

Nearly half of reef-building corals face elevated relative risk of extinction

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

To help resolve the divergence of opinion with referee 2, who did not wish to re-review the manuscript, one of the other referees was consulted and found the authors' response satisfactory, conditional on some further modifications to the text. The editor and referee 3 worked out a way to address the remaining concerns of referee 3 about the presentation of the findings in separate correspondence.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)

This paper addresses the topic of global extinction risks for tropical reef corals and provides a timely update to the 2008 paper on the same topic. Compared to the paper published 18 years ago, the current draft is much more comprehensive and expansive and appears already to have gone through a productive review process. The amount and diversity of data in this paper is staggering, and the conclusions are compelling. I do not have much to add to this contribution other than to urge publication asap, but see below.

The elephant in the room here, as with the 2008 paper, is a clear definition of what extinction risk means for tropical reef corals and just how few corals colonies of any one species have to be left to project extinction with accuracy? Failing to address this issue is to the detriment of the impact of this paper. Dietzel et al. 2021 (Nature Ecology + Evolution) stirred up a bunch of issues when their analysis describing population sizes (# colonies) suggested extinctions risks in the Pacific Ocean have been over stated. Moving ahead, it is important to reconcile these issues (Carpenter et al. 2008 + current ms vs Dietzel et al. 2021) to avoid having detractors of coral conservation (global polluters and the military-industrial complex) highlighting the weaknesses of conservation arguments. I would really like to see a set of strong statements that underscore the importance (once again – is anybody listening?) of large scale, accurate determinations of coral population sizes (# colonies, not % cover) and effective demographic approaches that that accurately enumerate the risks of extinction through things like PVA, etc. This suggestion should by any means hold up publication of this work, but it is critical to recognize what is truly needed to quantify the risks of extinction. We cannot risk having an important paper like the present submission being tarnished with “you’re wrong” (because of things like Dietzel et al. 2021: addressed in a very cursory manner here).

Referee #2

(Remarks to the Author)

Thank you for the opportunity to review this revised manuscript. While I appreciate the importance of the topic and the significant effort involved in assessing extinction risk across nearly 800 coral species, I have several concerns about the adequacy of the revisions that prevent me from recommending acceptance.

The central issue is the application of global averages across traits, thermal metrics, and climate responses for a highly diverse group of species. Reviewers 2 and 3 raised this concern substantively, and the revision does not appear to have meaningfully integrated species- or taxa-level variability in thermal tolerance, bleaching response, or adaptation potential, all of which have well-studied variability in the literature. For a species-level extinction risk assessment, this remains a significant limitation.

Related to this, I agree with the original referees that raise concerns over the sole reliance on degree heating weeks as a universal bleaching threshold across all regions and species. This warrants a more balanced qualification given the growing literature on the limitations of DHW as a predictive metric.

The treatment of coral adaptation potential in the methods does not fully reflect what the approach achieves. As noted by Reviewer 3, the methodology captures innate tolerance differences between taxa rather than intraspecific adaptive capacity, and the methods text should reflect this distinction more clearly and transparently, ideally with a conscientious sensitivity analysis to address these claims.

I also note that the choice of CMIP6 SSP5-8.5 emissions scenario is now identified as implausibly negative and removed from the suite of CMIP7 scenarios. Moderate scenarios are now favoured (references below) and I wonder if this will reduce the extinction risk and headline that 'most corals are doomed to extinction'? While the authors state they "applied two climate change scenarios (Shared Socioeconomic Pathways, SSP): SSP5-8.5, a worst-case scenario, and SSP2-4.5.." where is a sensitivity analysis / comparison of results between the two pathways? Additionally, I recommend removing SSP5-8.5 from consideration.

Taken together with the other concerns in agreement with the original reviewers, there is a question of the cumulative directional effect of several precautionary methodological choices that I expect to increase the overall extinction risk estimates possibly beyond reality; a future version of the paper would benefit from explicit discussion of how these decisions may or may not bias the conclusions towards increased extinction risk, and furthermore whether a continuation of the Carpenter et al. 2008 methods are warranted given the concerns raised about the validity of the original analysis.

Hausfather Z 2025 An assessment of current policy scenarios over the 21st century and the reduced plausibility of high-emissions pathways *Dialogues Clim. Change* (<https://doi.org/10.1177/29768659241304854>)

Hausfather Z and Peters G P 2020 Emissions—the 'business as usual' story is misleading *Nature* 577 618–20

Van Vuuren D P et al 2026 The Scenario Model Intercomparison Project for CMIP7 (ScenarioMIP-CMIP7) *Geosci. Model Dev.* 19 2627–56

Referee #3

(Remarks to the Author)

This is a long-awaited, important and timely contribution. This is the second comprehensive global Red List assessment of coral species and fundamentally new calculation of the Red List Index from 1998 through to 2024. While there have been numerous global analyses of the impact of climate change on many elements of species phenology and distribution thus far there are few systematic analyses of species extinction risk using credible risk frameworks. The IUCN Red List assessment process is the most rigorous and widely applied extinction risk framework. Corals and coral reefs have been shown to be the canary in the coal mine of temperature and acidification impacts. The authors have compiled an incredibly impressive spatial and temporal dataset of 877 individual species trends and applied structured inference to infer trends for the remaining 15 species, resulting in a revised Red List Index for all 892 coral species. The findings are stark, nearly half of all scleractinian coral species are threatened (44%) and extinction risk appears to be accelerating. This is worse than any other comprehensively assessed lineage (except for the small lineage of fern-like trees, cycads) and exceeds threat levels of amphibians (the terrestrial poster child for climate change) vertebrate group (Amphibians; 42%) and higher than the only other comprehensively assessed marine lineage (Chondrichthyans, 37.5%). The data compilation, structured analysis and application of the IUCN RL categories and criteria and the retrospective hindcasting are impressive. This is exactly the kind of rigorous, high-impact synthesis the conservation community needs, and the accelerating extinction risk signal documented here demands attention from both scientists and policymakers. Most of my critique related to the presentation and communication of the findings.

The five headline issues are:

1. Vague and under-specified vulnerability framework. "Vulnerability" is used interchangeably with "sensitivity" and "resistance" across the MS and SM, when these are formally distinct concepts — vulnerability being the combination of intrinsic sensitivity (a trait) and exposure to a threat (not a trait) offset by evolutionary adaptation. The SM reveals the terminology problems are not just stylistic but methodological — "vulnerability," "sensitivity," "susceptibility," "resilience," and "resistance" are used interchangeably and inconsistently throughout, with no formal definitions anchored to the standard hazard/risk framework ($V \sim \text{intrinsic sensitivity} \times \text{exposure to threat}$). Note that susceptibility is used as a component of exposure and is a possible source of confusion for a wider readership, though I appreciate that susceptibility might be used synonymously with my use of intrinsic sensitivity. The absence of a clear definition and calculation method for the vulnerability framework, needs to be resolved systematically. (For original definitions see, Turner BL, et al. 2003. A framework for vulnerability analysis in sustainability science. *Proceedings of the National Academy of Sciences of the United States of America* 100:8074–8079).

2. Imprecise and non-standard IUCN terminology throughout. A systematic find-and-replace of key terms, anchored explicitly to IUCN criterion letters (A2, A3, B, etc.) and the IUCN threat classification framework, is needed across both the main MS and SM.

2. Communication of what the RLI does and what it means. The manuscript implies the Red List Index (RLI) resolves or predicts extinction risk, when it retrospectively tracks it. Compounding this, the narrative conflates increasing percent-threatened with a decreasing RLI value — these move in opposite directions and the distinction needs to be didactically explained for readers, as it is a likely source of confusion and a credibility risk.

3. Inconsistent and suboptimal presentation of results. Several structural issues undermine clarity: results should lead with percentages rather than raw species counts (e.g. "Almost half, 44%, of species..."); the Sankey plot would benefit from a true time axis reflecting the unequal intervals (1998–2008 vs 2008–2024); the map should be centred on the Pacific rather than 0° longitude to avoid marginalising Africa and the Mediterranean; and Chondrichthyes area completely assessed vertebrate aquatic Class and appear to have been omitted from comparative analyses on pages 6 and 10. I cannot see any upper and lower bounds for the RLI, under different assumptions w=of what DD could mean (ranging from LC to CR)

Chondrichthyan RLI values and lower and upper bounds, where: RLlow gives the best case: every DD species is assumed Least Concern (category weight 0) and RLhigh gives the worst case: every DD species is assumed Critically Endangered (weight 4). This gives the lowest possible RLI. See values below from Dulvy NK, Pacoureau N, et al. 2024. Ecological erosion and expanding extinction risk of sharks and rays. Science 386:eadn1477.

year RLI RLlow RLhigh

1970 0.962261 0.967139 0.86372

1980 0.899042 0.912093 0.808674

2005 0.795594 0.822018 0.718599

2020 0.768774 0.798666 0.695246

4. The defensiveness of pages 292–327 undermines confidence in the work. The extended caveats and rebuttals in this section read as responses to an adversarial review and are better suited to the SM. The authors should lead with confidence — "half of corals are threatened and the rate has accelerated" is a powerful, defensible headline — and reserve methodological justifications for the supplement, following the model of established RLI papers.

5. Merge methods and results in SM. I found myself asking questions that could have been resolved if the results came after each methods section.

Line-by-line comments on the main manuscript

The following points refer to the proof PDF by printed page and manuscript line number.

On page 4, lines 32–36: 'loss' seems passive and permanent. By loss is death of coral cover meant, which I presume is reversible, with subsequent regrowth in the absence of re-bleaching etc.? I appreciate the need for a narrative, but this term seems too vague. Further, the IUCN SSC is developing a Lost species paradigm to encourage data collection and search for species that have not been seen for over a decade, e.g. see the nascent Journal of Lost Species. I suggest avoiding the term and explaining in IUCN RL language.

On page 4, lines 35–42: Again, the word 'lost' is problematic. Also, half of corals globally, does that mean half or coral colonies, species or half of the areal coverage of coral cover?

On page 4, lines 41–48: What is a 'stressor' and what is a 'threat' - stay aligned to IUCN threat classification terminology and definitions. I see there are threats here that are well-aligned with those dealt with in the SM, check and smooth alignment.

On page 4, lines 60–64: Incorrect, this implies problems are solved by the RLI; instead, the RLI TRACKS extinction risk retrospectively.

On page 4, lines 66–70: Replace with 'five'

On page 4, lines 66–70: It would be helpful to align the sections of the SM with this more explicitly worded list of improvements.

On page 4, lines 68–72: I am not sure this is needed in the main MS, red list assessments aren't expert opinion, however worded in the previous Carpenter paper. see section (d) of Collen B, et al. 2016. Clarifying misconceptions of extinction risk assessment with the IUCN Red List. Biology Letters 12:20150843.

On page 4, lines 82–84: The red list website and high-level summary papers tend to report figures to one decimal place.

Consider what message to lead with leading with species number is taxon specific, hence most high-level RL papers lead with the headline percentage. I suggest a rewrite to Almost half (44%, 340 of 774) of species.... and keep going in this manner. Thus, the focus is on percentages rather than species numbers

On page 4, lines 86–90: Replace with 'most'.

On page 4, lines 86–90: Don't forget the IUCN classification of uncertainty, i.e. observed, estimated, inferred, suspected; avoid using those terms in this manner, instead just say, "were based on population reduction (criterion A)".

On page 4, lines 87–94: Suggest simplifying to "due to projected future decline (A2, n=310), historical decline in coral cover (A2; n=23), and one (Latin binomial) based on both A2 & 3"

On page 4, lines 92–94: Beware this is not the name of the criterion and without some qualifier this may trigger those who do not take the time to understand the criteria. I suggest calling "small geographic range and continuing decline" (see Carpenter

et al. rebuttal that made this error: Hughes TP, et al. 2014. Double jeopardy and global extinction risk in corals and reef fishes. *Current Biology* 24:2946)

On page 5, lines 4–7: What does this mean in IUCN terms? Seems vague and untethered to criteria and vulnerable to critiques such as the Hughes et al. paper.

On page 5, lines 10–12: Align table sequence with legend sequence, it seems odd to work backwards in time in the table.

On page 5, lines 82–84: delete for clarity.

On page 5, lines 82–86: It might be worth being clear that there have been no recorded/assessments declaring species as Extinct.

Beware the percent threatened has indeed increased but the RLI value DECREASED, take time to didactically interpret this for the readers and beware that this is a potential source of confusion.

So, 110 species changed in 10 years from 1998-2008, then 210 species from 2008-2024, this seems like an acceleration,? It would help interpretation of rates if the sanky plot has a real time axis with greater separation from 2008-2024 rather than a categorical one. One could also present rates as was done in the Chondrichthyan paper.

On page 6, lines 7–11: It is worth reminding the reader that these are the data-sufficient species, i.e. excluding the DD

On page 6, lines 11–15: The authors are missing class Chondrichthyes according to these criteria

On page 6, lines 15–17: Change to Extinct as this is a formal IUCN category.

On page 6, lines 21–26: Seem pointless, since it has already been established that ALL species have been assessed and the different numbers in each ocean basin can be added to a figure legend.

On page 6, lines 22–26: Search the MS for 'proportion' and 'percent' and check that they are not being used incorrectly, as done here.

The result of 'consistent' should be 'consistently high'

On page 6, lines 27–34: insert percentages for comparison to previous sentence.

On page 6, lines 33–38: Please list them somewhere to ensure they are clear priorities for data collection.

On page 7, lines 6–8: It seems unfair to slice the map along 0 longitude rather than down the middle of the Atlantic; this disfavours the continent of Africa and the Mediterranean Sea - which have striking issues over presentation of mid-Atlantic islands, which by comparison have relatively uninteresting results.

On page 7, lines 13–17: Replace 'being' with 'classified as'.

On page 7, lines 22–29: What are the statistics for this family?

On page 7, lines 26–31: There needs to be a caveat here, maybe written out in the SM in the Generation Length section, that this is likely an underestimate of risk for these species because they likely have a longer generation length than was assumed uniformly across the group.

On page 7, lines 29–31: how many are CR?

On page 8, lines 13–17: I see that vulnerability and resistance are not defined here and seems to be used synonymously with 'sensitivity' - this issue needs to be clarified across the MS and SM.

On page 8, lines 53–57: This sounds like an Allee effect, and it is worth stating that.

On page 9, lines 22–29: Urgency is not an argument to avoid DD, the abundance of evidence is the reason why DD is relatively low.

On page 9, lines 22–26: I appreciate that actual and potential criticisms need to be dealt with (and I see the MS has had at least one adversarial review but that seems more like a supplementary material section than this. I suggest the authors reconsider paragraphs from 292–327 as they undermine the work rather than improve confidence in it.

These issues are caveats to pretty much all RLI papers.

On page 9, lines 27–31: This is where the team should summarise why their assessments are precautionary, rather than leaning on the misguided Edgar paper.

On page 10, lines 35–42: Three points: (1) chondrichthyans have been omitted. (2) note the dissonant 'increase' when the RLI values are DECREASING, (3) I have not seen any RATE of change indices reported, i.e. change in RLI per year - which would be helpful to support change-over-time narratives.

On page 10, lines 36–38: Focus on the headline message, "nearly half" - note that the time frame is 202, but the RLI goes to 2024 - thus halfway to 2050 timeline for most climate projections and five years away from 2030 CBD targets.

Have confidence in the narrative, I suggest "Half of corals are at risk of extinction, and extinction rate has accelerated in the past 17 years.

On page 10, lines 37–50: Flip these sentences because the current sentence comparing to another dataset comes out of nowhere, whereas the following sentence foreshadows that the authors are next going to verify / compare their results to other studies.

On page 10, lines 37–41: Give that the other studies are mainly about ecosystem structure and function, this study COMPLEMENTS rather than corroborates.

On page 10, lines 58–62: Replace with 'before' or 'without'.

On page 10, lines 80–84: I am not familiar with this "70-90% to 99%" statement, but I agree it is hard to parse what this means. It seems to be a range and an extreme value? However, the proposed replacement is even harder to parse not only is the structure the same but is reversed. I think some interstitial language is needed to help the reader follow, i.e. "Instead of focusing on the loss of coral reefs we suggest inverting the statistic to focus on the area of coral reef remaining".

On page 10, lines 84–88: Change to percent.

On page 10, lines 92–96: If possible a number would be helpful. Also has the coral reef community given up on mitigation, i.e. advocating for reduced CO2 emissions? Also, it is not that clear to me (from this paper) whether the community has done much to reduce the fishing and disease threats.

On page 11, lines 4–9: These are individual or 'i-frame' interventions to a systemic problem that requires system-wide 's-frame interventions, see "It's on you: How corporations and behavioral scientists have convinced us that we're to blame for society's deepest problems" by Chater and Loeuvenstein 2026. This is the stark rebuttal to behavioural nudge theory that has been so pervasive for the past 15 years, that has muted the worldwide application of systemic policy responses.

On page 11, lines 13–17: This is a widely misused word, it means to reduce by one-tenth - is this what is meant here? Also 'decimated to local extinction' seems contradictory and needs reworking.

On page 11, lines 34–43: This is pulling the punch on causality with the passive 'shown'; further the terms 'diversity' connotes positive responses; I presume that these responses have been very negative.

On page 11, lines 45–49: Again, this term, 'loss' is doing a lot of what -- what is meant exactly.

On page 11, lines 45–49: What is the subject of this sentence?

On page 11, lines 53–55: AND climate change

On page 11, lines 59–63: Is it worth acknowledging that civil society has made transformative efforts at local scale (detailed under the restoration section) that have not been matched by the level of systemic response achievable only by governments and businesses – see my point about "It's on you" book?

On page 11, lines 70–74: What is this term, does this equate to Red List Assessors and Reviewers?

On page 11, lines 80–84: define them as Data Deficient here.

On page 11, lines 91–98: This should be the topic sentence. The preceding sentences doesn't make sense until the Categories & Criteria are laid out.

On page 12, lines 5–7: replace with 'decline'.

On page 12, lines 15–17: This is not correct, see point 8 in the IUCN Standards and Petitions Subcommittee. 2022. Guidelines for the appropriate uses of IUCN Red List Data. Gland, Switzerland: Standards and Petitions Subcommittee, IUCN Species Survival Commission. Report no.

On page 12, lines 21–25: Does this mean that any category estimates under the B criterion were highly conservative?

On page 12, lines 25–28: Does this mean 'example' or 'excluding', spell out please. I do not understand why these would be included, this needs elaboration somewhere.

On page 12, lines 33–35: Give the numbers of the IUCN RL updates, please.

On page 12, lines 43–47: Connect to Criteria numbers, i.e. A2, A3, A4.

On page 12, lines 77–81: This would be a good point to specify how vulnerability and its components are calculated.

On page 13, lines 25–27: To head off misconceptions (like Hughes et al. Current Biology), consider adding: ..."IN COMBINATION with 2 out of three conditions: severe fragmentation, continuing decline or extreme fluctuations." In other words, merge these sentences to ensure that the subcriteria are considered.

On page 13, lines 35–39: What is 'general abundance' and how does it differ from 'abundance'; what are the units/scale?

On page 13, lines 39–43: So, a coral can be 'susceptible' to bleaching, but 'vulnerable' to disease. The terminology around the vulnerability framework needs cleaning up to ensure consistent use throughout the MS and SM.

On page 13, lines 51–55: What is the difference between a threat and a stressor, answer using the IUCN framework.

On page 13, lines 57–59: Vulnerability is NOT a trait, it is the combination of 'intrinsic sensitivity' (which is the trait) and 'exposure to a threat/stressor' (which is not a trait, but can be disease propagules, temperature, pH, COTs, etc.)

Line-by-line comments on the SM

On page 2, lines 3–10: I suggest authors write a paragraph signposting and guiding the reader to the subsequent sections, rather than detailing taxonomy which is dealt with (and thus seems like a repetition) in a section below.

On page 2, lines 19–22: This isn't just a research decision it is fundamental to the IUCN Criteria and guidelines and needs to document as such. See last few paragraphs of section (d) in this paper: Collen B, et al. 2016. Clarifying misconceptions of extinction risk assessment with the IUCN Red List. Biology Letters 12:20150843.

On page 3, line 23: I reiterate the need for an opening sign-posting comment, as a fresh reader this section comes out of the blue, I suggest foreshadowing (in the Red List criteria paragraph that the basis for assessment is 'coral cover'

On page 3, lines 23–24: Be explicit as to whether this means gross coral cover across all species-, or species-specific coral cover.

On page 3, lines 27–30: Be explicit which Red List subcriterion is in play here, I presume it is A2(c), whereas species-specific trends would be A2(b)

On page 3, lines 39–41: What is proportion (0-1) vs. percent (0-100%) change?

On page 3, lines 40–42: When scaled by 3G does this not make it a 'reduction' or are the authors reserving this term for species-specific declines over 3G, rather than gross coral cover decline over 3G? Be as explicit as possible when connecting to RL categories, criteria and guidelines.

On page 4, lines 3–4: This seems fine because the RLI is always retrospectively inferred to suspected based on best available up-to-date information. Maybe this should be made clear in the Red List paragraph. Indeed, an explicit description of how the RLI is calculated might be helpful following the IUCN RL paragraph to forestall and contextualise the issue raised in this paragraph, e.g. see the structure of the SuppMat of Dulvy et al. 2024, Science 386, eadn1477

On page 4, lines 5–7: I am not following this calculation, I think the issue is that 'species distribution' is undefined. I presume 'geographic extent' is meant here and it would have units of km^2 ? So, then the calculation is multiplication of a proportion (0-1) by a percent (0-100), and is this percent positive or negative (i.e. a decline)?

On page 4, lines 7–9: What is vulnerability? This in hazard/risk analyses is usually defined as $V \sim f(\text{intrinsic sensitivity} * \text{exposure to threat})$ offset by adaptation. Further this sounds too similar to the IUCN Vulnerable category. Check and lay out what exactly is meant here. Note, I am reading the SM before reading the main text of the MS.

On page 4, lines 30–32: Be specific here, is A3 or A4 or a mix of both

On page 4, lines 38–40: decline or population reduction?

On page 4, lines 40–43: Are the authors saying that widespread species have geographic range extents that exceed the spatial extent of typical bleaching events, thus might have spatial refuge? If I have understood correctly this seems more explicit than wrapping this into another misused term of 'resilience'. This seems sensible methodologically, focus on communicating to a wider audience.

On page 4, lines 44–45: This seems like a mix of units, please elaborate.

On page 4, lines 44–45: There is a need to unpack vulnerability earlier in the SM, is susceptible synonymous with my term

'intrinsic sensitivity' from an earlier comment?

On page 5, lines 3–5: Elaborate here to be explicit that 30 m is the known limit of bleaching and thus below this limit constitutes refuge at depth.

On page 5, lines 3–6: This sentence is missing a punchline, such as "and thus was assessed as IUCN category X". At this point laying out some contrasting examples of bleaching susceptibility and depth range > IUCN category would help the reader follow along with the methodology. Again, take this with the caveat that I am reading the SM before reading the main MS.

On page 5, lines 6–8: This seems like an 'exposure' factor?

On page 5, lines 36–38: Is 'puberty' and 'becoming reproductive' being used synonymously with maturation, I suggest not adding another term to minimise confusion.

On page 5, lines 39–41: Use timespans, because 'ranges' can be confused for geographic ranges.

On page 5, lines 41–44: It would be helpful to consider a sensitivity analysis to some plausible values for broadcast spawners versus brooding species, even a comparison of 10 for broadcasters vs 15-20 for brooders, so see if changes in life history composition of communities might lead to variation in the RLI.

On page 6, line 8: Vulnerability to what, I presume coral bleaching, again, write for the wider reader.

On page 7, lines 4–17: There is no consideration of exposure in this definition, I suggest dialing back the trait name to 'Bleaching sensitivity/susceptibility'. Choose one, define and stick with it. Consider fixing for other sources of mortality below.

On page 7, lines 28–36: Given that one disease below is given special treatment, is it worth a sentence or two somewhere in the SM to explain the diseases encompassed by this term?

On page 7, lines 95–112: Here, trade numbers are 'exposure' whereas the species traits that lead to them being preferred to aquarists would be their intrinsic sensitivity trait. Neither is synonymous with vulnerability alone.

On page 8, lines 12–14: Is resilience synonymous with vulnerability? What is resistance and how does it relate to susceptibility?

On page 8, lines 15–16: There is no context for the reader to understand why this paragraph is needed.

On page 9, lines 16–17: Stick to the IUCN guidance on classifying uncertainty, I think 'inferred' is meant here.

On page 9, lines 36–37: Define non-genuine and reasons why.

On page 10, lines 65–67: The structure of this table would not be machine-readable, suggest making it so, i.e. without merging/splitting columns.

On page 10, lines 65–66: Generally, this is a very helpful table.

On page 12, lines 5–7: I think these are genuine changes, rather than non-genuine, again it is hard to tell because of the lack of description of terms.

On page 12, lines 101–102: Genuine or non-genuine?

On page 13, lines 54–59: This is purposefully NOT a proper noun in IUCN language and terminology, correct through including abbreviations in table columns.

On page 13, lines 83–84: For what assessment year/period?

On page 14, lines 110–111: For what assessment year/period?

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)

Thanks for letting me review the revised ms again. I generally liked the first submission and here the authors have attended to my comments (and many others from the other reviewers – you know you are in trouble when the response document is almost as long as the paper!) in an effective way. There is no doubt this is a very important paper with the potential to have a very large impact on the field of coral biology; it provides a timely and much needed update to Carpenter et al. 2008.

While being cognizant that it is not my job to have the authors write the kind of paper I would like (and I would not want this to happen), in my humble opinion the style and format of this paper seems to be unusual for Nature. A leading journal like Nature needs to publish the very highest quality articles that have the biggest impact and are very newsworthy. The current ms seems to depart from the strictest standards of high quality science and advance a product that seems more akin to the numerous IPCC texts that present the challenges and status of climate change issues to a broad audience. The present draft has broad and sweeping statements that are not fully supported by data, and I worry that doubters and spoilers will find it too easy to show that scientists have got it wrong again or have overstepped what can be said and be supported with data. I think the bottom line is that this paper is the ****best that can be done**** in terms of evaluating which corals are threatened by extinction, but the reality is that the data are ****just not available to do this job the way it needs to be done****. I would really like to see a strong articulation of this reality rather than telegraphing that many of the trends described in this paper are fact rather than first order approximations.

For example:

Line 141: "every fraction of a degree....coral survival" is a figure of speech that lacks scientific meaning and erodes the quality of the important message. Is 0.0001°C really going to have an effect on coral survival?

Line 142: "... risk of irreversible coral reef decline..", this is partly of the central issue: how do we know that reef degradation is irreversible, or is this another figure of speech? And what does "reef decline mean"? It is too colloquial – reduction in % cover of coral, lower net calcification, etc. have more meaning.

Line 43: "...to secure what remains of coral biodiversity" begs the question of what "secure" means and what biodiversity does remain?

Line 146: why does diverse need the prefix of "hyper" that just seems to add hyperbole to the statement?

Line 151: "... a decline of 16% of coral on reefs" – means what? A decline in cover? Decline in diversity?

Line 153: "...impact 14-15% of corals or reefs.." – means what? A percentage of or coral colonies, percentage of species, and what makes a "reef" in order to determine the percentage affected.

Referee #3

(Remarks to the Author)

The authors have done an excellent job of addressing my slew of minor comments, notwithstanding the challenge that the page/line numbers where a challenge for them to figure out. The MS is much more clearly aligned to IUCN RL categories and criteria, and the vulnerability framework is much more clearly elaborated. I will ask the authors to check the SM through as a species is ONLY vulnerable when there is the appropriate threat occurring in concert with the trait susceptibility Vulnerability = trait susceptibility X extrinsic threat. A species is susceptible in the absence of a threat, but only becomes vulnerable when the threat occurs, but not before.

I appreciate the Editorial direction to retain the caveats section. However, as put together it is somewhat of a salad of disconnected issues that not always relate back to the central issue of how this will make assessments more or less precautionary. Please note there is an irreversible cost to not taking a precautionary approach as those errors are irreversible (i.e. unnoticed extinctions). Addressing my minor comment below might help make these paragraphs more coherent and balanced in terms of the different risks of being precautionary vs. evidentiary.

Page 4, line 165-166. This issue is more accurately explained on line 190 I suggest simply dropping this sentence.

The Red List Index does not track extinction risk between successive Red List Assessments; it is retrospectively inferred from *genuine* changes in status and the time periods do not necessarily need to line up with phases of Red List Assessment. Some changes in Red List assessment status maybe non-genuine, for the 6 reasons listed in the IUCN guidelines - thus allowing for retrospective adjustment of previous categories in light of current knowledge.

P6, line 218–220. The authors continue to refer to 'percent' values using the term 'proportion'. Please check and correct. Percents are 0–100 whereas proportions are 0–1 – they are not synonymous terms.

P8, lines 299-300. This is not a correct interpretation; one Criterion does not trump any other. This is stated clearly in both the Collen et al. paper and the recent Akcakaya et al. 2026 paper. This mindset that PVA is 'better' comes from the use of PVA in the US Endangered Species Act. Please do not propagate this fallacious interpretation of the criteria.

P9, lines 316–17. This sentence is somewhat vague, the point is that traits need to be available for all species, rather than 'sufficiently comprehensive at global scale' which implies a geographic rather than taxonomic limitation.

Line 320–321. But IUCN assessments are at the global SPECIES scale, not subpopulation scale. And by 'data' does the team mean trait data, rather than any other kind of data, or something else.

Line 326. As with any other method the RL approach has potential for bias IN BOTH DIRECTIONS, both with consequences for estimation of extinction risk and decision making. From a conservation perspective it is better to be precautionary (and downlist a species as more evidence becomes available) rather than risk unnoticed extinction due to lack of management. The former is a reversible error, whereas the later is not, hence being precautionary is safer.

Either way this does not seem like suitable topic sentence for the material in this paragraph. This is a value-based decision, whereas the other sentences are caveats relating to data availability.

Line 329. It could equally understate risk, depending on the shape of the non-linearity.

Line 329–331. Finish the thought off, what does this mean for bias in risk assessment?

Line 333. “population sizes” - These are only relevant for the C & D criteria. Surely the priority is generation length by species - yet this is not mentioned or my suggested sensitivity analysis was determined to be 'out-of-scope' which it most definitely is not.

Line 334–5. It seems pointless to aim for PVA analysis for all corals when the E criterion has only been used around 100 times (Collen et al paper). Again, comprehensive trait data esp. generation length seems to be THE priority.

Line 398. Please finish this sentence off as follows, ... due to climate change (they say little about the effects of widespread overfishing).

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)

This is a massive amount of work that has gone through detailed reviews and the authors have done due diligence in responding to suggestions and comments on earlier drafts. This is important work that needs to be published. Within the constraints of what has been done and the criteria that have been applied, this is an effective summary of the state of the “coral world”. Nonetheless, I am still disappointed that the distinction between the meaning of declines in cover of formerly abundant species and the size of the population (number of colonies) remaining has not effectively been highlighted. That the population sizes (cover) of so many species have declined is not the same as imminent demographic extirpation or extinction; while this is possible, it is not certain, particularly when so much of the coral reef range remains unexplored and hidden to science. I worry about the risks of being shown to be wrong in public media, with the plight of the GBR being a good case in point where decades of declining condition seem to be contradicted by the current LTMP report of AIMS shows positive news. This has led to to widespread media attention to “The Great Barrier Reef refuses to die on schedule”. Parallels between claims of imminent extinction and the famous quote of Mark Twain (“The reports of my death..”) would be most damaging.

Referee #3

(Remarks to the Author)

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Nature manuscript 2026-04-11601 – RESPONSES TO REVIEWERS

Almost half of reef-building corals threatened with extinction

David O. Obura, Beth Polidoro, Francoise Cabada-Blanco ...

Editor comments	Author responses
1. For publication in Nature, we will need you to attend to all the referee requests for clarity and better consideration of limitations in the approach to assessing extinction risk in the main text.	
2. unlike referee 3, we think it's important that the limitations are acknowledged in the main text.	Noted, this section is retained within the main text with additions as per other comments below. Components of text that are extensively rewritten are highlighted with a comment but without Track Changes for readability; lines 285-350.
3. We will also need you to acknowledge the potential for bias in the outcomes (towards overestimation of extinction risk) in the main text.	The IUCN Red list methodology uses the precautionary principle, where assuming equal uncertainties in application of the criteria, the result with the highest red list category is chosen as the final result. We have added an explicit acknowledgement to the main text where we also address comment 6 from Referee #2 (see end of "Uncertainty vs urgency" section, lines 329-338). We note the potential bias for overestimation due to the precautionary nature of the RL methodology, including also how our use of indirect measures can both overestimate and underestimate population decline. See also comment 57
4. We do not need you to remove the SSP5-8.5 emissions scenario, as referee 2 suggests. But please can you also provide a comparison of extinction risk under the two scenarios, again in the main text.	See response to comment 11, where we specify that while both SSP5-8.5 and SSP2-4.5 were explored in the study, the results presented are exclusively for SSP2-4.5. We had not stated this clearly.
Referees' comments:	
Referee #1 (Remarks to the Author):	
5. This paper addresses the topic of global extinction risks for tropical reef corals and provides a timely update to the 2008 paper on the same topic. Compared to the paper published 18 years ago, the current draft is much more comprehensive and expansive and appears already to have gone through a productive review process. The amount and diversity of data in this paper is staggering, and the conclusions are compelling. I do not have much to add to this contribution other than to urge publication asap, but see below.	Thanks, we acknowledge the importance of the results and study, notwithstanding the many challenges noted by the prior and this review. We attempt to address all comments while upholding the integrity and importance of the results

6. The elephant in the room here, as with the 2008 paper, is a clear definition of what extinction risk means for tropical reef corals and just how few corals colonies of any one species have to be left to project extinction with accuracy? Failing to address this issue is to the detriment of the impact of this paper. Dietzel et al. 2021 (Nature Ecology + Evolution) stirred up a bunch of issues when their analysis describing population sizes (# colonies) suggested extinctions risks in the Pacific Ocean have been over stated. Moving ahead, it is important to reconcile these issues (Carpenter et al. 2008 + current ms vs Dietzel et al. 2021) to avoid having detractors of coral conservation (global polluters and the military-industrial complex) highlighting the weaknesses of conservation arguments. I would really like to see a set of strong statements that underscore the importance (once again – is anybody listening?) of large scale, accurate determinations of coral population sizes (# colonies, not % cover) and effective demographic approaches that that accurately enumerate the risks of extinction through things like PVA, etc. This suggestion should by any means hold up publication of this work, but it is critical to recognize what is truly needed to quantify the risks of extinction. We cannot risk having an important paper like the present submission being tarnished with “you’re wrong” (because of things like Dietzel et al. 2021: addressed in a very cursory manner here).	We thank the reviewer and agree on the central points raised. We have expanded upon our rationale underpinning the view that the findings of Dietzel et al., 2021 (that corals face low extinction risk) in the supplementary material. We consider the conclusions of that study are not relevant in the contemporary context largely due to the antiquity of the data (predating the 2-4th global bleaching events) and the limited taxonomic and geographic extent of the data used in that study. These are addressed in a response from Muir et al., (2022) which the reviewer does not acknowledged, cited in our text alongside Dietzel et al. (2021, 2022) We have addressed these in both the “<i>Uncertainty vs urgency</i>” section of the main MS and by adding a section in SM “<i>Interpreting Abundance and Extinction Risk</i>”. The beginning of this section explains what the extinction risk category means and how its assessment under the RL framework should be interpreted. We have revised our text to address the critique of the assessments in 2008: (i) Criterion A is based on population reduction (rate) and not size; (ii) expanding on why high abundance and widespread range is not sufficient to assume a reduced risk of extinction, especially under thermal stress. We also now state explicitly that estimating extinction probability under Criterion E would require spatially and temporally resolved density and demographic data, and we set out what would be needed to address the paucity of such data at the scope required for global assessments, framing the closing of this gap as a priority for the field and as the route to reducing uncertainty in future assessments.
Referee #2 (Remarks to the Author):	
7. Thank you for the opportunity to review this revised manuscript. While I appreciate the importance of the topic and the significant effort involved in assessing extinction risk across nearly 800 coral species, I have several concerns about the adequacy of the revisions that prevent me from recommending acceptance.	We note the concerns of the reviewer, which we attempt to address as constraints that are unavoidable in a global study of a highly diverse and key taxonomic group. Addressing them as best as possible (within the constraints of available resources, and understanding they cannot be fully resolved) is necessary, but the ‘weight of evidence’ should not only be on attempts to address the challenge (the study), but also recognizing that many forces both drive the trends and prevent allocation of sufficient resources (finances, experts) to address these global challenges. The perfect should not be the enemy of the good.
8. The central issue is the application of global averages across traits, thermal metrics, and climate responses for a highly diverse group of species. Reviewers 2 and 3 raised this concern substantively, and the revision does not appear to have meaningfully integrated species- or taxa-level variability in thermal tolerance, bleaching response, or adaptation potential, all of which have well-studied variability in the literature. For a species-level	We fully acknowledge this limitation, but we note that while the reviewer may state “all of which have well-studied variability in the literature” this is not equivalent to being able to find fully consistent trait/data inputs for a global analysis of 892 species. For example, the coral Traitbase (https://www.coraltraits.org/) is a valuable resource of information, but this is not the same as having consistent trait data across all species for a quantitative analysis. We reviewed available evidence of species-level data at the start of the study

extinction risk assessment, this remains a significant limitation.	(2019-2021) and chose those variables that could be most consistently applied across as many species as possible (i.e. minimizing those classified as Data Deficient) and building on the prior coral species assessment. We acknowledge the first reviews we received and shared for this submission (reviewers 2 and 3 cited in this comment), and that we (as here) “integrate species- or taxa-level variability” by further explanation and caveats, as there is no capacity to revisit the analyses. See comment 10 below on coral adaptation potential.
9. Related to this, I agree with the original referees that raise concerns over the sole reliance on degree heating weeks as a universal bleaching threshold across all regions and species. This warrants a more balanced qualification given the growing literature on the limitations of DHW as a predictive metric.	We acknowledge the limitations of DHW as a predictive metric. Accordingly, we have further qualified its use (lines 470-474) and provided alternatives undergoing validation for future studies in the Methods section.
10. The treatment of coral adaptation potential in the methods does not fully reflect what the approach achieves. As noted by Reviewer 3, the methodology captures innate tolerance differences between taxa rather than intraspecific adaptive capacity, and the methods text should reflect this distinction more clearly and transparently, ideally with a conscientious sensitivity analysis to address these claims.	We acknowledge that the study does not have an independent estimate for intraspecific adaptive capacity. We did not find such a trait clearly defined and usable across all species. Our approach is more extensively described in lines 226-230 (evidence for adaptation) and 250-265 (applying two trait scores to estimate ‘none’ versus ‘some’ adaptation capacity to 1C warming. While this provides only a very broad-brush two-level estimate for adaptation, we determined that it is methodologically more defensible than attempting more nuanced approaches with insufficient data, and not considering adaptation capacity at all.
11. I also note that that the choice of CMIP6 SSP5-8.5 emissions scenario is now identified as implausibly negative and removed from the suite of CMIP7 scenarios. Moderate scenarios are now favoured (references below) and I wonder if this will reduce the extinction risk and headline that ‘most corals are doomed to extinction’? While the authors state they “applied two climate change scenarios (Shared Socioeconomic Pathways, SSP): SSP5-8.5, a worst-case scenario, and SSP2-4.5.” where is a sensitivity analysis / comparison of results between the two pathways? Additionally, I recommend removing SSP5-8.5 from consideration. Hausfather Z 2025 An assessment of current policy scenarios over the 21st century and the reduced plausibility of high-emissions pathways Dialogues Clim. Change (https://doi.org/10.1177/29768659241304854) Hausfather Z and Peters G P 2020 Emissions—the 'business as usual' story is misleading Nature 577 618–20 Van Vuuren D P et al 2026 The Scenario Model Intercomparison Project for CMIP7 (ScenarioMIP-CMIP7) Geosci. Model Dev. 19 2627–56	We explored population reduction based on several SSP scenarios, using the two most plausible and defensible ones from the SSP scenario family. What was not clearly stated in the earlier text was that following comparison of SSP2-4.5 and SSP5-8.5, we based results solely on SSP2-4.5 on the basis of both its greater plausibility as an emissions pathway (see comment 12) and its closer match to findings on coral responses. This is stated clearly in key places in the text now - lines 180, 482-484, and SM226-228. Thank you, the references provided here are incorporated into our discussion on emissions scenarios. An initial reason for considering SSP5-8.5 was that only very minor changes in SST (and differences among scenarios) are expected to be observed until 2050 or 2060, the 3 generation lengths threshold for calculating declines under IUCN Red List methodology. Major differences among scenarios only emerge after these dates.

12. Taken together with the other concerns in agreement with the original reviewers, there is a question of the cumulative directional effect of several precautionary methodological choices that I expect to increase the overall extinction risk estimates possibly beyond reality; a future version of the paper would benefit from explicit discussion of how these decisions may or may not bias the conclusions towards increased extinction risk, and furthermore whether a continuation of the Carpenter et al. 2008 methods are warranted given the concerns raised about the validity of the original analysis.	Thank you - please see response to Editor comment 3, on addressing the precautionary approach of the Red List methodology and potential for 'bias' in the consideration of multiple lines of evidence. Note that this concern applies across all application of the Red List of Species methodology and are addressed in a broad literature beyond our consideration here of coral reefs, and addressed in references such as Collen et al. 2016, Mace et al. 2008, and the continuously updated guidance on the Red List, of which we applied v14 as the most updated during the analysis of this study.
Referee #3 (Remarks to the Author):	
13. This is a long-awaited, important and timely contribution. This is the second comprehensive global Red List assessment of coral species and fundamentally new calculation of the Red List Index from 1998 through to 2024. While there have been numerous global analyses of the impact of climate change on many elements of species phenology and distribution thus far there are few systematic analyses of species extinction risk using credible risk frameworks. The IUCN Red List assessment process is the most rigorous and widely applied extinction risk framework. Corals and coral reefs have been shown to be the canary in the coal mine of temperature and acidification impacts. The authors have compiled an incredibly impressive spatial and temporal dataset of 877 individual species trends and applied structured inference to infer trends for the remaining 15 species, resulting in a revised Red List Index for all 892 coral species. The findings are stark, nearly half of all scleractinian coral species are threatened (44%) and extinction risk appears to be accelerating. This is worse than any other comprehensively assessed lineage (except for the small lineage of fern-like trees, cycads) and exceeds threat levels of amphibians (the terrestrial poster child for climate change) vertebrate group (Amphibians; 42%) and higher than the only other comprehensively assessed marine lineage (Chondrichthyans, 37.5%). The data compilation, structured analysis and application of the IUCN RL categories and criteria and the retrospective hindcasting are impressive. This is exactly the kind of rigorous, high-impact synthesis the conservation community needs, and the accelerating extinction risk signal documented here demands attention from both scientists and policymakers. Most of my critique related to the presentation and communication of the findings.	We thank the reviewer for recognizing the importance of the analysis and effort, notwithstanding the many challenges of applying the IUCN Red List criteria of an entire taxonomic marine group. The very detailed comments on clarifications/ specificity of applying IUCN Red List terminology and frameworks are appreciated, and addressed in the line by line comments below.

The five headline issues are:	
14. 1. Vague and under-specified vulnerability framework. "Vulnerability" is used interchangeably with "sensitivity" and "resistance" across the MS and SM, when these are formally distinct concepts — vulnerability being the combination of intrinsic sensitivity (a trait) and exposure to a threat (not a trait) offset by evolutionary adaptation. The SM reveals the terminology problems are not just stylistic but methodological — "vulnerability," "sensitivity," "susceptibility," "resilience," and "resistance" are used interchangeably and inconsistently throughout, with no formal definitions anchored to the standard hazard/risk framework ($V \sim \text{intrinsic sensitivity} \times \text{exposure to threat}$). Note that susceptibility is used as a component of exposure and is a possible source of confusion for a wider readership, though I appreciate that susceptibility might be used synonymously with my use of intrinsic sensitivity. The absence of a clear definition and calculation method for the vulnerability framework, needs to be resolved systematically. (For original definitions see, Turner BL, et al. 2003. A framework for vulnerability analysis in sustainability science. Proceedings of the National Academy of Sciences of the United States of America 100:8074–8079).	Thank you, we appreciate the reviewer noting this inconsistency. The manuscript and Suppl. Mat. (including Tables SM2 and SM10) have been fully edited to correct this as per the comment and the IUCN Guidelines, as follows: - we specify that vulnerability may relate to an intrinsic trait of a species (susceptibility) in relation to a threat, exposure of individuals to the threat AND a combination of both. We thus use 'susceptibility' and 'exposure' where it is relevant to each, and 'vulnerability' where it is a combination of these AND where different traits are aggregated together (e.g. 'overall species vulnerability trait', 'susceptibility to disease/bleaching', 'exposure through depth/habitat', etc.). - These edits are carried through the tables and text in both documents, shown in track changes. - The descriptions of methods in both manuscript and SM are edited to clarify these aspects. - The term 'resilience' has been eliminated in relation to this, appearing just once (line 349) in relation to citing studies on coral reef resilience.
15. 2. Imprecise and non-standard IUCN terminology throughout. A systematic find-and-replace of key terms, anchored explicitly to IUCN criterion letters (A2, A3, B, etc.) and the IUCN threat classification framework, is needed across both the main MS and SM.	We have addressed these inconsistencies across the MS, mainly: - ensuring criterion A is referred to as population reduction and making the distinction with <i>habitat/coral cover decline</i> and <i>continuing decline</i> for subcriterion B (b) and criteria C - Improving clarity when needed for quantitative thresholds and subcriteria - Improving clarity on generation length definition and population size, specifying the working definitions applied in this assessment Within the MS these have also been reviewed and modified. Please see responses to specific comments below
16. 2. Communication of what the RLI does and what it means. The manuscript implies the Red List Index (RLI) resolves or predicts extinction risk, when it retrospectively tracks it. Compounding this, the narrative conflates increasing percent-threatened with a decreasing RLI value — these move in opposite directions and the distinction needs to be didactically explained for readers, as it is a likely source of confusion and a credibility risk.	The text has been altered to state that the RLI "tracks" changes in Red list status over time. We have also been careful to be explicit about the text referring to INCREASE in extinction risk and DECREASE in the value of the RLI, and have written the latter explicitly in the caption to fig. 1B.
17. 3. Inconsistent and suboptimal presentation of results. Several structural issues undermine clarity: results should lead with percentages rather than raw species counts (e.g. "Almost half, 44%, of species..."); the Sankey plot	We acknowledge the reviewer's issues on presentation of the results, but we don't believe these are suboptimal, and represent aspects focused on coral reefs and coral species. Each aspect is covered in detailed comments below, in comments:

would benefit from a true time axis reflecting the unequal intervals (1998–2008 vs 2008–2024); the map should be centred on the Pacific rather than 0° longitude to avoid marginalising Africa and the Mediterranean; and Chondrichthyes area completely assessed vertebrate aquatic Class and appear to have been omitted from comparative analyses on pages 6 and 10. I cannot see any upper and lower bounds for the RLI, under different assumptions w=of what DD could mean (ranging from LC to CR)	- #29 - numbers vs percentages of coral species, - #39 - time axis for the Sankey and aspects of unequal time intervals - we have done this, and - #48 - map format - no change made. - #18, 41 for inclusion of Chondrichthyes results, but not adding the upper/lower RLI confidence limits. We also note that some of these decisions have arisen in response to prior review comments e.g. see comments from Reviewer 2.
18. Chondrichthyan RLI values and lower and upper bounds, where: RLlow gives the best case: every DD species is assumed Least Concern (category weight 0) and RLhigh gives the worst case: every DD species is assumed Critically Endangered (weight 4). This gives the lowest possible RLI. See values blow from Dulvy NK, Pacoureau N, et al. 2024. Ecological erosion and expanding extinction risk of sharks and rays. Science 386:eadn1477. year RLI RLlow RLhigh 1970 0.962261 0.967139 0.86372 1980 0.899042 0.912093 0.808674 2005 0.795594 0.822018 0.718599 2020 0.768774 0.798666 0.695246	Thank you! We missed seeing that Chondrichthyes are comprehensively evaluated as well. We have added the RLI value, but not the low and high values to figure 1B, to not over-clutter the graph, and to focus on the between-taxonomic group considerations, rather than the influence of the proportion of DD species within each taxonomic group. We also use English terminology for the taxonomic groups for broader access to Fig. 1 B, rather than use Latin terms such as Chondrichthyes. The Dulvy et al. reference has been added as a source, to the caption.
19. 4. The defensiveness of pages 292–327 undermines confidence in the work. The extended caveats and rebuttals in this section read as responses to an adversarial review and are better suited to the SM. The authors should lead with confidence — "half of corals are threatened and the rate has accelerated" is a powerful, defensible headline — and reserve methodological justifications for the supplement, following the model of established RLI papers.	We acknowledge this sentiment, but the extensive have been necessitated by prior reviews, and see those of Reviewer 2. The Editor has recommended retaining this section in the main text. The biodiversity and conservation community is polarized on the Red List methodology, so we attempt to bridge this gap by highlighting the strengths and importance, but also acknowledging challenges and critiques. Extensive edits have been made to this section and in relevant sections of the Supplementary Material, so these are highlighted with a comment and without Track Changes to improve readability.
20. 5. Merge methods and results in SM. I found myself asking questions that could have been resolved if the results came after each methods section.	We acknowledge that the request from the reviewer. But given primary citation of the Red List Guidelines as the primary methods, followed by the prior study (Carpenter et al.) and THEN our final adaptations, we have considered that this blending of methods and results may not help readability of the Supplementary Material, and its use in future years to inform subsequent coral assessments. Further the need to present updated methods AND/OR intermediate results varies for different components of the study, and this would make the SM uneven. Accordingly, we have elected to keep the separate methods/results structure.
Line-by-line comments on the main manuscript	
The following points refer to the proof PDF by printed page and manuscript line number.	Note to Editor - the line and page numbers used by the reviewer do not match any that we can produce in the review process, and we were unable to access the pdf files sent to the reviewers for their comment. We requested such copies but did not receive any

	response - so we have done our best to match the reviewer's comments with likely text. This is difficult in many places as the reviewer cites 3-5 lines of text while referring to a single word or phrase within those lines that should be edited. Apologies if we have erred, but we have nevertheless assured revisions are fully compatible with our study, and with our interpretation of the comment. The line numbers we cite here were done for a Googled Docs version, and we note that on transfer to Microsoft Word the line numbers differ between the CLEAN and TRACK CHANGES versions submitted. We hope the reviewers can adjust.
21. On page 4, lines 32–36: 'loss' seems passive and permanent. By loss is death of coral cover meant, which I presume is reversible, with subsequent regrowth in the absence of re-bleaching etc.? I appreciate the need for a narrative, but this term seems too vague. Further, the IUCN SSC is developing a Lost species paradigm to encourage data collection and search for species that have not been seen for over a decade, e.g. see the nascent Journal of Lost Species. I suggest avoiding the term and explaining in IUCN RL language.	The reviewer notes a challenge with corals, that this study faces due to the variety of variables relevant to corals, and varied use and citation of them in sources, that the text must cite as accurately as possible. These include abundance of coral colonies (individuals), coral cover (area) and area of coral reef ecosystems (which may be accurately defined or not by a minimum cover of coral and/or other aspects of the ecosystem). The use of 'loss' is intentional - the cited study (Wilkinson 2008) used the term "reefs destroyed" in the relevant Table (its page 18) and further qualified this as "current losses", using coral cover as the variable measured. Acknowledging the concern of the reviewer, we have edited each statement of decline to be more specific (in Track Changes, lines 150-154), but later in the text retain the generic use of 'loss' to be inclusive of the multiple forms of coral/reef decline.
22. On page 4, lines 35–42: Again, the word 'lost' is problematic. Also, half of corals globally, does that mean half of coral colonies, species or half of the areal coverage of coral cover?	Line 154-155. Both studies cited here (Eddy et al and IPBES Global Assessment) refer specifically to coral cover, so this has been specified.
23. On page 4, lines 41–48: What is a 'stressor' and what is a 'threat' - stay aligned to IUCN threat classification terminology and definitions. I see there are threats here that are well-aligned with those dealt with in the SM, check and smooth alignment.	Line 157. We have shifted this to be more aligned with IUCN terminology, replaced 'stressor' with 'threat' (and a stressor is more narrowly defined as the mechanism through which a threat acts). A second use of the word 'stressor' was also changed to 'threat'.
24. On page 4, lines 60–64: Incorrect, this implies problems are solved by the RLI; instead, the RLI TRACKS extinction risk retrospectively.	Line 166. Noted, the word 'addresses' has been changed to 'tracked'
25. On page 4, lines 66–70: Replace with 'five'	Because the page/line numbering used is not consistent with our copies, I am assuming this means replace "a range of" with "five main" to be more explicit, with 'main' being added to address the comment below (#26)
26. On page 4, lines 66–70: It would be helpful to align the sections of the SM with this more explicitly worded list of improvements.	We acknowledge the interest of the reviewer in making this change in organization of the SM, as well as the earlier request with respect to interleaving methods and results in the SM. But given the complexity of the study and the SM, we don't feel we can do this reliably at this stage. This list of key improvements is intended as a high level synthesis/prioritization (so adding the term "main" after "five", whereas there are many sub-

	components and other more minor improvements that were made.
27. On page 4, lines 68–72: I am not sure this is needed in the main MS, red list assessments aren't expert opinion, however worded in the previous Carpenter paper. see section (d) of Collen B, et al. 2016. Clarifying misconceptions of extinction risk assessment with the IUCN Red List. Biology Letters 12:20150843.	Lines 170-1. Noted - we have removed 'expert opinion' here and in a second location in the main text, just leaving the improvement as "adding quantitative analysis". Full details are specified in the Supplementary Material as this is an important specification (that the prior assessment was based on expert opinion estimates for individual regions) for those in the coral reef world familiar with the earlier assessment.
28. On page 4, lines 82–84: The red list website and high-level summary papers tend to report figures to one decimal place.	Percentages in Table 1 have been edited to one decimal place, but for clarity narrative flow in the text, percentages (except where higher precision may be necessary) are cited with no decimals.
29. Consider what message to lead with leading with species number is taxon specific, hence most high-level RL papers lead with the headline percentage. I suggest a rewrite to Almost half (44%, 340 of 774) of species.... and keep going in this manner. Thus, the focus is on percentages rather than species numbers	Lines 176-184 (and subsequent). The number of species of warm water corals is a foundational characteristic of the taxonomic group as well as of different regions and sub-regions within the global distribution (there is an almost order-of-magnitude difference in the species pool between the Atlantic and Indo-Pacific, and between regions within the Indo-Pacific). As such, coral reef researchers use these numbers extensively and they convey significant information. Percentages, while conveying the comparison effectively, contain much less information for this taxonomic group and critical ecosystem. Acknowledging the need for both metrics, to reach both the coral reef community as well as broader communities, we choose to report both, but remain with the initial structuring of reporting key numbers as well as percentages. How we use this is reviewed throughout the text to not overburden readers with the raw numbers, but maintaining information content for readers. Both numbers and percentages are shown in Table 1.
30. On page 4, lines 86–90: Replace with 'most'.	Line 179. We assumed this refers to "the vast majority". We have deleted 'vast' to remain the "the majority" as a more specific term than 'most'.
31. On page 4, lines 86–90: Don't forget the IUCN classification of uncertainty, i.e. observed, estimated, inferred, suspected; avoid using those terms in this manner, instead just say, "were based on population reduction (criterion A)".	We assumed this refers to "estimates of" which has been deleted.
32. On page 4, lines 87–94: Suggest simplifying to "due to projected future decline (A2, n=310), historical decline in coral cover (A2; n=23), and one (Latin binomial) based on both A2 &3"	Lines 176-184. Thank you, the text has been simplified as suggested, with comparable citation of criteria throughout this paragraph.
33. On page 4, lines 92–94: Beware this is not the name of the criterion and without some qualifier this may trigger those who do not take the time to understand the criteria. I suggest calling "small geographic range and continuing decline" (see Carpenter et al. rebuttal that made this error: Hughes TP, et al. 2014. Double jeopardy and global extinction risk in corals and reef fishes. Current Biology 24:2946)	Thank you, the text has been clarified, and "small geographic range and continuing decline" used.

34.	On page 5, lines 4–7: What does this mean in IUCN terms? Seems vague and untethered to criteria and vulnerable to critiques such as the Hughes et al. paper.	We are unable to determine what this refers to, but have attempted to remove any language that is not consistent with IUCN terms.
35.	On page 5, lines 10–12: Align table sequence with legend sequence, it seems odd to work backwards in time in the table.	We would like to retain the year sequence as the current year is the focus of the study, so is the first information presented in the table. The other years are recalculations to calculate the Red List Index
36.	On page 5, lines 82–84: delete for clarity.	We are unclear what is recommended to be deleted, but we have reviewed and clarified the text to attempt to address the clarity requested.
37.	On page 5, lines 82–86: It might be worth being clear that there have been no recorded/assessments declaring species as Extinct.	This has been added to the text, with relevant references. Lines 198-199.
38.	Beware the percent threatened has indeed increased but the RLI value DECREASED, take time to didactically interpret this for the readers and beware that this is a potential source of confusion.	This is specified in the caption to figure 1B, that the RLI decreases from 1 to 0 (lines 216-217). To avoid cluttering the text we have left the specification in the caption, but made sure the text is clear.
39.	So, 110 species changed in 10 years from 1998-2008, then 210 species from 2008-2024, this seems like an acceleration,? It would help interpretation of rates if the sanky plot has a real time axis with greater separation from 2008-2024 rather than a categorical one. One could also present rates as was done in the Chondrichthyan paper.	Yes, this is an acceleration, but a prior review contested our comparison of the rate increase, also documented in the RLI figure (1B), so we had removed it. On the request of this reviewer we return mention of 'acceleration' as well as specifying the rate of decline in the RLI on a per decade basis, as this matches the generation length used here, and makes the small proportions more readable. Lines 200-202. We have implemented the quantitative x axis for the Sankey (Fig 1A).
40.	On page 6, lines 7–11: It is worth reminding the reader that these are the data-sufficient species, i.e. excluding the DD	This is specified more up-front in the bold caption sentence for fig. 1, and the caption is further simplified for readability.
41.	On page 6, lines 11–15: The authors are missing class Chondrichthyes according to these criteria	Thank you, the mean line for Chondrichthyes is added, but not the high and low estimates, as adding these for all taxonomic groups would clutter the graph excessively, and obscure the comparison among taxa. See response to Comment 18.
42.	On page 6, lines 15–17: Change to Extinct as this is a formal IUCN category.	Thanks, this has been done.
43.	On page 6, lines 21–26: Seem pointless, since it has already been established that ALL species have been assessed and the different numbers in each ocean basin can be added to a figure legend.	We are unclear what is recommended to be deleted. The specific differences between Indo-Pacific and Atlantic realms are fundamental for coral species distributions and coral reef ecosystems, so are important to report clearly.
44.	On page 6, lines 22–26: Search the MS for 'proportion' and 'percent' and check that they are not being used incorrectly, as done here.	We appreciate the reviewer is concerned about use of 'proportion' as a comparison, vs 'percentage' as the quantities, however we feel our use is correct. A percentage fixes the foundation/denominator for comparison to 100, and is a specific type of proportion (as per definitions). Quantitatively we show percentages as the common denominator enables comparison with the study and to others; but because in reality we are referring to the number of species by region, globally, etc - our use of the term proportion is correct, where that proportion is quantified using percentages. We have checked all

	usages of 'proportion' and feel they are correct, and not changed them.
45. The result of 'consistent' should be 'consistently high'	The comment is not entirely clear - we elected to state the proportion is consistent across regions, and cite the percentage numerically, but not to state it is consistently high across the regions. We have not changed the text.
46. On page 6, lines 27–34: insert percentages for comparison to previous sentence.	See response to Comment 44
47. On page 6, lines 33–38: Please list them somewhere to ensure they are clear priorities for data collection.	The scattered islands are visible in fig. 2C. Listing them explicitly as data gaps for prioritizing data collection is too fine-grain for this global paper (as they are also likely to be similar to nearby well-sampled islands). Many of the data challenges that are highlighted by this and other reviewers in the main comments are higher priorities for future work, so we do not feel it is appropriate or efficient use of word count, to list the locations in the text.
48. On page 7, lines 6–8: It seems unfair to slice the map along 0 longitude rather than down the middle of the Atlantic; this disfavours the continent of Africa and the Mediterranean Sea - which have striking issues over presentation of mid-Atlantic islands, which by comparison have relatively uninteresting results.	Slicing a global map always has challenges. We certainly do not agree with cutting up an ocean, as connectivity across the Atlantic is important, as it is across the Indian or Pacific oceans. The neatest alternative for an ocean focus is to slice through Africa/South Africa where the Atlantic and Indian Oceans meet - but that cuts the Mediterranean in two, whereas in the current map the Mediterranean is largely intact. We acknowledge the reviewer's concerns about this map choice, but we don't feel another choice is a better solution. In addition, there is no overlap between species in the two ocean basins and the Atlantic fauna (85 spp) is much smaller than the IP fauna (807 spp). A prior paper focuses on the Atlantic coral RL status (Gutierrez et al 2024 - 10.1371/journal.pone.0309354), includes similar spatial analyses centered on the Atlantic.
49. On page 7, lines 13–17: Replace 'being' with 'classified as'.	Thanks, this has been done. Line 235
50. On page 7, lines 22–29: What are the statistics for this family?	The % threatened statistic was added to the sentence referring to Pocilloporidae (28 species, or 61% of 46 species are in threatened categories). Line 242.
51. On page 7, lines 26–31: There needs to be a caveat here, maybe written out in the SM in the Generation Length section, that this is likely an underestimate of risk for these species because they likely have a longer generation length than was assumed uniformly across the group.	Thank you, this is an important point, and that longer generation times for longer lived coral species would result in higher extinction risk under Criterion A3 (Lines SM 307-310).
52. On page 7, lines 29–31: how many are CR?	The number 'three' was added to the text referring to the # of CR species in Poritidae. Line 244.
53. On page 8, lines 13–17: I see that vulnerability and resistance are not defined here and seems to be used synonymously with 'sensitivity' - this issue needs to be clarified across the MS and SM.	See response to the headline comment 14.
54. On page 8, lines 53–57: This sounds like an Allee effect, and it is worth stating that.	We agree, but to minimize text and interpretation of all observations we have resisted adding such comments to individual observations, so prefer for consistency to not do this here. Allee effects are cited in line 333.

55.	On page 9, lines 22–29: Urgency is not an argument to avoid DD, the abundance of evidence is the reason why DD is relatively low.	This text has been extensively rewritten on the basis of other comments, and we have removed this justification for avoiding the DD classification. Paragraphs and sentences (such as this one) that have been significantly altered are annotated with a Comment, but presented without Track Changes, as they were otherwise very difficult to read.
56.	On page 9, lines 22–26: I appreciate that actual and potential criticisms need to be dealt with (and I see the MS has had at least one adversarial review but that seems more like a supplementary material section than this. I suggest the authors reconsider paragraphs from 292–327 as they undermine the work rather than improve confidence in it. These issues are caveats to pretty much all RLI papers.	We follow the editor's recommendation to keep these paragraphs in the main text, so that critiques received from reviews, which correspond to critiques that many readers may also have, are dealt with transparently in the main text. It is true that they relate to most applications of the RL, but the broader readership is not as versed in the RL and needs these caveats to be explicit.
57.	On page 9, lines 27–31: This is where the team should summarise why their assessments are precautionary, rather than leaning on the misguided Edgar paper.	This paragraph has been condensed and streamlined to emphasize the actions taken by the study and introduce the following paragraphs which detail the actions, without unduly leaning on particular critiques, though the Edgar paper is still cited.
58.	On page 10, lines 35–42: Three points: (1) chondrichthyans have been omitted. (2) note the dissonant 'increase' when the RLI values are DECREASING, (3) I have not seen any RATE of change indices reported, i.e. change in RLI per year - which would be helpful to support change-over-time narratives.	Chondrichthyes have been added to the graph, thank you. We note the distinction between increase in extinction risk and decline in the RLI value, but the text does not confuse these issues, and the directionality is stated explicitly. We have added the change in index value, using a per decade value for easier reading, and to correspond to the generation time used for corals of ten years.
59.	On page 10, lines 36–38: Focus on the headline message, "nearly half" - note that the time frame is 202, but the RLI goes to 2024 - thus halfway to 2050 timeline for most climate projections and five years away from 2030 CBD targets.	Given the mismatch of page/line numbers we assume comments 59-63, refer to the first paragraph in the section "Coral reefs at a threshold" ... We have fully rewritten the paragraph for clarity and accommodating the comments received here as best we can interpret. More broadly, the same challenge with line numbers is relevant to all remaining paragraphs, and given the extensive edits/clarifications, we have accepted all changes, and the responses below point to specific lines for traceability and clarity.
60.	Have confidence in the narrative, I suggest "Half of corals are at risk of extinction, and extinction rate has accelerated in the past 17 years.	See response to comment 59
61.	On page 10, lines 37–50: Flip these sentences because the current sentence comparing to another dataset comes out of nowhere, whereas the following sentence foreshadows that the authors are next going to verify / compare their results to other studies.	See response to comment 59
62.	On page 10, lines 37–41: Give that the other studies are mainly about ecosystem structure and function, this study COMPLEMENTS rather than corroborates.	See response to comment 59

63.	On page 10, lines 58–62: Replace with 'before' or 'without'.	See response to comment 59
64.	On page 10, lines 80–84: I am not familiar with this "70-90% to 99%" statement, but I agree it is hard to parse what this means. It seems to be a range and an extreme value? However, the proposed replacement is even harder to parse not only is the structure the same but is reversed. I think some interstitial language is needed to help the reader follow, i.e. "Instead of focusing on the loss of coral reefs we suggest inverting the statistic to focus on the area of coral reef remaining".	This paragraph is a challenging one and has been fully rewritten to shorten it and provide greater clarity.
65.	On page 10, lines 84–88: Change to percent.	It is not clear what should be changed to percent.
66.	On page 10, lines 92–96: If possible a number would be helpful. Also has the coral reef community given up on mitigation, i.e. advocating for reduced CO2 emissions? Also, it is not that clear to me (from this paper) whether the community has done much to reduce the fishing and disease threats.	A number for the overlap of protection and climate refuges has been inserted, and referenced (line 368-372). We don't discuss mitigation/CO2 emissions directly here, but do call for economic actions to reduce emissions in the concluding sections, addressing the causal chain of CO2 emissions (following IPBES indirect/direct driver frameworks). The paper also does not go into extensive discussion on responses to fishing, disease and other threats, as word count space is highly limited. However the reference cite (Bates et al. 2019) does address protection from other threats.
67.	On page 11, lines 4–9: These are individual or 'i-frame' interventions to a systemic problem that requires system-wide 's-frame' interventions, see "It's on you: How corporations and behavioral scientists have convinced us that we're to blame for society's deepest problems" by Chater and Loeuvenstein 2026. This is the stark rebuttal to behavioural nudge theory that has been so pervasive for the past 15 years, that has muted the worldwide application of systemic policy responses.	We agree with the reviewer sentiment, but do not feel that bringing in behavioural theory is appropriate here and focus on the characteristics of restoration actions.
68.	On page 11, lines 13–17: This is a widely misused word, it means to reduce by one-tenth - is this what is meant here? Also 'decimated to local extinction' seems contradictory and needs reworking.	The text has been edited without the word 'decimated' to say "the 4th global coral bleaching event led to the local extinction of restored corals in Florida in 2023"
69.	On page 11, lines 34–43: This is pulling the punch on causality with the passive 'shown'; further the terms 'diversity' connotes positive responses; I presume that these responses have been very negative.	Line 393. The text now reads "...shown the wide gradient and diversity of impacts to corals and reefs.." to be explicit about the wide range (diversity) of negative impacts experienced by reefs.
70.	On page 11, lines 45–49: Again, this term, 'loss' is doing a lot of what -- what is meant exactly.	Line 393-395. With greater specificity of types of loss in earlier sections (citing from the literature and the analysis of data for this paper), we have replaced this with 'decline' to be broadly applicable to individuals, area and species (see response to comment 21)
71.	On page 11, lines 45–49: What is the subject of this sentence?	The question is not very explicit. We assume it refers to the sentence "Finally, investing in ... loss of corals is inevitable". The subject is "investing in effective management". Line 396?
72.	On page 11, lines 53–55: AND climate change	Its not clear if this is a recommendation to add "climate change" somewhere in these sentences. Our intention is that 'climatic conditions have worsened' is inclusive of 'climate change'. Line 403-4.

73.	On page 11, lines 59–63: Is it worth acknowledging that civil society has made transformative efforts at local scale (detailed under the restoration section) that have not been matched by the level of systemic response achievable only by governments and businesses – see my point about “It’s on you” book?	We have considered this, but this interpretation goes beyond what many detractors and other reviewers of this work would accept (see earlier reviews and Reviewer 2 here). So we have not included this, and let it be an implication of the work and we follow it up beyond the paper, but it is not mentioned in the text.
74.	On page 11, lines 70–74: What is this term, does this equate to Red List Assessors and Reviewers?	It is not clear which term is referred to in the 5 lines of text cited! The terms “Red List Assessors and Reviewers” is meaningful to those familiar with the Red List methods, but not a priori to others. If it refers to ‘specialists’ we have changed this to a more generic ‘experts’, as used in many assessments. Line 413.
75.	On page 11, lines 80–84: define them as Data Deficient here.	Again, the comment is not specific enough to know what “them” refers to in 5 lines of text. We have simplified the sentence defining the Data Deficient category, now in line 430.
76.	On page 11, lines 91–98: This should be the topic sentence. The preceding sentences doesn’t make sense until the Categories & Criteria are laid out.	Again, the comment is not specific enough to know which sentence is referred to. We reviewed the text to ensure clarity.
77.	On page 12, lines 5–7: replace with ‘decline’.	Again, the comment is not specific enough to know which word to replace. We reviewed the text to ensure clarity.
78.	On page 12, lines 15–17: This is not correct, see point 8 in the IUCN Standards and Petitions Subcommittee. 2022. Guidelines for the appropriate uses of IUCN Red List Data. Gland, Switzerland: Standards and Petitions Subcommittee, IUCN Species Survival Commission. Report no.	Corrections were made to this sentence and the reference to the guidelines document was removed.
79.	On page 12, lines 21–25: Does this mean that any category estimates under the B criterion were highly conservative?	This statement, which we presume refers to the sentence that starts with “In this context, using coral cover as a metric of reef status ...” (now in line 256) refers to Criterion A, so we don’t infer from it anything related to Crit B, which uses population abundances.
80.	On page 12, lines 25–28: Does this mean ‘example’ or ‘excluding’, spell out please. I do not understand why these would be included, this needs elaboration somewhere.	Example - it has been corrected to “e.g.”. Corals may be found on non-reef and ‘off-reef’ (adjacent to reefs) habitats, so the species ranges used are broader than those published for “coral reefs”. Valid in lines 258 and 438.
81.	On page 12, lines 33–35: Give the numbers of the IUCN RL updates, please.	The RL update numbers have been added: 2022-2 (Atlantic species) and 2024-2 (IP species). Lines 440.
82.	On page 12, lines 43–47: Connect to Criteria numbers, i.e. A2, A3, A4.	These have been inserted, plus further edits that are consistent later in the methods section. Lines 442-445.
83.	On page 12, lines 77–81: This would be a good point to specify how vulnerability and its components are calculated.	See response to the headline comment 14.
84.	On page 13, lines 25–27: To head off misconceptions (like Hughes et al. Current Biology), consider adding: ..."IN COMBINATION with 2 out of three conditions: severe fragmentation, continuing decline or extreme fluctuations." In other words, merge these sentences to ensure that the subcriteria are considered.	It is not clear what text this comment was referring to. Edits comparable to this have been added in line 447-448 (where the causes of reduction are ongoing or may not be reversible), 492-493 (evidence of fragmentation and/or a continued decline in habitat, population, or range size) and in Table SM1.
85.	On page 13, lines 35–39: What is ‘general abundance’ and how does it differ from ‘abundance’; what are the units/scale?	This is specified in Table SM2 (“Species that are rare were inferred to have a higher vulnerability than those that are common due to demographic and

	environmental stochasticity”, on a three-point scale) as cited in the prior sentence as the source for all 11 of these variable names. To keep the text minimized, we have not made any alteration.
86. On page 13, lines 39–43: So, a coral can be 'susceptible' to bleaching, but 'vulnerable' to disease. The terminology around the vulnerability framework needs cleaning up to ensure consistent use throughout the MS and SM.	See response to the headline comment 14.
87. On page 13, lines 51–55: What is the difference between a threat and a stressor, answer using the IUCN framework.	See comment 23, changed to 'threat' for consistency with IUCN framework.
88. On page 13, lines 57–59: Vulnerability is NOT a trait, it is the combination of 'intrinsic sensitivity' (which is the trait) and 'exposure to a threat/stressor' (which is not a trait, but can be disease propagules, temperature, pH, COTs, etc.)	See response to the headline comment 14.
Line-by-line comments on the SM	Note to editor - as with the manuscript file, we cannot use the page:line numbers used by the reviewer, but have done our best to track the source of the comments.
89. On page 2, lines 3–10: I suggest authors write a paragraph signposting and guiding the reader to the subsequent sections, rather than detailing taxonomy which is dealt with (and thus seems like a repetition) in a section below.	We thank the reviewer. We have replaced the opening paragraph with an overview of the SM structure (lines 49-53) and moved the taxonomic detail (nomenclature source and family composition) to the Coral taxonomy section (lines 101-111) and Tables SM8-SM9.
90. On page 2, lines 19–22: This isn't just a research decision it is fundamental to the IUCN Criteria and guidelines and needs to document as such. See last few paragraphs of section (d) in this paper: Collen B, et al. 2016. Clarifying misconceptions of extinction risk assessment with the IUCN Red List. Biology Letters 12:20150843.	We agree and have rewritten the paragraph to state explicitly that the application of the criteria and thresholds is fixed, ensuring comparability among taxa and reassessments and that only the data and estimation methods are updated between assessments, applying the best available information and the highest category rule. We now cite Collen et al., 2016 and the IUCN Guidelines for this principle and link it to our treatment of method-driven non-genuine changes
91. On page 3, line 23: I reiterate the need for an opening sign-posting comment, as a fresh reader this section comes out of the blue, I suggest foreshadowing (in the Red List criteria paragraph that the basis for assessment is 'coral cover'	We have addressed comment 89, including an opening overview of the SM structure and sections and included a topic sentence at the start of the “Coral Cover” section to state explicitly that change in this variable constitutes the principal evidence base for estimating population reduction
92. On page 3, lines 23–24: Be explicit as to whether this means gross coral cover across all species-, or species-specific coral cover.	we have now explicitly stated that this refers to “total or gross” coral cover
93. On page 3, lines 27–30: Be explicit which Red List subcriterion is in play here, I presume it is A2(c), whereas species-specific trends would be A2(b)	For species listed under A2, both b (population trend as an index of abundance appropriate to the taxon) and c (a decline in habitat quality due to climate change) were applied, and sometimes e (effects of disease pathogens). Text was added to the SM.
94. On page 3, lines 39–41: What is proportion (0-1) vs. percent (0-100%) change?	We have rephrased this sentence for clarity: “For each subregion, coral cover change was calculated as the proportional change in percentage cover from the starting value to the current value, over three generations”. Line 188.
95. On page 3, lines 40–42: When scaled by 3G does this not make it a 'reduction' or are the	Coral loss here is used explicitly to refer to the assessment of loss in total coral cover made by

authors reserving this term for species-specific declines over 3G, rather than gross coral cover decline over 3G? Be as explicit as possible when connecting to RL categories, criteria and guidelines.	experts at that time of the first 2008 assessment (Carpenter et al. 2008). Then we: - use “Coral cover change” to refer to the quantitative change in coral cover calculated between time points - Coral cover decline when referring to coral cover applied as habitat decline or index of abundance (<i>sensu</i> Criteria A) - use “reduction” to refer to the species-specific declines for when the criteria (A2,3 and/or 4) are tested against the estimates obtained through our methodological framework (coral cover decline and ASB onset scenarios). We have made the changes where appropriate throughout the SM for consistency and clarity.
96. On page 4, lines 3–4: This seems fine because the RLI is always retrospectively inferred to suspected based on best available up-to-date information. Maybe this should be made clear in the Red List paragraph. Indeed, an explicit description of how the RLI is calculated might be helpful following the IUCN RL paragraph to forestall and contextualise the issue raised in this paragraph, e.g. see the structure of the SuppMat of Dulvy et al. 2024, Science 386,eadn1477	We have added an explicit description of the RLI and our approach to its calculation in this reassessment at the end of the “Methodology updates from Carpenter et al., 2008” section. Additionally, the RLI section is introduced in the new overview paragraph at the start of the SM. This establishes, before the recalculation is described, why we recomputed the 1998 and 2008 categories, contextualizing the issue in advance.
97. On page 4, lines 5–7: I am not following this calculation, I think the issue is that 'species distribution' is undefined. I presume 'geographic extent' is meant here and it would have units of km²? So, then the calculation is multiplication of a proportion (0-1) by a percent (0-100), and is this percent positive or negative (i.e. a decline)?	Lines 195-200. We have rewritten this to set out the calculation step by step. We now define the weight as the (dimensionless) proportion of the species' range in each subregion, state that the per-subregion coral cover values are signed percent cover change and clarify this is a range-weighted calculation of coral cover decline
98. On page 4, lines 7–9: What is vulnerability? This in hazard/risk analyses is usually defined as $V \sim f(\text{intrinsic sensitivity} * \text{exposure to threat})$ offset by adaptation. Further this sounds too similar to the IUCN Vulnerable category. Check and lay out what exactly is meant here. Note, I am reading the SM before reading the main text of the MS.	See response to the headline comment 14.
99. On page 4, lines 30–32: Be specific here, is A3 or A4 or a mix of both	It is A3 with subcriteria c and sometimes e. Text was added to the SM to the beginning of the ‘Thermal stress’ subsection. Lines 206-208.
100. On page 4, lines 38–40: decline or population reduction?	We have rephrased for clarity, specifying that the >80% projected under ASB refers to <i>coral cover</i> and that the application of the date for ASB was used to estimate <i>population decline</i>. Lines 201-213.
101. On page 4, lines 40–43: Are the authors saying that widespread species have geographic range extents that exceed the spatial extent of typical bleaching events, thus might have spatial refuge? If I have understood correctly this seems more explicit than wrapping this into another misused term of 'resilience'. This seems sensible methodologically, focus on communicating to a wider audience.	Lines 229-243. The reviewer has understood the method correctly. We have replaced the term “resilience”, which we agree is imprecise here, with an explicit statement of the mechanisms: because the onset of annual severe bleaching is asynchronous across a wide range, only the portion of the range crossing into annual severe bleaching within three generations contributes to the projected reduction, while the reminder acts as a spatial refuge that buffers this.
102. On page 4, lines 44–45: This seems like a mix of units, please elaborate.	It is not clear what the comment refers to, but we have edited all text for clarity and specificity.

103. On page 4, lines 44–45: There is a need to unpack vulnerability earlier in the SM, is susceptible synonymous with my term 'intrinsic sensitivity' from an earlier comment?	See response to the headline comment 14.
104. On page 5, lines 3–5: Elaborate here to be explicit that 30 m is the known limit of bleaching and thus below this limit constitutes refuge at depth.	Line 234 and 252-256. We have further elaborated on our 30m limit: this limit delineates the zone above which surface-driven severe bleaching typically acts, but is also the depth limit of our coral distribution range dataset (UNEP-WCMC). Nevertheless, deeper corals, including in mesophotic reefs, can experience bleaching and may have narrower thermal thresholds than shallower species, so this is an operational limit. Line 234 and 252-256.
105. On page 5, lines 3–6: This sentence is missing a punchline, such as "and thus was assessed as IUCN category X". At this point laying out some contrasting examples of bleaching susceptibility and depth range > IUCN category would help the reader follow along with the methodology. Again, take this with the caveat that I am reading the SM before reading the main MS.	We have expanded the example to carry the logic through the projected population reduction and referenced Table SM4 where worked examples are illustrated.
106. On page 5, lines 6–8: This seems like an 'exposure' factor?	This has been explicitly stated: "The application of the species' depth range as an exposure proxy". Line 251.
107. On page 5, lines 36–38: Is 'puberty' and 'becoming reproductive' being used synonymously with maturation, I suggest not adding another term to minimise confusion.	We have standardised on a single term "sexual maturity" and "become reproductive" to avoid implying distinct concepts. We have also clarified that age at maturity is distinct from generation length (the average age of reproducing parents), which together with the lack of longevity data is why a defensible generation length cannot be estimated and the 10-year value was retained. Lines 276-286.
108. On page 5, lines 39–41: Use timespans, because 'ranges' can be confused for geographic ranges.	Corrected: "these ranges" has been replaced with wording that refers explicitly to the spans of age at maturity and longevity, avoiding confusion with geographic range.
109. On page 5, lines 41–44: It would be helpful to consider a sensitivity analysis to some plausible values for broadcast spawners versus brooding species, even a comparison of 10 for broadcasters vs 15-20 for brooders, so see if changes in life history composition of communities might lead to variation in the RLI.	This is not within the scope of this study to assess reproductive mode in this way.
110. On page 6, line 8: Vulnerability to what, I presume coral bleaching, again, write for the wider reader.	See response to the headline comment 14.
111. On page 7, lines 4–17: There is no consideration of exposure in this definition, I suggest dialing back the trait name to 'Bleaching sensitivity/susceptibility'. Choose one, define and stick with it. Consider fixing for other sources of mortality below.	Without the page/line numbers and this comment refers to 13 lines its not possible to track what it refers to. We have reviewed the text to be cautious about consistency, and have defined trait names around susceptibility..
112. On page 7, lines 28–36: Given that one disease below is given special treatment, is it worth a sentence or two somewhere in the SM to explain the diseases encompassed by this term?	The explanation in Table SM2 is expanded to explain this. We have not named diseases due to the breadth of scope, but added reference to a recent review (Montilla et al. 2019)
113. On page 7, lines 95–112: Here, trade numbers are 'exposure' whereas the species traits that lead to them being preferred to aquarists	See response to the headline comment 14.

would be their intrinsic sensitivity trait. Neither is synonymous with vulnerability alone.	
114. On page 8, lines 12–14: Is resilience synonymous with vulnerability? What is resistance and how does it relate to susceptibility?	See response to the headline comment 14.
115. On page 8, lines 15–16: There is no context for the reader to understand why this paragraph is needed.	We have added an introductory sentence explaining why this paragraph is included. Namely, Bridge et al (2020) conducted an independent trait-based analysis reaching different conclusions about the relationship between traits and conservation status (IUCN categories) in corals. We clarify here how our traits approach differs and why divergent findings are expected. The purpose of the paragraph should now be clear on first reading. Lines 315-328.
116. On page 9, lines 16–17: Stick to the IUCN guidance on classifying uncertainty, I think 'inferred' is meant here.	The word “assumed” has been replaced with the appropriate IUCN data-quality qualifier “inferred”. Line 353.
117. On page 9, lines 36–37: Define non-genuine and reasons why.	We have defined non-genuine at first use, following the IUCN guidelines and we clarify that the differences from Carpenter et al 2008 are non-genuine, and that only genuine changes were included in the RLI calculation Lines. 362-367.
118. On page 10, lines 65–67: The structure of this table would not be machine-readable, suggest making it so, i.e. without merging/splitting columns.	This has been done, Table SM4.
119. On page 10, lines 65–66: Generally, this is a very helpful table.	We thank the reviewer, Table SM4.
120. On page 12, lines 5–7: I think these are genuine changes, rather than non-genuine, again it is hard to tell because of the lack of description of terms.	Again, without correct page/line numbers or citation of the text it is hard to be sure. We have specifically labelled non-genuine vs. genuine changes in table SM5 and SM6, and as explained in the methods section above “Red List Index”.
121. On page 12, lines 101–102: Genuine or non-genuine?	See response to question 120.
122. On page 13, lines 54–59: This is purposefully NOT a proper noun in IUCN language and terminology, correct through including abbreviations in table columns.	We assume this refers to the term “threatened” and abbreviation we use for it, “Thr”. We acknowledge that used in a sentence, in contrast to the IUCN proper nouns and abbreviations (e.g. Vulnerable, VU) ‘threatened’ is not capitalised. The only exception, grammatically, is at the beginning of a sentence when it must be “Threatened”, or similarly as the first word in a table cell. Similarly, the abbreviation “Thr” is used, which is not the same as proposing “THR” which would confuse it with the correct IUCN abbreviations (Tables SM5, 7 and 8).
123. On page 13, lines 83–84: For what assessment year/period?	It is not possible to deduce which lines these refer to, but we have specified 2024 for Table SM7, 8 and 9.
124. On page 14, lines 110–111: For what assessment year/period?	See response to question 123.

Title : Nearly half of reef-building corals face high relative risk of extinction

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Final revision file, 2 October 2026

Address Referee 1 comment:

This is a massive amount of work that has gone through detailed reviews and the authors have done due diligence in responding to suggestions and comments on earlier drafts. This is important work that needs to be published. Within the constraints of what has been done and the criteria that have been applied, this is an effective summary of the state of the “coral world”. Nonetheless, I am still disappointed that the distinction between the meaning of declines in cover of formerly abundant species and the size of the population (number of colonies) remaining has not effectively been highlighted. That the population sizes (cover) of so many species have declined is not the same as imminent demographic extirpation or extinction; while this is possible, it is not certain, particularly when so much of the coral reef range remains unexplored and hidden to science. I worry about the risks of being shown to be wrong in public media, with the plight of the GBR being a good case in point where decades of declining condition seem to be contradicted by the current LTMP report of AIMS shows positive news. This has led to widespread media attention to “The Great Barrier Reef refuses to die on schedule”. Parallels between claims of imminent extinction and the famous quote of Mark Twain (“The reports of my death..”) would be most damaging.

We thank the referee for their supportive assessment of the work and for pressing this point, which we agree is essential to how the paper will be received. We have revised the manuscript accordingly and explain our reasoning below.

On the distinction between declines in cover and remaining population size. We agree that population reduction and population size are distinct, and that a reduction, even a large one, is not equivalent to imminent demographic extirpation or extinction. This distinction is foundational to the Red List framework. Criterion A classifies species by the proportional reduction they have experienced or are projected to experience over three generations, not by the number of individuals remaining. A species classified as threatened under Criterion A is therefore not predicted to become extinct within that period, and neither the probability nor the timing of its extinction can be inferred directly from its category. We had set this out in the Supplementary Information (section “Coral abundance and extinction risk”). We recognise, however, that it was not sufficiently visible in the main text, and we now make it explicit in the abstract, Introduction, Results headings and the opening of the Discussion

On under-surveyed ranges. We agree that much of the reef range lacks species-level data, and we now state this in the abstract as a reason why classifications may be revised. We note that the assessment already accounts for unexposed parts of species' ranges in two ways. Projected population reduction excludes grid cells within each species' range that are not projected to experience annual severe bleaching within three generations, treating these as spatial refuges. Species whose depth range extends below 30 m have projected declines reduced accordingly

	(Supplementary Information, "Thermal stress" and "Species traits"). Classifications are also explicitly provisional: the precautionary principle selects the higher category where evidence is uncertain, and improved data will allow downlisting in future assessments if warranted, as the Discussion now states. On the Great Barrier Reef and public communication. We share the referee's concern about the risk of results being misread as forecasts. The most recent AIMS LTMP summary (2025–26) reports encouraging increases in coral cover in two of three regions. It also attributes this partly to a late-summer monsoon that eased heat stress, and notes that more frequent and cumulative disturbances have driven large fluctuations in coral cover over the past 15 years. Such fluctuations in aggregate cover at regional scale are compatible with the range-wide, species-level reductions assessed over three generations under Criterion A. Nonetheless, the referee's broader point is well taken: the paper must not imply that a Red List category is a prediction of extinction. Our revisions, including the new title, are intended to ensure that readers and the media encounter our results as standardised estimates of relative extinction risk, with their basis and limitations stated alongside them.
1. Address Editor recommendation: Just to clarify, we ran the revision by referees 1 and 3. Only referee 1 had further comments for the authors, which are copied below. Based on this additional input from referee 1, we will need you to modify the text to make the distinction that referee 1 is making crystal clear. Because of the strong concern this referee has about the extent to which the threat of extinction can be inferred, it might be wise to consider a title along the lines of 'Forty four percent of reef corals are now categorized as threatened under IUCN criteria'. See rest below by line number	We have revised the title to match the caveats emphasized by reviewer 1, but find it is too long and cumbersome in its full format. We have indicated our preference to NOT include the square-bracketed text at the end of the proposed title: Nearly half of reef-building corals face high relative risk of extinction [under IUCN criteria]
2 - Abstract L159-161: Please add a sentence here saying that declines in coral cover are not necessarily indicative of demographic extirpation or extinction risk because	[OUR LINE NUMBERS REFER TO OUR MANSUCRIPT WITH TRACK CHNAGES VISIBLE]

much of the coral reef range remains unexplored, or something to that effect.	We have added the requested caveat, merged with the final sentence on future data needs to limit length. It clarifies that Red List categories express relative extinction risk rather than a forecast of extinction, and that classifications may be revised as species-level population data become available, including from under-surveyed parts of species' ranges in lines 155-157. The earlier sentence in the abstract already states that population reductions were inferred from coral cover proxies because species-level trends are unavailable for most species.
3- Main L176-175: (please add something here saying that this is based on changes in coral cover, and so is an imperfect measure of species-specific coral decline and so extinction risk)	We have clarified that the 2008 classifications were based on coral cover as a proxy of decline, in lines 166-167. We emphasize the point made in the Methods and Supplementary Material that Red List categories are standardised estimates of relative extinction risk, comparable among species and assessed using varied data sources under the same criteria. Criterion A explicitly provides for population reduction to be inferred from habitat proxies where direct data are lacking, and this criterion has been used with proxies for many other taxa (e.g., mycorrhizal fungi). All methods for estimating extinction risk are approximations short of a full population census, including population viability analyses based on demographic data. The present reassessment applies the same framework, with quantitative improvements, so the two time points remain directly comparable.
4 - Main L185: Improve this sentence for clarity	We have improved the wording of two sentence sto be more specific about the IUCN RL method, that we reassessed a prior study, and the focus is on relative extinction risk. Now in Lines 174-177
5- Main L-201-202: (please be more explicit about the limitations here, and include the point about considering just one measure of thermal stress, expanded on in methods)	We now state the main limitations explicitly in the Introduction: reliance on coral cover as a habitat proxy, and the use of a single thermal-stress metric (Degree Heating Weeks, DHW). Both are expanded on in the Methods, which explain why DHW was used and note that other emerging metrics should be integrated in future analyses as they are validated. Now in lines 187-189.

6 L217-219: (Please amend to better reflect limitations in method, e.g. Rise in relative extinction risk/or something that makes clear this is relative extinction risk, extinction risk according to IUCN criteria)	We have revised headings to specify relative extinction risk, consistent with the Red List framework. Line 217 - “Corals face increasing relative extinction risk”
7 L220: (this needs softening, given the limitations in the approach)	"Genuine change" is an IUCN Red List term distinguishing category changes that reflect real changes in status within the Red List framework from those resulting from new information or revised methods; it underpins the Red List Index reported in this section. We have explained this better by more explicitly stating updated methods and data were applied to prior estimates, and citing “genuine change” as a Red List term. Further detail is contained in the methods and supplementary material. We have also replaced "assess" with "estimate" to better reflect the inferential nature of the approach and “status” for “category” to reflect the change is within the RL framework and not the “real” probability of extinction. Now in lines 218-220
8 L240: Patterns of extinction risk (same here)	We have revised headings to specify relative extinction risk, consistent with the Red List framework. Line 240 - “Patterns of relative extinction risk”
9 Line 383: Coral reefs at a threshold: Please soften given uncertainties	We have revised headings to specify relative extinction risk, consistent with the Red List framework. Line 350 - “Are coral reefs at a threshold”
10 L384-386: Really important to add another caveat here, specifying that this is based on data on aggregate coral cover decline, and not species-specific population data.	We have added the requested caveat to the opening sentence of the Discussion, specifying that these classifications express relative extinction risk inferred from proxies. The text added is “... relative risk of extinction under the RL framework, for most species, inferred from aggregate coral cover trends and projected thermal stress, rather than species-specific population estimates”. The rest of the paragraph was tightened to remain within the word limit. Now in lines 352-353

11 L408-411: Not sure about this sentence, could be interpreted as overstating the case given uncertainties, suggest removing. “At 1.5°C warming, with reduction to half of coral area and half of coral species threatened with extinction, are coral reefs committed to “70-90% loss” (at the start of the IPCC critical range) or “99% loss” (at the end of the GTPR critical range), and does a point along this gradient correspond to a “global tipping” threshold or “irreversible loss”?”	We have removed this sentence as suggested. The paragraph now closes on the uncertainty surrounding future committed loss, which better reflects the limits of current understanding.
12 L412: The start to this paragraph needs reworking to make clearer.	We have reworked the opening of this paragraph to state directly what the IPCC projections imply for remaining reef area (10–30% at 1.5°C; <1% at 2°C), and how current protected-area coverage compares. Following the removal of the preceding rhetorical question, the paragraph now introduces these reference points itself. Line 374-375
Deletion of the phrase “and the importance of each tenth of a degree increase”	We would like to keep this quantitative inference. The reviewer specified that the phrase “every fraction counts” as being unspecific, which is correct. It is used widely in public discourse on climate change and needs evidence and specificity to make it more concrete. This paragraph emphasises that the four global bleaching events, from 1998 up to 2024, span a warming range of approximately 1°C.. We don’t directly infer that each 0.25C of warming contributes to a global bleaching event, but we do hold that this attests to the importance of warming on the order of 1/10 of a degree for coral and coral reef responses. We believe this phrase is amply justified by the data presented, and applies a correction to the vague “every fraction counts” tht is demonstrably relevant for corals. Lines 412-425. We delete the final phrase on a tenth of a degree of warming in line 435.
13 L451: (think you need to specify of what)	“Biodiversity” has been inserted, and the end of the sentence altered to reflect this, citing the usage coined in the IPBES Global Assessment (2019) and discourse on the Global Biodiversity Framework. Line 426

14 L452: data on coral cover, which as it stands is	Thank you, done. Line 427
15 L453: to assess (e.g.) the relative extinction risk of corals (again, please state limitations),	Done, we include that coral cover is the best available data currently, and that this is for estimating relative extinction risk. We don't repeat further limitations explained earlier in the manuscript. Lines 427-428
16 L685-689: It would be good to mention this caveat in the main text too.	This text has been simplified and placed in the main text in the discussion on limitations of data. We have excluded some of the references used in the Methods section, but inserting this text requires some references, and pushes the total references in the Main Text up to 64. Lines 325-328. The corresponding methods text is now lines 681-685.
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