NOAA Pacific Islands Fisheries Science Center
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Status review of 82 Coral Species under the Endangered Species Act – Response to CIE Reviews

Biological review team (BRT) responses to review comments from the Center for Independent Experts (CIE) on the draft Status Review submitted for review by the BRT on October 28, 2010.

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Biological review team (BRT) responses to reviewer comments on the

"Draft Status Review Report of 82 Species of Corals

Under the U.S. Endangered Species Act"

(draft submitted by the BRT on October 28, 2010)

The original text of comments made by reviewers, and the BRT's responses to those comments, are included in the following sections and page numbers.

Sections 6-9 are the BRT's method of addressing the comments made by the three reviewers from the Council of Independent Experts (CIE). [Sections 1-5 reference response to internal NMFS comments.]

Section 6.

BRT responses to:
General comments Dr. Terry Hughes
(CIE Reviewer 1)

The text in black is from the General Comments by Dr. Terry Hughes. The BRT added their comments in red in this document.

Evaluate the adequacy, accuracy and application of data in the Status Review document.

The status review report is generally very well written, comprehensive and authoritative. The Biological Review Team (henceforth BRT) are to be commended for their professionalism.

1. In general does the Status Review include and cite scientific and commercial information available on the species, its biology, stock structure, threats, and risk of extinction?

While the bibliography is impressive, some highly pertinent information was omitted (see suggested insertions of references for specific sections of the report, pp.7-37, below).

We added the recommended additions, see attached specific comments spreadsheet.

I thought the summary of life histories and demography of corals was overly brief. I would prefer to have seen recruitment treated as a demographic process, whereas fecundity should be regarded as a life-history trait. (For example, *Montastrea annularis* variants are highly fecund but have low levels of recruitment). The storage effect is scarcely treated in the report, beyond an apparent misinterpretation by Edmunds (in press).

The BRT responded to this comment in the specific comments spreadsheet.

Other issues that would benefit from more coverage in the report are commonness and rarity of corals, and larval connectivity. Many species are naturally rare, and not necessarily more vulnerable. Clearly rare corals have been able to spawn and fertilize gametes throughout their evolutionary history, i.e. they have evolved life history strategies that allow them to persist while remaining relatively rare. There is a growing literature on dispersal and connectivity of corals (e.g. brooders versus spawners) that is highly relevant for assessing extinction risk.

Important aspects of commonness and rarity of corals has been expanded in Chapter 4, and especially in section 4.2. While there are numerous examples from the terrestrial environment of certain species for which there are adaptive mechanisms and benefits of being rare, we are unaware of evidence of these processes in corals. Although asexual reproduction will allow corals to persist, the local limits to genetic heterogeneity are not necessarily adaptive. For most of the species we do not know if the species commonness was historical or not, with the exception of *Dendrogyra* since the 1970s. The issue of dispersal and connectivity has been expanded in the SRR, Chapter 2, Section 2.2.1.

On p86, List of strengths, first bullet point, the report states that "*All available relevant information was considered*." However, the report citations have a bias to recent publications and to work done on US territories, even when alternative information from earlier or from elsewhere in a species range is more rigorous and more informative. In many cases this can be improved by citing papers already in the bibliography more frequently. It's important not to extrapolate from Hawaii to the entire Indo- Pacific.

Additional references suggested by the reviewer have now been included, and we agree that it is important not to extrapolate from Hawaii to the entire Indo-Pacific.

The report authors need to be more careful to avoid citing secondary studies such as flawed metaanalyses, and erroneous, unrelated references. For example, on p129, fifth paragraph. "*A. lamarcki is…….susceptible to storm damage (Andres and Rodenhouse 1993, Alvarez-Filip et al. 2009)"* This is factually wrong, and the citations are completely erroneous. The peak abundance of *A. lamarcki* is deeper than 20m, making it far less susceptible to storm damage compared to most other Caribbean species (e.g. Woodley et al. 1981). Andres and Rodenhouse is a modeling study based on previously published data on growth and survival of *A. lamarcki* and three other species, from Hughes and Jackson (1985). In the model, storm mortality was arbitrarily set at double the empirically-based background rate. This is effectively made-up data on vulnerability to storms, cited in the report as a fact. Alverez-Filip et al. is a crude meta-analysis of reef rugosity, the decline of which is attributable by those authors to the loss of *Acropora*. They make NO mention of *Agaricia lamarcki*.

Endnote glitches in the draft document have been corrected. However, the BRT respectfully disagrees with the reviewer's general contention that secondary sources should be avoided. The scope of the charge to the BRT (evaluating coral status range-wide) necessarily required integration of information across large areas of geography and a vast literature. The inclusion and reference to secondary sources (reviews and meta-analyses) were important to fulfilling our charge.

The petitioner has also relied apparently on poor secondary information. For example, on p3, it states, "*To support this assertion, the petitioner cited Alvarez-Filip et al. (2009) in noting the dramatic decline of the three dimensional complexity of Caribbean reefs over the past 40 years, resulting in a phase shift from a coral-dominated ecosystem to fleshy macroalgal overgrowth in reef systems across the Caribbean*." This is very sloppy, a reflection on the petitioner, not the BRT. Alvarez-Filip et al (2009) is one of the most ill-informed meta-analyses I have seen. The authors collected published data from 49 studies of reef topography from the literature, from 1969 to 2008. This is not a valid citation for evidence of phase-shifts or macro-algal overgrowth. Most of the loss of topography reflects the decline of *Acropora*, which did not "result" in macro-algal blooms. As noted elsewhere by the BRT, the rise of macroalgae is attributable to overfishing of herbivores, pollution and the loss of *Diadema*.

Similarly, on Page 4, first paragraph, last sentence. "*The petitioner stated that Hoegh-Guldberg et al. (2007)* ***found*** *marked reductions in resilience accompanied by increased grazing requirements to facilitate reef recovery after* ***modeling*** *the impacts of a 20% decline in coral growth rate in response to ocean acidification on a Caribbean forereef*." A model makes predictions, which in this case remain untested. It does not "find". The 20% future reduction is a hypothesis that should not be misrepresented as an observation or fact.

The BRT chose not to respond or launch extensive direct critique of the petition.

Regarding threats, the report makes a case for global warming and ocean acidification as the most widespread future threats for corals, with more local impacts from pollution, overfishing, etc. that are superimposed on future effects of climate change. While it undoubtedly is true that climate will affect corals more in the future, an alternative view (that is arguably more accurate) is that roughly one third of coral cover have already been destroyed, and most of that loss (e.g. as documented in GCRMN reports) has preceded the recent and future impacts of climate change. For example, close to half of the loss of corals in the Caribbean took place before regional-scale

disease and coral bleaching were first recorded there in the mid- to late 1980s. Similarly, "local" impacts along the 2000km subtropical coast of China have destroyed all of that country's fringing reefs, and climate change did not played a significant role. Realistically, climate change impacts are superimposed on existing "local" ones, not the other way round.

The BRT acknowledges that local impacts have affected coral declines in many, often extensive, areas. However, our growing understanding of much of the widespread, regional-scale coral declines such as in the Caribbean region includes a strong likelihood that climate/temperature disturbances are a strong contributor to disease and bleaching mortality. Figure 3D in Gardner et al (2003) shows the greatest rates of coral decline during the decade from 1980-1990 corresponding to the initial acute coral disease and bleaching events (early 1982-1983 for *Acropora* disease and 1987 for early mass bleaching event).

On p58, third paragraph, the discussion on the ratio of predators and their prey could also be extended to include herbivory and the recovery of *Diadema*, There is a modern myth in the recent coral reef literature that coral loss inevitably leads to a permanent macro-algal bloom. Connell's 30-year study of disturbance and recovery illustrates the capacity of corals to bounce back from very low abundances. Providing they are not depleted by disease or overfishing, herbivores are not overwhelmed by an increased potential for macroalgal expansion following the loss of corals after every hurricane. A healthy coral reef recovering from a hurricane has low abundance of corals AND very little macroalgae. Sudden losses of corals do not invariably lead to phase-shifts, otherwise corals could never recover from a hurricane.

The reviewer's point is acknowledged. The general message the BRT is conveying is that the modern tendency for reefs in many areas to NOT recover in this way indicates that multiple stressors have compromised this recovery capacity.

2. Are methods used valid and appropriate?

The methodology is weak, as illustrated by the disparity in scoring by different members of the BRT. A major weakness of any analysis of coral vulnerability to extinction is the paucity of species-level abundance data at regional scales. The life cycle figures showing vulnerable stages didn't add much to the text. Some arrows were missing (see specific comments). In most cases the apparent lack of impacts on larval stages is due to a lack of appropriate studies.

Figures have been corrected. As for weak methodology, in the situation of "paucity of species level abundance data at regional scales" as noted by the reviewer, the BRT has followed a methodology implemented by previous BRT's. The "disparity in scoring by different members" is a crucial aspect of the methodology which conveys aspects of uncertainty in the scores. This disparity/uncertainty has been more explicitly described (with standard error and mean range of votes) in the revision of the report. The "apparent" lack of impacts on larval stages is acknowledged explicitly in Section 3.4: "Interactive and Unapparent Threats on Coral Populations."

The term "productivity" is unclear ("reproductive potential" or just "recruitment" would be better). The naturally low levels of recruitment and reproductive outputs of many species need to be viewed in the context of their mortality schedules and longevity. I would argue that low-recruiting, long-lived species such as *Montastrea* are LESS vulnerable, because their populations can withstand recruitment failure for decades (see e.g. Hughes and Tanner 2000, which the report cites).

The term "productivity" has been further defined in Section 4, and explicitly distinguished from "primary productivity". This term is used throughout an extensive literature on extinction risk and Population Viability Analysis. The BRT confers that, *all else being equal*, greater longevity of species such as *Montastraea* would make them less vulnerable than shorter-lived species. However, the reality is that *Montastraea* spp. are undergoing vast adult mortality as shown by multiple data sets shown in the report. The BRT has concluded that low species productivity combined with ongoing acute adult mortality (as manifest by the Caribbean *Montastraea* spp complex) combine to yield a high extinction risk.

The Carpenter et al. study is seriously flawed, and I can't find any explanation in the descriptions of methods in this report for why these particular 82 species have been proposed by the Centre for Biological Diversity, or why so many of them come from the Indo-Pacific, where most reefs remain in good condition. On p4, second paragraph, the report states:. "*The petitioner cited Bruno and Selig (2007) in stating that … As recently as 1000 to 100 years ago, this region averaged about 50% coral cover, but 20%–50% of that total has been lost: the petitioner cited the same source, stating that regional total coral cover averaged 42.5% during the early 1980s, 36.1% in 1995, and 22.1% in 2003.*" Bruno and Selig's (2009) compilation is unreliable because regional-scale data are too sparse, especially before about 1990. One-third of the records of coral cover used in their analysis (supposedly spanning the entire Indo-Pacific from 1968 to 2004) come from one habitat in one region (i.e. mid-depth reef slopes on the Great Barrier Reef) after 1997. Furthermore, the meta-analysis is weakened by consistent methodological differences (e.g. quadrats versus videos) among primary studies and monitoring programs undertaken in different regions and at different times. I have no idea how the petitioner can convert inadequate data from 1968-2008 into "*1000 to 100 years ago*"!

The BRT is not in a position to defend the methodology or report of the petitioner.

3. Are the scientific conclusions valid and appropriate?

Generally, no. Undoubtedly coral reefs are in rapid decline and need to be much better protected. However, I would argue that if *Montastrea annularis* goes extinct in the Caribbean, then so too will virtually all other scleractinians. I disagree with the assessment that *Agaricia lamarcki* is significantly less vulnerable than *M. annularis*. Similarly, if widespread, relatively hardy Indo-Pacific species like *Acropora aspera* and *Turbinaria* species go extinct, then so too will everything else. See more detailed comment below, p6, #1.

The BRT was charged with assessing risk for the 82 spp chosen a priori. Specific arguments on relative ranking of *M. annularis* vs. *A. lamarcki* are addressed in the specific comments spreadsheet. The BRT cautions against interpreting "significant" differences among any given risk ranking scores in the report.

4. Where available, are opposing scientific studies acknowledged and discussed?

This issue is generally not applicable to this report. One omission, however, is a cogent critique by Nancy Knowlton of Carpenter et al.'s approach for identifying vulnerable coral species, published in *Science*. This critique and the response by Carpenter et al. needs to be consider here, especially regarding the validity of selecting far more Indo-Pacific species, when the Caribbean is clearly much more degraded.

The BRT was charged with assessing risk for the 82 spp chosen a priori and had no influence on this selection.

5. Are the uncertainties assessed and clearly stated.

Generally, yes. The BRT do a commendable job highlighting many of the uncertainties in making these assessments (e.g. on page x of the Executive Summary, and on p74, p83). On p74, the report states "*It is not apparent that individual species would always increase or decrease in direct proportion to the overall change in coral cover …. the diverse ecology and life history of the range of candidate species would seem to suggest otherwise."* The BRT are being very polite here: Carpenter et al.'s assertion is patently flawed. The following comes from a review in the November 2010 issue of TREE:

The species composition and functional dynamics of corals invariably changes whenever cover increases or decreases. For example, major mortality agents for corals are all highly selective – storms affect tabular and staghorn species disproportionately, bleaching and disease affects physiologically resistant "winners" less than susceptible "losers", algal overgrowth impacts on encrusting species more than three-- -dimensional ones, corallivores select their preferred prey, and so on. Similarly, short-- -lived coral species are more vulnerable to recruitment failure compared to longer-- -lived ones. Weedier groups such as bushy acroporids and pocilloporids re-- -colonize faster, while some former spatial dominants that are long-- -lived may take centuries to regain their abundance. This two-- -step filter, differential mortality and replenishment, is changing the face of reefs worldwide. The convenient practice of measuring total coral and macroalgal cover obscures these important shifts in composition.

The report notes on p83, fifth paragraph, last sentence that *"In many cases, essentially no species specific information was available other than the taxonomic species description and some questionable geographic range maps."* I certainly agree with the first part, although I think the geographic range information is reasonably robust for the majority of species. In Chapter 6, the individual species accounts rely too much on Veron's three-volume taxonomic treatise. In particular, Veron's habitat descriptions (such as " most reef habitats") are of very limited value. His depth distributions for Caribbean species seem to have been borrowed from Goreau and Wells 1967, which also gives a "preferred" depth range that is more ecologically relevant. The IUCN distribution maps are based heavily on Veron 2000, so I don't see the point of showing both. Wallace's distribution data (e.g. page 207) are superior for *Acropora*.

The best available maps that were standardly available for all species were those of Veron and IUCN and that is why the BRT used them. We used additional information from literature or personal comments on the geographic range that provided some corrections to maps of both Veron and IUCN and these corrections are given in the text. The IUCN distribution maps differ from Veron's in a number of important cases and we made every effort to determine which was correct in each case. We felt it was best to take both into account to preempt criticism for ignoring one or the other.

Wallace's maps are very good for showing the specific location for each of the specimens she examined, but they are not superior for our purposes of showing the overall geographic distributions of each *Acropora* and *Isopora* species. For example, her maps show only three sites for *Isopora crateriformis*, two in Indonesia and one in Samoa. Our experience has shown that its distribution is similar to the one depicted by Veron and IUCN maps. On the other hand, we included her map for *Acropora verweyi* which she differs strongly from both Veron and IUCN in

Indonesia in the center of its range. She makes a compelling case for her version and so we felt we needed to recognize it as an alternative.

The BRT chose to include all information on depth ranges, not just the "preferred" depth range. The responses to climate change might include a shift to deeper depths at which some environmental factors might be less stressful. The occasional occurrence of a species at deeper than "preferred" depths might suggest that it has the ability to survive at lower light levels and other factors that change with depth. The BRT decided to include all information on a species, not just information on preferred or optimal regimes because we need to assess how the species might do when conditions change.

The Acidification section, repeated verbatim for every species in Chapter 6, is very weak: "*Unknown for this genus. However, in most corals studied, acidification impairs growth (Langdon and Atkinson 2005, Manzello 2010), in the case of Acropora palmata impairs fertilization and settlement success (Albright et al. in press 2010), and contributes to reef destruction (Hoegh-Guldberg et al. 2007, Silverman et al. 2009).*" The Langdon and Atkinson paper is flawed. The inference here is that *"acidification impares growth"* now, but most experimental studies show very limited effects of acidification before saturation states of about 0.9 or lower. Last line. *"…contributes to reef destruction (Hoegh-Guldberg et al. 2007, Silverman et al. 2009)".* No one has documented "reef destruction" from contemporary ocean acidification, for the simple reason that it hasn't actually happened. It could happen in the future. Please change the wording to reflect current reality, or state it as a prediction, or the best option is to delete the paragraph from each species section.

The BRT believes it is important to highlight these predicted future impacts on corals from ocean acidification. While the available manipulative experimental studies are necessarily abrupt, none have shown any evidence of acclimation or adaptation to higher CO_2 conditions, except limited buffering provided by enhanced heterotrophic nutrition. The acidification threat assessments were based primarily on future prediction of the effects and the BRT expects the future threats of acidification to increase (see Table 3.2.2). We added a sentence to each of the ISAs to qualify this threat "While ocean acidification has not been demonstrated to have caused appreciable declines in coral populations so far, the BRT considers it to be a significant threat to corals by 2100."

*Similarly, the paragraph on LBSP-related stresses does not reflect the uncertainties for individual species. "*Overall, LBSP-related stresses (nutrients, sediment, toxins, and salinity) often act in concert rather than individually, and are influenced by other biological (e.g., herbivory) and hydrological factors. Collectively, LBSP stresses are unlikely to produce extinction at a global scale; however, they may pose significant threats at local scales and reduce the resilience of corals to bleaching (Carilli et al. 2009, Wooldridge 2009)."* The choice of references is poor given the huge Caribbean literature.

For the Caribbean in particular, I think it is wrong to say that these stresses have only local impacts. Overfishing leading to macroalgal blooms is the major cause of the loss of corals over the past 30 years. Bleaching and disease are killing what is left.

The report acknowledges that the local threats are often applied (and so impact) over large scales, as described for trophic cascade/phase shift phenomena in the Caribbean. The reviewer is correct that there is a substantial Caribbean literature on this. We have cited some of it in our characterization of that basin in Chapter 2. However, for the purposes of the Individual Species Accounts, we specifically chose two recent references that address local vs. global impacts—one

from the Atlantic, and one from the Pacific. The ISAs are not the place to provide extensive discussion of this topic.

Evaluate the findings made in the Status Review

1. Are the results of the Extinction Risk Analysis supported by the information presented?

Generally, no. Page 3, third paragraph, line 4 states: *The petition asserted that all of the petitioned species have suffered population reductions of at least 30% over a 30–year period, relying on information from the IUCN.* Here, "asserted" is indeed the appropriate verb, since there is very little species-specific data on abundance. It would be worth explaining here how the various IUCN categories relate to different levels of population decline, and how Carpenter et al. 2008 came to their conclusions. In this context, the report needs to consider the literature on the use and misuse of IUCN criteria for listings. For example:
Keith, D.A. 2001. An evaluation and modification of World Conservation Union Red List criteria for classification of extinction risk in vascular plants. Conservation Biology 12, 1076-1090.

Akcakaya, H.R. et al. 2006. Use and misuse of the IUCN Red List criteria in projecting climate change impacts on biodiversity. Global Change Biology 12, 2037-2943.

Keith, D.A. et al. 2000. Sensitivity analyses of decision rules in World Conservation Union (IUCN) Red List criteria using Australian plants. Biological Conservation 94, 311-319.

Possingham H.P., Andelman S.J., Burgman M.A., Medellin R.A., Master L.L. & Keith D.A. (2002). Limits to the use of threatened species lists. TREE 17, 503-507.

Regan T.J., Burgman M.A., McCarthy M.A., Master L.L., Keith D.A., Mace G.M. & Andelman S.J.(2005). The consistency of extinction risk classification protocols. Conservation Biology 19, 1969-1977.

I'm baffled as to why these particular Caribbean species are considered more vulnerable than other that are not mentioned. I suppose this issue is beyond the brief of the BRT – they have to work with the list they were given. But why, for example, are other deep water *Agaricia* or *Leptoseris cucull*ata not included along with *A. lamarcki*? Or other species of *Mycetophyllia*? As noted elsewhere in this review, I think the *Montastrea annularis* complex would be among the last Caribbean species to go extinct.

The BRT is not responsible for the content of the petition. Reviewer's insistence that *Montastraea* spp. risk is low relative to other Caribbean species does not seem consistent with rates of mortality/population decline over the recent past, combined with negligible recruitment/recovery potential.

Section 7.

BRT responses to:
Specific comments Dr. Terry Hughes
(CIE Reviewer 1)

	A	B	C	D	E
1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
2	"*External fertilization, planktonic larval phases, cryptic settlement, and a long post-settlement period with high mortality are characteristic of many coral species, making their population dynamics very difficult to determine with confidence"*. This is somewhat overstated. Remember brooders have internal fertilization. There is a substantial literature on coral demography and population dynamics. The BRT have tended to leave out Indo-Pacific work not done in Hawaii.	This is intended as a general overview statement. In the biology and ecology sections (2.2 & 2.3) we expand on the issues raised, we feel that there is no need for change here in the Executive Summary. However, for clarification we rephrased the sentence to include external fertilization without appearing to exclude brooders.	exe summ	ix	4, para 3
3	"*While the BRT compiled information regarding species distribution within US waters (included in the individual species accounts), it was not considered in the assessment of extinction risk as the Endangered Species Act requires this assessment to be made range-wide for invertebrates".* The meaning of ~~–it~~" is unclear.	We removed the footnote entirely as that is unnecesary to have in the Executive Summary.	exe summ	ix	footnote
4	~~—~~*. demonstrated low population sizes*". There seems to be an inference here that naturally rarer species are more vulnerable. The evidence for that is pretty sparse.	We added "declines in abundance or ". The sentence now reads "…demonstrated declines in abundance or low population sizes, …"	exe summ	x	last para, 4 ln from bottom
5	I would prefer to see the term "coral" reserved for scleractinians.	The BRT recgonizes the important taxonomic distinction, but they believe it will be best to keep it in the ecological context of the Petition to include *Millepora* and *Heliopora* as "corals" other than scleractinians that contribute substantial calcium carbonate deposition to the reef framework.	ch1	1	
6	endangered species definitions. Can you define "*…a significant portion of its range*". For example, does Hawaii count as significant for a species that is pandemic from Africa to Polynesia?	The BRT chose to use the precise language of the ESA, which does not explicitly define "a significant portion of its range".	ch1	2	
7	"*Montipora dilatata was identified as an Species of Concern in 2004 based on the species being very rare and subject to the following factors for decline: 1) vulnerability to coral bleaching; 2) fresh water kills and exposure at extreme low tide; 3) habitat degradation and modification as a result of sedimentation, pollution, and alien alga invasion; 4) a limited distribution; and 5) damage by anchors, fish pots, swimmers, and divers*". This species has a tiny geographic range, being a Hawaiian endemic. Surely that was a factor in its listing?	Species of Concern are those species about which NMFS has some concerns regarding status and threats, but for which insufficient information is available to indicate a need to list the species under the ESA. The BRT mentions limited distribution as one of the factors for consideration, which covers this instance.	ch1	3	para1, last sentence
8	* The 50% loss of corals from bleaching is misleading, since it only refers to remnant populations affected by the 2005 bleaching event. Far more corals have been killed by local events over the past 30 years.	The BRT is not stating that this statement is true. The BRT is merely stating what was asserted in the petition.	ch1	3	para 3, ln 9
9	Reproduction is a life history trait (that trades off evolutionarily with longevity), but recruitment is not. Recruitment is mediated by hydrodynamics, the nature of the substrate, postsettlement competition, etc.	The BRT agrees and changed the "Reproduction and Recruitment"	ch2	2.2.1	Heading

	A	B	C	D	E
1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
10	"*The distribution and abundance of scleractinian corals reflects patterns of larval recruitment, asexual reproduction via fragmentation, mortality, regenerative capabilities, and aggressive interactions (Richmond and Hunter 1990)*". The inclusion of one mechanism of competition here is odd. Sexual and asexual recruitment increase numerical abundance, while mortality reduces it. Competition (from aggressive interactions, shading, allelopathy, etc) is one source of mortality, and so is predation, sedimentation, disease, etc.	The BRT agrees and deleted "aggressive interactions"	ch2	8	para1, 1st sentence
11	* "*Interspecific differences in the mechanisms of recruitment, dispersal, and mortality are likely important in determining the species composition of reef corals in different environments…".* Isn't this necessarily the case? If all species had the same birth and death rate they would share the same abundance. There is a rich literature on coral demography (as distinct from coral life histories), beginning with Connell (1973) that's missing here.	We include extensive description of demographic factors related to extinction risk in Chapter 4 of the document. We believe that relevant information from the demographic studies the reviewer refers to are included in these discussions (e.g., lack of recruitment of *Montastraea annularis* complex species reported in long term plot-based studies in Jamaica), we do not believe that an extensive review of long term plot-based studies (which we believe the reviewer is referring to) is particularly helpful in evaluating species-specific extinction risk.	ch2	8	para1, 3rd sentence
12	"*Most stony coral species employ both sexual and asexual propagation*". I doubt if "most" is justified. Even for those species that do employ both, loss of branches may not be an effective mode of propagation (e.g. Smith and Hughes 1999. JEMBE).	We changed the word "most" to "many"	ch2	8	para2, 2 sn
13	"*Brooded larvae may either live for a short time in the plankton (relative to most broadcast larvae) or crawl away from the mother colony* ." While the average peak settlement time is shorter for brooders, both brooders and spawners have a long tail to their larval duration distributions. Cite work by Bob Richmond, and more recently by Andrew Baird, David Ayre and others.	We changed the wording slightly to emphasize that this is a generalized characteristic of brooders. The reviewers point about tails is made two paragraphs later about *Heliopora* (30-100 day potential duration)	ch2	8	para3, 3rd sn
14	"*In laboratory cultures, Graham et al. (2008) quantified the survival of larvae from 5 broadcast-spawning coral species and identified three survival phases: a bottleneck of high initial rates of mortality, followed by a low, approximately constant rate of mortality, and finally, progressively increasing mortality after approximately 100 days* ." I don't see how this lab study supports the preceding sentence on mortality from predation.	We clarified that both extrinsic and intrinsic factors cause larval mortality. Grahame et al demonstrate intrinsic factors.	ch2	8	para23, last sn
15	Inconsistent notation on ages of each stage in life cycle – add in hours and days for the pre-settlement stages.	We removed duration/age notations from the figure and added relevant information to the caption.	ch2	9	Fig.2.2.1
16	* The one-sentence paragraph on connectivity is inadequate.	We added a sentence referring to the connectivity discussion in Chapter 4 for more details.	ch2	9	
17	"*Overall, older recruits (i.e., after they have survived to a size at which they are visible to the human eye, probably 1–2 yrs after settlement) appear to have similar growth and post-settlement mortality rates across species (van Moorsel 1988).*" It's 10 dangerous to extrapolate from this single Caribbean study (which was preceded by earlier work by Bak and by Rylaarsdam). These three studies mainly counted brooded juveniles up to 3-5cm in diameter, which have very different demographies compared to Indo-Pacific spawners	The BRT agrees and we have removed the sentence.	ch2	10	para3, last sn

	A	B	C	D	E
1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
18	~~-~~*Fragmentation is a common, and can be the dominant, means of propagation in many species of branching corals (Gilmore and Hall 1976, Davis 1977, Tunnicliffe 1981, Bak and Criens 1982, Hughes 1985,(Bythell 1990, Hunter 1993, Adjeroud and Tsuchiya 1999)".* Note the typo before Bythell. Most of these refer to one species of *Acropora* . For some species and some habitats, losing branches is maladaptive (see earlier reference to Smith and Hughes).	We fixed the typo. The Bythell 1990 citation was wrong and we removed it. We added a sentence with Smith &Hughes reference to clarify that fragmentation is not always functioning as adaptive reproduction.	ch2	10	para5,2nd sn
19	Typo. "*maintaine(d)* ".	Corrected	ch2	11	para1,2nd sn
20	"*This stored supply of lipids can serve as a reserve for some corals during periods of bleaching (Hughes et al. 2007...* ". This paper measured lipids as an indicator of sub-lethal stress on corals due to an experimental phase-shift. The authors make no reference to a reserve during bleaching.	We changed the citation to Porter et al 1989 and added Anthony et al 2007 and Rodriguez&Grotolli 2006	ch2	11	para2, 2 sn
21	. "*The biodiversity of coral reef ecosystems and high rates of primary production in relatively nutrient-poor waters are, to a great extent, the result of the structures built by corals and other calcifying reef organisms (Lewis 1981)* ". The nutrient-poor paradigm for coral reefs has been somewhat overstated in the older literature. Many Asian reefs have naturally high nutrient levels, with robust coral assemblages in relatively turbid water.	We moderated the sentence. The nutrient-poor paradigm is dealt with in the land-based sources of pollution section.	ch2	11	para3
22	"*Because fragmentation (asexual) and sexual reproduction occur simultaneously and to varying degrees in clonal species populations, genotypic diversity can vary widely, even at small spatial scales (Baums et al. 2006)* ". This was hardly the first study to make this observation for *Acropora palmata* . There is quite a lot of information on genotypic diversity of corals - e.g. a suite of papers by David Ayre and John Benzie cover a dozen or so brooders and spawners. Asexual brooding is another important issue.	We added Ayre&Hughes 2000 and Hunter 1993. In reference to asexual brooding, we added text in the reproduction section of this chapter.	ch2	12	para2,ln7
23	"*High diversity of corals on reefs has been described as a nonequilibrium state, requiring periodic moderate disturbance events to prevent fewer competitively superior species from dominating (Connell 1978)* ". This is very out of date and not relevant for assessing extinction vulnerability across a species' range. Connell was concerned with non-equilibrial diversity at the scale of small quadrats.	We removed the sentence.	ch2	12	para2,end ln3
24	~~"~~*.coral species themselves constitute on the order of only ~ 1000 species worldwide…* ". Assuming this means scleractinians, Carpenter et al. (2008) give a lower estimate of 845.	We added "scleractinian" for clarification but the term " ~ 1000 species"was left because this sentences is contrasting this relatively small number with the orders of magnitude larger number of supported species in coral reef ecosystems.	ch2	12	para2, ln9
25	~~"~~*.highly restricted ranges…clustered into marine biodiversity hotspots" (Roberts et al. 2002).* Roberts et al. confuse biodiversity hotspots with locations that have many endemics. For corals, they are not the same thing, e.g. the Caribbean, eastern Pacific and Hawaii are all depauperate, but have very high proportions of endemics. 11 Most Indo-Pacific corals have huge geographic ranges (see, for instance, Hughes et al. 2002. Ecology Letters), including some of the 82 considered here.	We adjusted the wording in response to the reviewers' comment.	ch2	12	para2, ln11

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
26	~~…~~.*societies (Moberg and Folke 1999) such as traditional and cultural uses, subsistence, tourism, and potential biomedical…*". Food security might be a better term than "subsistence".	We changed the wording according to the reviewers' suggestion.	ch2	13	para1, ln3
27	Pandolfi et al. 2005 is not the primary reference for the economic value of Florida's reefs.	We removed Pandolfi and added the primary reference Costanza 1997	ch2	13	para1, ln7
28	The 25°C–30°C range for coral reefs is reasonably accurate for Hawaii and the US Caribbean, but more generally many reefs elsewhere thrive outside these boundaries. As noted in the report a few lines later, seasonal variation (18°C–32°C) along the Great Barrier Reef exceeds this range in both directions.	We added the range (18°-32°C) to the sentence above, where it says coral live in a fairly wide range across geographic locations. We also modified the sentence to say "tend not to thrive" rather than "do not thrive".	ch2	13	para3, ln8
29	"*The moderately resilient, long-lived and relatively bleaching-insensitive families Agariciidae, Mussidae, and Faviidae, and the pioneer family Pocilloporidae were relatively tolerant of poor water quality (Fabricius et al. 2005).*" These generalizations of course have many exceptions given the wide range of life histories within each Family. Later, the report argues that *Agaricia lamarcki* is susceptible to bleaching. *Agaricia agaricites* is certainly not long-lived or bleaching-insensitive, and either are some species of Indo-Pacific *Pavona*.	The reviewer is right in stating that these are generalizations from a specific study, we have added a qualifying clause.	ch2	13	para4, end
30	"*The hydrodynamic conditions that influence coral reefs ...with flows dependent upon surface gravity waves (seas and swell), tides, topographic and equatorial upwelling, and largescale thermohaline circulation*". Add wind to the list.	We added wind according to the reviewers' suggestion.	ch2	13	last para, ln4
31	"*Such phase-shifts…… may be reversible (Ayre and Hughes 2000)*". A&H studied connectivity, which relates only very tangentially to reversibility of phase-shifts. The issues of hysteresis and the mechanisms of reversibility of phase-shifts are covered rather superficially in the report (e.g. recent work by Bellwood, Mumby, Hughes and others).	We removed the A&H 2000 reference and replaced it with Mumby 2009.	ch2	14	para3, ln9
32	~~…~~.*acute anthropogenic disturbances such as shipwrecks (Hatcher 1984, Work et al. 2008) or hurricanes"*. Shipwrecks are a long way down the list of human impacts. Insert "by" before hurricanes for clarity.	While shipwrecks are not a major threat, there are multiple occassions in the literature where they are described as precipitated phase shifts, which is why they are articulated here.	ch2	15	para1, ln2

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
33	"*Resilience is the capacity of a reef or population to recover from damage by a major disturbance such as a disease outbreak or tropical storm; in other words, its capacity to ~~-~~bounce back" from a disturbance rather than assuming an alternate (phase-shifted) state. The term resistance is somewhat different*". This is very unclear. Resilience is the capacity to absorb recurrent disturbances (i.e. to both resist and recover from them) and to adapt to change without undergoing a phase-shift to a fundamentally different system. Rod Salm's "resistance and resilience" distinction, which is alluded to here, is based on a flawed understanding of resilience theory.	The BRT respects the reviewer's point. The term "resilience" along with related concepts of alternate stable states and stability, has been used in the reef ecology/conservation literature (reviewed by Nyström, M., et al.2008. Capturing the cornerstones of coral reef resilience: linking theory to practice. Coral Reefs 27, 795-809) and in the broader ecological literature (reviewed by Beisner, B.et al 2003. Alternative stable states in ecology. Frontiers in Ecology and the Environment 1, 376–382) with various shades of meaning. Given the applied nature of this document, we believe that the (narrower) concept of resilience (i.e., tendancy or rate that some factor recovers to a level characteristic prior to some alteration) as we have here defined it are 1) consistent with vernacular understanding of the term (Merrium-Webster definitions are: "the capability of a strained body to recover its size and shape after deformation caused especially by compressive stress" or 2 : "an ability to recover from or adjust easily to misfortune or change"); 2) consistent with a substantial portion of the ecological literature (e.g., Carilli et al 2009); and 3) useful in distinguishing morbidity/mortality-based	ch2	15	para2, 1st sn
34	~~"~~*.with increased dominance by weedy brooding species (Green et al. 2008)".* You can't extrapolate Green's *Porites astreoides* story to the entire Caribbean. Other detailed trajectories of species composition have been documented in Jamaica (by Hughes and Connell 1999 and others), Belize (Rich Aronson) and in Curacao (Rolf Bak).	The work by Aronson et al in Belize and Panama showing shifts to *A. tenuifolia* also illustrate "dominance by weedy brooding species." Reference to Aronson et al 04 has been added.	ch2	15	para3, last sn
35	"*Caribbean-wide meta-analyses have suggested that the current combination of disturbances, stressful environmental factors, and potentially depensatory states have yielded poor resilience, even to natural disturbances such as hurricanes (Gardner et al. 2005). These wide-scale changes in coral populations and communities have impacted habitat complexity (Alvarez-Filip et al. 2009), and may have already begun feeding back in reduced overall reef-fish abundances (Paddack et al. 2009)".* Gardener et al. compiled data on coral cover only, and their analysis provides no information on why cover changed, on depensatory states or on the mechanisms underscoring resilience. The fish were depleted in most parts of the Caribbean long before the corals declined.	The BRT made no change. Gardner et al 2005 is a meta-analysis specifically designed to discern effects of hurricanes on Caribbean reefs. The authors compared surveys of coral cover across Caribbean sites both with and without a hurricane "hit" within its period of record. These authors conclude that a hurricane hit yields an acute average 17% loss of cover in the year it occurs, but that coral cover trends in the years following a hit match the Caribbean background rate of coral cover loss (~ 2% per annum). While fishing has clearly had long term impacts on Caribbean reef-fish assemblages (as described elswhere in the SRR), the Paddack et al paper (analyzing just total reef-fish density) shows 1) significant declines in density only since 1996, 2) similar magnitudes of decline accross geographic subregions, and 3) similar declines accross trophic groups. These patterns lead the authors to suggest that habitat degradation, in addition to fishing, is affecting Caribbean fish density.	ch2	15	last para, last 2 sn
36	"*The Indo-Pacific ...hosts much greater coral diversity than the Caribbean region (700 species compared with 65 species; Table 2.5.1".* Yes, but see comment earlier on 1000 species of corals.	We changed "of only ~" to "less than." See earlier response regarding 1000 species.	ch2	16	para3, 1st sn
37	~~"~~*The North Atlantic takes up atmospheric CO2 at about four times the rate at which the central Pacific takes it up (Sabine et al. 2004)"*. Is that true for the Caribbean versus the central Pacific, or is this more of a temperate-tropical comparison?	We deleted this sentence to avoid confusion.	ch2	16	para3, ln4

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
38	"*However, consensus is building that these buffering factors simply have put the Indo-Pacific on a slower journey down a similar road of decline rather than a qualitatively different trajectory (Bruno and Selig 2007, Galloway et al. 2009)". ~~D~~Done et al. (2008) determined that the corals on the Great Barrier Reef started losing their resilience in 1996."* Pandolfi et al (2003) stated this notion earlier. Done et al.'s precision in selecting 1996 is silly. Inshore reefs on the GBR have been in decline since the colonial era.	We added the Pandolfi et al 2003 and combined Done et al. in the same sentence and deleted the 1996 part of the sentence.	ch2	16	para3, ln9
39	~~"~~*...the Indo-Pacific and as of 2002–03 stand at around 20% live cover (Bruno and Selig 2007)*". This is a meaningless statement by Bruno and Selig. They have no data from most of the Indo-Pacific, and the information in recent years is dominated by data from the Great Barrier Reef monitoring program.	The BRT made no change. Fig 2a in Bruno and Selig 07 indicates that this estimate (actulaly 22.1) is derived from data from 390 sites of which 125 are in the GBR region. Eyeballing this figure, the GBR regional mean appears intermediate (i.e., there are plenty of regional means that are both higher and lower) suggesting that the prepoderance of sites in this region did not skew this overall mean. Hence, we feel that the reviewer's assertion of "meaningless" is not supported.	ch2	16	para3, last ln
40	~~"~~*...far eastern French Polynesia hosts less than 50 species, 10 genera, and 4 families (Veron 2000)*". Where exactly? The Marquesas? Veron is not the primary reference.	Glynn et al. 2007 (Paci Sci) on Easter island gives Presence/Absence of Zooxanthellate Scleractinian Coral Species at 19 localities in the Eastern, southeastern, and central Pacific Ocean in Appendix 2. This includes the Marquesas. This reference is now cited here.	ch2	17	para1, last ln
41	"*The BRT determined corals limited to the eastern Pacific, with approximately one third as many genera, less than half the species, less reef area, and high susceptibility to strong climate variability, were likely at even higher risk of extinction than those in the Caribbean, based on these regional attributes*". Peter Glynn has long extolled and documented the vulnerability of eastern Pacific corals, and he should be cited here. Why would lower species richness per se add to vulnerability? Add El Niño to the list of vulnerabilities.	El Nino effects are subsumed in "strong climate variability". We added a clause to emphasize that previous paragraphs describing vulnerability (including extensive Glynn refs) yielded this conclusion. Lower species richness provides evidence of vulnerability, not cause.	ch2	17	last para, last sn
42	Page 18, Table 3.1. The list of threats and their importance should really be tied to locations. For example, fishing and coastal construction is not a low risk along 2000km of China's coastline, and invasive species (lionfish) pose more than a negligible-low risk in the Caribbean. The table lists known threats – I would like to have seen more discussion of surprises (unknown threats), thresholds, and interactions between threats. "Drivers of change" might be a better mindset than "threats".	While the importance of the listed threats does indeed vary regionally and locally, this table is intended to show estimates of the importance of each threat to global extinction, as opposed to potential local extirpation. For clarification we changed the heading to "proximate threats".	ch3	18	
43	Page 19, second paragraph, last sentence. "*Meaningful progress in conserving and restoring coralreef ecosystems can be accomplished only by clarifying the social, economic, and cultural frameworks needed to address unsustainable human population growth and increasing pressures each human places on natural resources*". This comes across as too preachy, and a comfortable middle-class, western view of the world. I don't disagree with the sentiment, but the tone could offend some people.	The BRT agrees with the reviewers comment and has removed the sentence.	ch3	19	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
44	Page 19, third paragraph, last two sentences – typos. *~~…~~.billion in 12 years (1999) space (Population Reference Bureau 2010)….through the mid-1900s , and lessening of the mortality rate in many countries*".	We incorporated the recommended changes.	ch3	19	
45	Page 22, last paragraph, line 4. "*Human-induced emissions of CO2 are also accelerating, rising from 1.5 ppm yr-1 during 1990-1999 to 2.0 ppm yr-1 during 2000-2007 (Raupach et al. 2007, Canadell, 2007 #2013)*". I assume this should say that the rate of emissions is accelerating, or else emissions are rising annually "by" rather than "from". Delete the endnote numbers here and elsewhere.	We incorporated the recommended changes.	ch3	22	
46	Page 24, figure 3.2.3. Please put in Y-axis units on the left.	We added y-axis label: "avg. emission growth rate (%)."	ch3	24	
47	Page 24, first paragraph, line 5. Hoegh-Guldberg et al. (2009) is not the primary reference for temperature rises. They cite IPCC.	We replaced the reference.	ch3	24	
48	Page 24, last paragraph, line 5. *~~…~~..an acceleration of CO2 emissions in excess of the worst-case scenario used in the IPCC's Third and Fourth Assessment Reports*". This more or less repeats the last sentence on p22.	We revised the text to reduce redundancy.	ch3	24	
49	*Page 26, Table 3.2.1. Donner (presumably 2009?) is not the primary reference. I don't think a basinwide projection is very meaningful. For example, IPCC projections within the GBR-Melanesia province vary from no change at the equator to 3oC for the southern GBR (at 23oS) under A2 conditions.	The BRT made no change. Donner 2009 is the primary reference for the table as he pulled these data from the model output. While there is certainly finer scale variability, we think that this table a useful demonstration of patterns provides.	ch3	26	
50	Page 26, second paragraph, line 1. "*Bleaching and mortality of adult coral colonies are the most visible signs*" ….(insert) of the effects of Climate Change	We incorporated the recommended changes.	ch3	26	
51	*Section 3.2.2.1 Coral Bleaching. This section is rather poorly written compared to most of the report.	We have edited this section.	ch3		
52	*Page 26, second last paragraph, line 4. *~~…~~.an increase of only 1-2 °C above the normal local seasonal maximum can induce bleaching (Fitt and Warner 1995). At any location, a bleaching threshold can be determined at approximately 1 °C above…*". There's very little support for a 1-degree threshold. You need to explain degree-days.	While there may be greater predictive power to use of a variance-based threshold, there is considerable evidence for a 1 °C threshold at many reef locations. However, we did change the second sentence into: "Bleaching is best predicted using an index of accumulated thermal stress above a locally-established threshold."	ch3	26	
53	Page 26, last paragraph, line 2. *~~…~~.there is general agreement that thermal stress leading to bleaching and mass mortality has increased…*". Insert "has". Perhaps it would be clearer to say that the scale of bleaching and mortality has accelerated, with appropriate references?	We incorporated the recommended changes.	ch3	26	
54	Page 26, last paragraph, line 4. Typo. *~~…~~.was documented throughout various parts of the world (Williams and Bunkley-Williams 1990, Eakin et al. 2009) space (Wilkinson and Souter 2008) (Eakin et al. in press 2010).*	We incorporated the recommended changes.	ch3	26	
55	Page 27, first paragraph, line 1. *~~…~~.just showing real signs of recovery from a mass bleaching event in 1998 have recently experienced mass bleaching again in 2010 (Gillis 2010).*" Inappropriate reference to a newspaper article. There is a real literature on recovery from the 1998 event (e.g. Tim McClanahan, Nick Graham and others).	We added Wilkinson (2004) reference to recovery from 1998 bleaching event. This was the primary reference used for most of these areas in Baker et al. (2008) as well. The use of Gillis 2010 newpaper article is for 2010 event and we feel it is appropriate.	ch3	27	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
56	Page 27, first paragraph, last sentence. "*Unfortunately, most reefs have already surpassed that rate of warming in the last two decades (Strong et al. 2008) (Penaflor et al. 2009)*". This isn't true. The information from the Coral Triangle indicates that half of the region has experienced temperature changes of -0.1 to <0.2oC per decade. Reefs to the north have warmed more.	We edited this sentence to read "many". The BRT believed that the reference to over half of all global reefs exceeding that rate of warming is appropriate based on the Strong et al. (2009) reference, while some areas, including part of the Coral Triangle (Penaflor et al. 2009) are cooling slightly or warming more slowly.	ch3	27	
57	Page 27, second paragraph, line 3. "*Using global climate models…found that continued ocean warming will result…*". Models predict. They don't "find", show or demonstrate.	We incorporated the recommended changes.	ch3	27	
58	Page 27, third paragraph, line 1. "*Buddemeier and Fautin (Buddemeier and Fautin 1993) proposed that bleaching…".* Please tidy up the referencing in this section.	We fixed the citations.	ch3	27	
59	Page 27, third paragraph, last sentence. "*However, further work has indicated that this sort of adaptation may impart, at most, a 1.5 °C adaptability in bleaching thresholds (Baskett et al. 2009a), but even this provides some hope to corals in face of the warming expected to exceed at least 1 °C and more likely > 2 °C during 21st century (Donner 2009)*". Awkward English.	We edited the sentence and also added references to LaJeunesse lab work on symbiont-switching and reversion.	ch3	27	
60	Page 27, fourth paragraph. References to Gleason and Wellington 1993, Wellington and Fitt 2003, Kushmaro et al. 1996, and Kushmaro et al. 1997 are missing from the bibliography.	We added the missing references to biliography.	ch3	27	
61	*Page 27, sixth paragraph. "*Multiple climate change effects are likely to interact. A recent modeling study found..result in significant declines in reef health…*". Again, these are predictions, not findings. There is a rich literature on interacting impacts, with empirical evidence, which would be reviewed here in preference to an untested model. What exactly is reef health?	References to models throughout text refering to findings of the models are now stated as "predictions". The BRT feels there is no need to get into the great "reef health" debate here.	ch3	27	
62	Page 27, seventh paragraph, line 1. "*…causing pathogens to grow faster and be more virulent…".* Bruno at al. found a correlation between temperature and the *occurrence* of coral disease, but failed to demonstrate a mechanistic link. They may both be simply increasing with time. The study has no data on growth rates or on virulence.	We shortened the text to eliminate the reviewer's concern and rendundancy with later sentences.	ch3	27	
63	Page 28, first paragraph, line 1. "*Though partially a result of increased surveys to assess disease, observations of the number and severity of coral disease outbreaks over recent decades have shown a significant and concerning increase (Harvell et al. 2007) and the outbreaks are often either accompanied by or immediately following bleaching events (Jones et al. 2004, Lafferty et al. 2004, Muller et al. 2008, Brandt and McManus 2009, Miller et al. 2009) and the associated seasonal patterns of high seawater temperatures (Willis et al. 2004, Sato et al. 2009)*". The sentence is awkward and too long.	We edited the text.	ch3	28	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
64	Page 28, last paragraph, line 1. "*The calcium carbonate saturation state (Ω) describes the dynamics of the calcification process (Figure 3.2.6)*". This statement is misleading because it infers that calcification is more or less a physical process. The relationship between saturation state and calcification in corals is not linear, and many species can still calcify below a saturation state of 1 (when physically carbonate should dissolve). There is a large literature on this, by Allemande and others.	We edited the text.	ch3	28	
65	Page 29, first paragraph, line 1. "*Increasing saturation states above one tend to favor calcification…".* Yes, sort of, but see previous comment. Most coral species show little or no change in calcification as the saturation state is reduced experimentally from 3 to 2 or one. For many, it collapses suddenly around 0.8.	The BRT made no change. The reviewer's point is made in the subsequent sentences. The BRT was not aware of references to support the reviewer's supposition that "most coral species show little or no change in calcification as the saturation state is reduced...".	ch3	29	
66	Page 29, first paragraph, line 4. "*Many experts believe that coral reefs need an external saturation state of 4.0 or greater to thrive…*". That's just not true, which would explain the lack of citations in support of this statement; "external" isn't necessary. While Langdon has repeatedly made the 3.5 claim, repeated here on lines 4-5, many people have refuted it as being unfounded and alarmist. For example, see: Silverman et al. 2009. G*eophysical Research Letters*, 36, L05606.	We edited this sentence to remove the actual values and we now cite two substantive review articles. Silverman is now cited in table 3.2.2.	ch3	29	
67	Page 29, last paragraph, line 2. ~~"…~~.*spatial variation (figure 3.2.8)…".* This figure doesn't show spatial variation. Figure 3.2.10 does.	We incorporated the recommended changes.	ch3	29	
68	Page 31. The decline in pH in 1990-2005 is inconsistent between figures 3.2.8 and 3.2.9. The empirical evidence shows -0,04, the lower cartoon indicates about 0,10. Presumably the latter is incorrect?	The BRT made no change; 3.2.9 is a projection from before 1999 as is apparent from the 1999 reference.	ch3	31	
69	Page 32, Figure 3.2.10. Poor resolution figure. The use of two color scales isn't explained in the caption. Where is this figure mentioned in the text?	We replaced the figure with a higher quality version. The BRT added the text reference to this figure. The caption provides the information on the color scales: "(Bottom right) The difference between the GLODAP-based and CCSM-based 1995 fields. Note the different color scale of the difference plot."	ch3	32	
70	Page 33, first paragraph, last line and second paragraph, first line. "*For example, the coral Oculina arbuscula had minimal changes in skeletal accretion at aragonite saturation states from 2.6- 1.6, but a major reduction in accretion at a saturation state of 0.8 (Ries et al. 2010)"* and ~~"~~*A variety of studies conducted on corals and coral reef organisms (Langdon and Atkinson 2005) consistently show declines in the rate of calcification by corals with rising pCO2, declining pH, and declining carbonate saturation state".* These statements are contradictory. The *Oculina* example has a threshold, which is almost always the case, i.e. the decline in calcification is not "consistent". Most of the published examples are based on unrealistic laboratory studies.	We edited this sentence to reduce potential confusion. However, there is no conflict. The latter statement only indicates that they decline, not the function that describes them.	ch3	33	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
71	*Page 33, Figure 3.2.11. Langdon and Atkinson's study has been criticized because their analysis confounds location and taxonomy with carbonate state. In the caption, "Effect of…." should be "Regression of…". The 560 and 840ppm manipulations are suspect because they suddenly expose corals to conditions that will take place slowly over the next century. The relationship for any one species is almost always non-linear, as indicated earlier for Oculia, and not a straight line. Some coral cores show lower calcification in the past few years (possibly due to changes in pH), but no one has seen the drop in calcification predicted by this graph before then (e.g. since 1800 in long cores).	We edited the caption. The remaining comment is discussed in the text that follows.	ch3	33	
72	*Page 34, first paragraph, line 3. "*In addition to laboratory studies, recent field studies have shown a decline in linear extension rates in Porites spp. from the Great Barrier Reef (De'ath et al. 2009); and Thailand (Tanzil et al. 2009), and of Acropora palmata in Curaçao (Bak et al. 2009) that suggest that acidification already is significantly reducing growth of corals on reefs".* There are of course other potential causes of these declines, such as pollution, rising temperatures and disease causing physiological stress. The link to pH is very weak. Other field studies show no change in calcification despite temporal and spatial variation in pH.	We edited the text to include the reviewer's comment. The point may be valid, but there have been no studies that indicate any ability of corals to acclimate -- the key point made here.	ch3	34	
73	*Page 34, third paragraph, line 4. *~~…~~algae at CO2 levels expected later this century…Table 3.2.1"*. The summary of experimental studies exposing corals to manipulated seawater carbon chemistry (or related treatments) is useful, but how realistic are these? The thermal equivalent is dropping corals suddenly into water hot enough to kill them. There is no opportunity in these short-term experiments for the corals to acclimate or adapt, as they are likely to over a 50-100 year time-scale.	The point the reviewer makes may be valid, but the BRT is limited by the best evidence available. The BRT is not aware of studies that indicate any ability of corals to acclimate; we did not change the text.	ch3	34	
74	Page 34, fourth paragraph, line 4. *~~-~~Expected increases in CO2 will likely increase the rate of herbivory necessary to maintain conditions needed for recruitment of new coral colonies (Hoegh-Guldberg et al. 2007) (Figure 3.9)".* Presumably, this should be 3.2.12. The sentence is awkward because it infers that rising CO2 will cause an increase in herbivory. It isn't clear from this sentence or the figure caption that this is a model prediction. The caption should read "Model prediction of a *reduction in the resilience* ...". This figure is quite complicated and difficult to understand in isolation from the three cited papers. What you really need to make this point is a figure with coral growth rate along the x-axis, not grazing.	Figure reference was corrected. The BRT edited the sentence and caption for clarity. The BRT is not in a position to create a new graphic as this requires a new modeling study.	ch3	34	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
75	Page 36, first paragraph. The eastern Pacific is unusual for many reasons. Its low saturation state and high levels of bioerosion may or may not be a good predictor of future temporal trends. The case for linking bioerosion with saturation state would be stronger if the BRT could establish this link more broadly (e.g. at high versus low latitude reefs). There does not seem to be a consistently higher level of bioerosion at higher latitudes.	The BRT made no change. High latitude reefs have additional problems related to light and temperature, and generally do not have as great a reduction in saturation state. Comparable studies of high-latitude reefs have not been published.	ch3	36	
76	Page 36, first paragraph, line 10. ~~…~~*.such as hurricanes, vessel groundings, and anchoring."* The last two are trivial, as indicated elsewhere in the report.	The BRT made no change. These might not be trivial at lower calcification rates. Anchor damage causes large and long-lasting impacts in the eastern Pacific.	ch3	36	
77	*Page 36, first paragraph, line 12. "*Recent work has shown that topographic complexity has already been reduced in Caribbean coral reefs (Alvarez-Filip et al. 2009)* ." The loss of topography is primarily due to hurricanes and disease affecting *Acropora* . It has nothing to do with pH or bioerosion. If the point is that loss of corals affects associated species, there is a substantial literature that should be cited (Graham, Pratchett, Wilson, etc.).	The BRT made slight edits to this sentence. The reviewer's comments are in complete agreement with the sentences that immediately follow this one.	ch3	36	
78	Page 37, second paragraph, line 11. *…crustose coralline algae in mesocosm experiments in moderate OA treatments".* Explain OA. What does "moderate" mean?	We edited the text for clarification.	ch3	37	
79	Page 37, fourth paragraph, line 1. "*The IPCC Fourth Assessment Report (AR4) (IPCC 2007a) determined* concluded *that sea level will continue to rise...".*	We incorporated the recommended changes.	ch3	39	
80	Page 39, first paragraph, line 1. ~~"~~*Flooded shelves and banks at higher latitudes (greater than 15° N) may alter the temperature or salinity of seawater to extremes that can then impact corals during offshore flows."* Why would this phenomenon not occur closer to the equator, or in the southern hemisphere? I think the statement may just be referring to the Caribbean.	We removed the reference to "higher latitudes."	ch3	39	
81	Page 40, second paragraph, line 8. Typos. "..*the Walker circulation* space *(Ries et al. 2006). Vecchi et al. (Knutson et al.* year? *) examined changes in tropical…"*. Is the second sentence a reference to Vecchi et al. 2006 or to Knutson et al. of unknown year? Vecchi et al. 2006 does not appear in the bibliography.	We incorporated the recommended changes.	ch3	40	
82	Page 41, first paragraph, line 1. "*In another comparison of climate observations to models, Wentz et al. (Tissot and Hallacher 2003a) found that global* …". Is this a reference to Wentz et al. in an unstated year or a reference to Tissot and Hallacher 2003?	We edited the citation.	ch3	41	
83	Page 41, second paragraph, line 2. *…models…atmosphere system simulate* predicts *a weakening of Atlantic Thermohaline Circulation in response"*	We incorporated the recommended changes.	ch3	41	
84	Page 41, second paragraph, last line. Typo. "… (McMullen and Jabbour 2009)s…".	We fixed the typo.	ch3	41	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
85	Page 41, fifth paragraph, line 3. *…reduced the ability of coral reefs to recover from disturbance by slowing coral recruitment, growth, and fitness (Nystrom et al. 2000*). Slowing fitness doesn't make sense; it isn't a rate phenomenon.	We edited the sentence.	ch3	41	
86	Page 41, fifth paragraph, line 10. "*A recent modeling study out to 2099 found predicted that Montastraea-dominated Caribbean coral reefs are likely to maintain their community structure and function under any expected level of hurricane activity...".* This prediction (not finding) by Edmunds et al. contradicts an earlier modeling study of *Montastrea* by Hughes and Tanner (2000, Ecology).	We edited the sentence and also added a more recent reference (Thompson and Dolman 2010).	ch3	41	
87	Page 41,last paragraph, line 1. Buddemeier et al. (Buddemeier et al. 2004) argue that there is little evidence…	We incorporated the recommended changes.	ch3	41	
88	Page 42, first paragraph, line 10. Buddemeier et al. (Buddemeier et al. 2004)	We incorporated the recommended changes.	ch3	42	
89	Page 42, second paragraph, line 1. "*Iron- and clay-rich soils found on many Caribbean islands originated as dust from Africa……*". Hardly all of the soil!	We edited the sentence.	ch3	42	
90	Page 42, section 3.2.8. Seems very peripheral to the topic of the report, inconclusive.	The BRT made no change. We have several non-critical stressors that are discussed but do not reflect a major threat.	ch3	42	
91	Page 42, fourth paragraph, line 8. Typo. ~~*A*~~ *further challenge for the researchers..."*	We corrected the typo.	ch3	42	
92	Page 43, second paragraph. "*If aerosols and their interactions with clouds were the primary cause of dimming, a large part of current brightening is related to legislation and policies that have reduced air pollution*". Relevance? Is there any evidence to support this bold statement at a global scale? While car pollution may have been reduced in California, it certainly hasn't in Asia. "*Therefore, brightening is likely a restoration of insolation levels that would have existed without without industrial pollution.... relatively small changes in surface insolation will...likely have minimal effect on corals".*	The BRT made no change. The BRT agrees and concluded that these effects are too uncertain to incorporate into our analyses.	ch3	43	
93	Page 43, third paragraph, line 7. Typos. ~~—~~*.in latitudinal expansions space (Kleypas 1997). Buddemeier et al. (Buddemeier et al.) year reviewed possible consequences of global climate change…Although some have speculated that warming would allow coral reefs to migrate poleward to higher latitudes, Buddemeier et al. (Buddemeier et al.) year argued that such migrations would likely be impeded...otherwise form. Buddemeier et al. (Buddemeier et al.) year also suggested…"*	We incorporated the recommended changes.	ch3	43	
94	*Page 43, fourth paragraph. "*The rise of atmospheric CO2, and its concomitant impact on temperatures and ocean acidity, has already contributed to the deterioration of coral health and populations globally (Hoegh-Guldberg et al. 2007)*". I think the evidence for ongoing impacts of warming is unequivocal. But there is a lot of hype about what ocean acidification might do. Hoegh-Guldberg et al. do NOT demonstrate a global impact of acidification on corals "already".	We added a reference to Wilkinson 2008 as well in which this exact statement is paraphrased. The statement says that the rise of CO_2 has already contributed to coral declines through rising tempereratures and acidification. We changed the reference to acidification.	ch3	43	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
95	*Page 43, fourth paragraph. "*By the early 1980s, atmospheric CO2 levels had risen from pre-industrial levels of about 280 ppm to in excess of 340 ppm, and the return frequency of thermal stress events began to exceed the ability of many coral species to recover from bleaching and disease impacts, in some cases decreasing net coral reef structure (Alvarez-Filip et al. 2009)*". This sentence is poorly written and too long. By the early 1980s, most coral reefs around the world have not yet bleached. He year 1998 was the first regional-scale event outside the Caribbean and eastern Pacific. It's misleading to talk about return events before then. You could cite Hoegh-Guldberg (1999), but most of that study has been discredited. The compilation of reef topographic complexity by A-F provides no information on why coral structure collapsed. The primary literature indicates that hurricanes were a major cause.	We edited the sentence.	ch3	43	
96	*Page 43, fourth paragraph. "*Major coral disease outbreaks had begun across the Caribbean Sea in the 1970s*". This is wrong, which explains the lack of references. The first outbreak affecting *Acropora* was in 1976, and was restricted to a small part (5 hectares) of St. Croix. Some recent reviews and meta-analysis have also made this claim about early disease outbreaks, but there is no primary literature in support of the notion that widespread disease epidemics occurred before the mid- 1980s. There are not even anecdotes. There is a large literature from this period, including many long-term studies of coral assemblages in Jamaica, Panama, St. Croix, Belize, etc.	We have changed text and added references.	ch3	43	
97	*Page 43, fourth paragraph. Typo, "*Presently, atmospheric CO2…exceeding worst case scenarios used in modeling future climate change (CDIAC 2009* Close Bracket*, (IPCC 2007a)".*	We incorporated the recommended changes.	ch3	43	
98	Page 43, fifth paragraph, line 6. "*…slower than rates of anthropogenic CO2 increase, time to recovery is much greater than the length of the delay …".* Time to recovery of what?	We edited the text to clarify.	ch3	43	
99	Page 44, line 8. "*Thermal stress and resultant bleaching and disease are already killing corals and may have caused the first coral extinction*". Disease isn't *necessarily* associated with bleaching. It does seem to be associated with physiological stress, e.g. due to pollution, post-hurricane injuries, as well as stress from bleaching.	We replaced "resultant" with "associated". Not all disease is associated with thermal stress and bleaching. This is discussed in the disease section. However, that is not the point of the sentence. This only speaks to the bleaching and disease that result from thermal stress.	ch3	44	
100	Page 44, line 12. "*Between the direct (bleaching, acidification) and indirect (infectious disease) effects of rising temperatures…".* Same comment – infectious disease doesn't have to be triggered by high temperatures.	We made some small edits. Note: there is no statement here that thermal stress is a prerequisite for coral disease, only that it is one of the contributing factors.	ch3	44	
101	Page 44, Last Line. ~~…~~*.anthropogenic increases in atmospheric CO2, are likely* to be *the greatest threats to all …".*	We incorporated the recommended changes.	ch3	44	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
102	Page 45, fourth paragraph, line 1. "*There are two basic types of sediments on coral reefs—sediments that are generated in situ as bioeroding organisms break down the skeletons of corals and other reef organisms, and sediments that are terrestrial in origin*". A sedimentologist would cringe at this simplistic account. For example, *Halemeda* and foraminifera are major sources of nonterrigenous sediments. In the next sentence, add wind as a mechanism for re-suspending sediment.	We incorporated the recommended changes.	ch3	45	
103	Page 46, Figure 3.3.1. The text on page 47, line 1-2 indicates that arrows for settlers and juveniles should be higher than other life stages.	The BRT disagrees with the reviewer's comment. The text on lines 1-2 only notes that sediment affects settlement and survival; not necessarily that they are more vulnerable than other life stages.	ch3	46	
104	Page 46, first paragraph, line 6. "*…though the ability of a coral to survive sediment burial may be is size-specific (Gilmour 2002).*" Gilmour's study of fungids is not the best reference. See papers by Rolf Bak, Caroline Rogers and others.	Most sediment burial/rejection experiments deal with sediment size rather than colony size, and most field observations usually describe correlations between extant colony size and sediment stress (which often varies by species). Rogers 1990 has been cited as hypothesizing that small corals are more efficient sediment rejectors, but that work actually cites Dodge & Vaisnys 1977 as the source of that idea, which in turn mentions the idea as a theoretical construct in the introduction rather than one that was actually tested in the paper. The Gilmour reference was therefore retained here, with additional references to address the effects of polyp size and partial mortality.	ch3	46	
105	Page 46, second paragraph, line 1. "*In addition to direct mortality, sediment can induce sublethal effects, including histological disruptions (Vargas-Angel et al. 2007).*" Perhaps "revealed by" would be better than "including".	We edited the sentence and included the reviewers suggestion.	ch3	46	
106	Page 46, second paragraph, line 4. Reference format is wrong. "(*Dallmeyer et al. 1982, Riegl and Branch 1995, Telesnicki and Goldberg 1995, Te 2001) Philipp, 2003 #1539;Anthony, 2004 #1566;Weber, 2006 #1537}*".	We corrected the citation.	ch3	46	
107	Page 46, second paragraph, line 9. ~~…~~*.and can force corals to rely more heavily on asexual recruitment*". This is poorly worded. Again fungids are so different from other corals, I don't understand why this reference has been selected. Highsmith and others have argued that fragmentation in branching corals allows them to colonize sediments.	We edited this sentence and replaced the reference with Highsmith 1982	ch3	46	
108	Page 47, second paragraph, last sentence. These community-level effects are generated by direct and indirect effects, from sediment settling to the seafloor or turbid conditions in the water column.	The BRT was unsure what changes the reviewer recommended and made no changes.	ch3	47	
109	Page 47, third paragraph, line2. "*Human activity has increased riverine sediment inputs to the Great Barrier Reef over the past century*". True, but this is not just a GBR phenomenon.	We edited the sentence for clarity.	ch3	47	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
110	Page 47, third paragraph. Some of this text is duplicated earlier in the sea rise section. Line 4. "*Greater inundation of reef flats can erode residual soils and lagoon deposits (Adey et al. 1977, Lighty et al. 1978) and produce greater sediment transport (Hopley and Kinsey 1988)".* Reef flats are intertidal by definition and don't have soils. Do you mean coral cays?	This sentence was added to reflect that greater inundation would erode the shoreline and resuspend lagoon deposits. We clarified the text.	ch3	47	
111	Page 47, fourth paragraph, line 7. "*These natural sources may account for more material (nitrogen and phosphorus) than anthropogenic sources in highly developed areas such as the Florida Keys (Leichter et al. 2003)* ." Surely this is a typo?	This was not a typo; Here is a quote from Liechter et al 03 "The estimates presented in Table 3 suggest that nitrogen and phosphorus inputs to the slopes of the Florida Keys reef tract from internal bores are at least as large and possibly as much as 20-40 times greater than estimated daily inputs to near-shore waters from waste water and storm water. T" p.1403. We clarified the sentence to specify wastewater and stormwater inputs.	ch3	47	
112	Page 48, first paragraph, line 8. "*Nitrogen and phosphorus can both decrease calcification mass*". But not equally, and not under ambient conditions experienced by most reefs.	We agree with the point made by the reviewer and clarified the text accordingly.	ch3	48	
113	Page 48, second paragraph, line 3. "*~~…~~.settlement, and shift species to more asexual reproduction…*". The verb "shift" makes it sound like the corals make a decision. There is no evidence for a compensatory shift, ie. larval recruitment may be curtailed, but asexual recruitment continues at the same (or reduced) level.	We adjusted the wording in response to the reviewer's comment.	ch3	48	
114	Page 49, first paragraph, lines 1-3. "*~~C~~oral reproductive mode...planula production...fecundity...Reefs in eutrophic waters have lower densities of juveniles (Tomascik 1991)"*. "Highly polluted" would be a more accurate term than "eutrophic". The lower density of recruits in Tomasik's study is probably due to post-settlement survivorship rather than local reproductive output.	"Eutrophic" is the word that the author of this study used to describe the conditions of his study. The review does not explain rationale for 'highly polluted' being a prefereable term. We added the possibility of lowered post-settlement survivorship as the reviewer suggested.	ch3	49	
115	Page 49, third paragraph, line 8. "*Nonetheless, the role of nutrient enrichment in reef community phase-shifts remains controversial (Hughes et al. 1999a, Lapointe 1999, Szmant 2002, Bruno et al. 2009)*". Bruno et al. (2009) do not address this issue at all. They compiled existing data on coral and macroalgal cover.	We removed the Bruno et al. 2009 citation.	ch3	49	
116	Page 49, fourth paragraph. "*For example, coral larvae settle at high rates in algal turfs and crustose coralline algae, while the fleshy macroalgae Laurencia and Hypnea differed substantially in the degree to which they inhibited coral settlement (Diaz-Pulido et al. 2010)*". The second phrase is awkward.	We edited the sentence.	ch3	49	
117	P49, The fourth paragraph is not well-written, and each of the 3 sentences is awkward. Line 2. "*~~…~~.also being realized*" should be "also being revealed". Missing entirely is a summary of the literature on differential susceptibilities of corals in the Caribbean and elsewhere to macro-algal blooms.	We edited the paragraph and added Nugues et al. 2004 citation.	ch3	49	
118	Page 49, Section 3.3.1.3. The writing in this section is much better and more authoritative.	No change needed.	ch3	49	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
119	Page 50, second paragraph, line 11. "~~…~~*.copper Acropora cervicornis and Montastrea faveolata bioaccumulated the metal…*". I think most people use "bioaccumulate" in the context of accumulation up the food chain. Here, "uptake" would be clearer.	We edited the sentence.	ch3	50	
120	Page 52, second paragraph, line 1. "*Many coral reefs are heavily influenced by open-ocean seawater, creating relatively stable salinity conditions over the long term (Coles and Jokiel 1992)*". An odd construct. Most marine creatures are influenced by water.	The BRT feels that no change is needed. As the introductory sentence to a section on salinity, the sentence is simply emphasizing the point that salinity fluctuation is rarely experienced by reef organisms (hence probably constitutes a stress when it does).	ch3	52	
121	Page 52. Somewhere in this section there needs to be a discussion of haloclines and freshwater lenses, creating depth profiles in impacts of floods.	The BRT agrees with the reviewers comment and has added some text.	ch3	52	
122	Page 52, second paragraph, last sentence. "*Responses to salinity are controlled in part by behaviors, such as polyp retraction and mucus production (Muthiga and Szmant 1987, Manzello and Lirman 2003), and by exchange of osmotically active particles between the coral and its zooxanthellae (Mayfield and Gates 2007)*". What are these mysterious particles? Do you mean ions? There are additional mechanisms beyond these two.	We added examples of osmoregulatory particles.	ch3	52	
123	Page 53, third paragraph, line 1. "*Most salinity stresses to corals are driven by rainfall, or the lack thereof".* The latter is trivial except on landlocked shallow bodies of water. Is there a single study on hypersalinity affecting corals from the Caribbean, beyond the ancient paper on sponges by Walton Smith (1941)?	The BRT made no change. Porter et al 1999 show hypersaline intrusion from Florida Bay to the outer Florida Keys reef tract (i.e. warm hypersaline water on the bottom/reef surface).	ch3	53	
124	Page 54, first paragraph, line 6. Extended droughts can produce reef salinities of 40–71 ppt (Walton Smith 1941), and corals exist in hypersaline waters in areas such as the Red Sea. See comment above. What is the reference for the Red Sea? What does a "reef" mean in "reef salinities"? A lagoon?	We clarified the text and added a Red Sea reference (Falkowski et al 1984).	ch3	54	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
125	Page 54, second paragraph, line 1. Disease is broadly defined as ~~*any*~~ *impairment that interferes with or modifies the performance of normal functions...(Wobeser 1981)".* This isn't a useful definition. Most people use the term for pathogenic impairment only.	The BRT respectfully disagrees. The US Coral Disease and Health Consortium, in drawing on various medical models in human and wildlife health and epidemiological fields has adopted such a broad definition (Woodley et al. 2003. Coral Disease and Health: A National Research Plan. National Oceanic and Atmospheric Administration, Silver Spring, MD. 66 pp.; p.17). This broad definition provides for the full scope of phenomena (and related investigations needed) that affect coral health in profound ways, especially in the context of evaluating extinction risk. These phemomena range from reproductive impairment from toxological exposure to genetic defects, to the complex mechanisms of immunosuppression that may, in corals, be mediated by an interaction of environmental conditions and microbial symbionts (e.g., Ritchie K.B., 2006. Regulation of microbial populations by coral surface mucus and mucus-associated bacteria. Marine Ecology Progress Series 322, 1-14.). Given the poor knowledge of the etiology of most of the devastating coral diseases, we have no understanding of whether they result from a classic 'pathogen', an otherwise symbiotic microbe, a toxicant, etc. The next sentence articulates that the report is using the term 'disease threat' more in the sense that the Reviewer is	ch3	54	
126	Page 55, first paragraph, line 3. Here, the emergence of disease in the Caribbean is dated as the early 1980s, but earlier in the report on p43, you claim (incorrectly) that widespread outbreaks date back to the 1970s. The diseases are not "new" in the sense that they have not newly evolved. Also, in the sentence, ~~—~~*.and growing recognition of impacts on corals in the Indo-Pacific basin have followed (Green and Bruckner 2000, Sutherland et al. 2004, Bruno et al. 2007, Harvell et al. 2007, Galloway et al. 2009),* Sutherland et al is the only paper in this list that has ANY data on Indo-Pacific disease. Cite the primary literature, please. If it doesn't exist, you shouldn't just parrot an unsubstantiated claim made in a superficial review or by a crude metaanalysis of coral abundance.	We corrected the references on 1980s disease in Caribbean. We use the term "emergent" disease rather than "new" disease for this reason (they may have existed before but were not influential in coral populations). The reference list begun with Green and Bruckner 2000 supports the entire sentence it appears in which includes "rapid increases in the description of new diseases affecting corals, pervasive impacts throughout the Caribbean region". We respectfully disagree with the reviewer that Green and Bruckner 2000 and Bruno et al. 2007 do not consitute "data" on coral disease in the Pacific. The vast scope of the (global) assessments this team was assigned to undertake requires integration of the effects of threats throughout species wide ranges. The Global Coral Disease Database and the meta-analysis of Bruno et al. 2007 were judged by the BRT to provide reasonable evidence that the scope of coral disease in the Indo-Pacific region is significant in affecting coral demography.	ch3	55	
127	Page 55, first paragraph, Line 6: which two species are they? I presume they are both from the Caribbean. Miller et al.'s paper documents a 60% loss in total cover, not for just a single coral species.	We clarified the text; Miller et al. 2009 Table 3 shows *Montastraea* complex cover drops from 17.51 to 5.42 or 69%.	ch3	55	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
128	Page 57, Section 3.3.3. The predation paragraph is somewhat disjointed. Is the focus here on coral physiology or bioerosion? Can you convert 100 chaetodontid bites into a mass of tissue removed, to make it more comparable to the scarid data? Presumably it is tiny in comparison. Parrotfish play an important positive role in removing dead coral skeleton (work by David Bellwood and others), promoting recovery after bleaching.	Yes, a focus on either physiology or bioerosion would produce a slightly disjointed paragraph. The focus is on predation as a chronic and frequent energy drain on corals. Both physiological processes and calcium carbonate deposition involve metabolic energy costs which tie things together. It would indeed be a fine study to compare the energy drain from chaetodontid and scarine (formerly scarid) bites and perhaps this study can be undertaken someday, but doing so is outside the scope of this report. Likewise, the positive aspects of bioerosion by scarines is a good subject for further study, but this paragraph is focused on the costs to coral metabolism rather than the benefits towards recruitment of corals.	ch3	57	
129	Page 57, first paragraph, line 11. "*Schools of Bolbometopon can be 30 to 50 fish and so the school…"*. Or substantially more. The largest I've seen is about 300.	We added in the text that schools of *Bolbometopon* can be substantially larger.	ch3	58	
130	Page 58, second paragraph, line 1. ~~*In*~~ *undisturbed conditions, the distribution of corals is considered the status quo even though the realized niches…".* Awkward and very unclear. I would just start with the second sentence.	We edited the text and added references.	ch3	58	
131	Page 58, second paragraph, last line. ~~…~~*.can impede or even prevent the recovery of the coral populations".* "Hinder" might be better than "impede". But often it doesn't. Many corallivores, including some chaetodontids switch to alternative prey when corals are depleted.	We replaced "impede" with "hinder" and agree with the point made by the reviewer for an additional factor that contributes to the ability of predation by fishes to prevent the recovery of coral populations. Fishes such as puffers are quite generalized and so the absence of coral as prey does not affect the population density of their predators. If predators remain abundant or at least common when the prey are absent, this makes it especially difficult for corals to successfully replenish their population. Actually, chaetodontids are probably relatively unimportant in preventing the replenishment of coral populations, at least in comparison to the effects of certain species of puffers and filefishes.	ch3	58	
132	Page 58, final paragraph. "*Although there has been a strong theoretical interest in establishing networks of marine protected areas to promote larval subsidies from upstream populations, recent quantitative field studies have shown that the larval supply is generally more local and selfseeding than theoretically predicted, despite the current speeds and the potential longevity of the larval stage in the life history (Sammarco and Andrews 1989, Cowen et al. 2006)".* The term "theoretical interest" sounds odd. The choice of references is inappropriate since Sammarco and Andrews did not measure dispersal (they measured recruitment onto floating panels at different distances from a reef and inferred that the larvae came from it), and Cowen et al. is a modeling study. There is a substantial literature that does measure fish and coral dispersal directly (e.g. by Jones, Warner, Ayre, and many more).	The BRT agrees with the reviewer's comment and we replaced the previous references with a much more pertinent and approprite one, Warner and Cowen 2002 Bull Mar Sci on local retention of production in marine populations: evidence, mechanisms, and consequences.	ch3	58	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
133	*Page 58, final paragraph. "*Coral colonies are sessile and for spawners or brooders to fertilize one another, they must be within a few meters of each other (Littler et al. 1989a, Coma and Lasker 1997, Aronson and Precht 2001a, Hoegh-Guldberg et al. 2007)".* Most of these references provide NO support for this statement. The most relevant one, by Coma and Lasker, looked at a gorgonian and not a scleractinian. The others are reviews of completely different topics. Perhaps they have a throw-away line about Allee effects, but that's all. Clearly, most corals have densities that are lower than one every few meters, as noted elsewhere in the report. I'm not aware of any primary literature on this topic for corals. "*Steneck (2006) explained how the size of the ~~d~~dispersal kernel" or the distance over which larvae can subsidize downstream populations is determined by the effective population size (number of reproductively mature colonies of a species within a few meters of one another) of the source population".* This definition of "effective population size" (a key concept in population genetics) is incorrect, and so the sentence doesn't make sense. The Steneck reference is a *Science* commentary, which is	The BRT made some changes to the first sentence in the reviewer's comment and re-arranged the paragraph. Although Comer and Lasker studied the dilution of gametes of gorgonaceans rather than scleractinians by water currents in the field, it is not clear how the water motion would dilute the gametes of spawning gorgonaceans and scleratinians differently. There might be differences in the way the gametes of these two subclasses are affected by water motion, but at this time it is not clear how this would be. Certainly a comparative study is needed, but at this time the best available evidence (currently only from gorgonaceans) indicates that dilution is a problem for sessile spawning organisms. The commentary paper by Steneck seemed to not be a summary, but an essay on further implications, i.e., Steneck did not cite primary reference, but instead presented some new concepts that might be an important byproduct of the heuristic Cowen et al. 2006 study. The "effective populations size" is used as the number of individuals in the population capable of reproducing, which is a standard definition.	ch3	58	
134	Page 59, section title. "*Synergistic effects of predation"* and disease (?)	The BRT made no change, predation here is synergistic with population size of prey, rate of healing of prey, and disease of prey.	ch3	59	
135	Page 59, first paragraph, line 2. "*Healing rate time increases non-linearly with lesion size…".*	We incorporated the recommended changes.	ch3	59	
136	Page 59, second paragraph. "*In response to chronic and intense chaetodontid predation, coral polyps may be withdrawn into their calices for long periods of time, and eventually the polyps can increase nematocyst density (Sammarco 1980)".* Polyps do not withdraw into their calices. Sammarco's paper was on sea urchins, so this sentence doesn't seem credible.	The BRT agrees with the reviewer's comment, we replaced the Sammarco reference with Gochfeld 2004 and removed "into their calices."	ch3	59	
137	Page 59, second paragraph. "*It is reasonable that as the coral populations decline, the predation becomes more focused and therefore intense, the energetic cost to the coral becomes greater and healing of lesions might become slower, and the fecundity of the colony may be reduced. This interaction between concentration of predation and population size of the prey can become a positive feedback once a threshold is crossed".* Already stated, in the dispensation section, p.58.	The BRT made no change, this is a key concept in both locations.	ch3	59	
138	Page 59, fourth paragraph, line 4. Ayling and Ayling (1997) reference is missing from the bibliography.	We added the missing references to the bibliography.	ch3	59	
139	Page 59, fourth paragraph, line .6-7. …"*The most probable usual cause of outbreaks is considered to be nutrient runoff from land that boosts phytoplankton blooms, which in turn provide food for the larvae of the predators and facilitate abundant recruitment*". Add references to Birkeland, Fabricius and D'eath 2010 (Ecology).	We incorporated the recommended changes.	ch3	59	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
140	Page 59, fifth paragraph. This distinction between chronic predation and acute outbreaks in terms of their ecological impact is driven by the disparity between the generalist diet of *Drupella* and *Acanthaster* compared to the more specialist diet of other corallivores that don't reach high densities. These two outbreaking species still have a generalized diet at low densities, so the inference in this paragraph isn't quite right. The two species do not consume alcyonaceans. There is a substantial literature on recovery following crown-of-thorns outbreaks, with influxes of coral recruits (e.g. by Colgan, Moran, etc), which should be cited here.	Although some species of predators tend to be more specialized (and thereby tend to be selective) than other species of predators, the generalists usually tend to be increasingly selective as prey become more abundant. Ecological "impact" is also strongly affected by the change in selectivity of generalists as the prey changes in population density. Morgan Pratchett (2010. Changes in coral assemblages during an outbreak of *Acanthaster planci* at Lizard Island, northern Great Barrier Reef (1995–1999). Coral Reefs 29:717-725) and Morgan Pratchett et al. (2010. Recent disturbances augment community shifts in coral assemblages in Moorea, French Polynesia Coral Reefs on-line) both document that although most of the species of coral in the area appear in its diet, COTS generally selectively focuses on the relatively fast-growing *Acropora* and *Montipora* and this selective predation by the generalist seastar (a generalist because its diet will include almost all the species of scleractinians, even at low COTS densities), the nature of the change it brings about in the coral community structure is a function of the relative abundance of the predator and the overall	ch3	59	
141	Page 60, first paragraph, line 1. ~~—~~.*process is called a trophic cascade effect of removal of top predators".* Awkward. There is a huge body of literature on this (Hughes, Steneck, Hay, etc) that long pre-dates the 2007 studies.	The BRT made no change. There is indeed a huge body of literature on trophic cacades that dates back to over half a century in limnological, rocky seashore, and coral reef studies. Here we just cited a couple of the more recent important papers and the reader can go back to the roots starting with the more recent.	ch3	60	
142	Page 60, first paragraph, last line. "*Therefore, they are only generally present in their natural state on remote Pacific islands (Stevenson et al. 2007, Sandin et al. 2008)".* Don't forget the remote parts of the Indian Ocean, Papua New Guinea.	We changed the sentence to include these remote areas.	ch3	60	
143	Page 60, section 3.3.3.5. I didn't find the summary very useful.	The BRT thinks this section clearly summarizes the key points that are conceptually important for the managers.	ch3	60	
144	Page 60, fourth paragraph, line 1. "*There are fundamental differences in ecosystem-level processes between coral reef and pelagic fisheries*". So?	The BRT thinks this is very important for resource managers and rarely realized.	ch3	60	
145	Page 60, fourth paragraph, line 4. "*Fishing, or even overfishing, by humans does not influence the process of upwelling…*". That seems a little too obvious.	The BRT thinks that it is usually not realized by the public and decision makers that overfishing on coral reefs can have effects at the level of ecosystem processes and also bring about phase-shifts because overfishing pelagic populations has not historically affected the pelagic ecosystem processes. This is obvious to coral-reef scientists, but not at all obvious to the public and to decision-makers.	ch3	60	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
146	Page 60, fourth paragraph, line 9. *~~…~~.removal of fishing pressure in marine no-take reserves can restore coral recruitment…* ". Removal of macroalgae is the key issue, so perhaps this sentence should point to removal of fishing pressure on herbivores in particular; "restore" is a loaded term, and "rebuild" might be better. Mumby showed higher coral recruitment, less macroalgae, and more grazing inside a no-fishing reserve. But the amount of macroalgae in these reserves is still substantial compared to the historic baseline, and coral recruitment is nowhere near as high as that recorded elsewhere (Jamaica, St. Croix, Bonaire) before the *Diadema* die-off. The effects of recovering *Diadema* on macroalgae and coral recruitment should also be discussed here. *Diadema* are still virtually absent in the Bahamas.	The next two paragraphs point to the removal of herbivores in particular as recommended. "Restore" has been replaced with "rebuild". *Diadema* and *Echinothrix* have always been virtually absent in the central Pacific. Perhaps phytoplankton as food for echinoid larvae is less reliable in the oceanic tropics than in continental tropic waters.	ch3	60	
147	Page 60, fifth paragraph, last sentence. "*Under these conditions of topographic complexity with substantial populations of herbivorous fishes, as long as the cover of living coral is high and resistant to being affected by environmental changes, it is very unlikely that the algae will take over and dominate the substratum".* The writing is unclear, and the inference here is incorrect. A healthy reef can lose all of its corals (e.g. from recurrent cyclones) and still recover without flipping to persistent blooms of macroalgae. See, for example, Connell's epic work from Heron Island or Colgan's studies of recovery following severe outbreaks of *Acanthaster*.	Although examples can be found of corals recovering from events of nearly total devastation, numerous examples are well known of phase shifts to algal communities and so we do not wish to say it is very unlikely that the algae will take over. We feel that it is relatively safe to say that corals will prevail and algae are unlikely to take over under the conditions we listed. We do not want to go too far out on a limb.	ch3	60	
148	Page 60, sixth paragraph, line 5. *~~…~~.collapse into an alternative stable state or ~~p~~hase shift" (Mumby et al. 2007b)* . These concepts were originally demonstrated for coral reefs by Done (1992), and Hughes (1994).	The BRT made no change. The concept of alternative stable states in natural communities is indeed at least half a century old. Eugene Odum had it in his Fundamentals of Ecology in the 1950s. John P. Sutherland wrote a frequently cited review in American Naturalist in 1974 Multiple stable points in natural communities. But here we are just citing a recent one and a literature review of the history of the concept might dilute the text too much.	ch3	60	
149	Page 61, Figure 3.3.7. Overfishing and destructive fishing practices shouldn't be combined, because they are so different. Fishing of herbivores leading to algal blooms also affects coral fecundities (Tanner 1996).	The BRT has separated reef fishing and destuctive fishing into 3.3.4 and 3.3.5 as requested.	ch3	61	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
150	Page 61, first paragraph, line 1. "*Although algae can have a negative effect on adult coral colonies, the ecosystem-level effect of algae is mainly by the inhibition of coral recruitment*". I agree that recruitment-failure due to algal blooms is very important (and depending on the storage affect, it impacts some species more than others). But, I think you have understated the role of differential mortality due to overgrowth by macroalgae of established corals. For example, blooms of *Lobophora* have smothered many deep-water corals in the Caribbean, with platey morphologies being more susceptible than others. There are a dozen or more studies showing this, mostly from Jamaica and Curacao.	The BRT made no change. While the more limited historical observation opportunities in deeper environments may have given the impression that *Lobophora* were "smothering" live coral, there are now cumulating reports of acute coral mortality events affecting deeper Caribbean coral popualtions (over 20-50 m) with *Lobophora* increases often following (Miller&Williams 2006; Nugues&Bak 2008; Menza, et al. 2007. A deep reef in deep trouble. Continental Shelf Research 27, 2224-2230; Smith et al 2010). Nugues and Bak (2008. Long-term dynamics of the brown macroalga *Lobophora variegata* on deep reefs in Curaçao. Coral Reefs 27:389–393) specifically note that increases in *Lobophora* cover at 20 and 30m in Curacao most likely resulted from coral mortality rather than the algae "smothering" live coral. Indeed, at least several studies specifially suggest that most spp of intact scleractinians can effectively resist *Lobophora* competition (Nugues&Bak2006, De Ruyter Van Steveninck, et al. 1988), at least in Curacao.	ch3	61	
151	*Page 62, first paragraph, line 3 onwards. Typo. "*Raymondo and colleagues 2009 space found that overfishing appears to increase the frequency of coral disease*".	We fixed the typo.	ch3	62	
152	*Page 62, first paragraph, "*Fishing activity usually targets the larger apex predators*". But for most reefs and reef fisheries today, this is ancient history.	The BRT revised these two sentences to try to clarify the findings of the Raymundo et al 2009 paper. Actually, a recent study in SE Oahu showed that fishers target larger fishes even when the larger fishes are very rare. The average time to catch an ulua in eastern Waikiki is over 40 hours, yet ulua (*Caranx ignobilis*) is the primary target. The fishers still like to spend time fishing even when they rarely catch a targeted fish.	ch3	62	
153	*Page 62, first paragraph, "*When the predators are removed, corallivorous chaetodontids become more abundant*." This needs a reference. There is some evidence to support it from Australia (Williamson and Russ compared in and outside no-take areas), but my impression is that most degraded reefs around the world have lost their predatory fish AND their chaetodontids.	The BRT agrees with the reviewer's comment. If the reefs are degraded to the extent that the coral cover has been substantially decreased, then the corallivorous chaetodontids also decrease. We believe this is referring to where the predatory fish are removed by fishing pressure rather than reef degradation. We added Raymondo et al. 2009 as a reference.	ch3	62	
154	*Page 62, first paragraph, "*Corallivorous chaetodontids can transmit disease from one coral colony to another as they move around and take bites from each coral colony.*" The evidence for this is scant.	We added citation of Aeby, G.S., and Santavy, D.L. 2006. Factors affecting susceptibility of the coral *Montastraea foveolata* to black-band disease. MEPS 318: 103-110 which demonstrates in laboratory experience that transmission by chaetodontid bites can occur. However, they also show this can be countered by predation on the black-band disease itself so there are two process simultaneously countering each other. Nevertheless, they have demonstrated that bites can occasionally transmit disease.	ch3	62	
155	*Page 62, first paragraph, "*As they become more abundant, they transmit disease more thoroughly*". As far as I know, nobody has documented an increase in chaeotodontids, while showing that they have also caused an increase in disease. This paragraph needs numerous supporting references for each statement to be credible.	The BRT revised these two sentences to try to clarify the findings of the Raymundo et al 2009 paper.	ch3	62	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
156	*Page 62, second paragraph, line 2. "*There is general agreement that habitat degradation is the most important threat to the long-term recovery of exploitable fisheries stocks (Benaka 1998)"*. How general? Surely the biggest impediment to fish recovery is ongoing fishing. The Benaka reference, a symposium abstract, is woefully inadequate. You could cite work on fish recruitment after 1998 in the Indian Ocean and Pacific by Graham, McClanahan, Wilson and Pratchett. I don't understand why these 30 or so papers are ignored in favor of an obscure abstract.	We edited the sentence and replaced the citation.	ch3	62	
157	*Page 62, second paragraph, line 9. "*Trawls clearly dislodge and abrade corals.. .*". No sane trawler captain would approach a coral reef. This phrase seems very hypothetical – it appears to be confusing tropical coral reefs the much more real issue of trawling in deep-sea cold water coral assemblages.	The BRT made no change. The phrase is used to introduce the main point of the sentence on traps and impacts of inadvertent trap movement. While not judging on sanity, at least one BRT member has observed trawler crews removing coral that had become entangled in nets.	ch3	62	
158	Page 62, third paragraph, line 3. *—.explosive or toxic chemicals…are not as well documented in Caribbean waters".* The issue here is not documentation. Bombs and cyanide are not an issue in the Caribbean.	The BRT made no change. The use of toxic chemicals ('bleach, formalin, and gasoline') in collecting for the aquarium trade, at least, has been reported in Puerto Rico (Sadovy 1991, http://www.drna.gobierno.pr/oficinas/arn/recursosvivientes/negociado-de-pesca-y-vida-silvestre/la) and bleach in subsistence fisheries in Haiti and the Dominican Republic (Linton et al. 2002).	ch3	62	
159	Page 62, fourth paragraph, line 2. "… *live corals (64%) and live rock (95%) for the aquarium…*". It isn't clear that these are proportions of global trade(?)	We reworded the sentence to clarify that these are the U.S. proportion of global trade.	ch3	62	
160	Page 62, fourth paragraph, line 6. "*Much harvest of …".* Poor English.	We rephrased the sentence to "Much of the harvest…"	ch3	62	
161	*Page 62, fifth paragraph, line 1. "*The numbers of aquarium fishes taken from coral reefs is about 20 times the numbers of live coral taken (Tissot et al. 2010)".* What is the point of comparing numbers of juvenile fish with corals? This reference comes from Hawaii, so what is its global relevance? Certainly, in terms of biomass or ecological impact, harvesting corals from the tropics is more important than *Nemo* . Hawaii might or might not be an exception, but it is a trivial proportion of the global coral reef ecosystem.	Later in the paragraph it is clearly explained how the collection of reef fishes and featherduster worms is more harmful then the collection of corals per se. This is because the dislodging of corals on a large scale with nets and towels, and the use of cyanide in the Philippines is quite damaging to the reef structure while the collection of corals themselves is more discrete and done with the intention of keeping the coral alive for sale.	ch3	62	
162	Page 62, fifth paragraph, line 9. "*According to the World Wildlife Fund, six thousand divers in the tropical Pacific inject…33 million heads…* ". How credible is this statement?	The BRT does not have information to question the credibility of this statement by the World Wildlife Fund. Even if their estimates are skewed one way or another, the point of inclusion in the SRR is simply to demonstrate that this threat exists. We added "According to three precautionary estimates, the reef-degrading capacity of the cyanide fishery for food fish on Indonesia's coral reefs amounts to a loss of live coral cover of 0.047, 0.052 and 0.060 m2 per 100 m2 of reef per year (Mous et al. 2000)."	ch3	62	
163	Page 63, first paragraph. The writing in this paragraph is especially disappointing. "*Stony corals are generally sessile and externally fertilized* …". Apart from fungids, corals are overwhelmingly sessile, while brooders by definition have internal fertilization (Kerr, Baird quantify the prevalence of brooders).	We edited the sentence to read: "Since stony corals are predominantly sessile and most are externally fertilized (~75%), sustainability of spawning populations depends …"	ch3	63	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
164	Page 63, first paragraph. "*There may be thousands of colonies of a particular species in an archipelago, but if they were nearly all more than 10 m apart (Coma and Lasker 1997), depensatory Allee effects will have commenced*". The report fails badly to discuss current knowledge of commonness and rarity in corals. Depending on what is meant by an "archipelago", a common species could have a population size of many millions. Cleary, most corals are much less abundant, and have always been relatively rare. The 10m concept, based on a single species of gorgonian, is a very, very poor argument that dispensatory effects have "already commenced". I would pick this as the least convincing statement in the entire report.	We have rewritten the paragraph in consideration of this point.	ch3	63	
165	Page 63, first paragraph. "*Hence, the practices of aquarium trade collectors matter (for what, why? Where?) and they should? structuring their harvest to leave colonies in close proximity to each other? can reduce species level threat from what?. A similar precaution should be taken with brooding corals."* So, are the preceding sentences referring to spawners only? ~~The~~ *The local coral? communities can replenish themselves if they have local reproductive stock, but they cannot replenish themselves from populations kilometers away"*. Why on earth not? The author of this paragraph seems to have confused fertilization processes with larval dispersal.	We think what was meant by replenishment from populations far away is the difference in the regularities and numbers of recruits required for maintaining biogeographic distributions versus the regularities and numbers of recruits required to maintain populations subjected to predation, competition, usual physical damage and aquarium trade collection. The text refers specifically to *Heliopora.* We edited the text to clarify this.	ch3	63	
166	Page 63, second paragraph, line 3. ~~…~~. *so if a fish becomes scarce it is not targeted until its stock recovers".* Unfortunately, that sentiment is wishful thinking. In particular, it simply doesn't apply to the mixed (largely artisanal) fisheries of coral reefs globally.	We agree with the point made by the reviewer and deleted the sentence.	ch3	63	
167	Page 64, second paragraph, line 1. "*Collection of some coral reef animals for trade has caused virtual elimination of local populations, major changes in age structure, and promotion of collection practices that destroy reef habitats (Tissot et al. 2010)".* The reference here is a 4-page paper from Hawaii. What animals? Is it reasonable to extrapolate this modest study to the rest of the world? Obviously, there is a broader literature.	The reference was provided to give some examples of local effects, but we did not extrapolate to the rest of the world. We concluded that the effects of the aquarium trade was minor compared to the effects of overfishing, sedimentation, climate change, etc.	ch3	64	
168	Page 64, second paragraph, line 7. "…*the size of corals targeted for collection was smaller than exceeded the minimum reproductive size…".* Depending on the species, most corals start to reproduce when colonies are about 5-10cm in diameter. Did Ross (1984) really show this?	We made the suggested change.	ch3	64	
169	*Page 64, fourth paragraph, line 4. "…*BRT considered storm events to have the potential to significantly reshape the zonation of coral communities* …". What is the rationale/evidence for this statement? What is the timeframe? Where?	Essentially storms are small-scale (along the storm track, spatial scale depends on wave energy which in turn is driven by storm size and strength), discrete events from which reefs, in the absence of other stresses, can recover. However that depends on storm interval.	ch3	64	
170	*Page 65, first paragraph, line 10. ~~P~~*reliminary stabilization of loose fragments and other rubble is more likely when accomplished by reductions in wave energy is moderate or low …".* (cite appropriate references by Highsmith, Smith, etc).	The BRT made no change. The reference used here is a review/synthesis paper, and adequately supports the statement made.	ch3	64	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
171	Page 66, first paragraph, line 1. "*Storm waves are much longer in duration and often bring significant rainfall, while tsunamis add additional disturbance…*". Very poor writing. Waves have a wavelength and frequency. What do you mean by duration? Waves do not "bring" rainfall. Perhaps you mean "coincide with", but if so, then where, when? Of course, tsunamis bring additional disturbance - rarely, and in a few places.	We edited the text for clarification. The duration refers to the storm, not the waves per se.	ch3	65	
172	*Page 66, first paragraph, line 9. "*~~…~~.hurricanes are correlated with reduced recruitment of massive species (Crabbe et al 2008)*". This sweeping, ill-informed statement is based on a 4-page modeling study.	We deleted the statement.	ch3	66	
173	Page 66, second paragraph, line 3 onwards. "*The northern GBR has lower cyclone risk than elsewhere in the system…"*. Not exactly. Historically, cyclone frequency is highest in the middle of the GBR, and declines rapidly to the south as well as northwards. Puotinen's study is confined to the Australian side of the Coral Sea, rather than all of the world's *~~n~~on-equatorial (poleward of ~ 5° latitude) oceanic atolls…".*	Puotinen specifically states the GBR is at risk the least amount of the time (p.114, p.117), but the statement refers to risk of damage rather than cyclone frequency as the reviewer interpreted. The text has been clarified. The statement about atoll latitudes is indeed an erroneous statement and has been removed. The sentence was also modified to clarify that estimates risk based on hindcast models of wave conditions from historical storm tracks, rather than estimating risks of storms themselves.	ch3	66	
174	*Page 66, third paragraph. This paragraph is very flawed. "*Caribbean-wide, hurricanes have resulted in an average reduction in coral cover of ~ 17%, with no evidence of recovery for at least eight years (Gardner et al. 2005)".* A disappointing feature of this section of the report, is that it meekly repeats earlier assertions, without assessing their credibility. The 17% and 8-year metrics, as average "Caribbean-wide" responses, are not convincing. The range is 0-100%, and an average is meaningless.	The data supporting the statement in the text are given in Figure 2a in Gardner et al., and the -17% change in coral cover has a bootstrapped 95% confidence interval of -27.8 to -13.5 (p. 179). Of course some storms are worse than others and damage is patchy, but no range information is given in the paper to support the reviewer's comment. Nevertheless, the original statement was poorly written and subject to misinterpretation, and so this section was reorganized somewhat using examples from other paragraphs to illustrate the reviewer's point.	ch3	66	
175	*Page 66, third paragraph. "*In the Pacific, the substantial fetch….is somewhat offset by generally higher growth rates in the Pacific*". The writing is poor, so I can only assume that "higher growth" is relative to the Caribbean? Of course, the Indian Ocean also has a large fetch and big swells. Growth rates of corals vary latititudinally, so even if you compare genus by genus between the Caribbean and Pacific, high latitude Pacific corals grow more slowly.	We edited the text and the phrase about higher growth has been deleted.	ch3	66	
176	*Page 66, third paragraph. "*Patterns of storm damage and recovery can follow intermediate disturbance hypothesis (Aronson and Precht 1995), or create a mosaic of shifting steady states (Done 1999). However, despite storm-induced variability at local scales, coral reefs are relatively stable at landscape scales (Bythell et al. 2000)".* Sorry, this just doesn't make sense. What patterns of damage and recovery? What is stable – diversity, composition, cover?	We agree that the statements here are too vague and brief. Instead, some of the references here are rewritten as examples to address the range/variability the reviewer mentiones in line 177 of this spreadsheet.	ch3	66	
177	Page 67, section 3.3.7.1. This section on invasive species should highlight the introduction to the Caribbean of the *Diadema* disease, and of lionfish.	The lionfish is highlighted in another section. We agree the *Diadema* disease might be from Hawai'i where a similar disease struck *Diadema* and *Echinothrix* in 1981-1982, but we know of no solid evidence that it was introduced to the Caribbean. Is there evidence that it was not latent in the Caribbean all along?	ch3	67	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
178	Page 68, second paragraph, last line. "*In Hawaii, there are 287 introduced marine invertebrate species,...and relatively few have become established* …". How many are "established"?	All 287 are established in the sense that they are apparently maintaining viable populations, but only about 5 have become noticeably abundant and have taken over a substantial amount of space.	ch3	68	
179	Page 68, third paragraph, last line. Add the Caribbean origin of *Carijoa riisei* , and note that it is an octocoral rather than a scleractinian.	We noted that *Carijoa riisei* is an octocoral. Although *Carijoa riisei* was originally found and described in the Caribbean, recent genetic studies at HIMB indicate that it probably (at least possibly) originated in the South Pacific and was possibly introduced on the bottom of old wooden sailing ships three or four centures ago. This is not yet decided so we will not bring it up in this report.	ch3	68	
180	Page 69, first paragraph, last line. "~~…~~*.and the two black corals experienced niche compression".* In plain English, does this mean their depth range has been compressed by extirpation from shallower sites?	We changed the wording for clarification.	ch3	69	
181	Page 70, fifth paragraph, line 2. "*Impacts to reef food webs... significant changes in the coral reef fish complex, with unknown synergistic impacts to the corals".* Fish assemblages? Synergistic interactions between what and what?	We changed "synergistic" to "cascading" for clarification.	ch3	70	
182	Page 70, fifth paragraph -~~O~~*verfishing is typically thought of as a human-induced issue"* . Seems rather obvious. Delete, and remove the "However" from the last sentence.	The BRT made no change. We decided to leave it in as a heuristic perspective.	ch3	70	
183	Page 70, section 3.4 Heading. "*Interactive and Unknown CrypticThreats to on Coral Populations"*	We changed the section heading to "Interactive and Unapparent Threats on Coral Populations"; we know these types of threats are out there, but we cant predict exactly what they are or how they mainfest or interact.	ch3	70	
184	*Page 71, second paragraph, line 9. "~~…~~*.release of some coral pests such as butterflyfish…".* It is ridiculous to call butterflyfish a pest. Degraded reefs generally lose their corals, their top predators, AND their chaetodontids. The notion of reefs being over-run by butterflyfish is not supported by the literature.	We changed term to coral "predators", "pests" was used in the sense of an organism that is harmful to another, not necessarily one that is "over-run".	ch3	71	
185	Page 71, second paragraph, last line. "~~…~~*. bleaching resistance west (West and Salm 2003)".*	We corrected the citation.	ch3	71	
186	Page 71, third paragraph, line 1. "*Cryptic effects*…". Cryptic larval settlement is a well-established term, and Bellwood et al's 2004 *Nature* paper talks about a cryptic loss of resilience, but "cryptic effects" isn't very clear.	We changed the term to "unapparent effects".	ch3	71	
187	*Page 71, third paragraph, line 7. "*…there are no known approaches to quantify what the effect of that reduced fecundity would mean for coral recruitment".* Not true. Hughes et al (2000, Ecology) measured the relationship between spatio-temporal variation in fecundity and recruitment by acroporids. They found that declines in coral fecundity and spawning have a disproportionate effect on recruitment.	We edited the text to include the reviewer's comment and added "but there are very few examples with comparable field data. Hughes et al. (2000) measured the relationship between spatio-temporal variation in fecundity and recruitment by acroporids. They found that declines in coral fecundity and spawning have a disproportionate effect on recruitment."	ch3	71	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
188	Page 71, final paragraph, line 9. ~~...~~*.fishing reduced coral cover by 51%"*. How? Is this a spatial comparison between fished and non-fished reefs?	The BRT made no change. A long-term data set of hard coral cover from Kenyan reefs was used to examine the independent and interactive effects of two stressors: fishing and a temperature anomaly in 1998 that caused mass coral bleaching and mortality. While both stressors decreased coral cover, fishing by 51% and bleaching by 74%, they did not interact synergistically. Instead, their combined effect was antagonistic or weakly additive. The observed nonsynergistic response may be caused by the presence of one dominant stressor, bleaching, and cotolerance of coral taxa to both bleaching and fishing stressors. Consequently, coral bleaching has been the dominant driver of coral loss on Kenyan reefs and while marine reserves offer many benefits to reef ecosystems, they may not provide corals with a refuge from climate change.	ch3	71	
189	Page 72, first paragraph, Last line. ~~...~~. *the following four parameters at a variety of spatial scales: 1) abundance, 2) productivity, 3) spatial structure, and 4) diversity".* Of what? For example, productivity and diversity usually refer to ecosystems, not individual species.	We clarified that parameters are evaluated at the species scale. Productivity and diversity as used in this evaluation are defined later in this section. As defined, productivity and diversity are appropriate parameters for species-level evaluation.	ch4	72	
190	Page 72, second paragraph, Last line. "*In very few cases have studies considered the actual number and demographics of distinct genets (Baums et al. 2005, 2006)".* Genetic studies by Ayre, Benzie and others certainly have. Genet-level demography is a feature of Joe Connell's work, because he followed genets from recruitment for 30 years.	We added references to Ayre&Hughes 2000 and Coulson 2001.	ch4	72	
191	*Page 72, first paragraph, Last line. "*It is useful to note that productivity (sensu fisheries) is often a better indicator of extinction risk than overall abundance—a large population can be quite vulnerable if it lacks resilience and conversely a relatively small population can be robust if it has high productivity (Fig. 4.1.1)".* It's not quite so simple. I don't like the term productivity as used rather vaguely here. You seem to be talking about reproductive or regenerative potential. Largely missing from the report is the concept of the "storage effect". In brief, a long-lived, low fecundity species (with low productivity, as used here) is often very resilient because the population can persist for decades with little or no recruitment. You seem to be arguing the opposite.	We added a paragraph about productivity and life history stategies (r and K selection, storage effects) to clarify. Species with storage effects can show stable population sizes in the face of short-term environmental variabily compared to species without. However, long-lived species with low productivity are vulnerable to extinction if adult mortality increases or productivity declines below the already naturally low levels.	ch4	72	
192	Page 72, first paragraph, Last line. "*If there are directional changes…these types of data provide less confidence as a basis for estimating extinction risk".* You could state this more strongly by pointing out that linear extrapolation will almost always under-estimate the risk.	We added the sentence "In the case of increasing anthropogenic threats, the stationarity assumption is violated and a simple extrapolation of historic trend data will tend to underestimate risk."	ch4	72	
193	Page 74, section 4.2. Abundance and Productivity Regenerative Capacity of Corals Page 74, fourth paragraph, line 8. Typo. Italicize Dendrogyra cylindrus.	We made no change to the caption heading and fixed the typo.	ch4	74	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
194	Page 74, fourth paragraph, line 9. –*The only comprehensive data were for the few species of Montastraea".* Data on what? What are the references for *Montastrea*? If you mean long-term species-level data on abundance and demography, then this statement is too strong. For example, in the Caribbean, species-level trends over 20+ years have been documented by Bak, Hughes, Rogers, and others.	We chagend the sentence to "Time-series data were available for the candidate *Montastraea* species partially because they make up such a predominant part of live coral cover. "	ch4	74	
195	Page 74, fifth paragraph. This is a key part of the report, and I couldn't agree more.	No change needed.	ch4	74	
196	Page 75, first paragraph, line 1-2. "*For some of the Montastraea species, data are available on juvenile recruitment (Edmunds et al. 2010 in press)".* I assume this refers to the Caribbean *M. annularis* complex, and not to *M. cavernosa* or the more numerous Pacific species of *Montastrea*? The characteristically low levels of recruitment by *M. annularis* have been widely documented over the past 30 years (Bak and Engle, Rylaarsdam, Hughes and Jackson, Szmant, etc). Edmunds apparently confirms this well-known pattern: "*These data provide valuable information on rates of sexual reproduction* …" I haven't seen the Edmunds et al paper yet, but I don't see how recruitment data tells you anything about rates of sexual reproduction.	We added the term "candidate" *Montastraea* spp. and the term "successful" sexual reproduction for clarification. Additional discussion of *Montastraea* recruitment (with additional citations) is provided in the ISA.	ch4	75	
197	Page 75, fifth paragraph, line 3. So what does the Richards (2009) PhD thesis have to say about effective population size?	We added a pointer to Section 4.2 where this is discussed.	ch4	75	
198	Page 76, Section 4.5. The report would benefit here from a summary of the extinction debt concept.	We added a paragraph on extinction debt to Section 4.5.	ch4	73	
199	Page 77, third paragraph, line 4. "*eggs must be released within a short distance (2-5 m) of a spawning male for successful fertilization to occur (Lacks 2000)".* It is not justifiable to extend this study of a fungid to all other scleractinians.	We added the sentence "It is not clear the extent to which these experimental studies can be extened to all corals, " and a pers. comm.	ch4	77	
200	Page 77, third paragraph, second last sentence. "*Hermaphroditic brooding corals may be at greater risk of spatial isolation than are spawning corals because of reduced dispersal distances".* Dispersal distances of what? There seems to be some confusion here about dispersal of gametes versus larvae. David Ayre and colleagues compared levels of gene flow in nine species of brooding and spawning corals, showing that the former tended to have more local dispersal (of larvae). But this paragraph seems to be focused on dispersal of gametes.	We rephrased this sentence for clarification.	ch4	77	
201	Page 77, fourth paragraph, line 3. "*However, anthropogenic physical disturbances and chemical pollution decrease the fecundity of corals by decreasing the size distribution of corals and by reducing the energy available for producing gametes".* This is an incomplete list of mechanisms. For example, overfishing and nutrients promote algal blooms that can reduce coral fecundity and growth.	We added, "and other factors" to indicate that anthropogenic physical disturbances and chemical pollution are only examples.	ch4	77	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
202	Page 78, first paragraph, line 5. …The top figure comes from Hughes et al (TREE, 2005), which was reproduced in Steneck's commentary. "*As fecundity decreases, the distance at which population replenishment converts to biogeographic range extension decreases*". The figure caption is garbled "*As habitats are disturbed and become unavailable for coral recruits, habitat availability becomes synergistic with fecundity, fertilization, and connectivity".* See the original caption in Hughes (2005).	We changed figure capation and replaced the citation with Hughes 2005.	ch4	78	
203	Page 79, first paragraph. This paragraph is not well written. Line 2. "….*over-predation (a second predation event before the first has healed or lost individuals are replaced) decreases exponentially with increased coral abundance and increases linearly with increased healing time (Fig. 4.6.3)*". "Over-predation" is a flawed concept, since most adult corals receive chronic, low levels of grazing on reefs that have normal populations of corallivores. Overpredation is normal predation, and the term as defined is unwarranted. The figure is very poor. Most of the 3-dimensional surface (especially the curved part to the top left) is extrapolated. For example, the healing time axis stretches from 20-80 days, but the observed durations span only 30-50. The y-axis should probably be "rate" rather than "probability", assuming the data are empirical and not from a model.	An operational definition of "over-predation" is given in the 3rd sentence of 3.3.3.1 as "Over-predation becomes effective when the rate of predation relative to the rate of healing or population replenishment of prey crosses a threshold in which the process of predation becomes depensatory, i.e., could produce positive feedback, preventing the recovery of coral populations." When the reviewer stated that "Overpredation is normal predation" we realized that the concept was not clearly expressed. Overpredation does not exist as a rate of predation by itself, but is meaningful only in the context of rates of prey healing and successful recruitment. It is indeed a flawed concept when taken as a process by itself. The processes of recovery in the prey population must be included. Although the data are empirical, and although each of the biological and ecological processes are in terms of rates, the change from "normal predation" to "overpredation" involves relative rates in terms of probabilities. The rates of predation might stay the same, but the probabilities of starting a downward trend in prey populations could still increase because of changes in prey growth or	ch4	79	
204	Page 79. Line 5. ~~—~~*…the probability of escaping over-predation increases with … individual size (Jayewardene et al. 2009)*". Every study to date has shown that the probability of escaping partial mortality from predation and other processes DECREASES with colony size (e.g. Bak, Hall and many others).	We think we are actually in agreement with the reviewer. The probability of escaping partial mortality does indeed decrease with colony size (or, the probabililty of encountering partial mortality increases with colony size). Over-predation is unsustainable and leads to compete mortality. As Jayewardene et al. 2009 showed with field experiments, as the colony size increased, the probability of total mortality decreases and the probability of partial mortality increases. We believe we are in total agreement. We clarified the wording.	ch4	79	
205	Page 79, Figure 4.6.3. The caption seems to confuse coral cover and colony size.	We changed the caption to say "coral cover."	ch4	79	
206	Page 80, first paragraph, line 3. "*…corallivorous chaetodontids, became more abundant and transmitted more coral disease as they fed"*. This is speculation. Raymundo et al may have said this, but they certainly didn't show it.	We changed the text to "They hypothesized that…"	ch4	80	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
207	Page 80, second paragraph, line 3. "*…once algae cover more space than even non-depleted herbivore populations can graze, the process becomes depensatory because the algae occupy more space than the herbivores can control (Williams et al. 2001)…".* This is wrong, but unfortunately the notion proposed by Williams has been widely repeated. If "even non-depleted herbivore populations" can't control macroalgae, then all reefs would undergo a phase shift whenever a hurricane occurs. Yet, healthy reefs bounce back.	We edited this paragraph to "Just as predators can outpace the recovery abilities of prey at low population levels, once algae cover more space than the herbivore populations can effectively graze, the process becomes depensatory. This is because the algal population can expand making it even less likely that the algae can be controlled by herbivores {Williams, 2001 #604}, yielding reduced recruitment habitat for coral larvae."	ch4	80	
208	Page 81, the second paragraph on colony size and its importance is somewhat superficial and underreferenced. For example, there is no mention of size-based population models or size-based fecundity and survival schedules in the coral demography literature. "*The eighth and final process, colony size,...*". Colony size is not a process.	We added more references on size based fecundity and a reference to size-structured coral population models. Now we refer to the process of "decreasing colony size" rather than just "colony size".	ch4	81	
209	Page 81, Final sentence "*However, there are some circumstances in which small colony sizes are advantageous (Shenkar et al. 2005)".* What are they?	We added the sentence, "For example, smaller colonies of *Oculina patagonica* appear less vulnerable to bleaching than larger colonies (Shenkar et al. 2005)."	ch4	81	
210	Page 81, third paragraph, line 1. "*Several of the depensatory processes described above could result in the loss of sexual reproduction within the species*". A whole species loses its capacity to reproduce? This is overstated. Maybe you mean "curtailed"?	We edited the text to clarify that we are refering to "successful" sexual recruitment.	ch4	81	
211	Page 82, first paragraph. "*The BRT would consider a species that lost the ability for successful recruitment…This issue is of some concern in species such as those of the Montastraea annularis complex that show very low levels of successful sexual reproduction (Edmunds et al. 2010 in press)".* This text seems to confuse sex with recruitment. Hughes and Tanner (2000 Ecology) document recruitment failure as a critical issue for Caribbean corals.	We are refering to a loss of successful sexual recruitment (as opposed to asexual "recruitment"). For clarification we added the text, "The BRT does not expect that species will lose the ability to produce gametes, but rather that through a depensatory process (or processes), sexual reproduction results in no new recruits that enter the population. A species in this situation would likely be far along an extinction trajectory. " We also added Hughes and Tanner (2000) reference on recruitment failure.	ch4	82	
212	Page 83, second paragraph. "*The Critical Risk Threshold describes a condition where the species is of such low abundance, or so spatially disrupted, or at such reduced diversity, that extinction is extremely likely*". Unfortunately, the lack of species-specific data is a major impediment to assessing extinction risk in corals, as outlined elsewhere by the BRT.	We agree with the reviewer's comment and, as is stated in the comment, the issue of poor data is addressed thoughout the BRT report.	ch5	83	
213	Bibliography: There are some formatting errors such as missing italics for species names.	We formatted all references.	Bibliography1-5		
214	Page 129, second paragraph depth range. The depth ranges here for *Agaricia lamarcki* (3-76m) might be technically correct, but they convey a false impression of this species' normal depth range. Adult colonies of *Lamarcki* are rarely found in any abundance shallower than 20m, except on vertical walls or steep slopes. On most reefs, this species' cover peaks at 30-45m.	We amended the text to indicate the species rarely occurs in shallow waters and is basically found at depth.	ch6	129	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
215	*Page 129, third paragraph. "*A. lamarcki has increased (Bak and Nieuwland 1995) or shown no decline in abundance in the Netherlands Antilles over the last 30 years (Bak et al. 2005), even though other non-agariciid corals in the same area have decreased".* I fail to see how a paper published in 1995 can show tends in the past 30 years. The species comparison referred to here is confounded with depth – Bak's work shows greater changes at shallower sites. If *A. lamarcki* really has stayed stable or increased (it hasn't), then why is it included in this report?	A. lamarcki's inclusion is a result of the petitioner, not the BRT. The text has been clarified as to the time period addressed by Bak's work. The species comparison in the text is not confounded by depth (see Fig. 4 in Bak et al 2005), but the text has nevertheless been clarified that the comparision is for deep reefs only.	ch6	129	
216	*Page 129, fourth paragraph. "*The specific life histories of this species is unknown".* Wrong. Its life history and demography is better known than the vast majority of corals. For example, read Hughes and Jackson 1985 (Ecological Monograph). It provides information on size-specific growth, mortality, and recruitment. The life history of this species is among the best known in the Caribbean.	The sentence is a mis-statement and refers to reproductive strategy rather than life history per se. We changed the text accordingly.	ch6	129	
217	Page 129, fifth paragraph. "*...its average growth rate of ~ 5 mm/yr (range: 0–1.4 cm/yr) is low relative to its congeners*". This statement isn't robust unless it is clear about species and depth. Ironically, the citation used here is Hughes and Jackson 1985 (see previous comment about appropriate references for specific life histories of this species). "Congener" in Hughes and Jackson refers to *Agaricia agaricites* , mostly in much shallower water. In deep water, growth of *A. lamarcki* is faster than other *Agaricia* species that are at the lower edge of their depth range.	We agree with the reviewer's comment about confounding species and depth, and that *A. agaricites* occurs in shallower water. The species comparision is probably not necessary for the purpose of this review; it is rather the growth rate of *A. lamarcki* that is most important so the sentence has been amended to reflect that.	ch6	129	
218	*Page 129, last paragraph. "*The overall life history characteristics of A. lamarcki are roughly parallel to those of Montastrea annularis, that is, based on low overall recruitment rates, high survival, and high partial mortality (Rogers et al. 1984)".* This isn't really true. I can't find any support for this statement in Caroline Roger's 1984 paper. Hughes and Jackson's 1985 Ecological Monograph documents faster growth, higher recruitments, and lower size-specific survival in *Agaricia lamarcki* compared to *M. annularis* at the same site and depth.	The statement in the status review is based on the 4th full paragraph on page 74 of the Rogers reference. The sentence has been amended to include the Bak and Luckhurst reference, and the locations for the results presented. Those results have been contrasted with the Jamaica information from Hughes and Jackson.	ch6	130	
219	Page 130, first paragraph, last sentence. *~~The~~ The congener Agaricia tenuifolia replaced Acropora cervicornis…".* Why is this relevant?	We have deleted the sentence.	ch6	130	
220	Page 130, fifth paragraph. Typo? *~~Although~~ Although its platy morphology could make it sediment-susceptible, A. lamarcki is inefficient at actively rejecting sediment (Bak and Elgershuizen 1976)".*	This was indeed erroneous information from a previous edit. We amended the text.	ch6	130	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
221	Page 133, last paragraph, line 1. "*Published and unpublished records indicates Mycetophyllia ferox is rare (< 0.1% species contribution and <0.8 colonies/10 m2) in Florida (2010) and rare (0.8 colonies/100 m) in Puerto Rico (AGGRA database online at http://www.agrra.org)*". This is isn't very convincing. Sure, *M. ferox* is less abundant than some Caribbean species, but it cannot be described as rare. What does "species contribution" mean? Almost one colony per 10m2 converts to a very substantial population size across the Caribbean. The Puerto Rican data seem to be number of colonies along linear transects? Were the Florida and Puerto Rican data collected at the appropriate depth and habit for this species?	We removed the qualitative term "rare" and left the quantitative density and cover estimates. The survey used by Wagner et al 2010 sampled across multiple habitat types/dpeths and AGRRA targets two specific depth strata; both concentrate within comfortable SCUBA depths (<15 m).	ch6	133	
222	Page 134, second paragraph, last line. "*Recruitment of this species appears to be very low, even in studies from the 1970's (e.g., (Good et al. 2005) reported zero settlement)*". What? Were Good et al slow to publish their 1970's data? Perhaps this should be a citation to Bak and Engel, Rylaarsdam, etc.	We corrected the reference to Dustan 1977.	ch6	134	
223	*Page 135, first paragraph, line 2. *—.with a mean of 70% probability and a wide range of votes (10%–99%).* Isn't this disparity of considerable concern?	As described elsewhere in the document, the range/variance of the risk assessment votes provides an important piece of information, namely a measure of uncertainty relative to the BRT's assessment of its extinction risk. In this case, the 10%-99% reported range was incorrect and has been corrected to 33%-99%.	ch6	135	
224	Page 137, in Habitat section. "*Most reef environments (Veron 2000)*". This statement by Veron is illinformed.	In the absence of corrective info provided by the reviewer, no change has been made.	ch6	137	
225	Page 138, fourth paragraph, line 4. Reference format. "*In contrast, (Oxenford et al. year report that 100% of the 15 colonies they observed in Barbados …*". Not an impressive sample size.	We fixed the citation.	ch6	138	
226	Page 139, second paragraph, line 2. *—.anomalous report of 6000 pieces imported by Portugal from Mozambique in 1996 — probably in error)*". Of course it is.	We deleted "probably in error."	ch6	139	
227	Page 141, last paragraph. Depth range: 2–72 meters (Carpenter et al. 2008b). According to Goreau and Wells (1967) this is the combined depth range of both conspecifics.	We replaced the Carpenter depth range with Humann's range of 3-50 m).	ch6	141	
228	Page 142, Second paragraph. "*D. stokesii is described as a gonochoric spawner*". Reference?	We added a Hoke 2007 reference and other information from this thesis as well.	ch6	142	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
229	Page 144, Second paragraph, line 1. "*While there now is general acceptance that these represent three valid species, long-term monitoring data sets and earlier ecological studies did not distinguish among them".* I'm not sure if this general acceptance is true. Veron (2000) considers them to be a single species. Certainly the standard spelling for the genus in the Indo- Pacific is unchanged.	While Veron (2000) does list them as combined, this work was published prior to several of the more definitive papers describing the complexities of hybridizaiton and genetic relatedness such as Levitan et al 2004, Fukami et al 2004. We changed "general" to "reasonable". This acceptance is further illustrated by most authors referring to "*Montastraea annulari*s complex" or "*Montastraea annularis* sensu lato" or "sensu stricto" in much of the current published literature. The validity of scientific spellings are not determined by frequency of use or consensus. In 1895, the ICZN (International Commission on Zoological Nomenclature) established the International Code of Zoological Nomenclature and the ICZN decides all questions about interpretation of the code and publishes decisions in the Bulletin of ICZN. Dr. Stephen D. Cairns of the Smithsonian Museum of Natural History is an expert coral taxonomist, and so the BRT uses as the standard reference for name spellings: "Cairns, S.D., B.W. Hoeksema, and J. van der Land. 1999. Appendix: List of extant stony corals. Atoll Research Bulletin 459."	ch6	144	
230	*Page 144, third paragraph, line 5. "*There is ample evidence that it has declined dramatically throughout its range, but perhaps at a slower pace than its fast-paced Caribbean colleagues, Acropora palmata and Acropora cervicornis, and most other Caribbean species. While the latter began their rapid declines in the early to mid 1980's, declines in M. annularis complex (where?) have been much more obvious in the 90s and 2000s, most often associated with combined disease and bleaching events".* The best data on relative declines of these and other species comes from Jamaica. The 1990s-2000 date is incorrect for the "beginning" of the decline in most places that have data – note the contraction in following sentences about substantial losses in Florida in 1975-1982. The decline from 10% in 2003 to 3% cover by 2009 reported from the US Virgin Islands example (on page 145) is a trivial loss compared to much earlier declines that are well documented at this location.	The BRT found this comment confusing and were not clear why the reviewer inserts " and most other Caribbean species." This sentence is to summarize trajectories range-wide (individual locations are described in the following paragraphs). The sentence states that the declines in MAC have been more obvious in the 1990s and 2000s, not that they began then. Hughes 1989 reports a dramatic increase in *Montastraea annularis* cover at one site in Jamaica from 1983-1987.	ch6	144	
231	Page 147, first paragraph, line 1. Typo. "*All three of the Montas t raea* …" Throughout pages 147 and 148. Typo. "*Montas t raea annularis"*.	We corrected the typos.	ch6	147	
232	Page 147, second paragraph, line 4. ~~…~~*.the Caribbean also report them to…".*	We edited the text as suggested.	ch6	147	
233	Page 147, second paragraph, line 4. "*Edmunds (Edmunds et al. 2010 in press) states that the ~~s~~torage effect" (large, replenishing recruitment events that happen rarely) hypothesized to operate in these species, was never actually documented on any Caribbean reef since the initiation of quantitative ecological study in the 1960's".* Edmunds is confused. The storage effect creates a mixed age population that builds up over time. The longevity of *Montastrea* has allowed it to persist in the virtual absence of recruitment, while species such as *Agaraicia agaricites* that have week storage are much more vulnerable to recruitment failure. See Hughes and Tanner (2000).	We changed the wording to clarify Edmunds point (replenishing recruitment events for Caribbean MAC are PRESUMED to occur but have NEVER been observed). It is not clear to us if the reviewer is disputing wether MAC display a "storage effect" or simply if it was poorly described in the text.	ch6	147	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
234	Page 147, second paragraph, line 4. "*Mortality in Montastrea in the Florida Keys is size-specific, with small juveniles suffering higher mortality (Smith et al. 2006) — so even if a pulse event occurs, not all settlers will become reproductive adults*". This is an incredibly naïve statement. All corals have type 3 survivorship.	We deleted this sentence.	ch6	147	
235	Page 147, second paragraph, line 4. ~~—.~~*degree of fragmentation fission and clonal reproduction".* Fragmentation is usually only used to describe breakage of the skeleton.	We edited the text as suggested.	ch6	147	
236	Page 148, first paragraph, first sentence. "*Given the rapidly developing genomic tools for this species, cellular and transcriptomic mechanisms for bleaching and thermal stress are being elucidated for this species (Desalvo et al. 2008), as well as certain aspects of geographic and genetic variability in the molecular responses to thermal stress (Polato et al. 2010), which may enable more accurate predictions of potential evolutionary adaptation to warming*". Sentence is too long.	We revised the text.	ch6	148	
237	Page 148, fourth paragraph, Predation. The paragraph lacks focus, including issues such as bioerosion and colonization by damselfish. The first sentence doesn't make sense since the biogeographic range of *Acanthaster* doesn't overlap with this species of coral. Line 8-9. ~~—.~~*parrotfish biting can impede colony resilience to bleaching (Rotjan and Lewis 2006)".* The term "resilience" is used inappropriately here. I think you mean the capacity of a colony to recover from bleaching, and not the capacity of an ecosystem to avoid shifting to an alternate stable state. More generally than the Rotjan and Lewis study, grazing by parrotfish on macroalgae is critical for promoting ecosystem resilience to bleaching. *Though it is not predation per se, bioerosion…".* Of course it isn't predation. Delete this phrase. Maybe you should broaden the heading.	We replaced "resilience" with "recovery". We agree that *Acanthaster* does not have an overlapping range with MAC. The sentence uses them as a CONTRASTING example of an outbreaking corallivorous predator. Strictly non-predatory interactions put in separate paragraph referred to as 'pests'.	ch6	148	
238	Page 151, Depth range. "*0.5–40 m (Weil and Knowton 1994, Carpenter et al. 2008b)*". 40m is far too shallow. *Montastraea* is still abundant at 60m.	The abundant deep populations are generally understood to be *M. franksi* with *M. faveolata* having a somewhat less deep distribution. The reviewer provides no reference to replace those supporting the current statements so no changes were made.	ch6	151	
239	Page 153, Characteristics, line 1. Typo. "*Montastraea franksi is distinguished by larg large, unevenly arrayed polyps…".*	We fixed the typo.	ch6	153	
240	Page 156, Global Distribution, line 2. ~~—.~~*but may be absent from Bermuda…".* We don't need two distribution maps.	For consistency, the BRT has decided to present the two maps for all species as in some cases there are discrepancies.	ch6	156	
241	Page 160, Bibliography. The Carpenter reference is included twice.	We deleted one of the Carpenter references.	ch6	160	
242	*Page 202. As noted, the references need to be tidied up.	We fixed all the references.	ch6	202	
243	*Page 230. Global distribution.* I think "medium" as a descriptor of the geographic range of *Acropora globiceps* is misleading. Most corals have enormous ranges. This abundant species stretches from the Andaman Islands in the Indian Ocean to the easternmost parts of French Polynesia.	We replaced "medium" for "88th place out of 114"	ch6	230	

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1	**CIE Reviewer #1 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
244	Page 238. I have seen *Acropora jacquelineae* in American Samoa. Veron's map is wrong – this species is also common in Papua New Guinea and the Solomon Islands.	We do state that although the IUCN does not list this species in American Samoa and PNG we do report it with references to support these observations.	ch6	238	
245	Page 320. Bibliography. The reference for Carpenter et al. is duplicated again. The American Samoa records are frequently based on Mundy's report. What is the full reference?	We fixed all the references.	ch6	320	
246	*Page 509. Table. 7.1. My assessment would place *Agaricia lamarcki* before *Montastrea annularis* . Many of the Pacific species are abundant and have very large geographic ranges. If species like *Acropora aspera, Pavona cactus, Porites nigrescens,* the Isoporans or *Turbinaria peltata* go extinct, then all corals will. Listing all of these species as vulnerable isn't credible. *Acropora palmerae* appears twice. It is highly resistant to bleaching and remains abundant across French Polynesia.	The BRT ranked *Montastraea annularis* higher than *Agaricia lamarki* because of the extensive data showing high levels of population decline throughout its range for *M. annularis* , combined with very low recruitment/recovery potential. While *A. lamarki* may have undergone (or be undergoing) population declines as well, data was not available to document this, it has always been rarer than *M. annularis* , its recruitment potential, though apparently low, seems higher than *M. annularis* , and its primary distribution in deep/mesophotic habitats suggests potential buffering from surface-based threats. We deleted one of the erroneous *Acropora palmerae.*	ch7	509	
247	Page 511. The Indonesian attribution is very unlikely to be accurate given the absence of this species across the western and central Pacific.	This is indeed the message we hoped to convey, although we could not state it as a proven fact.	appendix	511	
248	Page 512. Acidification and LBSP. But remember that *Millepora* is not a scleractinian, so these comparisons to "corals" are less relevant.	The BRT feels that as both hydrocorals and scleractinians are aragonitic, they may have similarities in their processes of calcifications.	appendix	512	
249	Page 513. Last paragraph. Typos. "*geo-graphically*" and reference formatting.	We fixed the typos and references.	appendix	513	

Section 8.

BRT responses to:
Review comments Dr. John McManus
(CIE Reviewer 2)

	A	B	C	D	E
1	**CIE Reviewer #2 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
2	In general, does the Status Review include and cite the best scientific and commercial information available on the species, its biology, stock structure, habitats, threats, and risks of extinction? "The habitat information is nearly adequate, but additional emphasis should be placed on the roles of non-structural coral communities. The report clearly defines structural coral reefs. However, this report is focused not on the ecology of structural reefs, but rather on the risk of extinction of certain coral species."…"It is noted, however, that the descriptions of the habitats on a per-species basis within the report are not at all dependent on the limited definition of habitat in the early portion of the report. Thus, a simple clarification in these early pages will suffice to convey the appropriate concepts concerning extinction risk. "	We added text and references in section 2.2.3 to emphasize coral populations may be extensive in 'non-reef communities.'	General comment		
3	CO2 -> CO^2	We corrected the typo.	Table of contents	iv	
4	3.3.4 does the fishery section include increased fishing pressure due to losses and changes on land?	Very good point. We added two paragraphs at end of 3.3.4	Table of contents	3.3.4	
5	6.14.x All *Monitpora* should be *Montipora*	We corrected the typo.	Table of contents	6.14	
6	6.19 *Pacheseris should be Pachyseris.*	We corrected the typo.	Table of contents	6.19	
7	Page ix – The process is like a highly informal Delphi approach, but non-anonymous. Each member makes a judgment call based on much the same gathered information but differing field and other experiences. Worrisome, but perhaps the best that could be done?	Given the statuary time limitations required, the BRT could not identify realistic alternative approaches. The BRT recognizes the limitations of the approach, which focused on extension data and information gathering, frank, impartial discussion, anonymous voting, and a process designed to clearly demonstrate uncertainty.	Exec Summary	ix	
8	Page xii – Has multiples of the following entries:	We corrected the typo.	Exec Summary	xii	
9	Acanthastrea brevis 2	We corrected the typo.	Exec Summary	xii	
10	Acanthastrea hemprichii 2	We corrected the typo.	Exec Summary	xii	
11	Acanthastrea ishigakiensis 2	We corrected the typo.	Exec Summary	xii	
12	Acropora acuminata 2	We corrected the typo.	Exec Summary	xii	
13	Acropora horrida 2	We corrected the typo.	Exec Summary	xii	
14	Acropora palmerae	We corrected the typo.	Exec Summary	xii	
15	2 Acropora paniculata 2	We corrected the typo.	Exec Summary	xii	
16	Acropora polystoma 2	We corrected the typo.	Exec Summary	xii	
17	Acropora vaughani 2	We corrected the typo.	Exec Summary	xii	
18	Astreopora cucullata 2	We corrected the typo.	Exec Summary	xii	
19	Barabattoia laddi 2	We corrected the typo.	Exec Summary	xii	
20	Pavona diffluens 2	We corrected the typo.	Exec Summary	xii	
21	Physogyra lichtensteini 2	We corrected the typo.	Exec Summary	xii	
22	Pocillopora danae 2	We corrected the typo.	Exec Summary	xii	
23	Pocillopora elegans (W. Pac) 3	We corrected the typo.	Exec Summary	xii	
24	Porites horizontalata 2	We corrected the typo.	Exec Summary	xii	
25	Porites napopora 2	We corrected the typo.	Exec Summary	xii	
26	Porites nigrescens 2	We corrected the typo.	Exec Summary	xii	
27	Also *Porites pukoensis* is treated both as a clade and as a species in the same table.	We corrected the typo.	Exec Summary	xii	
28	It includes the following misspellings (correct spelling in last column), also found elsewhere in the document:	We corrected the typo.	Exec Summary	xii	
29	Dichocoenia	We corrected the typo.	Exec Summary	xii	
30	Pavona	We corrected the typo.	Exec Summary	xii	
31	Turbinaria	We corrected the typo.	Exec Summary	xii	
32	Parentheses should be corrected for: dilatata/flabellata(/turgescens) and patula(/verrilli)	We added parentheses because those spp were not petitioned but were included in the CRT based on best available science. We added an explanation in the caption.	Exec Summary	xii	
33	*Dichocoenia stokesii* should be Dichocoenia stokesi according to usage in works by Veron, Romano and Cairns.	We changed it to *stokesi.*		1	
34	"A coral is a marine invertebrate, not a vertebrate species; therefore, it may not be subdivided into distinct population segments." This is a meaningless sentence. Whether or not one can divide a species' range into distinct population segments has nothing to do with its being vertebrate or invertebrate. Note that the average fish stays longer in the open water as a larva than the average broadcast coral planulae (despite some individual species exceptions), and brooded coral planulae stay much closer to home (see McManus and Meñez 1997). I suggest simply stating that "Most corals have planulae that drift widely with ocean currents, and so subdivision of the species ranges into distinct segments is not practical in terms of the assessment of vulnerability." McManus, J.W. and Meñez, L.A.B.. 1997. The proposed Spratly Island international marine park: biological considerations. Proc. 8th Intl. Coral Reef Symp. 2:1943-1948.	The reviewer is correct on the biology, but the text in the status review derives its meaning from legal definitions rather than biological statements, and therefore is not meaningless. However, a sentence has been added to capture the reviewer's point.		3	

	A	B	C	D	E
1	**CIE Reviewer #2 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
35	If the petitioner mentioned the criteria for selecting these particular US-hosted corals and not others, it would be good to mention this.	The petitioner mentioned multiple criteria, most of which are listed in the BRT status review. Additionally, the petitioner noted that all petitioned species are listed as threatened, endangered, or critically endangered by the IUCN. The BRT chose not to include this information because IUCN listings are not directly equivalent to listings under the Endangered Species Act, and we did not wish to confuse the issue by introducing terminology that conflicts that of section 1.1.1 in this document.		3	
36	"Cnidaria include members of both the class Hydrozoa (fire corals) and true stony corals (class Anthozoa, order Scleractinia)." Suggest change this to "Within the Cnidaria are fire corals (part of the class Hydrozoa) and true stony corals (class Anthozoa, order Scleractinia)."	Changed to 3 orders as suggested.		5	
37	„genenetically' - > genetically	We corrected the typo.		7	
38	"(i.e. cryptic species adding to diversity)." The term „cryptic' usually means hard to see. Suggest deleting this phrase.	We changed the text to: "previously undescribed species."		7	
39	"(*Montipora, Porites* clades)," change to "(some clades within the genera *Montipora* and *Porites*),"	We amended the text according the reviewer's suggestion.		7	
40	„earth' without the definite article should be capitalized.	We amended the text according the reviewer's suggestion.		7	
41	"Today's reefs are less than 10,000 years old as they are found on shallow seafloors that were dry land during the last glacial period (Siddall et al. 2003)." Reefs as geomorphological structures can be much older. Suggest change to "Today's reef ecosystems are less than 10,000 years old as they are found on shallow seafloors that were dry land during the last glacial period (Siddall et al. 2003)."	We amended the text according the reviewer's suggestion.		7	
42	Omitted asexual "bail-out" strategy of *Seriatopora* (at least hystrix, but possibly others). See: Sammarco 1982. Polyp Bail-Out: An Escape Response to Environmental Stress and a New Means of Reproduction in Corals. Mar. Eco. Prog. Ser. 10: 57-65	Given the limited ocurrence of this mechamism in the literature, let alone its documentation in the field, the BRT does not feel that polyp "bail-out" rises to the status of a reproductive mode. This reference will be added to the Seriatopora species description.		10	
43	Need citation for last sentence on genetics. Suggest: Baums, IB; Miller, MW; Hellberg, ME 2006. Geographic variation in clonal structure in a reef-building Caribbean coral, Acropora palmata. Ecological Monographs [Ecol. Monogr.]. Vol. 76, no. 4, pp. 503- 519. Nov 2006. (already in the references?).	We revised the sentence.		10	
44	„maintaine' -> maintained	We corrected the typo.		11	
45	By now, the document is showing signs of uneven level of scientific jargon. Earlier, all terms were well defined. Later, terms such as mixotrophy are used without specific definitions. Perhaps there should be a glossary, or better use of in-line definition.	We defined "mixotrophy" in the text and included definitions of other scientific jargon when deemed needed.		11	
46	It is important to explain about nonstructural coral reefs, given that the report is about coral survival and not reef ecology per se. Corals grow fine on hard substrates other than high-relief limestone reefs, and these communities may well support large proportions of coral populations. The distinction between structural reefs and „coral communities' was in early papers by Tom Goreau Senior on Jamaica and Saipan. However, the term „coral community' was later used by Done to describe communities of coral on structural reefs. Thus, the term nonstructural coral community is recommended. The term „coral reef' is often applied to these nonstructural coral communities, even when the corals are growing on sandstone on volcanic rocks. See: McManus, John. 2001. Coral Reefs. p. 524-534 In: J.H. Steele, S.A. Thorpe and K.K. Turekian (eds.) Encyclopedia of Ocean Sciences. Academic Press, London. 2000 pp. Or (using the term „nonreef coral communities') McManus, J.W. 1988. Coral reefs of the ASEAN Region: status and management. Ambio 17(3):189-193	We added text to emphasize prevalence of corals in "nonstructural coral communities."		11	
47	The "Loss of resilience" sentence is missing one "and". Otherwise this paragraph is very well done.	We corrected the typo.		15	
48	*Acropora palamata -> Acropora palmata*	We corrected the typo.		15	
49	Should add „approximately' to "(700 species…."	We amended the text according the reviewer's suggestion.		16	
50	Should add 'may' to "have provided substantial buffering…"	We amended the text according the reviewer's suggestion.		16	
51	Biogeography handled very well.	Thanks!		16	
52	"This has come about through.." sentence missing „and'.	We corrected the typo.		17	
53	Should list the corals restricted to the Eastern Pacific, or modify the sentence to read "local extinction" and otherwise adjust it.	Added specific reference to *Pocillopora elegans* and *Psammocora stellata*. While *Porites lobata* is also found in the eastern Pacific, this is a more complex issue that surrounds *Porites pukoensis* that we do not feel is appropriate here.		17	
54	"The BRT was not tasked with considering Section 4(a)(1) Factor 4 (Inadequacy of Existing Regulatory Mechanisms)." So, another report should be prepared to cover this prior to threat status determination.	PIRO and SERO are jointly preparing a report on regulatory mechanisms.	ch3	18	table 3.1

	A	B	C	D	E
1	**CIE Reviewer #2 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
55	The sentence following '*' this is very unclear, has inappropriate capitalizations, and perhaps should be broken down into multiple sentences with more clarifications, especially concerning insolation.	We revised the Table and caption.	ch3	18	table 3.1
56	I would have made nutrients and sedimentation „medium', given the rise of coastal construction and deforestation.	The BRT voted low-medium for these primarily because there remain many coral reef areas these are not a significant threats that would lead to potential extinction. The BRT does recognize and Chapter 3 discusses the fact that in many local areas, these land-based threats are the most significant threat and that they result in significant local degradation of reefs and could cause local extirpation.	ch3	18	table 3.1
57	Table 3.1.1 caption needs clarification regarding offset of columns in all but last column.	We clarified the caption.		19	table 3.1.1
58	The population impacts should include a paragraph relating them to the US coral ranges – such as the problems of population rise on small islands (e.g. limits to farm size leading to increased fishing pressure) and in South Florida (e.g. increased tourist development).	Reviewer's suggestion declined. The requirement of the status review is to address the potential extinction risk across the full range of the species in question.	3.1	20-21	
59	Table 3.2.1 needs much more extensive explanation (symbol definitions) and a year for Donner. What is "Commit"? What does all that mean?	We added text to the caption for clarification and corrected the citation.	3.2	26	table 3.2.1
60	It matters how long the warming occurs (multiple day has worse effects than one day). Maybe Al Strong has published this somewhere?	We amended the text for clarification.	3.2	26	
61	'Baker' should be „Baker et al.' or something like that.	We fixed the citation.	3.2	27	
62	„recognizes' -> „recognized'	We corrected the typo.	3.2	27	para 4, ln2
63	"(Polovina et al.)" needs year	We fixed the citation.		28	
64	(Albright et al.) needs year.	We fixed the citation.		34	
65	Also repeated table header.	We corrected the table		34	
66	Table 3.2.1 references appear to have been cut off. They should be there, perhaps in numbered form with list relating numbers to authors as footnote.	We fixed the table.		35	3.2.2
67	"Buddemeier et al." needs year	We fixed the citation.		43	
68	Sediment can come directly from land during storms without only going through rivers.	We assumed the comment refers to the first sentence of the last paragraph in this section and deleted "riverine.		47	
69	"community structure. Elevated nutrients" -> "community structure. This may be particularly the case when herbivory has been reduced, as by overfishing or disease. Elevated nutrients"	We amended the text according the reviewer's suggestion.	3.1.1.2	47	last para
70	Table 3.3.1. Delete the extra „int's.	We corrected the typo.		54	
71	"history (Sammarco and Andrews 1989, Cowen et al. 2006). Coral colonies" -> "history (Sammarco and Andrews 1989, Cowen et al. 2006). However, even the arrival of a few larvae over a great distance may be important in cases of re-establishment following local extinction on a reef. Coral colonies"	Inserted " That said, it is also recognized that even the arrival of a few larvae over great distance may be important in cases of re-establishment following local extinction on a reef."	3.3.3.1	58	last para
72	Need introductory mention of corallivorous snails, sea-stars, etc. even though discussed later.	Modified introductory sentence to "Predation on some genera of corals (especially *Acropora, Montipora, Pocillopora,* and *Porites* in the Pacific and *Montastraea, Acropora,* and some species of *Porites* in the Atlantic) by many species of fish and invertebrates (e.g. corallivorous snails and seastars)"...	3.3.3	57	para 1
73	"depensatory Allee effects will have commenced" -> "depensatory Allee effects may have commenced" Note that this depends on the species and its reproductive needs, as well as local currents and other factors.	We revised that paragraph and deleted that part of the sentence.	3.3.5	63	below fig 3.3.8
74	"practices (Green and Shirley 1999). An additional" -> "practices (Green and Shirley 1999). There are often concerns raised that permitting the export of cultured dead or live corals may open up the trade in non-cultured corals, because of difficulties in tracking and enforcement.	Added "Due to difficulties in tracking and enforcement, concerns are often raised that permitting the export of cultured dead or live corals may increase pressure on the trade in non-cultured corals."	3.3.5	64	para2
75	Is it Carajoa or Carijoa? You used both.	We changed it to the correct spelling *"Carijoa"* thoughout the document.		69	
76	"invasion is potentially" -> "invasion has potentially"	We amended the sentence according to the reviewer's suggestion.		70	
77	Mention that the congener hystrix also exhibits „bail-out' phenomenon, not yet established for this species.	We added a reference as requested.		200	
78	*Anacropora puertogalerae* and A spinosa are yet more species not found in US waters. Does this make it less relevant, or will the threat category ultimately affect trade in these species?	How potential threats might affect trade is a management/policy consideration, and the BRT cannot address this in the status review document.		299	
79	Large patches of an unknown Euphyllia form large patches in silty coral communities of Outer Ambon Bay Indonesia, indicating a tolerance for sedimentation. McManus, J.W. and Wenno, J.J. 1981. Coral Communities in Outer Ambon Bay, Indonesia: a general assessment survey. Bulletin of Marine Science 31(3):574-580. That reference has the following useful quote: "Corals with large polyps may be able to withstand siltation better than some with smaller polyps (Roy and Smith, 1971)" Roy, K. J., and S. V. Smith. 1971. Sedimentation and coral reef development in turbid water: Fanning Lagoon, Pacific Science 25: 234-248, Good work on species info. Frustratingly, lots of unpublished observations abound, but they cannot be used for this (predation on Isopora, etc.)	We appreciate the useful Ambon Bay reference and have incorporated it. The Roy and Smith paper discusses growth forms more so than polyp size. The polyp size issue has been addressed by Stafford-Smith and others and has been addressed elsewhere in the status review.		473	

Section 9.

BRT responses to:
Review comments Dr. Bernhard Riegl
(CIE Reviewer 3)

	A	B	C	D	E
1	**CIE Reviewer #3 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
2	"Photographs…by Charles Veron". His name is actually John Edward Norwood, Charlie is the nickname (supposedly dating back to his very early childhood days).	The BRT corresponded with Dr. Veron on his preference of being cited/credited.		ii	
3	provides a succinct overview of rationale and process	Thanks	Exe Summ		
4	"The Scleractinia have diversified into multiple families, all of which exploit the ability to form complex colonies." The Fungiidae are actually mostly solitary.	We amended the text according to the reviewer's comment.	ch2	2.1.1	5, para 2
5	Evolutionary history: The reef-less interval at the beginning of the Cenozoic is exaggerated by Veron (2008) and this is echoed here. Reefs were present, albeit not widely distributed, already in the Danian, which immediately follows the K/T event. It is, in fact, possible that in some areas (Paris basin) some Cretaceous taxa may have actually survived into the Dania. Be it as it may, coral reefs were alive and kicking already in the Paleocene (many well-developed reefs particularly in North Africa and the Middle East) and not only in the Eocene. This should be corrected, since p.202 of the report correctly states that Acropora arose in the Paleocene. So, here's a contradiction: if there were no reefs in the Paleocene, where would this genus have arisen (it could have arisen in non-reef habitats, but that's not the fact)? What we do have, however, is an approximately 10 million year reefless period in the lower Triassic, following the Permo-Triassic extinction of the Paleozoic corals.	The BRT agrees with the reviewer's comments and made the appropriate changes. We added a sentence on the reefs of the Paleocene. However, we did already have the word "relatively" before "reefless", and as the reviewer noted, reefs were always around, "albeit not widely distributed". In the Paleocene, most consisted chiefly of low-diversity (less than 5 coral species, Montaggioni and Braithwaite 2009), and some consisted mainly of coralline algae. As the reviewer stated, a number of Cretaceous coral taxa (genera and families) survived into the Dania, and continued to the present.	ch2	2.1.3	7
6	"Edinger and Risk (1995) speculated that this pattern in the Atlantic was driven by lower rates of extinction of brooders relative to broadcast spawners during the Caribbean Oligocene-Miocene extinction event" True, but Glynn (2009) has taken exception to that (Glynn PW (2009). Survival of brooding and broadcasting reef corals following large scale disturbances: is there any hope for broadcasting species during global warming? Proc 11th Int Coral Reef Sym, 368-37 (see text in last paragraph on p.371)).	We added a contrasting Glynn&Colley 09 discussion of enhance broadcaster fitness/survivorship.	ch2	2.2.1	8, para 2, last sn
7	An explicit mortality schedule for a scleractinian is given by Harriott (Harriott VJ (1985). Mortality rates of scleractinian corals before and during a mass bleaching event. MEPS 21:81-88).	We added the reference to Harriott.	ch2	2.2.1	8, last para
8	Sentence incomplete. "Fragmentation is a common…" insert "process" or "accurrance" or delete "a".	We deleted "a."	ch2	2.2.1	10, last sn
9	Section calcification and reef building. The report might considering mentioning here that coral skeletons are aragonite, and reef cements aragonite or high-Mg calcite, just to lead easily into the later acidification debate (and just in a sentence).	We amended the text according to the reviewer's comment.	ch2	2.2.3	11
10	Clonality and genetics "If there is low genotypic diversity within individual stands and/or across the region, it might suggest that a clonal species' status is under much greater extinction risk than would be judged from its overall abundance.": the term "extinction debt" (Tilman D et al. 1994. Habitat destruction and the extinction debt. Nature 371:65-66) could be introduced here, since this is really what is being referred to.	Our understanding of the concept of extinction debt relates more to habitat loss and connectivity than to genotypic diversity per se. We added this concept in Chapter 4.	ch2	2.2.4	11
11	Last sentence in the section refers to adaptation of Arabian corals to high temperatures, but the sentences before only refer to adaptation to low temperatures. Coral reefs in the Arabian Gulf are not only selected to withstand some of the lowest temperatures (the lowest temperatures are cited by Veron 1995 for Japan, with 4 deg C, if I remember correctly) but regularly the highest. Also bleaching not only occurred in 2010, but in 1996, 1998, 2002 and 2010. The effects of repetitive mass mortality due to increasing heat, and the temperature adaptation, are described in Riegl and Purkis (Riegl and Purkis (2009). Model of coral population response to accelerated bleaching and mass mortality in a changed climate. Ecol Mod 221: 192-208). Bleaching in the Gulf occurs if temperatures are maintained at >35 or 36 deg C for over 3 weeks (Riegl (2002). Effects of the 1996 and 1998 positive sea-surface temperature anomalies on corals, coral diseases and fish in the Arabian Gulf. Mar Biol 140:29-40.) The bleaching information is reviewed in Baker at al (2008) ECSS, cited in the references. Also corals in American Samoa have been shown to survive to 35 deg C (Craig P et al. 2001.	We added Riegl 2002 and additional text on bleaching events in the Arabian Gulf.	ch2	2.3.2	13

	A	B	C	D	E
1	**CIE Reviewer #3 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
12	The Adjani et al (2006) phase-shift reversal has been disputed by Quinn and Kojis (Quinn NJ, and Kojis BL (2008) The recent collapse of a rapid phase-shift reversal on a Jamaican N coast coral reef after the 2005 bleaching event. Revta Biol Trop 56(Suppl 1): 149-159).	We amended the text according to the reviewer's comment.	ch2	2.3.4	14, 10 ln from bottom
13	Contrasts between Caribbean and Indo-Pacific Seas: This section is a bit confused. The unique Caribbean fauna is a result of the closure of the Isthmus of Panama and many of the typical taxa arose after the closure (the *Acroporas* for example). The inheritance from the Tethys is less important, since that ocean includes the Pacific. So any inheritance would be on both sides of the isthmus. The story in the Caribbean is one of a slow step-down of the old Indo-Pacific fauna, and a gradual rise of the new Caribbean fauna. The relevant papers are those (many) by Budd and Johnson (ex., Budd AF, Johnson KG (1999). Origination preceding extinction during late Cenozoic turnover of Caribbean reefs. Paleobiology 25:188-200).	It is true that there has been a gradual loss of cosmopolitan corals and a gradual rise of unique Caribbean corals. As Fukami et al. (2004) point out, the Atlantic faviid and mussid are more closely related to each other than to their congeners in the Pacific. We added reference to Fukami et al.'s findings. These complications of taxonomic diversity are indeed confusing, but this complex issue is not the main point of attention. 80% of the more than 100 Mio-Pliocene reef coral species in the Caribbean became extinct 4 – 1.5 million years ago (Budd and Johnson 1999). We are not aware of any such extinction in the Indo-West Pacific. Of the corals living today, over 20 genera (e.g., Pocillopora, Isopora, Turbinaria, etc) went extinct in the Caribbean but are still abundant and widespread in the Pacific. This is an important note for background for climate change and scleractinians. It is interesting that the Caribbean faviid-mussid taxonomic group might be unique to the Caribbean. This might combine two Caribbean genera into one, further reducing the Caribbean count. But it is the recent (in geological time) extinctions in the Caribbean that are the point of emphasis.	ch2	2.5	16
14	Threats to coral reefs. I note that coastal construction is considered a low threat. In some regions of the world, notably the Arabian, coastal construction is considered the primary threat. And it may well be so in other areas (especially small island states) as well (Sheppard et al (2010). The Persian/Arabian Gulf: A young sea in decline. Mar Pollut Bull 60: 13-38; Sale et al (2010). The growing need for sustainable ecological management of marine communities of the Persian Gulf. Ambio DOI:10.1007/s13280-010-0092-6).	The BRT made no change. We acknowledge that coastal construction likely consitutes an acute or even primary threat in some regions. However, our task is to rank threats on a range-wide (i.e., global) basis.	ch3		18
15	"…across each ocean province from Donner (Donner)" . Is this is supposed to mean Donner et al (2005) or Donner (2009)?	We corrected the citation (Donner 2009).	ch3	table 3.2.1	26,1st ln
16	the bracketing needs to be fixed. Several citations can fit within a single set of brackets.	We corrected the citation.	ch3	3.2.2.1	26, last para
17	the bracketing needs to be fixed. Several citations can fit within a single set of brackets.	We corrected the citation.	ch3	3.2.2.1	27,1st & 2nd para
18	Negri et al. citation needs the year specified.	We corrected the citation.	ch3		28. 2nd para
19	Suggest renaming to "Changes to water column stratification";	We renamed the section heading as suggested to : "Changes to water column stratification (less mixing, less nutrients)."	ch3	3.2.2.3	28
20	Polovina et al: citation needs year specified.	We corrected the citation.	ch3	3.2.2.3	28
21	another useful citation might be: Silverman J, Lazar B, Cao L, Caldeira K, Erez J (2009). Coral reefs may start dissolving when atmospheric CO2 doubles. Geophys Res Lett 36, L05606, doi:10.1029/2008GL036282, 2009.	We added the suggested reference.	ch3	3.2.3	29, 1st para
22	the citations need the year specified	We corrected the citation.	ch3	Fig. 3.2.8	31
23	the citations need the year specified	We corrected the citation.	ch3	Fig. 3.2.9	31
24	the citations need the year specified	We corrected the citation.	ch3	Fig. 3.2.10	32
25	remove double brackets in citation "((Schneider and Erez 2006))"	We corrected the citation.	ch3		33, 2nd para
26	fix citations	We corrected the citation.	ch3		34, para 3,4
27	fix page break to avoid splitting table.	We fixed the table.	ch3		34, bottom

	A	B	C	D	E
1	**CIE Reviewer #3 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
28	While acidification demonstrably will lead to a decline in crustose coralline algae (CCA) , does it automatically lead to more growth of fleshy algae? This is implied by sentence “(Jokiel et al. 2008) showed dramatic declines (86%) in the growth rate of CCA and other reef organisms (250% decline for rhodoliths), and an increase in the growth of fleshy algae at CO2 levels expected later this century. The decrease in CCA growth, coupled with rapid growth of fleshy algae, will result in less available habitat, and more…” Also, the sentence should not begin with a bracketed term	The depiction of the study is correct. Following sentence qualified to apply when ecosystems follow the pattern seen in mentioned study. "The fleshy algae increase presumably occurs because plant species that have an inefficient carbon capturing mechanism, can have elevated rates of photosynthesis with increased CO2." We changed the wording of the first sentence to avoind starting with a bracket.	ch3		34, para4
29	There is a reference to a figure 3.9, but no such figure exists in the text.	We changed it to Fig. 3.2.12.	ch3		34, para4
30	“The final well-documented impact of ocean acidification (falling carbonate saturation state) is a reduction in the structural stability of corals and reefs, which result both from increases in bioerosion and decreases in secondary cementation.” This is an overstatement. The only study that clearly shows less cementation is that of Manzello on reefs that are anything but typical for the tropics. The precipitation of cements is a much more subtle process than is generally given credit for in the biological literature. It is, by the way, the “primary” way of binding reefs together, so “secondary cementation” is a bit of a misnomer. The organisms themselves bind far less than the cements. For an extreme view on this (not subscribed to by all, or even many) use Silverman J,Lazar B, Cao L, Caldeira K, Erez J (2009). Coral reefs may start dissolving when atmospheric CO2 doubles. Geophys Res Lett 36, L05606, doi:10.1029/2008GL036282, 2009	We changed "secondary" cementation to "reef" cementation. We also added other references that identify the Eastern Tropical Pacific as having the highest recorded rates of bioerosion, lack of cements, fragility, and sitting precariously close to tipping point to net erosion. We added the Silverman reference to the end of the paragraph.	ch3	3.2.3.2	36
31	(Albright et al. in press 2010). Is this in press or published?	It has been published and the citation is changed to 2010.	ch3	3.2.3.3	37, 1st para
32	clean up: “Kuffner et al. (Kuffner et al.) and Jokiel et al. (Jokiel et al.) have…”	We corrected the citation.	ch3	3.2.3.3.	37, 2nd para
33	clean up “Blanchon and Shaw ((Blanchon and Shaw 1995)) argued..” Also clean up the last line and consolidate citations with a single bracket.	We corrected the citation.	ch3	3.2.4.2	37, last para
34	clean up “… ((Neumann and Macintyre 1985))…”	We corrected the citation.	ch3	3.2.4.2	39, ln3
35	clean up: “Blanchon et al. (Blanchon et al. 2009)..” to Blanchon et al (2009)…	We corrected the citation.	ch3	3.2.4.2	39, para 3
36	sentence doesn’t make much sense to me “These surface ocean currents are highly variable over a broad range of spatial and temporal scales, most notably seasonal and inter-annual time scales associated with the El Niño-Southern Oscillation (ENSO).” ENSO operates on an approximately 4-5 year scale, but does not affect reefs world wide other than via teleconnections. Reword a bit.	The text is correct. The order of these two sentences were reversed to point out that the ENSO impact is driven by teleconnections.	ch3	3.2.5	40, 3rd ln
37	clean up: “Vecchi et al. (Knutson et al.) examined changes in tropical Pacific..” and “..it is largely due to anthropogenic climate forcing {Vecchi, 2006 #2248}.”	We corrected the citation.	ch3	3.2.5	40, last para
38	clean up: “…In another comparison of climate observations to models, Wentz et al. (Tissot and Hallacher 2003a) found that..”	We corrected the citation.	ch3	3.2.5	41, 1st ln
39	highly repetitive text: As for density-driven circulation of the ocean interior, many general circulation models of the coupled ocean–atmosphere system simulate a weakening of Atlantic Thermohaline Circulation in response to enhanced greenhouse warming (Latif et al. 2000). Both surface warming and freshening in high latitudes, the so-called sinking region, contribute to the weakening of the Thermohaline Circulation in these models. Some models even simulate a complete breakdown of the Thermohaline Circulation at sufficiently strong forcing (Canadell 2007).	We clarified and condensed the section as requested.	ch3	3.2.5	41,para 2
40	*From p.40, the formatting issues are written into the text and outlined in the accompanying pdf file, so the comments here are sparser. MW added text changing comments in RED.*				
41	“Updated research continues to support this IPCC assessment (Ward et al. 2006).” But this citation is older than the IPCC 2007 report by one year. So how can it be "updated research"?	We corrected the citation to Knutson et al. 2008.	ch3	3.2.6	41, para1, last ln

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1	**CIE Reviewer #3 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
42	“Iron- and clay-rich soils found on many Caribbean…”. Clay forms as detritus when rocks break down. So the clays are mostly locally-formed, but additionally receive input via dust (there is more iron in many Caribbean soils than can be locally produced just by breaking down country rock).	We amended the text according to the reviewer's comment.	ch3	3.2.7	42, 2nd sn
43	really it's the trees that support the Bromeliads. Maybe say "supplements with nutrients" or such...	We amended the text according to the reviewer's comment.	ch3	3.2.7	42, 3rd sn
44	"A further challenge for the researchers is to incorporate the effects of global dimming/brightening more effectively in climate models, to understand their impact on climate change better." Delete "for the researchers"; add "better" in front of understand, delete "better" at the end.	We amended the text according to the reviewer's comment.	ch3	3.2.8	42, last sn
45	insert citation of CDIAC 2009	We changed the citation to WDCGG 2010.	ch3	3.2.10	43
46	"and resultant bleaching and disease are already killing corals and may have caused the first coral extinction." Add "known" before coral and add ref "(Glynn....)" at end.	We amended the text according to the reviewer's comment.	ch3	3.2.10	44, ln 9
47	change "continuing rising temp" to "continuously .."	We changed the wording to "continued warming."	ch3	3.2.10	44, ln 3 from end
48	Note: the stronger the scatter, the less light will be available to the coral - unless all the light gets absorbed, as is the case in dense plumes of fine material. So, I think, it is more the light absorption that matters here.	We modified the text to focus on absorption rather than scattering.	ch3	3.3.1.1	45, para1, halfway
49	but not advected out of the system…”. Aren't things usually ADvected INTO a system? Maybe better to state "transported out of the system"	We amended the text according to the reviewer's comment.	ch3	3.3.1.1	45, para 2
50	: “In highly energetic environments where currents… (Larcombe and Woolfe 1999, Larcombe et al. 2001).” I think that the environments in which these authors worked are not necessarily high-energy....they are just very muddy. For comparison also see the recent papers by Perry et al. in similar environments.	The main point intended was that turbidity and sediment accumulation may be disconnected from sediment supply in some areas. We modified the text to clarify this.	ch3	3.3.1.1	p.45, last line
51	“…with long oceanographic residence time,…” maybe better "water residence time"....but it might be better to change the sentence and make it clearer.	We changed the wording to "restricted circulation."	ch3	3.3.1.2	49, last para
52	“…For example, other stresses…”. Stresses like what exactly?	We amended the text to specify the stresses.	ch3	3.3.1.2	49, last para
53	wouldn't you expect toxins also to act on the larvae? Certainly one could poinson a planula?	We added a line to the pelagic planula as well.	ch3	fig. 3.3.3	
54	“…reefs and those away from the plume were unaffected)…” There is no plume or river mentioned in that sentence. So…what plume?	We clarified the text to specify the Fitzroy River.	ch3	3.3.1.4	53, para 2, halfway
55	“In undisturbed conditions, the distribution of corals is considered the status quo even though the realized niches of the affected corals can be a minor component of their fundamental niches and their realized niches might be in suboptimal environments.” This sentence is unintelligible and should be reworded.	We rephrased this paragraph.	ch3	3.3.3.1	58, para 1
56	Yes, see the parallel in plants. Trees often suffer complete (100%) loss of propagules due to seed predation. Yet, they haven't evolved a defense against seed predators, because as long as the population is maintained, no evolutionary pressure is exerted. Same in *H. coerulea*, ...it's just another "tree". No change to text required…just a thought	We appreciate the interesting thought. (The reviewer noted no change to the text was required)	ch3	3.3.3.1	58, para 3
57	"Therefore, they are only generally present in their natural state on remote Pacific islands.." Change "generally present" to "common"	We amended the text according to the reviewer's comment.	ch3	3.3.3.4	60, para 1, last sn
58	All else being equal, a species with high abundance is at less extinction risk than a population at low abundance because small populations are more vulnerable to the negative impacts of environmental fluctuations, genetic problems, catastrophic events, and other issues. Higher productivity is also an indicator of low extinction risk.” This may be true in a majority of cases, but not in species that have a dynamics accruing high extinction debt. There are many examples of common species going precipitously near-extinct (the Caribbean *Acropora* and *Monstastraea* are a fine example), while rare species persist. That said, and given the general paucity of data on most coral species, I believe that the approach taken and the cited argument are acceptable.	We added a paragraph on extinction debt. In discussion of abundance productivity, we added the sentence: "This is one of the reasons it can be difficult to predict a species vulnerability to extinction based on its current abundance (another reason being potential “extinction debt”, discussed below)."	ch4	4.1	72

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1	**CIE Reviewer #3 Comments on the Draft SRR (Oct 2010)**	**BRT Response:**	**Chapter**	**Page #**	**Line(s) #**
59	"At larger spatial scales, geographic distribution becomes important for "spreading the risk" among multiple populations." Yes, but only if we assume that the population acts as an open population or a well-connected metapopulation (in the strict Hanski-sense), i.e. one where the sub-populations exchange propagules relatively frequently. As corals show (especially the Caribbean *Acropora* and *Montastraea*), wide distribution and ecological dominance do not necessarily insure against precipitous population decline. If recruitment is mainly local, a high extinction debt ensues that, when due to be paid, can be (near-)catastrophic. Thus, even though it may sound a bit paradoxical, the rare species may have a lower extinction likelihood if its local populations accrue less extinction debt, i.e. if the subpopulations are very well connected. Rarity has been demonstrated a realistic survival mechanism in some plants and animals (the plants being better models for the corals).	We added a paragraph on extinction debt that addresses these issues raised.	ch4	4.1	73, para1
60	Thermal Stress: "The genus is also highly susceptible to bleaching in the western Indian Ocean (McClanahan et al. 2007) and has caused local extirpations in the tropical Eastern Pacific (Glynn and de Weerdt 1991)." Highlighted - no additional comment.	A genus can't cause local extirpation. We clarified the text accordingly.	ch6	Millepora foveolata	170
61	distributional chart: *H. coerulea* definitely does not occur in the Arabian Gulf and the N.Arabian Sea.	We added this information to the text.	ch6	Fig. 6.7.2	178
62	also note that Glynn et al. (2007) suggest that due to poor description of the types, there is very little reason to separate P. elegans from P. verrucosa and suggested the two to be likely synonymous.	We amended the text and cited Glynn et al. 2007 and Reyes-Bonilla 2002.		6.8 (Pocil. preamble)	182, last para
63	distributional chart: also occurs on Easter island (Glynn et al. 2007).	We amended the text and cited Glynn et al. 2007 and Reyes-Bonilla 2002.		Pocil. ele; Fig 6.8.6	191
64	"despite previous records from central Pacific (and wasn't seen by Wells (Wells 1954))." I don't understand why this statement is here, given that Wells (1954) is not cited in the previous sentences.	The BRT believes that the Wells reference relevance is because it was a previous survey at Bikini that did not record the species. We amended the sentence for clarification.		Seriatopora aculeata	199
65	Disease: I think it's not only emerging diseases, but diseases in general.	The BRT agrees with the reviewer's comments and deleted "emerging."		Seriatopora aculeata	200
66	Just for info: Note: Riegl and Purkis (2009) Ecol Mod 220:192-208 calculated the recruitment rates needed to recover such populations. This might be helpful for the discussion of the Critical Threshold.	We added the reference.		Acropora preamble	203, para 4
67	Yellow Band Disease in the Arabian Gulf (Korrubel and Riegl 1996; Riegl 2002) takes a big toll on Acropora populations. Spreads up to 2 cm per week.	We added the references.		Acropora preamble	204, para1
68	"Lateritic soils are typical of tropical islands." as long as they are volcanic. Better say "Typical of high tropical islands" or "of volcanic tropical islands".	We amended the sentence and added "volcanic."		Acropora preamble	204, last para
69	*A. horrida*, bleaching: *A. horrida*, if it ever existed in the Arabian Gulf (it is listed by Riegl 1999) went locally extinct after the 1996 and 1998 bleaching events. Might be useful to evaluate overall threat.	We amended the text to include the information given by the reviewer.		Acropora horrida, Threats	235
70	*A. pharaonis*, thermal stress: *A. pharaonis* became locally extinct in the SE Arabian Gulf after the combined impacts of the 1996, 1998 bleaching events (Riegl 2002, Mar Biol 140:29-40)> might be useful info to evaluate overall threat.	We amended the text to include the information given by the reviewer.		A. pharaonis, Threats	263
71	distribution: *I. palifera* exists at Bassas da India and on the SE African mainland until northern KwaZulu/Natal. I. cuneata is not proven to exist (source: Riegl B (1995) A revision of the hard coral genus Acropora in SE Africa. Zool J Linn Soc 113: 249-288).	We amended the text to include the information given by the reviewer.		Isopora cuneata, Global distribution	316
72	*I. cuneata*, thermal stress: *I. cuneata* was a common species in the *A, palifera* zone of the Chagos, which got almost completely wiped out in 1998 and has not regenerated (Sheppard et al 2002 Erosion versus recovery of coral reefs after 1998 El Nino: Chagos Reefs, Indian Ocean. Ambio 31(1) 40-48).	We amended the text to include the information given by the reviewer.		Isopora cuneata, Thermal stress	317
73	Correct spelling to "Pachyseris"	We corrected the typo.			406,1st ln