Dear Mr. Lauchstedt,  
  
I have received the reports from the Topic Editor and the reviewers of your manuscript, "Species distributions and two morphological traits predict the IUCN conservation status in reef corals", which you submitted to Coral Reefs.  
  
Your manuscript requires a major revision based on the Topic Editor and reviewer comments.  Their comments can be found below.  Please also check the website for possible reviewer attachment(s).  
  
The revision should address all points raised by the reviewers in a point-by-point reply to the comments of the reviewers and editor.  Your reply should be submitted either as a separate "Response to Reviewers" file or included in the textbox provided during the resubmission process.  The new manuscript must be uploaded as the "manuscript file", and a version of the original manuscript with all changes marked (e.g. track change mode or colored/highlighted text where changes have been made) should also be submitted as a "Track changes" file.  
  
In order to submit your revised manuscript, please access the journal web site:  
  
Your username is: AndreasL  
If you forgot your password, you can click the 'Send Login Details' link on the EM Login page at <https://www.editorialmanager.com/core/>  
  
We look forward to receiving your revised manuscript before 06 Oct 2019. Please contact the editorial office if you need additional time.  
  
With kind regards,  
Morgan S. Pratchett  
Editor in Chief  
  
  
COMMENTS TO THE AUTHOR:  
  
Dear Dr Lauchstedt,  
thank you for submitting your revised manuscript "Species distributions and two morphological traits predict the IUCN conservation status in reef corals" to Coral Reefs, and for your efforts in addressing the reviewers comments. I have now received reports from two reviewers: one of the original reviewers (see below) , and a 'new' reviewer (see attached). Both reviewers agreed that the manuscript is an improvement on the previous submission, however some of the revisions have raised additional issues. In particular, i ask that you consider the comments Reviewer #2 raises regarding the Pleistocene analysis. I agree with the reviewer that statements suggesting that threat-levels to corals were greater during the Last Interglacial than today are difficult to reconcile, and that this would be better presented as a separate manuscript. Reviewer 3 also raises several interesting points that i ask you consider in revising your manuscript.  
  
I look forward to receiving your revised manuscript.  
  
Best regards,  
Andrew Hoey  
  
Reviewer #2:   
The revised paper does a better job of describing the main result; the identification of easy-to-measure or simplified traits that predict IUCN conservation status in corals. The authors have adequately responded to my original comments, and now correctly define traits and distinguish between actual extinction risk and IUCN conservation status. The results provide a valuable demonstration of the utility of traits, and are applied to data deficient and fossil corals in interesting and useful ways.   
  
However, unfortunately the revisions have highlighted further problems that were hidden away or unclear in the original manuscript, primarily concerning the Pleistocene analysis. The revised abstract (line 34-36) states: "Applying our model to corals of the Last Interglacial (c. 130 ky ago), we find that the threat-levels were greater overall, and especially larger in the Caribbean than today."   
  
The notion that corals were more threatened 130ky ago than today seems like a confusing statement, which is difficult to reconcile with what I know of coral reefs today. While the traits may suggest that corals were more threatened, their pre-human environment and stable composition presumably would not. The authors are qualified to make historical inferences of this sort, but this counterintuitive result is not given nearly enough attention, especially in the discussion. This shows a lack of attention to detail, and high potential to be misinterpreted.   
  
In addition, which species were included in the LIG analysis in Figure 4? How similar are their occurrences to present day taxa, and what changed about them except for the two extinctions? The legend in Figure 4 says there are 3,184 occurrences of corals (curiously the same as the data-deficiency analysis), but it's not certain which species were included and where.  
  
In summary, although I think the LIG analysis is novel and exciting, it is not given proper justice in it's current form, and has led to spurious or poorly-explained results. I feel the only way to reconcile this is by removing the biogeographic analysis of LIG corals in Figure 4 and Table 2 (which should probably be a separate paper), and focussing only on the two corals that went extinct. What were their traits, what would their predicted threat status be, and what does this mean for our assessment of extinction risk? Another conceptual or quantitative figure focussing only on these two species would be helpful. If the authors insist on keeping the biogeography of the LIG in this paper, the explanation of the methods and results must be drastically improved and discussion expanded following points above, and I'm not sure there is sufficient room left to do so.   
  
Other minor suggestions:  
  
Revised line 62-66. Long sentence. Please shorten or split up.   
  
Revised lines 89-91. I don't see the value of mentioning 'supertraits' here in the methods, perhaps move to the discussion.   
  
Revised line 177-178: These two sentences at the start of the results are highly methodological. Consider moving them to the methods.   
  
Line 214-218 (also revised line 33): The data-deficiency analysis in Figure 3 and Table 1 is not just an analysis of vulnerability, but also of data-deficiency. Consequently, the majority of Coral Triangle Acropora in the vulnerable/data-deficient group may highlight the capacity of Indo-Pacific Acropora to be data-deficient, not just vulnerable. These ideas need to be backed up with more statements in the discussion (and not in the results section as on line 217).   
  
Line 279-281: I'm don't think Acropora should be classified in the same growth rate category as Dendrophyllidae and Euphylliidae. Please clarify. Also, growth rates are not the only important thing about growth forms that influence vulnerability. Please expand.  
  
Revised line 318: The last sentence on "super traits" does not read well and finishes off a relatively poor/stunted summary paragraph.   
  
Line 492-500. Figures 2 and 3 legends are poorly written. Please shorten the sentences, and include only the most important information about the figure in the first sentence.

Reviewer #3:

Overview

Overall, I think that the problem the authors are trying to tackle is an interesting and potentially impactful one. However, there are a few weaknesses in structure that make it difficult to follow in terms of the overall scope and how the various results and analyses work towards specific objectives and overall aim. Additionally, a more thorough testing of the robustness of the ANN model should be undertaken. This is a solid effort, and I’m recommending that the authors resubmit with major revisions. I look forward to reading the next version of the manuscript.

Introduction

The introduction is very short, and while I understand that introductions shouldn’t include extraneous details or be overly long, I think there is still space to set up the aims and objectives more clearly and state why this work is necessary. It would be helpful to outline in the last paragraph what the main aim of the study was, and the questions asked/aims that help to achieve it. It would be beneficial to introduce information around why the Pleistocene analysis is important, as well as why the biogeographic analyses are included.

The introduction states the drawbacks of how IUCN categories are designated (expert opinion, anecdotal reports, etc), and goes on to discuss how quantitative, trait-based approaches can be useful for the assessment of conservation status which I believe is a fair point. However, the study uses traits to predict ICUN categories rather than, for example, comparing IUCN and trait-based approaches to predict observed conservation metrics. I feel like this paragraph sets up the idea that this work will improve on previous IUCN approaches, which is slightly misleading. A better trait-IUCN link would be to discuss how species-level traits are easier to obtain than population sizes and so may help scale and speed up ICUN category designations.

Methods

It’s not clear if any cross validation on the ANN model was undertaken. The authors state that they “… compared the number of cases in which the modelled status differed from the actual status…”, but its unclear if this is referring to cross validation. For example, leaving out 30% of the data before fitting the model, and predicting the classes/probabilities from the left-out data and comparing to their actual classes. The could be done multiple times to test the generality and robustness of the model across different inputs.

A more thorough exploration of the predicted vs observed categories would also be welcome. Confusion matrices, which provide information on false positives, false negatives, and other classifier metrics such as balanced accuracy, are one way of exploring this type of problem. The function “confusionMatrix” from the caret package is useful and easy to use. Kappa values also provide a good estimate of balanced accuracy as well. I would suggest using figures to visualise the performance of the model where possible. Using predicted probabilities instead of classes might help with this.

The introduction of supertraits in the methods (ln 90) should be in the introduction and expanded on as it’s not immediately clear how the idea of supertraits fits into this work. Are the authors trying to find supertraits? Or are they applying a potential supertrait to a new problem?

It’s not clear what is added by using the AUC from the text, or what the expectation would be to determine if the model was performing well using this assessment method.

The authors mention in one of the AUC tests that the IUCN statuses were treated as binary variables (ln 164). This approach could be used to run a binary logistic regression that could act as simpler first pass attempt as the problem that the ANN model could be compared to. I would imagine the ANN would outperform the logistic regression in this case, but how much better it performs would be interesting to see and provides a good frame of reference.

Reading the text, I wondered if imputing the 5% data is necessary? 5% is not a lot of missing data, and 90-95% complete data would still leave around 620 Species to be used in the analysis without introducing any artefacts/biases and would help simplify the methods by avoiding the need to explain the imputation process. If the imputation is truly worthwhile, a better explanation or statistical test to justify it would be welcome.

Why classify growth form into a binary variable? This results in a lot of potentially useful information being lost. While I agree that 12 growth forms are perhaps too many, and that some are likely to be more redundant than others (e.g. hispidose, caespidose), there has been an extensive amount of work showing that there are differences between growth forms that go beyond a binary massive-branching split. I’m also unsure what the authors mean by reducing the effect of trait plasticity (ln 94), further elaboration on this point would be welcome. Partial categorisation into a few key growth forms that are clearly different would allow for more subtle differences to potentially be found. I also disagree that a higher number of categories would cause overfitting by including more information in the explanatory side, and this could be tested via cross validation regardless. Part of the benefit of adopting methods such as ANN is their ability to deal with subtleties in complex data. One approach would be to test different models using a different number of classifications (e.g 12, 7 and binary).

It’s not clear to me what the relevance of the paleo analysis is to the main aim of the paper, but this could be made more obvious with a more developed introduction and statement of the aims of the paper.

In the same vein, it needs to be made clearer why the authors results are compared to previously published results, again possibly as a question/aim at the end of the introduction. As currently written this comparison jumps in at the end of the methods and is surprising. Generally, everything in the methods should already make sense given the content of the introduction.

I would appreciate the opportunity to see the data and scripts used in the manuscript.

I would also encourage the authors to make the final versions of the scripts and data available online following publication, although the decision to do so or not will not have any bearing on my final recommendation to publish.

Results

For the data deficient corals that remained unable to be classified, it would be interesting to see what the distribution of the probabilities for the classes were. You state that a category was only assigned if there was more than 60% probability, but if the joint probability of EN and VU was high, say 80%, then I would argue you would be able to classify it as EN/VU, VU, or EN depending on whether you wanted to be conservative or not. Regardless of choice, that would arguably be a great step forward from data deficient, even if a single category can’t be assigned with high confidence.

The result where 76% of threatened corals are non-massive, and 74% of the non-threatened species are massive is a clear and potentially impactful result, essentially stating potentially any colony that falls into the authors’ branching category can be assigned a threatened status. Whilst further examination of this result is needed strengthen it (e.g. binomial models, cross validation), if robust it is potentially a very useful heuristic. However, if it is robust, it may somewhat undercut the need for a more complex ANN approach, depending on how it is framed.

Discussion

On line 241, the expectation by chance would only be one out of five if all classes had equal numbers. For example, if one class made up 90% of the data, and your accuracy of predicting that class was 70%, that would be worse than if you had randomly drawn classes weighted by the occurrence in the data. Balanced accuracy and kappa values tackle this issue.

The inclusion of supertraits in the implications for future work seems like a strange point to end on. It’s still not clear by this point how supertraits fit into this study.

Personally, I think one of the biggest potential achievements in the manuscript is the ability to infer IUCN categories for data deficient species. I think more attention should be given to these results throughout the manuscript and in the discussion. As is, the mix of supertraits, the paleo analysis and the biogeographical analysis dilute the paper by perhaps trying to do too much, although they may all become cohesive if they are more explicitly linked to an overarching aim.

Figures

The maps look good, but there seems to be a large emphasis on them (three figures), when the main aim of this manuscript is (in my mind) about predicting IUCN categories and not the biogeography of extinction risk. I would suggest swapping some of these figures out for a graphical representation of the ANN prediction performance.

Figure 2. It seems like only one legend is needed given that the values and colours of the top legend are the same in the bottom one.

Table 1. remove the colouring for the headings.

Overall style

The manuscript could make use of subheadings to break the methods and results into clearer blocks of text (similar to the discussion). Different heading in the results for each objective would also help.

There are a few areas that would benefit from further proofreading for style and clarity.

 ####################################################################################