Reviewer #1: The manuscript by Lauchstedt et al. applies machine-learning techniques to coral traits and models IUCN threat categories. They conclude that a model based on four traits can accurately predict threat status and uses it to assign a status to previously data deficient species, and compare the susceptibility of fossil corals by applying the model to the Last Interglacial Episode.   
  
This manuscript presents a useful analysis, and I appreciate their consideration of coral traits to an applied problem, IUCN threat categories. However, I think the novelty of this work is oversold; all told, the analysis adds IUCN status for 77 previously data deficient species, 17 of which "showed no linkages to compilations of trait expressions" (L74-75), leaving 60 species with new IUCN status. Of these, the most urgent are the VU or EN categories (sensu Foden et al. 2013) - here, there are 21 and 8 species, respectively. But we are never told anything about these species in the main text - what are they? Does it make sense they are threatened?

We thank the reviewer for a constructive review, and we welcome the opportunity to explain why we think the paper is indeed novel. The key novelty of our paper is that, with only four basic attributes, we can retrieve the exact IUCN status of reef corals with 76% accuracy. Not only can previously data deficient corals be given an IUCN status, but the approach also allows us go back in time and assess the distribution of fossil assemblages.

Regarding the specific question on the nature of the new categorization of previously data deficient species, we had checked the literature but information is very scarce, which is perhaps the main reason for data deficiency. We have added a brief section outlining this issue.

Citations and a new chapter in the discussion were added: L286-294

Fig. 1 suggests the main difference is geographic range - the reader is left wondering whether morphological or life history traits matter?

Yes they do! Geographic range is indeed the dominant variable to explain IUCN category in these univariate plots (72% of variance from LC to CR versus for example 33% for water depth again LC to CR). The true influence of each trait only emerges in the ANN, which is now specified in the main text.

In all, the authors need to provide a stronger reasoning for the conservation impact of the "formal incorporation into the assessment of extinction risk may help improve the assessment of risk status of currently unevaluated species" (L51-53), otherwise this paper is better framed as a methods paper.

We now state explicitly the key advantage of using simple attributes in risk assessments: cost-efficacy (L 317-318). The paper is still heavy on methods, but we argue that our advanced methods are a prerequisite for a successful categorization of risk assessments and hence need be explained in full.

The analysis on the LIG paleo feels ad hoc; I am left wondering how an IUCN analysis of reefs thousands of years ago is a relevant comparison to the challenges faced by contemporary coral reefs.

With the LIG, we intended to show another application of our approach, namely to assess the match between IUCN categories and actual extinction (see above). In addition, the last interglacial is assumed to have been warmer than today making climate change a potential driver of extinction and our model allows identifying potential hotspots.

We rewrote the discussion L295-314

For example, the manuscript concludes with a consideration of paleo vs contemporary reefs that is a confusing ending to the manuscript. A broader conclusion to help put the main messages of the manuscript in context would benefit the reader.

We rephrased our ending returning the main motivation of our work and its future implications.

L 315-320

Overall, the manuscript feels weakly grounded in the existing literature and it is unclear what are the objectives of this work, or how this would move the field forward. Carpenter et al. 2007, Madin et al. 2016, Huang 2012 feel poorly developed in the Introduction and Discussion; I am left wondering why this manuscript's analysis is novel given the work already published on this topic.

The novel aspects of our paper were stated more clearly in this revision. Discussions of Carpenter et al. 2008, Madin et al. 2016, Huang 2012 were expanded in the introduction and discussion.  
L42-46, 56-58, 93-94, 276-277, 318-320  
Minor comments  
Adding some indications of significant post-hoc analyses would be nice for Fig 1.   
Results of Kruskal-Wallis Tests for the comparison of medians were added to the figure caption L349-352 and are stated in the text L184-185.  
Methods - Madin et al. 2016 has also demonstrated skewed taxonomic coverage of traits.

This is exactly the reason why we ended up using just four traits. The imputation approach was used to fill minor gaps but traits missing in more than 5% taxa were excluded (see methods).   
Why was growth form reduced to a binary variable or branching vs non-branching?? This seems like a gross oversimplification.   
We now defend our approach in the main text more explicitly. In short:

1. The CTB lists 12 different growth forms. To include them all in the analyses could cause model overfitting, where it is difficult to decide which growth form is really most relevant.

2. It is reasonable to condense the growth forms, because, although some growth forms are single categories like hispidose, corymbose and caespidose, their basic shape is still arborescent/branching and therefore we found it reasonable to pool them.

A rationale for the pooling is now given and the pooling scheme described in L94-98

L147-149 Why were these two particular species selected, re: "two extinct Caribbean species Pocillopora palmata and Orbicella nancyi (Pandolfi et al. 2001) was used to determine whether predicted extinction risk was actually greater than average for these species". Not clear why this is relevant.

This is relevant because these two species are the only taxonomically verified coral species in the Caribbean with confirmed extinction after the LIG. All other nominal extinctions are based on old literature without revisions. Pending further studies, we cannot us more corals to check for the match between inferred IUCN and extinction.

Section on single traits starting L228; how do findings relate to supertrait of Colony mass per area (CMA) proposed by Madin et al. TREE 2016? I think the authors can do a better job placing their research and discussion in the context of recent literature.   
This paper was already cited by us and we admit the possible advantages of a supertrait approach. However, our constraint on data completeness prevented us from exploring the entire suite of traits. In any case, we now mention the term ‘supertrait’ explicitly and suggest that corallite size may be close to its original definition as “a trait or combination of traits that capture a large amount of variation for a broad range of biological, ecological, and evolutionary processes.”

Amendments made in l. 89-91, 276-277, 318-320

Reviewer #2: The paper entitled "Reef coral traits predict extinction risk" by Lauchstedt et al. presents a test of the link between species-level metrics and their IUCN risk category. The findings are followed by a biogeographical analysis showing that low-richness regions (e.g. the Caribbean) contains the highest proportion of 'critically endangered' species, while regions of higher species richness (e.g. the Coral Triangle) may contain proportionally more 'threatened' species. Their model corroborates and updates the results of a previous study (Carpenter et al 2008 Science 321:560-563). The most novel and exciting component of the paper, in my opinion, is the use of models generated from the analysis to make inferences about the vulnerability of Pleistocene species that have subsequently become extinct. Nevertheless, while the quantitative analyses of the paper appear to be highly sophisticated, the interpretation of the results are, in my view, currently overstated or approached from the wrong angle. Below I outline major comments on the core themes of the paper (coral traits/extinction risk) that can help to address these issues. Most of the changes can be achieved by modifying the wording of the title, abstract and main text. Since I am not familiar with neural network models/machine learning algorithms, I am not qualified to comment extensively on the statistical techniques used.     
  
Major changes:  
  
Coral traits: Two of the four traits analysed are not strictly traits (Maximum Water Depth and Geographic Range). Trait-based analysis has great potential in ecology and conservation but its impact relies on consistent understanding and definitions. The authors cite Violle et al. (2007) Let the concept of the trait be functional! Oikos 116:882-892 - a widely cited review that clarifies exactly what we mean by traits. In their study, Violle et al. conclude that traits are "Any morphological, physiological or phenological feature measurable at the individual level, from the cell to the whole-organism level, without reference to the environment or any other level of organization." While this review is widely cited by coral reef ecologists, it's key message is often ignored. Maximum depth and geographic range are not traits as they cannot be measured at the individual level. Instead they are components of species distributions. I am aware that these species distribution parameters are listed on [coraltraits.org](http://coraltraits.org) and have been described as traits in the coral reef literature (e.g. Darling et al. 2012 and elsewhere) but for the purposes of clarity this has to change. This means changing the title (for example, "Species distributions and traits predict IUCN conservation status in reef corals," or something similar), and modifying the text accordingly throughout the manuscript.

We thank the reviewer for her/his very constructive review. We agree that some of our prose may have been phrased from the wrong angle. Being aware of the different concept of traits among publications, we had first distinguished between geographic and life-history traits but then subsumed under the general term traits. We agree that the new title reflects better reflects the content of the paper and adopted it. We now distinguish between spatial and morphological attributes and only use trait when applicable.

L27, 29, 38, 80, 82, 86, 108, 126, 129, 176, 195, 239, 244, 276, 245, 211, 317  
  
Extinction risk: The utility of the IUCN categories for reef-building corals is a controversial issue that remains open for debate. Indeed, many believe that the information used to place species in IUCN categories is insufficient to determine extinction risk (even for species that are not listed as 'data deficient'). My suggestion, therefore, is to avoid stating that this study is a test of 'extinction risk' and more a test of the association between traits and IUCN categories. The title that I have suggested above ("Species distributions and traits predict IUCN conservation status in reef corals") is a good start, as it avoids stating upfront that you have measured extinction risk.

We agree with the reviewer that IUCN status may not necessarily reflect true extinction risk. However, quoting the IUCN “The IUCN Red List of Threatened Species … uses a set of quantitative criteria to evaluate the extinction risk”. So, for the purpose of our study extinction risk and IUCN status may by used interchangeably, especially when termed “extinction risk categories” as we now do

The results of this study lend increasing support for the relevance of the IUCN categories as it seems they associate with attributes of corals that could be linked with extinction. At the same time however, the categories did not predict extinction for 1 out of 2 Pleistocene corals, suggesting that despite their association with key traits, the capacity of IUCN categories to predict extinction risk may still be limited.

This is a good point, which is now emphasized in the discussion. Specifically, we address that while the prediction of IUCN categories is great, the prediction of actual extinctions is not.

L302-304.

Statements of these results should be followed by a discussion of how the IUCN categories can be improved and the different metrics (traits or distributions) that are required to build a better picture of conservation status in corals.

It was the motivation of this paper, to find easy-to-gather attributes that could partly replace IUCN criteria that require extensive montoring (e.g., Criterion A, population reduction). We did not intend to improve the IUCN categories, which hopefully clear in this revised draft.

Minor comments:  
  
Lines 33-35: I'd remove the finding that 'Removing geographic range from the predictors slightly lowered the explanatory power' from the abstract, and replace with more critical results. What did the analysis of Pleistocene corals reveal? What about the biogeographic analysis?   
Abstract revised accordingly. Pleistocene distributions have been added L34-36.

Lines 42-45: After taking a look at the supplement for Carpenter et al. 2008, it seems as though some 'traits' were included in their analysis to define IUCN categories (e.g. maximum depth). Does this mean that there is an element of non-independence in the analysis of the relationship between traits/distributions and IUCN risk, and if not why not? Please clarify here.

Carpenter et al. used trait information as additional information for assigning threat statuses for LC and NT species (page 5, v.). The source of the information for their trait ‘lower depth’ is not to be found in their SOM and was probably derived from expert knowledge. As the CTD is not cited, neither is Madin et al. 2016, we therefore assumed that the data used for our analysis is not congruent with the one used by Carpenter at al. and therefore a non-independence can be ruled out.  
  
Line 50-51: Surely climate change in combination with other stressors generates extinction risk?

Explicit reference to climate change deleted

Line 105: Please define 'great circle distance.'

Great circle distance is the shortest distance between two points on the surface of a sphere, measured along the surface of the sphere. This info has been added to the manuscript L100-102.  
  
Line 110: Does "Range area" refer to geographic area size in the CTD?

Yes, Range area is trait nr. 138 in the CTD as calculated from GIS shapefiles

Lines 154-156: What exactly is plotted inside these hexagonal grids? Proportions? Means?

As now stated in the figure captions (L354-367), inside the grids, the percentages of species falling in one of the IUCN categories indicated, are plotted.

Line 163: Why wasn't growth rate used in the model if it had good correlation with threat status?

We limited the traits used to those with less than 5% missing data. Growth rate, unfortunately had 84.3% missing data across species and was thus excluded.

Line 168: Perhaps repeat the list of the four traits again here.

Done. L190-191

Line 265. What are the potential reasons for the disagreement between this study and Foden et al? I'd suggest that fast-growing taxa could be at lower risk of extinction if they are 'weedy' (i.e. have high reproductive output), suggesting that more complex life history traits need to be considered.

Darling et al. 2012 states: “For example, small corals with brooding reproduction, fast growth rates and high population turnover are expected to be ‘weedy’ (Knowlton 2001), while large, slow-growing colonies of massive corals are expected to be more tolerant to chronically stressful or variable environments (Jackson & Hughes 1985; Soong 1993; Rachello-Dolmen & Cleary 2007).”

We agree that more complex life history traits could be considered, but this was not directly the scope of this paper. The use of other traits, or the aggregation of more traits to “supertraits” (Madin et al.) would be of beneficial, but most of these traits are very incomplete in the CTD. We now discuss supertraits in the text.

L 89-91, 276-277, 318-320

Lines 275-277: Sentence starting with "Major advantages…" The statement about redundancy is wrong. The Caribbean has lower trait redundancy compared to most other regions, as demonstrated in a range of studies. There is no evidence that the phylogenetic patterns measured in Huang & Roy 2015 relates to symbiont diversity/coral trait diversity.

We admit that this statement was wrong and we rewrote the whole paragraph L296-314  
Spelling/Grammar:   
All done but probably obsolete due to fundamental changes.  
Line 95: Spelling (\*functions). Also delete 'in the CTD.' Done  
Line 104: Delete 'directly' Done  
Line 138: Comma after 'test' Done  
Line 162: Delete 'a species could inhabit' Done  
Line 163: Insert 'the' before 'best' Done  
Line 164-166: Sentence beginning with 'Although' needs rewording Done