Review of NOAA Technical memorandum NMFS-PIFSC-XX: Status Review Report of 82 species of Corals under the US Endangered Species Act

Reviewer:

Terry P. Hughes

Director, ARC Centre of Excellence for Coral Reef Studies,

James Cook University, Townsville, QLD 4811, Australia

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).

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).

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.

One 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.

The report authors need to be more careful to avoid citing secondary studies such as flawed meta-analyses, 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*.

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.

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.

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.

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.

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 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*"!

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.

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.

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 Acidification section, repeated verbatim for every species in Chapter 6, is very weak: "*<u>Unknown for this genus</u>. 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.

*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.

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.

Specific Comments

Many of my specific comments below are minor, but there are many of them, and some are much more substantive. I hope they are useful. I have marked the latter with an asterisk (*). Page numbers refer to the numbers in the document, and quotes from the report are in italics.

Executive Summary

Page ix, third paragraph, line 4. "*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.

Page ix, footnote. "*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.

Page x, last paragraph, 4 lines from the bottom: *"... 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.

Chapter 1

Page 1, I would prefer to see the term "coral" reserved for scleractinians.

Page 2, 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?

Page 3, first paragraph, last sentence. "*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?

*Page 3, third paragraph, line 9 onwards. 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.

Chapter 2. This is exceptionally well written and comprehensive.

2.2.1 Heading. 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, post-settlement competition, etc.

Page 8, first paragraph, first sentence. "*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.

*Page 8, first paragraph, third sentence. "*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.

Page 8, second paragraph, second sentence. "*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).

Page 8, third paragraph, third sentence. "*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.

Page 8, third paragraph, last sentence. "*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.

Page 9, Figure 2.2.1. Inconsistent notation on ages of each stage in life cycle – add in hours and days for the pre-settlement stages.

*Page 9, The one-sentence paragraph on connectivity is inadequate.

Page 10, third paragraph, last sentence. "*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 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

Page 10, fifth paragraph, second last sentence. *"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).

Page 11, first paragraph, second sentence. Typo. "*maintaine(d)*".

Page 11, second paragraph, second sentence. "*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.

Page 11, third paragraph. "*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.

Page 12, second paragraph, line 7: "*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.

Page 12, second last paragraph, end of third line. "*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.

Page 12, second last paragraph, line 9. *"…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.

Page 12, second last paragraph, line 11. *"…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.

Most Indo-Pacific corals have huge geographic ranges (see, for instance, Hughes et al. 2002. Ecology Letters), including some of the 82 considered here.

Page 13, first paragraph, line 3. *"...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".

Page 13, first paragraph, line 7. Pandolfi et al. 2005 is not the primary reference for the economic value of Florida's reefs.

Page 13, third paragraph, line 8. 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.

Page 13, fourth paragraph, second last sentence. "*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*.

Page 13, last paragraph, line 4. "*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.

Page 14, third paragraph, line 9. "*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).

Page 15, first paragraph, line 2. *"...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.

*Page 15, second paragraph, first sentence. "*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.

Page 15, third paragraph, last sentence. *"…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).

*Page 15, last paragraph, last two sentences. "*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.

Page 16, third paragraph, first sentences. "*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.

Page 16, third paragraph, line 4. *"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?

Page 16, third paragraph, line 9. "*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)". "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.

*Page 16, third paragraph, last line. *"…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.

Page 17, first paragraph, last line. *"...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.

Page 17, last paragraph, last sentence. "*The BRT determined corals limited to the eastern Pacific, <u>with approximately one third as many genera, less than half the species</u>, 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.

Chapter 3

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".

Page 19, second paragraph, last sentence. "*Meaningful progress in conserving and restoring coral-reef 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.

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*".

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.

Page 24, figure 3.2.3. Please put in Y-axis units on the left.

Page 24, first paragraph, line 5. Hoegh-Guldberg et al. (2009) is not the primary reference for temperature rises. They cite IPCC.

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.

*Page 26, Table 3.2.1. Donner (presumably 2009?) is not the primary reference. I don't think a basin-wide projection is very meaningful. For example, IPCC projections within the GBR-Melanesia province vary from no change at the equator to 3^{o}C for the southern GBR (at 23^{o}S) under A2 conditions.

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

*Section 3.2.2.1 Coral Bleaching. This section is rather poorly written compared to most of the report.

*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.

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?

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).*

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).

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 <0.2^{o}C per decade. Reefs to the north have warmed more.

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.

Page 27, third paragraph, line 1. "*Buddemeier and Fautin ~~(Buddemeier and Fautin~~ 1993~~)~~ proposed that bleaching…".* Please tidy up the referencing in this section.

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.

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.

*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?

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.

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.

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.

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.

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.

Page 29, last paragraph, line 2. *"…spatial variation (figure 3.2.8)…".* This figure doesn't show spatial variation. Figure 3.2.10 does.

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?

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?

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.

*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).

*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.

*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.

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.

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.

Page 36, first paragraph, line 10. *"…such as hurricanes, vessel groundings, and anchoring."* The last two are trivial, as indicated elsewhere in the report.

*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.).

Page 37, second paragraph, line 11. *...crustose coralline algae in mesocosm experiments in moderate OA treatments".* Explain OA. What does "moderate" mean?

Page 37, fourth paragraph, line 1. "*The IPCC Fourth Assessment Report (AR4) (IPCC 2007a) ~~determined~~ concluded that sea level will continue to rise...".*

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.

Page 40, second paragraph, line 8. Typos. "*..the Walker circulationspace(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.

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?

Page 41, second paragraph, line 2. *…models…atmosphere system ~~simulate~~ predicts a weakening of Atlantic Thermohaline Circulation in response*"

Page 41, second paragraph, last line. Typo. "… (McMullen and Jabbour 2009)~~s~~…".

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.

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).

Page 41,last paragraph, line 1. Buddemeier et al. ~~(Buddemeier et al.~~ 2004~~)~~ argue that there is little evidence…

Page 42, first paragraph, line 10. Buddemeier et al. ~~(Buddemeier et al.~~ 2004~~)~~

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!

Page 42, section 3.2.8. Seems very peripheral to the topic of the report, inconclusive.

Page 42, fourth paragraph, line 8. Typo. *"A further challenge for ~~the~~ researchers..."*

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"*.

Page 43, third paragraph, line 7. Typos. *"...in latitudinal expansionsspace(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…"*

*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".

"*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.

"*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. Typo, "*Presently, atmospheric CO2…exceeding worst case scenarios used in modeling future climate change (CDIAC 2009Close Bracket, (IPCC 2007a)".*

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?

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.

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.

Page 44, Last Line. *"…anthropogenic increases in atmospheric CO2, are likely to be the greatest threats to all ...".*

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 non-terrigenous sediments. In the next sentence, add wind as a mechanism for re-suspending sediment.

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.

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.

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".

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}*".

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.

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.

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.

Page 47, third paragraph. Some of this text is duplicated earlier in the sea rise section. Line 4. "*Greater inundation of reef flats can <u>erode residual soils</u> 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?

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?

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.

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.

Page 49, first paragraph, lines 1-3. *"Coral 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.

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.

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.

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.

Page 49, Section 3.3.1.3. The writing in this section is much better and more authoritative.

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.

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.

Page 52. Somewhere in this section there needs to be a discussion of haloclines and freshwater lenses, creating depth profiles in impacts of floods.

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.

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)?

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?

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.

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 meta-analysis of coral abundance.

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.

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.

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.

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.

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.

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).

*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 "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 misquoted here. You should be citing the primary papers that Steneck was summarizing.

Page 59, section title. "*Synergistic effects of predation*" and disease (?)

Page 59, first paragraph, line 2. "*Healing ~~rate~~ time increases non-linearly with lesion size…*".

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.

"*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.

Page 59, fourth paragraph, line 4. Ayling and Ayling (1997) reference is missing from the bibliography.

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).

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.

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.

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.

Page 60, section 3.3.3.5. I didn't find the summary very useful.

Page 60, fourth paragraph, line 1. "*There are fundamental differences in ecosystem-level processes between coral reef and pelagic fisheries*". So?

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.

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.

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*.

Page 60, sixth paragraph, line 5. *"…collapse into an alternative stable state or "phase shift" (Mumby et al. 2007b)*. These concepts were originally demonstrated for coral reefs by Done (1992), and Hughes (1994).

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).

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.

*Page 62, first paragraph, line 3 onwards. Typo. "*Raymondo and colleagues 2009spacefound that overfishing appears to increase the frequency of coral disease*".

"*Fishing activity usually targets the larger apex predators*". But for most reefs and reef fisheries today, this is ancient history.

"*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.

"*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.

"*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.

*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.

*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.

Page 62, third paragraph, line 3. *"…explosive or toxic chemicals…are* <u>*not as well documented*</u> *in Caribbean waters".* The issue here is not documentation. Bombs and cyanide are not an issue in the Caribbean.

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(?)

Page 62, fourth paragraph, line 6. "*Much harvest of … ".* Poor English.

*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.

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?

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).

"*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.

"*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 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.

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.

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.

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?

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?

*Page 65, first paragraph, line 10. *"Preliminary 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).

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.

*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.

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 *"non-equatorial (poleward of ~ 5° latitude) oceanic atolls…"*.

*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.

"*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.

"*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?

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.

Page 68, second paragraph, last line. "*In Hawaii, there are 287 introduced marine invertebrate species,...and <u>relatively few</u> have become established…*". How many are "established"?

Page 68, third paragraph, last line. Add the Caribbean origin of *Carijoa riisei*, and note that it is an octocoral rather than a scleractinian.

Page 69, first paragraph, last line. *"…and the two black corals experienced <u>niche compression</u>"*. In plain English, does this mean their depth range has been compressed by extirpation from shallower sites?

Page 70, fifth paragraph, line 2. "*Impacts to reef food webs... significant changes in the <u>coral reef fish complex</u>, with unknown <u>synergistic</u> impacts to the corals"*. Fish assemblages? Synergistic interactions between what and what?

"Overfishing is typically thought of as a human-induced issue". Seems rather obvious. Delete, and remove the "However" from the last sentence.

Page 70, section 3.4 Heading. "*Interactive and Unknown ~~Cryptic~~Threats to ~~on~~ Coral Populations*"

*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.

Page 71, second paragraph, last line. *"… bleaching resistance ~~west~~ (West and Salm 2003)"*.

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.

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.

Page 71, final paragraph, line 9. *"…fishing reduced coral cover by 51%"*. How? Is this a spatial comparison between fished and non-fished reefs?

Chapter 4.

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.

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.

*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.

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.

Page 74, section 4.2. Abundance and ~~Productivity~~ Regenerative Capacity of Corals

Page 74, fourth paragraph, line 8. Typo. Italicize Dendrogyra cylindrus.

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.

Page 74, fifth paragraph. This is a key part of the report, and I couldn't agree more.

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.

Page 75, fifth paragraph, line 3. So what does the Richards (2009) PhD thesis have to say about effective population size?

Page 76, Section 4.5. The report would benefit here from a summary of the extinction debt concept.

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.

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.

Page 77, fourth paragraph, line 3. "*However, anthropogenic <u>physical disturbances</u> and <u>chemical pollution</u> 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.

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).

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. Over-predation 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.

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).

Page 79, Figure 4.6.3. The caption seems to confuse coral cover and colony size.

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.

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.

Page 81, the second paragraph on colony size and its importance is somewhat superficial and under-referenced. 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.

Final sentence "*However, there are some circumstances in which small colony sizes are advantageous (Shenkar et al. 2005)"*. What are they?

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"?

Page 82, first paragraph. "*The BRT would consider a species that lost the ability for successful <u>recruitment</u>…This issue is of some concern in species such as those of the Montastraea annularis complex that show very low levels of successful <u>sexual reproduction</u> (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.

Chapter 5

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.

Bibliography: There are some formatting errors such as missing italics for species names.

Chapter 6

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.

*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?

*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.

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.

*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.

Page 130, first paragraph, last sentence. *"The congener Agaricia tenuifolia replaced Acropora cervicornis…".* Why is this relevant?

Page 130, fifth paragraph. Typo? *"Although its platy morphology could make it sediment-<u>susceptible</u>, A. lamarcki is <u>in</u>efficient at actively rejecting sediment (Bak and Elgershuizen 1976)".*

Page 133, last paragraph, line 1. "*Published and unpublished records indicates Mycetophyllia ferox is rare (< 0.1% <u>species contribution</u> and <u><0.8 colonies/10 m^2</u>) in Florida (2010) and rare (<u>0.8 colonies/100 m</u>) in Puerto Rico (AGGRA database online at <u>http://www.agrra.org)</u>*". 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 $10m^2$ 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?

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.

*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?

Page 137, in Habitat section. “*Most reef environments (Veron 2000)”*. This statement by Veron is ill-informed.

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.

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.

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.

Page 142, Second paragraph. “*D. stokesii is described as a gonochoric spawner*”. Reference?

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.

*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.

Page 147, first paragraph, line 1. Typo. “*All three of the Montastraea …*”

Throughout pages 147 and 148. Typo. "*Montastraea annularis*".

Page 147, second paragraph, line 4. *"…the Caribbean also report them to…"*.

Page 147, second paragraph, line 4. "*Edmunds (Edmunds et al. 2010 in press) states that the "storage 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).

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.

Page 147, second paragraph, line 4. *"…degree of ~~fragmentation~~ fission and clonal reproduction"*. Fragmentation is usually only used to describe breakage of the skeleton.

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.

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.

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.

Page 153, Characteristics, line 1. Typo. "*Montastraea franksi is distinguished by ~~larg~~ large, unevenly arrayed polyps…"*.

Page 156, Global Distribution, line 2. *"…but may be absent from Bermuda…".* We don't need two distribution maps.

Page 160, Bibliography. The Carpenter reference is included twice.

*Page 202. As noted, the references need to be tidied up.

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.

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.

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?

Chapter 7

*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.

Page 511. The Indonesian attribution is very unlikely to be accurate given the absence of this species across the western and central Pacific.

Page 512. Acidification and LBSP. But remember that *Millepora* is not a scleractinian, so these comparisons to "corals" are less relevant.

Page 513. Last paragraph. Typos. "*geo-graphically*" and reference formatting.

Appendix I: Background Material

NOAA Technical memorandum NMFS-PIFSC-XX: Status Review Report of 82 species of Corals under the US Endangered Species Act.

Appendix II: Statement of Work for Dr. Terry Hughes (James Cook University)

External Independent Peer Review by the Center for Independent Experts

Status Review of 82 Species of Coral

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance the predetermined Terms of Reference (ToRs) of the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in **Annex 1**. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.org.

Project Description: A Status Review of 82 species of coral was conducted by a team at the Pacific Islands Fisheries Science Center pursuant to a petition for NMFS to list 83 coral species and designate critical habitat for them under the Endangered Species Act. Of the petitioned species, 8 occur in the Atlantic and 75 in the Pacific. NMFS has found that the petitioned action may be warranted for 82 of the 83 species; the status review is for these 82 species. The draft Report of the status review team is the subject of the peer review. For each coral species, the report presents and evaluates information on the species' distribution, biology, abundance trends, natural and anthropogenic threats, and danger of extinction throughout all or a significant portion of its range. The Terms of Reference (ToRs) of the peer review are attached in **Annex 2**.

Requirements for CIE Reviewers: Three CIE reviewers shall conduct an impartial and independent peer review in accordance with the SoW and ToRs herein. The combination of required expertise of the CIE reviewers shall include working knowledge and recent experience in the biology and ecology of corals, population dynamics of marine invertebrates, quantitative assessment of extinction risk. Each CIE reviewer's duties shall not exceed a maximum of 10 days to complete all work tasks of the peer review described herein.

Location of Peer Review: Each CIE reviewer shall conduct an independent peer review as a desk review, therefore no travel is required.

Statement of Tasks: Each CIE reviewers shall complete the following tasks in accordance with the SoW and Schedule of Milestones and Deliverables herein.

Prior to the Peer Review: Upon completion of the CIE reviewer selection by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later the date specified in the Schedule of Milestones and Deliverables. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, and other pertinent information. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

Pre-review Background Documents: Two weeks before the peer review, the NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports for the peer review. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review.

Desk Review: Each CIE reviewer shall conduct the independent peer review in accordance with the SoW and ToRs, and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs can not be made during the peer review, and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator.** The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements.

Contract Deliverables - Independent CIE Peer Review Reports: Each CIE reviewer shall complete an independent peer review report in accordance with the SoW. Each CIE reviewer shall complete the independent peer review according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

1) Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review.
2) Conduct an independent peer review in accordance with the ToRs (**Annex 2**).
3) No later than REPORT SUBMISSION DATE, each CIE reviewer shall submit an independent peer review report addressed to the "Center for Independent Experts," and sent to Mr. Manoj Shivlani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and Dr. David Die, CIE Regional Coordinator, via email to ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.

25 October 2010	CIE sends reviewer contact information to the COTR, who then sends this to the NMFS Project Contact
28 October 2010	NMFS Project Contact sends the CIE Reviewers the report and background documents
1-15 November 2010	Each reviewer conducts an independent peer review as a desk review
19 November 2010	CIE reviewers submit draft CIE independent peer review reports to the CIE Lead Coordinator and CIE Regional Coordinator
3 December 2010	CIE submits the CIE independent peer review reports to the COTR
10 December 2010	The COTR distributes the final CIE reports to the NMFS Project Contact and regional Center Director

Modifications to the Statement of Work: Requests to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent substitutions. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on substitutions. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW and ToRs. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Applicable Performance Standards: The contract is successfully completed when the COTR provides final approval of the contract deliverables. The acceptance of the contract deliverables shall be based on three performance standards:

(1) each CIE report shall completed with the format and content in accordance with **Annex 1**,

(2) each CIE report shall address each ToR as specified in **Annex 2**,

(3) the CIE reports shall be delivered in a timely manner as specified in the schedule of milestones and deliverables.

Distribution of Approved Deliverables: Upon acceptance by the COTR, the CIE Lead Coordinator shall send via e-mail the final CIE reports in *.PDF format to the COTR. The COTR will distribute the CIE reports to the NMFS Project Contact and Center Director.

Support Personnel:

William Michaels, Contracting Officer's Technical Representative (COTR)
NMFS Office of Science and Technology
1315 East West Hwy, SSMC3, F/ST4, Silver Spring, MD 20910
William.Michaels@noaa.gov Phone: 301-713-2363 ext 136

Manoj Shivlani, CIE Lead Coordinator
Northern Taiga Ventures, Inc.
10600 SW 131st Court, Miami, FL 33186
shivlanim@bellsouth.net Phone: 305-383-4229

Roger W. Peretti, Executive Vice President
Northern Taiga Ventures, Inc. (NTVI)
22375 Broderick Drive, Suite 215, Sterling, VA 20166
RPerretti@ntvifederal.com Phone: 571-223-7717

Key Personnel:

NMFS Project Contact:

Jerry Wetherall
Pacific Islands Fisheries Science Center
2570 Dole Street, Honolulu, HI 96822
Jerry.Wetherall@noaa.gov Phone: 808-983-5386

Megan Moews
Pacific Islands Fisheries Science Center
1601 Kapiolani Blvd., Suite 1110, Honolulu, HI 96814
megan.moews@noaa.gov Phone: 808-944-2120

Annex 1: Format and Contents of CIE Independent Peer Review Report

1. The CIE independent report shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations, and specify whether the science reviewed is the best scientific information available.

2. The main body of the reviewer report shall consist of a Background, Description of the Individual Reviewer's Role in the Review Activities, Summary of Findings for each ToR in which the weaknesses and strengths are described, and Conclusions and Recommendations in accordance with the ToRs.

3. The reviewer report shall include the following appendices:

   Appendix 1: Bibliography of materials provided for review

   Appendix 2: A copy of the CIE Statement of Work

Annex 2: Terms of Reference for the Peer Review

Status Review of 82 Species of Coral

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

1. 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?
2. Are methods used valid and appropriate?
3. Are the scientific conclusions factually supported, sound, and logical?
4. Where available, are opposing scientific studies or theories acknowledged and discussed?
5. Are uncertainties assessed and clearly stated?

Evaluate the findings made in the Status Review.

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

All information associated with the Status Review document is to remain strictly confidential until the Status Review is posted to the PIFSC website and/or the Federal Register by NMFS.