorology)	Text has been modified accordingly.
441	52049	30	35	44	35	44	The author team might consider a more qualified or conditional framing for the "reduced mixing and ventilation." (Katharine Mach, IPCC WGII TSU)	Text has been modified accordingly.
442	35922	30	35	51	0	0	afer: ""Persian Gulf 1996 and 1998" add and in "2010 and 2011". (Bernhard Riegl, Nova Southeastern University)	Agreed, we have added the dates.
443	52050	30	36	2	0	0	Section 30.5.7. All calibrated uncertainty language used in this section should be italicized. Please note that the phrase per the uncertainties guidance for authors is "medium confidence," rather than "moderate confidence." Additionally, all usages of "likely" should be checked to ensure they are per the uncertainties guidance for authors (reflecting a probabilistic basis for their assignment); casual usage of this reserved likelihood term should be avoided. Along these lines, please check page 36, lines 16, 37, 44, 49, 52; page 37, lines 11, 13, 20, 21, 22. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
444	43262	30	36	2	37	25	The report on Deep Sea changes is one of several sections which lack a regional focus. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	(page 36, line 2 PGB – noted. But the deep ocean waters have great similarities, data are sparse, and the style chosen here reflects that reality – it is not possible at this time to parse the ocean basins separately given the space and data available.). Perhaps we can add a little granularity around deep sea vent communities etc - even if we say they are unlikely to be affected. This was also deliberate because we did not have the space and there is not enough known about regional differences in terms of climate impacts. For example, we could have looked at black smokers and those communities on the sea floor, but there's very little to say in that respect. Therefore we chose to represent deepwater habitats is a single region.
445	52051	30	36	11	36	11	Where the word "importance" is used, it would be helpful to clarify that this is important in terms of the deep-sea, presumably also in terms of understanding changes and impacts associated with climate change? (Katharine Mach, IPCC WGII TSU)	Have changed "importance" to "interest"
446	48135	30	36	27	36	40	"highly likely" is used twice. Please rephrase or replace by a likelihood statement that is consistent with the uncertainty guidance note - considering the available evidence (this requires verification and is thus not limited to an editorial change). (Philippe Marbaix, Université catholique de Louvain)	Text changed to: "Most organic matter entering the deep ocean is recycled by microbial systems at relatively shallow depths [Buesseler et al., 2007], at rates which are temperature dependent."

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
447	46653	30	36	31	0	0	Need to explain what the 'age' of deep water means. (Neville Smith, Bureau of Meteorology)	Term explained. Text now reads: "For the deep Atlantic Ocean, the mean age of deep-waters (mean time since last exposure to the atmosphere) is ~250 years; the oldest deep waters of the Pacific Ocean are >1,000 years old. "
448	52052	30	36	31	36	31	It might be clearest to specify that these ages indicate the time for which the waters have been deep waters (not the overall time that the molecules have been on planet Earth, for example). (Katharine Mach, IPCC WGII TSU)	(Note – as above)
449	47359	30	36	36	36	52	I would be careful about "overinterpreting" the Laws results. Statistically, temperature explained most of the variance in the export ratio in a cross-habitat comparison of very diverse systems ranging from equatorial to polar. That doesn't mean the relationships derived have any predictive power with respect to (comparatively small) future changes within any given system. (Also numbers given are in GtC; should use SI units.) (James Christian, Government of Canada)	We respectfully disagree – We don't have a lot of choice. I can't find any other papers that are explicit about the T dependency of the export flux. It makes good common sense too. The units of GtC could be converted to SI units, but I think the GtC is widely used in IPCC reports?)
450	45601	30	36	44	36	52	Please specify the time period over which the change of food supply to the deep sea is estimated. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	(page36, line 44 – PGB – The papers by Smith et al on the changing particle flux to the deep sea and their climate dependency are cited here. The changes in flux are hinted at already, and this goes along with declining O2 in the mid water.)
451	47360	30	36	45	36	45	"food supply from sinking particles to the deep sea is about 20% of net primary productivity" No. Export from the euphotic zone is around 20% but the vast majority of this is remineralized above 1000 m (see e.g. Karl et al 1996 op cit). (James Christian, Government of Canada)	Jim is correct. Text now reads: "Quantifying these effects is difficult since complex ecosystem responses are likely and information is sparse. Food supply to depths below the euphotic zone is about 20% of net primary productivity. The vast majority of this is recycled by microbes at depths shallower than 1,000. The net result is that with warming of the upper ocean food supply to the deep sea will decrease. If the temperature dependence of models such as that of Laws et al. (2000) is correct, then warming of the upper ocean by 2.5ºC (which is projected to occur under RCP 6.0 and 8.5; Table 30.4) could reduce the fraction exported to the deep sea by about 5%. "
452	47361	30	36	51	36	51	"a maximum reduction in food supply to the deep sea from 20% to 16% of global net primary production" or 20% of the current amount, assuming constant NPP. I can't really follow how this number was derived, but I do not believe that this is an upper limit. The export ratio could easily change by this much. And the phytoplankton will be smaller on average, so even without any temperature effect on remineralization rate, the remineralization length scale should decrease. (James Christian, Government of Canada)	We dont understand the reviewers problem here. If we have a Q10 of 2.5 then a change of 2 degrees changes rates by 20%. Do the math. He is right that may not be the maximum. How about "The T dependence of microbial reactions alone predicts a change in rates of 20% with medium confidence. The total change may well be larger due to effects on T on decreasing phytoplankton average size.")
453	41273	30	37	0	0	0	Section 30.5.7.2 does not touch with the vertical transportation of the carbon by biology and surface and deep layer interaction by zooplankton and mesopelagics. It is better to be discussed. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Agreed - have added sentence "The ocean changes in T, O2, and CO2 may also increase the vulnerability of the daily vertical migration of mid-water populations."
454	37281	30	37	6	0	0	"Consequently, two changes will have greater impact on oxygen decline than the solubility effect alone (high confidence)." Which two changes? Or should this be "Thus, increased microbial activity and reduced oxygen solubility at higher temperatures will have additive impacts on the decline of oxygen." (Erica Head, Fisheries and Oceans Canada)	Agreed - text changed to: "Thus, increased microbial activity and reduced oxygen solubility at higher temperatures will have additive impacts on the decline of oxygen (high confidence). "
455	46654	30	37	17	0	0	Does "very likely" mean > 90% probability? Perhaps need more evidence. (Neville Smith, Bureau of Meteorology)	This has been corrected as part of the overall changes to the use of calibrated terms.
456	52053	30	37	17	0	0	Section 30.5.7.2. Citations should be provided for statements in this section. (Katharine Mach, IPCC WGII TSU)	We have added citations where possible.
457	46655	30	37	23	37	25	This 'urgent need' contrasts with the surety attaached to many of the previous statements. (Neville Smith, Bureau of Meteorology)	We have deleted the word "urgent".
458	43263	30	37	30	0	0	One wonders why the respective coastal chapter 5 is referred to here but not chapter 6 that would also talk about human systems in the oceans. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	OK. We have added mention of Chapter 6)
459	47362	30	37	30	37	30	"WGI Ch 5" Is this right? Ch 5 is "Information from Paleoclimate Archives" (James Christian, Government of Canada)	We mean WG2 chapter 5 – good catch - this has now been corrected.
460	43264	30	37	30	44	14	Rather than a global treatment of economic sectors, this text, in a regional chapter, should address these WGIII aspects well integrated with WGI and II issues and specifically for the regions and the interest of individual countries and their economies as suggested above. As written the text is to some extent complementary but overlaps with the respective treatments in chapter 6. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We have reworked the text in several places to try and expand to include this point of view.
461	46656	30	37	35	37	37	Bordering on policy prescriptive. Would delete. (Neville Smith, Bureau of Meteorology)	We respectfully disagree. It is important to indicate that other factors interact with climate change in the ocean, representing both a challenge and an opportunity. We'll look at this statement and make it clearer that it's not a policy description but an opportunity et cetera.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
462	45602	30	37	36	37	36	Certainly other factors that just warming (e.g. OA and hypoxia) call for mitigation and adaptation strategies. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	Agreed - The text has been modified accordingly.
463	46657	30	37	53	0	0	can be minimised' Why? Reference? (Neville Smith, Bureau of Meteorology)	Have rewritten text to eliminate the confusion with respect to the phrase "can be minimised".
464	41274	30	38	0	0	0	Section 30.6.3.1 does not tough with the importance of spawning region of fishes. Many species has spawningn region in the shallower seas wich is influenced by anthropogenic factors. For example, sea level rise may destroy the spawning region of herrings. This kind of inreversible and critical issues must be argued. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	Have taken this nuance into account in the rewrite of the text.
465	52147	30	38	0	0	0	“Section 30.6.2 :Multiple stressors are well documented Section 30.6.3 : Economic sectors : Real life problems faced by the coastal small scale fisheries in the developing countries have been well addressed. Their problems can only be solved by the respective governments through introduction of cooperative societies (Shelley Bhattacharya, Visva Bharati University)	We thank the author for their comment.
466	38567	30	38	22	0	0	Section 30.6.3. Does deep sea carbon sequestration deserve a brief paragraph here (or elsewhere)? As the global carbon market matures, there could be increased demand for (and profit from, and ecological impacts of) attempting to sink carbon dioxide into the deep. (Christopher Harley, University of British Columbia)	We agree and have added an extra section on deep Sea Carbon sequestration. “The economic impact of deliberate CO2 sequestration beneath the sea floor has earlier been reviewed (IPCC Special Report, 2005). Active CO2 sequestration from co-produced CO2 into sub-sea geologic formations is proceeding in the North Sea, and in the Santos Basin offshore Brazil. It is likely that these activities will increase as off shore oil and gas production increasingly produces fields with high CO2 in the source gas and oil.”
467	46658	30	38	35	0	0	"less vulnerable than dependents on single species or markets" ? (Neville Smith, Bureau of Meteorology)	We have corrected the text make it clearer.
468	43063	30	38	37	38	38	This is dependent on the assumption that markets prevent switching is it not? Commercial fisheries that can adapt their targetting may have greater ability to adapt socially and economically than those tied to specific locations (and resource mixes) due to social and cultural constraints. (Beth Fulton, CSIRO Marine and Atmospheric Research)	As per previous comment - remove ambiguities, vagueness around this statement
469	38838	30	38	40	0	44	Probably one of the more interesting papers related to fisheries and climate change could be the paper of [Sherman, K. Belkin, I. M.Friedland, K. D. O'Reilly, J.Hyde, K. 2009. Accelerated Warming and Emergent Trends in Fisheries Biomass Yields of the World's Large Marine Ecosystems. Ambio, 38(4): 215-224] because anlyse different fishing areas in relation with explotation intensity and climate change. I suggest to incorporate to this section (Ricardo Anadon, University of Oviedo)	Have looked at paper and have considered including it.
470	43064	30	38	40	38	44	This generalisation is fairly gross as it will vary significantly region to region (e.g. Australia vs other places at similar latitudes) and be more skewed even within a region than the simple statement indicates (i.e. state has the potential to be misleading). (Beth Fulton, CSIRO Marine and Atmospheric Research)	The modify text to take on the involve nuances.
471	52054	30	38	41	38	45	For these statements, the author team might consider indicating its degree of certainty through use of calibrated uncertainty language per the guidelines for authors. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
472	46659	30	38	44	0	0	"they strongly suggest the fully understand .." ? (Neville Smith, Bureau of Meteorology)	Correct the language here
473	46660	30	39	1	0	0	This is potentially a key finding, so long as there is adequate evidence behind it (Neville Smith, Bureau of Meteorology)	Agreed.
474	46661	30	39	7	39	11	Better referenced to relevant section. (Neville Smith, Bureau of Meteorology)	We are exploring this.
475	46662	30	39	16	0	0	Seasonal early warning systems probably deserve some space here. (Neville Smith, Bureau of Meteorology)	We are exploring this.
476	46663	30	39	26	39	29	Bottom scouring from deep sea currents also rate a mention. (Neville Smith, Bureau of Meteorology)	Agreed. We have changed sentence to: “..from instability and bottom scouring from deep sea currents of the unconsolidated sediments …”
477	52055	30	39	30	39	32	For these statements, the author team should also consider and cross-reference the special report on extremes (chapter 3) and the working group one contribution to the 5th assessment report. (Katharine Mach, IPCC WGII TSU)	Agreed. Have included cross-references on extremes from chapter 3 and a special working group.
478	46664	30	39	32	39	33	As SREX noted, early warning systems are a powerful tool for mitigating the impactas of severe events. Improvements are likely to arrive faster than the the changes in intensity (as we experiecned with TC Yasi and Lua in Australia). TCs are likely to be more intense but also less frequent though SREX lacked confidecne in this finding. (Neville Smith, Bureau of Meteorology)	Have included linkages and concepts from SREX throughout this section.
479	37729	30	39	45	0	0	I think more information on shipping and local impacts on warming need to be included in this section. Add(?): However, increase shipping, and the concurrent increase economic activity, will increase the emission of black carbon. The black carbon will in turn increase local warming and ice-melt that further increases economic activity and shipping. .[Lack, D. A.; Corbett, J. J., 2012: Black carbon from ships: a review of the effects of ship speed, fuel quality and exhaust gas scrubbing. Atmospheric Chemistry and Physics Discussions, 12(1)3509-3554] [Levitsky, M., 2011: Black Carbon and Climate Change: Considerations for International Development Agencies. World Bank Washington, DC. 40 pp.] (George Backus, Sandia National Laboratories)	Have included text and lack a towel Lack
480	38199	30	40	7	40	7	is there any documented evidence of changes in tourist patterns being driven by perceived degradtion of ecosystems by climate change or is this all simply assumed? (THOMAS SPENCER, University of Cambridge)	We agree that this is not well understood or documented. We have added references where possible.
481	52056	30	40	7	40	43	"likely" on lines 7, 9, 43 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .

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482	52057	30	40	9	40	11	For this statement, the author team may wish to consider a more qualified or conditional framing. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
483	36015	30	40	20	0	0	perhaps cite Chapter 6 where this is discussed in more detail? (Frank Whitney, Institute of Ocean Sciences)	We have made greater linkages to chapter 6.
484	38839	30	40	25	0	26	Is not clear howw large areas in remote regions can sustain biodivversity of the ocean, and their functionality and services. In many cases tha humana action has no effect (capture of CO2 by the ocean), or has a limited effect on migrant species (protect some areas for tuna fishing -reproduction or recruitment areas - but intensively fished in other areas). Is nor clear how effienent will be to preserve oceans, but can avoid damages in pristine areas. Probably reducing fishing pressures and avoiding some fishing gear - benthic trawls- and incorporating the local fishermen will be more effective in many coastal areas. The co-management, stewardship or other suggested in literature possibilities were more effective and more realistic ideas. At this respect I suggest some references that could be very valuable for euristic discussion and analysis for the future [Worm, B. Hilborn, R. Baum, J.K. Branch, T.A. Collie, J.S. Costello, C. Fogarty, M.J. Fulton, E.A. Hutchings, J.A. Jennings,S. Jensen, O.P. Lotze, H.K. Mace, P.M. McClanahan,T.R. Minto, C. Palumbi, S.R. Parma, A.M. Ricard, D.Rosenberg, A.A. Watson,R. Zeller,D. 2009. Rebuilding Global Fisheries. Science, 325: 578-585; Castilla, J.C. and Fernández, M. (1998) Small-scale benthic fisheries in Chile: on co-management and sustainable use of benthic invertebrates. Ecological Applications 8:S124-S132; Carpenter, S.R. Folke, C. Norström, A. Olsson, O. Schultz, L. Agarwal, B. Balvanera, P. Campbell, B. Castilla, J.C. Cramer, W. DeFries, R. Eyzaguirre, P. Hughes, T.P. Polasky, S. Sanusi, Z. Scholes, R. Spierenburg, M. 2012. Program on ecosystem change and society: an international research strategy for integrated social–ecological systems. Current Opinion in Environmental Sustainability, 4: 1-5] (Ricardo Anadon, University of Oviedo)	We will consider this in our rewrite.
485	46665	30	40	26	40	28	Delete - policy prescriptive. (Neville Smith, Bureau of Meteorology)	We respectfully disagree - setting aside large areas of the ocean is an adaptation options designed to build ecological resilience. We need to talk about these things - we have added to this section.
486	46666	30	40	31	0	0	I do not understand the term 'social security' in this context. (Neville Smith, Bureau of Meteorology)	Agreed - we have modified the sentence to make this clearer, and have modified the term social security to "reduced social vulnerability".
487	46667	30	40	33	0	0	There was material in 30.5 that could lead to a HAB discussion here (Neville Smith, Bureau of Meteorology)	We have added discussion of HABS - we agree with the reviewer.
488	46668	30	40	43	40	45	Seems out of scope here. (Neville Smith, Bureau of Meteorology)	Respectfully disagree. These impacts that come from broadscale ocean changes.
489	49772	30	40	48	0	0	Section 30.6.5.2. This "Social Security" language should be changed to avoid misinterpretation. This is not a general term in the United States. (Ryan Rykaczewski, Princeton University)	Agreed, see above.
490	52058	30	41	2	41	18	"likely" on lines 2, 4, 18 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
491	52059	30	41	10	0	0	Section 30.6.6. For statements in this section, the author team should consider providing further citations. (Katharine Mach, IPCC WGII TSU)	We have rewritten the section and added references
492	46669	30	41	17	41	19	This has not been assessed here. (Neville Smith, Bureau of Meteorology)	See above
493	46670	30	41	19	0	0	What are the climate related uncertainties facing shipping? Risks there seem far less than for fishing. (Neville Smith, Bureau of Meteorology)	It says "uncertainties" not "risks". ACTION: No change
494	46671	30	41	27	41	32	These statements are too broad, and in the latter part, likely out of scope. (Neville Smith, Bureau of Meteorology)	We have rewritten this section and added detail and references
495	52060	30	41	28	41	31	The author team should ensure wording here that avoids potential interpretations of policy prescription. Additionally, "very high confidence," as calibrated uncertainty language on line 28, should be italicized. (Katharine Mach, IPCC WGII TSU)	We have rewritten this section and add references
496	52148	30	42	0	0	0	Section 30.6.7 : Excellent discussion which should be highlighted so that the present report can give a direction to the respective governments to alleviate the plight of these communities deteriorated due to climate change.” (Shelley Bhattacharya, Visva Bharati University)	We thank the reviewer - but have modified the text to make it a little bit more explicit.
497	46672	30	42	1	42	16	Very general. More review than assessing. (Neville Smith, Bureau of Meteorology)	We have modified text to improve this aspect.
498	52061	30	42	21	0	0	Section 30.7.1. For the key assessment findings presented in this section, the author team should use calibrated uncertainty language to indicate its degree of certainty in the conclusions. Additionally, the author team should provide the line-of-sight references to the sections of the chapter supporting each conclusion. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
499	46673	30	42	23	42	25	Sweeping and over the top statements. (Neville Smith, Bureau of Meteorology)	We have modified the text to be less over the top.
500	52062	30	42	49	42	49	"likely" -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
501	36016	30	42	51	0	0	...increased upwelling and reduced sea ice, whereas others may suffer due to spreading hypoxia or ocean acidification. (Frank Whitney, Institute of Ocean Sciences)	We have modified the text as suggested.
502	52063	30	43	12	43	42	"likely" on lines 12, 14, 40, 42 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	Will do as part of the general activity of going through the manuscript and checking calibrated uncertainty language where it is already used and finding places where it needs to be used.
503	46674	30	43	14	43	15	Sweeping and general (Neville Smith, Bureau of Meteorology)	We have completely rewritten section and have reduced the number of general and sweeping statements.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
504	46675	30	43	17	43	23	Delete. Far too general. (Neville Smith, Bureau of Meteorology)	We have modified the text and made it more specific, deleting pieces that are too general.
505	38840	30	43	28	0	54	In the same sense of my nexts comments, I suggest to incorporate the need of more detailed analysis of changes at regional and local scales; the aim will be to understand the appropiate scales to observe and predict scales. Now the use of satellites, metanalysis, or global scale change analysis, obscures the addecuate understanding, and produce in some cases misinterpretations at the representative scales of physical and biological processes. (Ricardo Anadon, University of Oviedo)	We have modified the text and made it more specific, deleting pieces that are too general.
506	52064	30	43	31	43	33	On line 31, is the word "described" more accurate than "undertaken"? Additionally, the author team might also consider indicating the role of drivers aside from climate change. (Katharine Mach, IPCC WGII TSU)	We have modified text accordingly and have adopted this term.
507	46676	30	43	36	0	0	Regional rather than local. (Neville Smith, Bureau of Meteorology)	Good point - modified accordingly.
508	46677	30	43	41	0	0	based on the DOCUMENTED impact .. (Neville Smith, Bureau of Meteorology)	Good point - modified accordingly.
509	46678	30	43	47	0	0	"land-cean temperature"?? Wind? (Neville Smith, Bureau of Meteorology)	We have modified the text and corrected the typo and made the meaning of the sentence more explicit.
510	37128	30	43	52	44	1	I would add two recent references regarding precedents in the geological records, and rephrase the text as follows: The impacts of ocean acidification on marine organisms and ecosystems (Figure 30-16G) has emerged as a major concern especially given the robust evidence that the current chemistry of the ocean is outside where it has been for at least 20 million years [Pelejero et al., 2010]. At current rates of atmospheric CO2 increase, the acidity of the ocean will surpass any seen over the last 40 million years [Hoegh-Guldberg et al., 2007; Raven et al., 2005, Pelejero et al., 2010]. In fact, the anthropogenic rise in CO2 and lowering of pH is occurring at a rate not seen for hundreds of millions of years, so the oceans are entering an unknown territory possibly involving major ecosystem changes [Hönisch et al., 2012]. References: [Pelejero et al., 2010] Pelejero C., Calvo E., Hoegh-Guldberg O. 2010. Paleo-perspectives on ocean acidification. Trends in Ecology & Evolution 25: 332-344. [Hönisch et al., 2012] Hönisch B., Ridgwell A., Schmidt D.N., Thomas E., Gibbs S.J., Sluijs A., Zeebe R., Kump L., Martindale R.C., Greene S.E., Kiessling W., Ries J., Zachos J.C., Royer D.L., Barker S., Marchitto Jr. T.M., Moyer R., Pelejero C., Ziveri P., Foster G.L., Williams B. 2012. The geological record of ocean acidification. Science 335: 1058-1063. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	Have added a modified version of this text to section 30.3.2.2.
511	41275	30	44	0	0	0	Section 30.7.3 does not denote the importance of comarative study and laboratry experiments. It is better to touch them. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We have added text along these lines to the relevant final sectons.
512	38698	30	44	1	44	3	I do not think that the recent scientific literature provides the necessary evidence to support the statement that observed reductions in coral calcification rates can be attrribued to ocean acidification, as yet. (Janice Lough, Australian Institute of Marine Science)	We agree and point out that the text doesn't say that there is undeniable evidence regarding the impacts of ocean acidification on calcification of corals. We point to a growing literature and recognise the degree of uncertainty we still have with respect to this aspect of the impacts of ocean acidification.
513	46679	30	44	1	44	6	References? (Neville Smith, Bureau of Meteorology)	Statement has been reorganised and references added as appropriate.
514	37129	30	44	4	44	4	I would change 'greatest' by 'earliest', which is more precise. By the end of the century, the global detrimental effects of ocean acidification could well be largest in tropical areas than in the high latitudes... (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	Agreed. Tax has been modified accordingly.
515	46680	30	44	14	0	0	"major erosion ... regulating may" Do not understand (Neville Smith, Bureau of Meteorology)	This sentence has been rewritten to be clearer.
516	52065	30	44	32	0	0	Section 30.7.3. As appropriate, the author team might consider providing references to relevant chapter sections for statements here. (Katharine Mach, IPCC WGII TSU)	We have added more references to back up the statements in this section.
517	52066	30	44	38	44	45	"likely" on lines 38, 41, 45 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
518	41276	30	45	0	0	0	I could not understand the meaning of the section 30.8 "Case Studies". It is better to declare the meaning of the case studies in the top of the section. (Shin-ichi Ito, Fisheries Research Agency, Tohoku National Fisheries Research Institute)	We believe this is clear to most people.
519	48136	30	45	8	45	8	"highly likely" needs to be replaced by a statement consistent with the guidance on uncertainty - e.g. use "very likely" if appropriate, or if not appropriate, rephrase and use a wording that does not suggest a likelihood. (Philippe Marbaix, Université catholique de Louvain)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
520	52067	30	45	11	45	11	The author team should consider the wording on this line to ensure phrasing that is not prescriptive. (Katharine Mach, IPCC WGII TSU)	We have reduced the prescriptiveness of statement
521	38699	30	45	19	45	46	Not sure how informative this "case study" is. (Janice Lough, Australian Institute of Marine Science)	We respectfully disagree with the reviewer - understanding how upwelling might vary in a warmer world is important - case study was included because there is considerable debate over what will happen to upwelling in a warmer world. We will consider this perspective.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
522	38841	30	45	24	0	26	Several other authors refers about changes in seasonality, intensity, and impacts of upwelling, not only as general intensification, if not understanding how the upwelling events occurs, their frecuency, intensity, duration, probably the key question to understand the potential impacts of such importantant hydrographic signature of the oceans [Llope, M., Anadón, R., Viesca, L., Quevedo, M., González-Quirós, R., Stenseth, N.C. 2006 Hydrography of the Southern Bay of Biscay shelf break region: integrating the multi-scale physical variability over the period 1993-2003. J. Geophys. Res. 111, C0921 (doi:10.1029/2005JC002963); A.C . Iles, T.C. Gourier, B.A. Menge, J.S . Stewart, A.J . Haupt and M.C. Lynch. 2012. Climate-driven trends and ecological implications of event-scale upwelling in the California Current System. Global Change Biology (2012) 18, 783–796, doi: 10.1111/j.1365-2486.2011.02567.x; Ar´stegui et al, 2011]. I suggest to incorporate because done differents perspectives of the problem, indeed in an clearly intensifying upwelling, the California current area. (Ricardo Anadon, University of Oviedo)	We have added extensive discussion in reference to seasonality and other long-term patterns of variability. See above.
523	38842	30	45	26	0	28	I suggest to incorporate the interesting paper of [Rykaczewski, R.R. Checkley, D.M. 2008. Influence of ocean winds on the pelagic ecosystem in upwelling regions. PNAS, 105 (6): 1965-1970 related to changes in the physical forcings like curl upwelling, as an example to the complex behaviour in the very active systems. Is necessary to understand properly each situation to project the future impacts. As example, in a in preparation paper, we encounter in the last 20 year a increased zooplankton biomass. This increase occurs at the same time that primary production first decline and then increase buit with a different seasonality. For us is a ecological surprise, unexpected result. How many unexpected results waiting for our detailed account of events during long term studies? (Ricardo Anadon, University of Oviedo)	Will incorporate this into the case study on the influence of climate change on upwelling.
524	36894	30	45	26	45	26	Reference Perez et al. (2019) does not appera listed in the Reference section: Pérez, F. F., X. A. Padin, Y. Pazos, M. Gilcoto, M. Cabanas, P. C. Pardo, M. D. Doval, and L. Farina-Bustos. 2010. Plankton response to weakening of the Iberian coastal upwelling. Global Change Biology 16:1258-1267. (Antonio Bode, Instituto Espanol de Oceanografia)	We have added this reference.
525	37130	30	45	26	45	26	I would add a reference to the assertion, including also the Iberian region, thus rephrasing the sentence as: '… while upwelling clearly weakened in the Iberian/Canary region [Pérez et al., 2010; Pardo et al., 2011]'. References: [Pérez et al., 2010] Pérez F.F., Padín X.A., Pazos Y., Gilcoto M., Cabanas M., Pardo P.C., Doval M.D., Farina-Busto L. 2010. Plankton response to weakening of the Iberian coastal upwelling. Global Change Biology 16: 1258-1267. [Pardo et al., 2011] Pardo P.C., Padín X.A., Gilcoto M., Farina-Busto L., Pérez F.F. 2011. Evolution of upwelling systems coupled to the long-term variability in sea surface temperature and Ekman transport. Climate Research 48: 231-246. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	We have Considered this within the section on the Canary current.
526	36893	30	45	26	45	27	However, despite the estimations of reduced nutrient inputs and primary production as a consequence of reduced upwelling, in situ measurements revealed decadal increases in primary production in the same region (Bode et al., 2011; Chavez et al., 2011). Indeed, Perez et al. (2010) (and also Bode et al., 2011) did not show evidences of decreasing nutrient concentrations. A mentioned later in this paragraph (l. 29-31) intense upwelling may lead to reduced primary production because of excessive turbulence, therefore a small reduction in upwelling intensity may benefit primary production nearshore (as observed in the northern limit of the Canary upwelling) . (Antonio Bode, Instituto Espanol de Oceanografia)	We have Considered this within the section on the Canary current.
527	38700	30	45	49	46	39	Again, not sure how informative this coral reef "case study" is. Section is also repetitive regarding corals and bleaching and appears to rely on new analyses (Figure 30-18) rather than recent literature (Janice Lough, Australian Institute of Marine Science)	We have reduced the amount of discussion about coral reefs and have combined our box with that of chapter 5's box on coral reefs.
528	46681	30	46	1	46	30	A lot of this is covered in the body of the assessment. (Neville Smith, Bureau of Meteorology)	Agreed, we have reduced this here.
529	52068	30	46	2	46	2	"High confidence," as calibrated uncertainty language, should be italicized. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
530	52069	30	46	3	46	20	"likely" on lines 4, 17, 20 -- If this term is being used per the uncertainties guidance for authors (reflecting a probabilistic basis for its assignment), it should be italicized. The author team should avoid casual usage of this reserved likelihood term. (Katharine Mach, IPCC WGII TSU)	As above, we have reviewed our use of calibrated language, and the associated formatting, throughout the manuscript .
531	45603	30	46	17	46	30	It should be mentioned whether, and if so, how potential impacts of OA are accounted for in these models. (Astrid Wittmann, Alfred Wegener Institute for Polar and Marine Research)	As has been discussed elsewhere, the impacts of ocean acidification on corals remains uncertain with few studies showing the field evidence. On the other hand, there is abundant experimental evidence. we have added a sentence along these lines, however.
532	49773	30	47	9	47	11	This statement seems incorrect. On the contrary, there is a lack of a global relationship between primary productivity and fish catches. See the recent paper by Friedland et al. (PLoS One, 2012) that highlights this point. (Ryan Rykaczewski, Princeton University)	"On a global scale" or should we say "on a large scale/ regional scale" it is correct. Just see our extentive table. But on a local scale it is NOT correct.
533	36017	30	47	27	0	0	accepting that fish production may rise in subpolar seas, losses to temperature sensitive salmon stocks and hypoxia stressed groundfish will disrupt existing fisheries of the N Pacific. (Frank Whitney, Institute of Ocean Sciences)	Has been commented above. All species have their natural thermal habitats. ACTION: No change
534	36516	30	47	27	47	28	see comments on the credibility of such forecasts of catch potential and the need to keep consistent across chapters (6, 7, 18,30) (Keith Brander, DTU)	Yes, and there are some adverse resaponses within the high latitude systems ACTION: Text modified
535	36018	30	48	1	0	0	I think it's important to stress that slowing the rate of change gives ecosystems more opportunity to adapt (changing migration patterns, locating new breeding grounds, etc) (Frank Whitney, Institute of Ocean Sciences)	Agreed - we have added a sentence along these lines.
536	38568	30	48	1	0	0	FAQ 3. This is all true, as far as making comparisons between marine and terrestrial environments. Within an environment, however, slower rates of warming (or other change) may be very important in allowing time for acclimitization or adaptation. That should be mentioned here, as it provides some motivation for slowing rates of change even if we cannot immediately reverse climate change trends. (Christopher Harley, University of British Columbia)	Agreed - we have added a few sentences along these lines. See previous comment.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
537	52070	30	48	2	48	2	Still in broad terms, is it possible to be more specific about what is meant here by "major" and "natural systems." (Katharine Mach, IPCC WGII TSU)	We have modified the text to make this clearer.
538	36019	30	48	15	0	0	perhaps "..provides half the oxygen for every breath we take.."? Some might be confused about how we only breath oceanic oxygen on every second breath, although I like the idea. (Frank Whitney, Institute of Ocean Sciences)	We have modified the statement to make it clearer.
539	37120	30	55	15	55	16	Journal, volume and pages for this ref. are: Oceanography 22: 36-47. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	We have modified the reference accordingly.
540	35923	30	57	47	0	0	citation lacks journal reference (Bernhard Riegl, Nova Southeastern University)	We have corrected the reference.
541	37122	30	58	13	58	14	This paper has already been published. The correct and complete reference is: Hoegh-Guldberg O. 2012. The adaptation of coral reefs to climate change: Is the Red Queen being outpaced? Scientia Marina 76: 403-408. (Carles Pelejero, ICREA and Institut de Ciències del Mar, CSIC)	We have corrected the reference.
542	35924	30	58	19	0	0	citation lacks journal reference (Bernhard Riegl, Nova Southeastern University)	We have corrected the reference.
543	35925	30	62	44	0	0	citation lacks journal reference (Bernhard Riegl, Nova Southeastern University)	We have corrected the reference.
544	35999	30	72	0	0	0	Table 1. Some amount of primary productivity does occur in the deep sea around e.g. hydrothermal vents and methane seeps. Chemosynthesis produces enough food to support lush communities over small areas. I suggest an asterisk and note might be appended to this table to acknowledge such areas of non-photosynthetic primary productivity. (e.g. Levin, LA et al. Diversity of bathyal macrofauna on the northeastern Pacific margin: the influence of methane seeps and oxygen minimum zones. Marine Ecology 31 (2010) 94–110) (Frank Whitney, Institute of Ocean Sciences)	We have corrected the table, figure and legend.
545	47363	30	72	0	0	0	Table 30-1: equatorial upwelling systems are not found along the eastern "edge" of the oceans. They are found along the equator, with a bias towards the eastern side of the basins but extending thousands of km into the open ocean. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
546	52071	30	72	0	0	0	Table 30-1. The author team should provide citations for the numbers presented here and, as relevant, for some of the facts in the area descriptions. Additionally, for the percentages presented, it would be clearest to indicate what the denominator is (are polar areas excluded?). (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
547	47364	30	73	0	0	0	Table 30-2: Do the significance tests take account of autocorrelation? There will likely be a fair amount of autocorrelation in these time series so simple linear regression with N-2 degrees of freedom gives an inflated estimate of the significance. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
548	41761	30	73	0	73	0	Table 30-2 Caption states six ocean regions but seven mentioned in Table 30-1 and listed in Table 30-2. (Juergen Weichselgartner, University of Kiel)	We have corrected the table, figure and legend.
549	47365	30	74	0	0	0	Table 30-3: Was CanESM2 really excluded for RCP4.5? We were one of the first groups to post data for all of the experiments except RCP6.0. (James Christian, Government of Canada)	There is a paper by Rojeli et al 2012 that compares and directly and they are broadly similar - will need to include this paper - Rogelj, J., M. Meinshausen, and R. Knutti (2012), Global warming under old and new scenarios using IPCC climate sensitivity range estimates, Nature Climate Change DOI: 10.1038/NCLIMATE1385
550	41762	30	74	0	74	0	There is an asterisk in the title of column one in Table 30-3 that is not defined. (Juergen Weichselgartner, University of Kiel)	We have corrected the table, figure and legend.
551	47366	30	75	0	0	0	Table 30-4: It would be good have some explanation of how the values for the semi-enclosed seas are derived since the models do not resolve these. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
552	47367	30	76	0	0	0	Figure 30-1should state the sensor used or data source. NASA flew more than one ocean colour mission during the period in question. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
553	52072	30	76	0	0	0	Figure 30-1. This figure is very helpful for understanding the assessment in the chapter. In the 1st sentence of the caption, it might be clearest to indicate that the oceans have been separated into these regions for the chapter 30 assessment. Additionally, the author team might consider numbering regions in this figure such that the numbers match the subsections of 30.5.X, for ease of reference for the reader. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
554	54033	30	76	0	0	0	Figure 30-1: Since Ch6 6 has a figure very similar to Figure 30-1 (although they divided oceans into 51 “units”), you may want to coordinate with them to highlight why you have divided the regions differently from Ch 6. Also, consider a possibility to combine Figure 6-1 and 30-1. (Yuka Estrada, IPCC WGII TSU)	We disagree respectfully with the reviewer. Given that we are regional chapter, we needed to define how we were dividing the world up into regions. 51 regions would have been unmanageable as we have discussed in our text. On the other hand, for chapter 6 which is focused on organisms and ecosystems, we can see that a focus on a multitude of smaller elements might be appropriate.
555	36895	30	76	1	76	1	Fig. 30-1. The limits of the Canary Current upwelling system (zone 5) must be extended to the shelves off Portugal and N Spain (Aristegui et al., 2009; Pérez et al., 2010). See comment to Ch. 30, p. 20 (Antonio Bode, Instituto Espanol de Oceanografia)	We have corrected the table, figure and legend.
556	52073	30	76	0	0	0	Figure 30-2. As a minor point, it is not clear why the data point for PDO is in bold. (Katharine Mach, IPCC WGII TSU)	Added late in the scheme - will modify original diagram carefully.
557	47368	30	76	0	0	0	Figure 30-2. The abbreviations should be defined in the caption. NPI is never defined anywhere. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
558	54034	30	76	0	0	0	Figure 30-2: Abbreviations should be spelled out. It seems to be confusing to have “The global scale” dotted line in the pane. It may be sufficient to have an arrow below the x axis indicating the limit of the global scale. Any reason why the PDO is emphasized (with thicker line)? (Yuka Estrada, IPCC WGII TSU)	Agreed. See above.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
559	41763	30	76	0	76	0	The caption of Figure 30-1 should either define or reference the location of the definitions of the numbered ocean regions in the figure. (Juergen Weichselgartner, University of Kiel)	We have corrected the table, figure and legend.
560	43265	30	76	1	93	0	The figure collection is impressive. As mentioned before graphs reflecting synthetic analyses across working groups I to III would match the role of a regional chapter more than the depiction of material which could equally be seen in the global views discussed in the sectoral chapters (WGI: Figures 2, 3, 4, 5, 6, 7, 13, 14, 17 WGII: Figures 8, 9, 10, 11, 12, 15). Figures 16, 18, 19 are strongly regional and would benefit from inclusion of the roles and responses of neighbouring countries and regional economies as known. (Hans-O. Pörtner, Alfred-Wegener-Institute for Polar and Marine Research)	We partially disagree. The focus of our chapter is the ocean from a regional perspective. We chose a particular way in which to separate the world into seven major regions. This was manageable. If you look at how we have been examined each of the physical, chemical and biological variables, you will see that this is consistent with our regional focus. We have not divided the world on government or economic grounds for reasons which we have outlined in our text - principally because we were looking for regions that made sense from the perspective of the ocean not the nations of the planet. Chapter 6, on the other hand, was to examine marine systems which essentially means looking at organism, community and ecosystem responses. Therefore, having global maps makes no sense in terms of this remit chapter 6. On the other hand, there is plenty of opportunities for chapter 6 to feed its very relevant information on the impact of climate change on the organisms, communities and ecosystems into these regional to global perspectives we have provided.
561	47078	30	77	0	0	0	Figure 30-3. Panel A is misleading because it includes regions with no statistical evidence of warming or cooling. My criticism was sent by email. (Skip McKinnell, PICES)	Not quite sure understand this problem.
562	47369	30	77	0	0	0	Figure 30-3 delete "SST" from legends (lower 2 panels) (James Christian, Government of Canada)	We have corrected the table, figure and legend.
563	52074	30	77	0	0	0	Figure 30-3. For panel A, it would be helpful to expand the color bar scale to facilitate its interpretation by the reader, potentially matching the style used for the other panels. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
564	47370	30	78	0	0	0	Figure 30-4 "defined as any year that has a thermal anomaly"? Presumably there is a threshold value but it is not stated here. I can not make sense of "the maximum in the monthly climatology". (James Christian, Government of Canada)	We have corrected the table, figure and legend.
565	52075	30	78	0	0	0	Figure 30-4. For both panels of this figure, the author team might consider using a broader range of colors (full green to full red) to facilitate interpretation by the reader. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
566	47371	30	79	0	0	0	Figure 30-5 "solar insolation"? A and D state ocean surface. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
567	38566	30	80	0	0	0	Figure 30.6. All of the reds blend together, making it difficult to tell which regions, if any, are undersaturated. Would it be possible to add a line on the map to indicate the contour where the saturation state equals 1? Failing that, can there be a stronger gradient in the color code at values near 1? (Christopher Harley, University of British Columbia)	We have changed the colouring on the figure to make this clearer.
568	52076	30	80	0	0	0	Figure 30-6. If there is an additional citation reflecting the source of this figure, it should be clarified. (Katharine Mach, IPCC WGII TSU)	These figures were calculated from a model which Ch30 CA Dr Long Cao has generated. We have added a reference to wear this model was first recorded.
569	36896	30	80	1	80	1	Fig. 30-6. The units for the scale bars in pannel A must be pH (not ppm). Indicate the units for the scale in pannel B. (Antonio Bode, Instituto Espanol de Oceanografia)	This has been changed.
570	52077	30	81	0	0	0	Figure 30-7. In the 1st sentence of the figure, it would be beneficial to provide citations supporting the phrase "widely accepted," perhaps with more specificity regarding what is meant by "threatening" as well. Additionally, where "critical regions" are described, it would be helpful to indicate why they are critical. Also, is it possible to indicate why certain regions are not included--due to limitations in data availability? Finally, for an unfamiliar reader, it might be helpful to clarify in the 1st sentence how to interpret this critical value (in terms of expectations for occurrence of hypoxia above and below the boundary). (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
571	47372	30	82	0	0	0	Figure 30-8 maybe change "California Current" to "northeast Pacific". Not all locations shown are within the CC. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
572	52078	30	82	0	0	0	Figure 30-8. For panel A, it would be helpful to indicate how many studies fall into each of the 3 categories. Additionally, for panel B, the author team might clarify how to interpret "frequency"--is it the number of studies or the number species? (Katharine Mach, IPCC WGII TSU)	We have added the number for each category of A into the legend. We have clarified the legend for B by adding explanation of an observation
573	54035	30	82	0	0	0	Figure 30-8: A. C and D may be included as insets of A. Observations Panel: Line between circles should be removed, or use a bar chart for frequency. Lines make the data look like continuous values. (Yuka Estrada, IPCC WGII TSU)	C and D are zoomed in sections are A. For panel B we have replaced the line with a bar graph
574	41764	30	82	0	82	0	Figure 30-8B does not state which axis accompanies each set of data included. (Juergen Weichselgartner, University of Kiel)	We have updated the legend to be clearer
575	47373	30	83	0	0	0	Figure 30-9 change "predictions" to "projections" (James Christian, Government of Canada)	We have made this change

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
576	54036	30	83	0	0	0	Figure 30-9: I find this figure hard to read. The sample size may be stated as (n=14) next to the label on the left (e.g., Mammals (n=14), Seabirds (114), Turtles (8)…) while the significance level can be listed to the right of each row (closer to the mean and SE bars). Since there is nothing less than 0.5, consider using a broken x-axis to reduce “empty” space on the left side of the charts. Also, instead of using proportion on the X-axis, it maybe intuitive for readers to understand if percentage was used, and it will be consistent with the caption. (Yuka Estrada, IPCC WGII TSU)	We have decided to keep the sample size on the right of the figure. Moving the numbers to the left as suggested made the figure harder to read. We have also decided against a broken x axis as this can confuse readers and the empty space sends a visual message. We will change the axis title to percentage
577	54037	30	84	0	0	0	Figure 30-10: Change the orientation of the chart to be consistent with Figure 30-9. See comments on Figure 30-9 for sample numbers and significance level. (Yuka Estrada, IPCC WGII TSU)	We would prefer the keep the orientation as is, this is showing results from a different analysis to 30.9
578	41765	30	84	0	84	0	The signifigance notations are described in the caption but are not shown. (Juergen Weichselgartner, University of Kiel)	We will correct this
579	52079	30	85	0	0	0	Figure 30-11. For panel A-C, it would be helpful to clarify the scale used on the axes. Additionally, what is meant by "1701 of 838"? 1701 studies of 838 species? Finally, what do the numbers within the circles of panel D stand for? (Katharine Mach, IPCC WGII TSU)	We are removing panels A-C as these do not add much useful additional information then panel D. Panel D is the figure developed with Chp 18 Detection and Attribution
580	54038	30	85	0	0	0	Figure 30-11: Change the color of the label and arrows so that the marker will stand out more. The caption should be changed to Quantitative and Qualitative evaluation … since D is a qualitative figure? Need explanations on numbers within each circle in D. Title of Y-axis should be consistent for all four charts? (Yuka Estrada, IPCC WGII TSU)	We are removing panels A-C as these do not add much useful additional information then panel D. Panel D is the figure developed with Chp 18 Detection and Attribution. We have updated the legend to explain the numbers in the circles.
581	41766	30	85	0	85	0	Figure 30-11 states in the caption "…1701 of 838…". Perhaps it should be reversed? The sentence is unclear. (Juergen Weichselgartner, University of Kiel)	We are removing panels A-C as these do not add much useful additional information then panel D so this comment is no longer relevant. Panel D is the figure developed with Chp 18 Detection and Attribution
582	36007	30	86	0	0	0	correct caption (C. SST). Are data monthly averages? (Frank Whitney, Institute of Ocean Sciences)	We have corrected the table, figure and legend.
583	47374	30	86	0	0	0	Figure 30-12 subpanels within panels B and C are not identified as to which represents which region in panel A. Data sources are not specified. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
584	52080	30	86	0	0	0	Figure 30-12. It would be helpful to clarify if all data presented here originate from the citation mentioned on the 2nd line of the figure caption. Additionally, is it possible to label the plots in B and C (also noting that C seems to be mislabeled in the figure caption)? (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
585	54039	30	86	0	0	0	Figure 30-12: White polygons need labels or names in A. Needs caption for (i)-(v) and (a) vs (b). Current caption is not corresponding with labels on charts in B and C. What are the dots and lines representing in B and C? Should have clear labeling linking between regions in A and charts in B and C. (Yuka Estrada, IPCC WGII TSU)	We have corrected the table, figure and legend.
586	41767	30	86	0	86	0	Figure 30-12 contains several shortcomings. 1) Section C is not defined in the caption; 2) The plots in section B and C cannot be related to the regions defined in section A; 3) What is meant by the labels b) and a) in the right hand corners of the first plots in section B and C?; 4) In the caption is says that chl-a is shown in section B, but it is section C (and vice versa). (Juergen Weichselgartner, University of Kiel)	We have corrected the table, figure and legend.
587	36897	30	86	1	86	1	Fig. 30-12. Label pannels A, B and C with region names. Correct the attribution to pannel C in the figure caption. (Antonio Bode, Instituto Espanol de Oceanografia)	We have corrected the table, figure and legend.
588	47375	30	87	0	0	0	Figure 30-13 RCP names truncated. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
589	52081	30	87	0	0	0	Figure 30-13. The author team should specify the source of data presented here. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
590	54040	30	87	0	88	0	Figure 30-13, 14: Needs label on legend (what is the color scale representing?) (Yuka Estrada, IPCC WGII TSU)	We have corrected the table, figure and legend.
591	52082	30	88	0	0	0	Figure 30-14. The author team should specify the source of data presented here. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
592	47376	30	89	0	0	0	Figure 30-15. Magenta lines are not explained. I think there is too much text in the caption and some of it should be moved to the text. (James Christian, Government of Canada)	We have corrected the table, figure and legend.
593	52083	30	89	0	0	0	Figure 30-15. Based on the legend provided, is not clear how to interpret the 3 reddish fish that also include a "?" (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
594	54041	30	89	0	0	0	Figure 30-15: I am not sure if having this map adds any information. Since the regions described in this figure are corresponding with the Figure 30-1, a small table or even text seem to be sufficient to present this set of information. (Yuka Estrada, IPCC WGII TSU)	We have corrected the table, figure and legend.
595	54042	30	89	0	0	0	Figure 30-16: See comments on Figure 30-15. (Yuka Estrada, IPCC WGII TSU)	We respectively disagree, the figure provides a summary for reader
596	52084	30	90	0	0	0	Figure 30-16. It would be helpful to explain the letters labeling each box. Additionally, the author team should consider providing references to sections of the chapter supporting these summary conclusions. It also would be beneficial to consider assigning calibrated uncertainty language for the changes characterized. In a few places, it may be more informative to indicate why a change is "rapid" or "dramatic," in addition to the adjective or adverb used. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
597	36898	30	90	1	90	1	Fig. 30-16. The limits of the Canary Current upwelling system must be extended to the shelves off Portugal and N Spain (Aristegui et al., 2009; Pérez et al., 2010). See comment to Ch. 30, p. 20, and to Fig. 30-1. (Antonio Bode, Instituto Espanol de Oceanografia)	This has been done for both Figure 30.1 and Figure 30.16
598	52085	30	91	0	0	0	Figure 30-17. The author team might consider providing background citations for this figure, as a reference for the reader. (Katharine Mach, IPCC WGII TSU)	We have corrected the table, figure and legend.
599	54043	30	91	0	0	0	Figure 30-17: Need clear description for b) in caption. (Yuka Estrada, IPCC WGII TSU)	We have corrected the table, figure and legend.

#	ID	Ch	From Page	From Line	To Page	To Line	Comment	Response
600	47377	30	92	0	0	0	Figure 30-18 Why are there no changes during the historical period? (James Christian, Government of Canada)	Because it didn't change. What you're looking at here is the exceedances of a threshold that was reached around the 1970-1980 period
601	54044	30	93	0	0	0	Figure 30-19: Use legend in a cart itself. Numbers in x-axis label are not consistent with Figure 30-1 and Table 30-1. Either remove those numbers to avoid confusion or use the consistent numbering scheme throughout the chapter. Side by side bars would look cleaner. (Yuka Estrada, IPCC WGII TSU)	Need to consider changing