

Widespread Ecological Novelty Across the Terrestrial Biosphere

Corresponding Author: Dr Matthew Kerr

Version 0:

Decision Letter:

18th October 2024

*Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your co-authors.

Dear Dr Kerr,

Your manuscript entitled "Widespread Ecological Novelty Across the Terrestrial Biosphere" has now been seen by three reviewers, whose comments are attached. As you will see from the reports, they find the work of interest but also raise a number of concerns that will need to be addressed. Therefore, we would like to see a revised manuscript and a responses to all the reviewers' comments to take a decision on publication.

Please note that Reviewer #3's comments have not reduced our editorial interest in the work, but we expect that additional analyses will be needed to address some of the criticisms of Reviewer #1 and Reviewer #2, and we concur with the latter on the need to ensure reproducibility of the work.

We are committed to providing a fair and constructive peer-review process. Do not hesitate to contact us if there are specific requests from the reviewers that you believe are technically impossible or unlikely to yield a meaningful outcome.

When revising your manuscript:

* Highlight all changes in the manuscript text file and provide it in Microsoft Word format.

* Include a "Response to reviewers" document detailing, point-by-point, how you addressed each reviewer comment. If no action was taken to address a point, you must provide a compelling argument. This response will be sent back to the reviewers along with the revised manuscript.

* If you have not done so already please begin to revise your manuscript so that it conforms to our Article format instructions at <http://www.nature.com/natecolevol/info/final-submission>. Refer also to any guidelines provided in this letter.

* Include a revised version of any required reporting checklist. It will be available to referees (and, potentially, statisticians) to aid in their evaluation if the manuscript goes back for peer review. A revised checklist is essential for re-review of the paper.

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Note: This URL links to your confidential home page and associated information about manuscripts you may have submitted, or that you are reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage.

We hope to receive your revised manuscript within 8 weeks. If you expect the process to take much longer, please let us know. We will be happy to consider your revision so long as nothing similar has been accepted for publication at Nature Ecology & Evolution or published elsewhere.

Nature Ecology & Evolution is committed to improving transparency in authorship. As part of our efforts in this direction, we are now requesting that all authors identified as 'corresponding author' on published papers create and link their Open Researcher and Contributor Identifier (ORCID) with their account on the Manuscript Tracking System (MTS), prior to

acceptance. ORCID helps the scientific community achieve unambiguous attribution of all scholarly contributions. You can create and link your ORCID from the home page of the MTS by clicking on 'Modify my Springer Nature account'. For more information please visit <http://www.springernature.com/orcid>.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

[redacted]

Reviewer expertise:

Reviewer #1: novel ecosystems, biodiversity

Reviewer #2: novel ecosystems, invasion biology, macroecology

Reviewer #3: spatial ecology, paleoecology

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

Thank you for inviting me to review "Widespread ecological novelty across the terrestrial biosphere". In this manuscript, the authors construct global maps of ecological novelty based on three broad processes leading to novel conditions (climate change, defaunation, floristic disruption), arguing that this understanding is required prior to work that considers the ecological consequences of novel conditions. Interestingly, they find that protected areas and key biodiversity areas are as exposed to novelty as areas lacking any protection. They correctly point out that these changes imply many current policy objectives are unattainable, and provide a valuable discussion around this aspect in the main text.

This was, on the whole, an easy paper to read and it tackles an important subject with multiple consequences across disciplines. It will, therefore, be of interest to the readership of NEE. I have quite a few comments, in order of the manuscript, not necessarily in order of importance, that I believe could be considered by the authors to clarify their message and justify some of their methodological choices.

(a) One of the key facets of the definition of novel ecosystems (e.g. line 30) (at least according to e.g. Hobbs et al.) is that they have gone beyond thresholds precluding any return to historical conditions. Given multiple rapid environmental changes that do not look like they are returning to any historical state any time soon, this is likely a moot point, but the authors may want to reflect how their approach considers these thresholds.

(b) Line 48 - also subsequent abandonment of that land

(c) Could line 53 be unpacked a little more - how does knowing what drives novel conditions help set conservation and restoration goals? Do different drivers lead to different goals, and if so how?

(d) Line 58 - I am uncertain what is meant by an 'ecosystem trajectory destabilising' - given that immediately before (line 56) the authors suggest that novel conditions might actually preserve ecosystem functioning, which sounds like it will be more stable, not less?

(e) Line 61 - could the reasons why the drivers and/or consequences of novelty vary greatly between systems also be unpacked more - what is the underlying rationale? Presumably in some areas climate is changing less than in others; invasive species coming into systems varies etc. Perhaps it would be helpful to make this explicit - and whether there are any interactions amongst the different drivers. It would be great if a conceptual framework could be put forward as to what these relationships could be - and then how what is mapped subsequently fits on that framework.

(f) Line 65 - I would explain precisely what is meant by 'locally novel' conditions given this is the key focus of the manuscript. What spatial scale is being considered. And how this considers (or not) thresholds.

(g) Line 77-78 - By looking at mean temperature and precipitation separately, does your measure of novelty potentially underestimate the degree of change? If there are specific relationships between temperature and precipitation that have become disrupted - perhaps more recently than when any one response is outside the modern range?

(h) It is a philosophical point as to whether humans are a part of, or apart from nature (line 81). The authors clearly consider 'natural' to be without human impact. Is this in the frame of industrialisation, or even before that? E.g. megafaunal extinction from earlier societies? It might be worth explaining the frame of reference explicitly here.

- (i) Line 84 - In a similar vein, I would explicitly state what is being considered by 'intactness'.
- (j) Line 99: In considering an 'undisturbed habitat' how are abundance shifts following, say, volcanic disturbances considered i.e. disturbance that is not human-caused but represents a natural state. And, following on line 100, can the change in intactness be anything other than negative?
- (k) Line 107 to 112 - How do you account for the two metrics per driver when working out these >0.33 and >0.66 . As I understand it, the >0.33 would be if 1 out of 3 drivers was maximum, but how are the two metrics within it combined? Or is it just implicit, and that both metrics, having also been scaled between 0 and 1, are both at 1 for a given driver?
- (l) Line 123 is somewhat of a repeat of the previous sentence given climate change and defaunation come out as close to being the same (high) novelty.
- (m) Line 134-135: I found this confusing - how can Hedges G be < -0.71 (line 135) and then $= -0.45$ later in the same line? Please clarify. (Is that then compared to polar? This seems clearer with the comparisons on line 137 - 138).
- (n) It would be helpful (line 144) to provide more information earlier in the paper as to what is actually meant by 'key biodiversity area' given concepts such as biodiversity hotspots and so forth.
- (o) Line 146 - Is it a one-off comparison, or do you choose multiple equal numbers of cells (e.g. through bootstrapping) to identify whether or not the novelty in a network of protected areas and/or key biodiversity area is different to unprotected? I presume there are no climate regions where there are more protected cells than unprotected cells? Did you mask out urban, agricultural comparisons here (given e.g. the earlier statement on line 71 about not being interested in agriculture, forestry and urban areas).
- (p) Line 165 on - Higher globally compared to what? And where do key biodiversity areas fit in this comparison - or are key biodiversity areas and protected areas somehow combined in these comparisons?
- (q) Line 185 - Are the drivers really independent - might you expect intactness to relate to climate change (or other drivers such as nitrogen pollution) for instance as abundances change in response to these pressures?
- (r) Line 194 - Is the underestimation directly linked to the earlier paragraph that there are other axes that have not been considered in your analysis? Otherwise, please can you provide further justification of your underestimation statement and/or clarification.
- (s) Line 206 - And functional consequences? Not just diversity consequences?
- (t) Line 212 - What baseline? Who decides what is a relevant baseline?
- (u) Line 243 - I think, in the original conception of biodiversity hotspots, they already were widely threatened by novelty-inducing processes.
- (v) Line 244 - Where is the justification for this statement. Could it be that systems are just different e.g. see Hobbs and 'degraded or different'.
<https://onlinelibrary.wiley.com/doi/10.1111/rec.12336>
- (w) Line 269 - It is unclear to me how clearly the figure shows this highest novelty in tropical, arid and temperate regions.
- (x) Line 281 - It needs to be explained earlier in the manuscript what constitutes the higher protection status. Also line 322.
- (y) Extended data 4 - please clarify why 'climate', 'community change', 'defaunation' used on axes in this figure whereas elsewhere it is 'climate change', 'defaunation' and 'disruption' (as per the phrasing in the main ms). Note that the global triangle has 'climate', 'community change' and 'disruption' - Is this a mistake in labelling as community change and disruption are presumably synonymous given the other labels?
- (z) Methods (line 448 on). The first sentence (line 450 on) needs rephrasing - it is very confusing.
- (aa) Line 463 - how did you consider correlations between temperature and precipitation (see earlier remark).
- (bb) Line 472 - Is this phrasing the correct way around - if ice was earlier in time than the novelty condition, then presumably the novelty condition was closer to the present day and should remain within the analysis?
- (cc) Line 475 - I am unfamiliar with these databases - how robust are their predictions of modern ranges in the absence of human pressures - and how far back does this absence of human pressure go?
- (dd) Line 491 - I found this an interesting decision given introductions are a key part of novelty? I.e. is it justifiable to consider only defaunation as leading to novelty? Or is this another reason why your results are conservative (see comment (r)).
- (ee) Line 500 - how did you account for Botanic Gardens? I was involved in some work where it became apparent that some

species occurrences were related to this - and not non-native introductions per se.

(ff) Line 527 - how does this method account for e.g. National Parks in the UK - which are considered protected by some, but are likely not in the frame of this work?

(gg) Line 540 - In the main text I would make the decision explicit and also outline what constitutes a 'small' (or larger) effect size in relation to Hedges G.

Reviewer #1 (Remarks on code availability):

I am not expert at coding and did not have time to review it in detail.

Reviewer #2 (Remarks to the Author):

The authors map novelty in the terrestrial biosphere and at a global scale based on novel climatic conditions (abiotic component), defaunation and floristic disruption (abiotic component) representing three dimensions of novelty. Further analyses comprise a closer look at novelty in protected areas and key biodiversity areas. The topic of the manuscript is a valuable contribution to current debates about novel ecosystems and ongoing discussions in restoration ecology and biogeography about biodiversity and conservation baselines. While the concept of the paper is mostly convincing and the manuscript is well-structured and written I would like the following issues to be addressed by the authors, mostly concerning the methodology and terminology.

The terms “drivers of novelty”, “indicator of novelty” and “dimensions of novelty” are used in the manuscript, which seems confusing. The authors should be more explicit if they consider climatic novelty, defaunation and floristic disruption to be drivers of novelty or rather indicators/dimensions? I would opt for these metrics to be indicators/dimensions of novelty as the underlying drivers are rather land use change, human activity etc. Please consistently use one of the terms and explain how you perceive the link between novelty on the one hand and climatic novelty, defaunation and floristic disruption on the other hand. This requires changes throughout the manuscript.

The summary paragraph and the introductory paragraph of the main text mention global transformation of the Earth by human activities and human land use. These sentences are not backed up by literature. Here are two inspirational papers: Gordon, J.D., Fagan, B., Milner, N. et al. Floristic diversity and its relationships with human land use varied regionally during the Holocene. *Nat Ecol Evol* 8, 1459–1471 (2024). <https://doi.org/10.1038/s41559-024-02457-x>
Venter, O., Sanderson, E., Magrach, A. et al. Global terrestrial Human Footprint maps for 1993 and 2009. *Sci Data* 3, 160067 (2016). <https://doi.org/10.1038/sdata.2016.67>

You mention that novel ecosystems have become widespread in natural and semi-natural landscapes. It seems, however, contradictory to the reader how a landscape can still be natural while novel ecosystems are widespread? Can this contradiction be explained to the reader in the main text?

The third paragraph of the main text mentions the research gap concerning novelty at local and regional scales. The fourth paragraph explicitly mentions that the aim of the manuscript is to quantify novelty and the underlying drivers locally. But can the manuscript actually achieve this? The analysis is rather a global one and your data has a resolution of 10 km x 10 km. Therefore, I would delete the word “local” from the manuscript and rather state that your analysis is global.

In the main text protected areas and key biodiversity areas are not mentioned until the presentation of the results. As these are an essential part of the manuscript these need to be mentioned early in the main text, for example in paragraph four of the main text where the aims of the manuscript are clarified.

What does the focus on only wild and semi-wild systems, as stated in paragraph four, imply? All maps and subsequent analysis are global. Not even urban and agricultural areas are excluded from your analysis. If this statement only leads to the zoom-ins in figure 1 please rather delete it as it evokes different expectation.

You measure climatic novelty based on mean annual temperature and annual precipitation sum. Firstly, in the fourth paragraph the precise term “annual precipitation sum” is missing and only “precipitation” is mentioned. Secondly, why did you decide to use these metrics as, for example, annual precipitation sum can stay the same with ongoing climate change while temporal precipitation patterns can change in a region?

Did you consider the interrelatedness of climatic novelty, defaunation and floristic disruption? Floristic disruption can, for example, be caused partly by changes in climate.

Please consistently use either the term “alien species” or “nonnative species” (also written “non-native” in the manuscript) throughout the entire text.

The terms “wild and semi-wild” as well as “natural and semi-natural” are used in the text. Are they refereeing to the same? If yes, please consistently use only one term.

At the end of the introductory main text you present your novelty scale that ranges between 0-1 and how you divided it into

three categories. Firstly, I was wondering why you decided on three categories and secondly, it is confusing why this scale is developed but in the proceeding paragraph you start presenting your results using a novelty score of 0.2. I would advise to stick to using your three categories when presenting your results.

It seems obvious to expect differences of climatic novelty when dividing your result by climatic region. Have you considered using another categories for your subanalysis such as biome classifications that are not mainly based on climate (see Fischer et al., 2022 for inspiration: <https://onlinelibrary.wiley.com/doi/full/10.1111/geb.13574>)?

You state that your results concerning floristic distribution might depend on data availability for nonnative species. If that is highly likely, why did you include regions of low data availability in your analysis and not blank out those regions? The global novelty maps may be misleading because this explanation is not given in the figure or its subheader. To make your global analysis and maps reflect true patterns and not data availability I would advise to reduce it to areas with sufficient data availability for all three metrics. There are packages available to evaluate sampling bias in biodiversity collections, e.g., the R package *sampbias* (<https://cran.r-project.org/web/packages/sampbias/sampbias.pdf>).

When presenting your results you write: "When directly compared, protected areas are equally exposed to novelty as unprotected areas at the global scale ($G = 0.03$, Figure 3), confirming our hypothesis." I could not find that hypothesis in the main text.

Your analysis is based on raster data. Therefore, I was wondering how you converted the data on protected areas (provided as vectors) into raster data. Did you consider a pixel to be a protected area only when it was covered 100% by an according polygon declared as a protected area? Did you consider using different thresholds of coverage (e.g., 100%, 75%, 50%)? This might change your results concerning novelty in protected areas. Furthermore, did you exclude protected areas that have the status "proposed" and which are not actually implemented? This information is not given in your methods description.

You state that your results likely underestimate local novelty. Can you elaborate why you come to this conclusion?

In figure 2a the legend is missing, colours are not explained. Only at a second view it becomes clear that the climatic regions are named in b. Significant differences should be indicated in the figures, with for example letters above the boxplots. Additionally, the n (sample size) for each category would be helpful to assess the figure. If you decide not to present p -values in your figures an extra figure summarising the effect sizes might be a valuable addition as those numbers are hidden in the SI and in the text but are not visually represented.

Why not stick to the exact same colours in figure 2b as in figure 1? The „climate novelty“ colour looks different.

It might be a valuable addition to represent global values in figures 2 a and b as you did in figure 3.

In the methods description please use a new paragraph for describing floristic disruption. Is it now combined with the defaunation metric in one paragraph which makes it hard to find.

For the calculation of the Biodiversity Intactness Index you mention the use of abundance data. Do you really use abundance data and if yes, which data source did you use?

Please check the reproducibility of your code, as the source of the reference raster you used (HYDE data ?) is not clear and therefore getting the data and reproducing your results is not possible. For reproducibility it would help to indicate data sources also in the R Code.

Reviewer #2 (Remarks on code availability):

I could not easily reproduce the code.

The source of the reference raster used (HYDE data ?) is not clear and therefore getting the data and reproducing your results is not possible. For reproducibility it would help to indicate data sources also in the R Code.

Why is the raster package still being used in the code as it is not supported anymore and not mentioned to be relevant for the data analysis in the methods section?

Reviewer #3 (Remarks to the Author):

I have now read "Widespread Ecological Novelty Across the Terrestrial Biosphere" and carefully considered the full context and implications of the paper. The authors consider three components of ecological novelty globally: climate change, defaunation, and invasive species. They assess how novel modern ecological systems (1980-2019) are relative to those from the past 300 years. The paper demonstrates widespread novelty, with some variance according to biome, protection, or key biodiversity areas. Overall, I find that this paper is interesting, but it does not do enough to demonstrate or evaluate the implications of these differing types of novelty for ecosystem function or conservation. Without substantial rewriting and sharpening of the overall messaging, I think this would be a fantastic contribution to a more subject-specific journal.

Version 1:

Decision Letter:

7th January 2025

Dear Dr. Kerr,

Thank you for submitting your revised manuscript entitled "Widespread Ecological Novelty Across the Terrestrial Biosphere" (NATECOLEVOL-24092568A). On the basis of the reviewers' feedback, I am pleased to inform you that we will be happy in principle to publish it in Nature Ecology & Evolution, pending minor revisions to satisfy the reviewers' final requests and to comply with our editorial and formatting guidelines.

We are now performing detailed checks on your paper to prepare a checklist detailing our editorial and formatting requirements. In order to complete these checks, we require a copy of the manuscript file in an editable format (Microsoft Word or LaTeX), as we can not proceed with the PDF version at this stage. Could you provide the Word (or LaTeX) version of the paper by email at your earliest convenience?

Once we receive this document, we will send you the checklist in about a week. Please do not upload the final materials or make any revisions until you receive this additional information from us.

Thank you again for your interest in Nature Ecology & Evolution. Please do not hesitate to contact me if you have any questions.

[redacted]

Reviewer #1 (Remarks to the Author):

SUMMARY

I enjoyed reading this revised manuscript, and believe the authors have addressed many of the comments raised by previous reviews. This paper remains an important step in understanding exposure to novel conditions across the globe - and will encourage more questions to be addressed. Indeed, in some ways, it raises more questions than answers (e.g. what would happen if interactions were considered, what are the wider biodiversity and functional consequences), but I believe that raising of more questions to be a strength.

I have a few remaining comments that will hopefully further clarify the authors' approach; in particular, I think some further justification of looking at both protected areas and KBAs is worth bringing out - and there also needs to be some clarification around the comparisons as there are discrepancies between what is put in the main text and what is shown in figures (see detailed comments below). Getting a colleague, unfamiliar with the subject area, to read through any revised submission may also be useful to make sure the messages are clear and supported throughout.

DETAILED COMMENTS (in order of the manuscript, not of importance)

Line 41-42: Can something be pre-Homo sapiens if stated as anthropogenic novel states? Doesn't anthropogenic imply Homo sapiens, or can it comprise other members of the genus?

Line 93 - "...additionally due to their impacts on biodiversity patterns at large spatial scales" needs a reference.

Seems odd that the first floristic disruption metric is presented in one paragraph (line 109 on) together with other metrics, and intactness (the other floristic disruption metric) in its own paragraph (line 115 on). It appears more explanation is required for intactness (and this also addresses a previous review point) but I think it worth re arranging these sections to be more logical. In addition, I would suggest line 121 on should be a separate paragraph too, as this zooms back out to consideration of all three measures of exposure.

Line 124: decades to centuries (??).

Line 131: "...across the three evaluated dimensions."

Line 151: Suggest useful to include description/definition of terrestrial protected areas and key biodiversity areas within this opening section. It remains unclear why it is useful to look at both e.g. what additional information do you get by looking at e.g. KBAs rather than protected areas? And how are KBAs and/or protected areas defined? [I realise more of a definition is provided around lines 206 on; it remains unclear why both metrics are useful to investigate though].

The explanation of how this comparison was carried out is much improved - although see point below about line 230. As well as the already included explanation, it may help address my previous point by discussing/presenting to what extent

protected areas and KBAs spatially overlap - for instance, do all KBAs exist within protected areas (hence why the comparison is with areas outside of formal protection for both**) or can KBAs be outside of formal protection?

**NOTE on line 230 you state the comparison for KBAs is with 'non-Key Biodiversity Areas' but in the earlier methods section you state it is with areas outside of protection for both analyses i.e. line 215-216 "...tested KBAs against areas not marked as having formal protection". Please clarify. [Having read more of the paper and e.g. supplementary figures such as Extended Data Figure 8, I assume that 215-216 is a typographical error, and the comparison is with areas that are not KBAs as opposed to unprotected areas, which would make more intuitive sense. Explaining the spatial overlap or otherwise of these different areas (globally or within the different climate regions) may help understand why both]

Line 237/238: Capitalise "key biodiversity areas" to be consistent with elsewhere.

The results from line 217 to 218 seem to suggest that KBAs have lower novelty than elsewhere (44% compared 58%) but line 252 states that "KBAs are just as exposed to novelty". Please can you explain this - as the percentage difference actually seems quite large in this case? See also my remarks on Figure 3, which seem to suggest that all pairwise comparisons are significant, which suggests that there is a slightly lower exposure to novelty (in both protected areas and KBAs) rather than the take home that they are equally exposed - but you can highlight the relatively low effect size? [As you do in the Methods where you point to how you assess pairwise comparisons]

Line 292: As well as understanding the biodiversity dynamics, it is likely important to understand the functional dynamics of current and future novel ecosystems (which I think is mentioned after, but could be explicit here too?).

Line 330: Suggest 'less' as opposed to 'decreasingly'.

Figure 1 e and h highlight potentially important remarks about the data - is there really such a dramatic difference in proportional contribution of drivers of novelty in parts of Sardinia but not others (Figure 1e - north vs south of the island; see also other Spanish islands in Med) and/or such different drivers of novelty in New Zealand compared to south eastern Australia (Figure 1h) (although this latter comparison may be more realistic?). It may also be useful, on Figure 1, to provide an example point in the ternary plot showing how it is x% defaunation, y% climate and z% disruption? [I note which axis is which is more clearly presented on Extended Data Figure 4]

On Figure 3 - if all pairwise comparisons are significant, how can areas be considered equally exposed? I realise in some cases effects sizes are small but even so...? See also comment around line 217-218 vs line 252.

Line 674: Would be easier to understand this if the three novelty dimensions were placed immediately after stating 'three novelty dimensions'. Otherwise it looks as though climate change, defaunation and floristic disruption are 'two conceptually related metrics'. This phrasing is confusing.

Reviewer #2 (Remarks to the Author):

The authors created global maps of ecosystem novelty and their three proposed drivers which are climate change, defaunation and floristic disruption. Based on the first round of reviews, the authors added clarifying information to the main text, improved their methods, and changed results accordingly. Additionally, the R code was revised to enable reproducibility. As the revision of the manuscript was mostly thorough, I would like to ask the authors to make only a few minor changes to their manuscript.

I.52-54: Please simplify the sentence and check if the word "ecosystem" or "ecosystems" is required here. You could write: "However, global assessments of what drives the emergence of ecosystem novelty is, with few exceptions, missing.

I.75: The term "direct anthropogenic interventions" might be more suitable as climate change, mentioned in the second half of the sentence, is also an anthropogenic intervention, although indirect.

I.82: Why "but"? What about using another connector here?

I.123: Here, you are still using the term "nonnative" instead of "alien".

I.125: ... "they interact"... . Please delete "will" as they are already interacting.

I.155-159: Please strengthen the argumentation behind your second hypotheses. By adding "...although..." to your hypothesis it seems quite weak.

I.175: Here, you use the term "nonnative" again. Please consistently use one term, i.e. alien.

I.237-238: Please change to "Key Biodiversity Areas". Please check where the term needs to be changed to capital letters in the manuscript. It can still be found written differently in other paragraphs as well.

I.285-288: Instead of stating that you think your results are robust please give arguments.

I.287: Please remove “certainly” and replace it with “likely”.

I.314: Please use the term “invasive alien species”.

I.325-327: The first concluding sentence can be written in a more convincing and correct way, e.g.: “In conclusion, our results show that today’s biosphere is majorly exposed to multiple degrees of novel conditions. Protected areas and Key Biodiversity Areas are equally exposed to novelty as unprotected areas and areas of low biodiversity importance.”

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Reviewer #1 (Remarks to the Author):

Thank you for inviting me to review "Widespread ecological novelty across the terrestrial biosphere". In this manuscript, the authors construct global maps of ecological novelty based on three broad processes leading to novel conditions (climate change, defaunation, floristic disruption), arguing that this understanding is required prior to work that considers the ecological consequences of novel conditions. Interestingly, they find that protected areas and key biodiversity areas are as exposed to novelty as areas lacking any protection. They correctly point out that these changes imply many current policy objectives are unattainable, and provide a valuable discussion around this aspect in the main text.

This was, on the whole, an easy paper to read and it tackles an important subject with multiple consequences across disciplines. It will, therefore, be of interest to the readership of NEE. I have quite a few comments, in order of the manuscript, not necessarily in order of importance, that I believe could be considered by the authors to clarify their message and justify some of their methodological choices.

- Thank you to Reviewer 1 for their thoughtful and detailed feedback. We have addressed all comments below in full and believe the message of the manuscript has been made clearer, as have a number of the analytical and methodological choices.

(a) One of the key facets of the definition of novel ecosystems (e.g. line 30) (at least according to e.g. Hobbs et al.) is that they have gone beyond thresholds precluding any return to historical conditions. Given multiple rapid environmental changes that do not look like they are returning to any historical state any time soon, this is likely a moot point, but the authors may want to reflect how their approach considers these thresholds.

- We added the missing part of the Hobbs definition of novel ecosystems, and changed the wording of the definition slightly as to not be repetitive with preceding sentences. (Lines 32-33, 40)
- We also added a sentence clarifying that we are mapping the magnitude of the process that could result in a novel ecosystem, rather than specifically testing the timing or emergence of a novel state. (Lines 40, 80)

(b) Line 48 - also subsequent abandonment of that land

- We added this point and included a new reference discussing novel ecosystems in abandoned land. (Line 50)

(c) Could line 53 be unpacked a little more - how does knowing what drives novel conditions help set conservation and restoration goals? Do different drivers lead to different goals, and if so how?

- We added an extra sentence here that touches on these issues, which are expanded upon in the discussion, by giving two concrete examples. (Lines 57-60)

(d) Line 58 - I am uncertain what is meant by an 'ecosystem trajectory destabilising' - given that immediately before (line 56) the authors suggest that novel conditions might actually preserve ecosystem functioning, which sounds like it will be more stable, not less?

- We removed the word “destabilising” and changed the sentence to focus on “unpredictability”, which is our real focus here. (Line 65)

(e) Line 61 - could the reasons why the drivers and/or consequences of novelty vary greatly between systems also be unpacked more - what is the underlying rationale? Presumably in some areas climate is changing less than in others; invasive species coming into systems varies etc. Perhaps it would be helpful to make this explicit - and whether there are any interactions amongst the different drivers. It would be great if a conceptual framework could be put forward as to what these relationships could be - and then how what is mapped subsequently fits on that framework.

- We added additional discussion to this point, specifically at the end of the paragraph discussing our drivers and the pathways to generating a novel ecosystem. We discuss here that these drivers almost certainly interact, but that these interactions will be spatially inconsistent and require quite a lot of unpacking to test. Our goal here is not to investigate the mechanisms and consequences of our novel drivers, but rather discuss the exposure to locally novel drivers. We do agree that such an exploration would be helpful for the field, particularly in the exploration of the diversity dynamics within novel ecosystems, and a future paper focused on this fact would be pertinent. (Lines 75-76, 121-130)

(f) Line 65 - I would explain precisely what is meant by 'locally novel' conditions given this is the key focus of the manuscript. What spatial scale is being considered. And how this considers (or not) thresholds.

- We expanded the definition of “locally novel” here, more explicitly drawing from the definition of both local and regional novelty from Ordonez and Gill (2024), which is what we mean here. We also made the scale that our metrics are being calculated at explicit in the text. (Lines 73-74)

(g) Line 77-78 - By looking at mean temperature and precipitation separately, does your measure of novelty potentially underestimate the degree of change? If there are specific relationships between temperature and precipitation that have become disrupted - perhaps more recently than when any one response is outside the modern range?

- This is true, but not something our metrics can specifically capture – although something a more specific future study could address. We added text addressing this in the discussion section on how our metrics underestimate novelty (which has changed from other revisions too). (Lines 261-264)

(h) It is a philosophical point as to whether humans are a part of, or apart from nature (line 81). The authors clearly consider 'natural' to be without human impact. Is this in the frame of industrialisation, or even before that? E.g. megafaunal extinction from earlier societies? It might be worth explaining the frame of reference explicitly here.

- We added the time frame of the mammals included in PHYLACINE to convey the frame of reference we are using as “natural” (Lines 97-98)

(i) Line 84 - In a similar vein, I would explicitly state what is being considered by 'intactness'.

- We added an extra few words to define intactness, and specifically contrast it to the other disruption metric (Line 100)

(j) Line 99: In considering an 'undisturbed habitat' how are abundance shifts following, say, volcanic disturbances considered i.e. disturbance that is not human-caused but represents a natural state. And, following on line 100, can the change in intactness be anything other than negative?

- We changed the word “undisturbed” to “before alteration by modern industrial society” in order to be more specific about the BII assumptions. We included the reference to the formulation of BII by Scholes and Biggs. (Line 116)
- We removed the reference to negative intactness shifts here. In the methods we also clarified intactness further, and how we modified it in our analysis. To briefly summarise: using the formulation of the BII from Newbold *et al* and prior authors, the modelled change in abundance of native plants is used. Therefore, if the abundance increases (through changes to land use which favour the plant in the models) then the BII is greater than 1 (Figure S4 and Table S3 in Newbold *et al* 2016 for examples). As here we consider any change in original abundance as novelty (albeit it is only low novelty in this case) we use our formulation.

(k) Line 107 to 112 - How do you account for the two metrics per driver when working out these >0.33 and >0.66. As I understand it, the >0.33 would be if 1 out of 3 drivers was maximum, but how are the two metrics within it combined? Or is it just implicit, and that both metrics, having also been scaled between 0 and 1, are both at 1 for a given driver?

- We reworked this section so that the process and methodology is clear from the start. (Lines 131-148)

(l) Line 123 is somewhat of a repeat of the previous sentence given climate change and defaunation come out as close to being the same (high) novelty.

- We reworded this slightly so that Figure 1b is better explained and the text is less repetitive. (Lines 167-170)

(m) Line 134-135: I found this confusing - how can Hedges G be < -0.71 (line 135) and then = -0.45 later in the same line? Please clarify. (Is that then compared to polar? This seems clearer with the comparisons on line 137 - 138).

- This was a wording error and we have corrected the text. It is correct that this is when compared to polar. (Line 193-194)

(n) It would be helpful (line 144) to provide more information earlier in the paper as to what is actually meant by 'key biodiversity area' given concepts such as biodiversity hotspots and so forth.

- We have now expanded the definition to reflect the definition given in the references and in other papers which use the same dataset for KBAs. We have also expanded the definition of protected areas to be clear here. (Lines 208-211)

(o) Line 146 - Is it a one-off comparison, or do you choose multiple equal numbers of cells (e.g. through bootstrapping) to identify whether or not the novelty in a network of protected areas and/or key biodiversity area is different to unprotected? I presume there are no climate regions where there are more protected cells than unprotected cells? Did you mask out urban, agricultural comparisons here (given e.g. the earlier statement on line 71 about not being interested in agriculture, forestry and urban areas).

- We have clarified that we compare multiple equal numbers of cells at the protected area level (i.e. each protected area has its own set of comparisons), along with changing the analysis and visualisation slightly as to be clearer (Lines 211-214)
- We did not mask out these areas, and we have now clarified our reasoning earlier in the manuscript along with changing the specific wording on line 71 (now line 82)

(p) Line 165 on - Higher globally compared to what? And where do key biodiversity areas fit in this comparison - or are key biodiversity areas and protected areas somehow combined in these comparisons?

- We have reworded each sentence in this section to make the comparisons clearer, as part of reworking this analysis entirely (Lines 205-248)

(q) Line 185 - Are the drivers really independent - might you expect intactness to relate to climate change (or other drivers such as nitrogen pollution) for instance as abundances change in response to these pressures?

- We added some examples of how these could interact, and clarified why we don't address them specifically. (Lines 125-130)
- See also our reply to comment (e), where we also discuss this from a different context.

(r) Line 194 - Is the underestimation directly linked to the earlier paragraph that there are other axes that have not been considered in your analysis? Otherwise, please can you provide further justification of your underestimation statement and/or clarification.

- We significantly expanded this section to examine potential underestimation for each axis. We also added a sentence to expand on the other axes. (Lines 260-288)

(s) Line 206 - And functional consequences? Not just diversity consequences?

- We expanded this to explicitly include functional and phylogenetic diversity. (Line 292)

(t) Line 212 - What baseline? Who decides what is a relevant baseline?

- We have changed baseline here to "pre-human alteration" to match the definition of a novel ecosystem, and opt not to expand on the philosophical and scientific issues with restoration baselines more broadly. (Line 298)

(u) Line 243 - I think, in the original conception of biodiversity hotspots, they already were widely threatened by novelty-inducing processes.

- Here we only refer to KBAs as areas marked for needing protection – we added this wording to make this more clear (Line 296)

(v) Line 244 - Where is the justification for this statement. Could it be that systems are just different e.g. see Hobbs and 'degraded or different'.

<https://onlinelibrary.wiley.com/doi/10.1111/rec.12336>

- We reworded the start of this section so that the sentence fits with our discussion around the results for individual drivers (Lines 289-303)

(w) Line 269 - It is unclear to me how clearly the figure shows this highest novelty in tropical, arid and temperate regions.

- In reality, these difference are indeed very small – and this supports our result of novelty exposure being widespread. We updated how we frame this result to be more explicit about this. (Lines 185-187)

(x) Line 281 - It needs to be explained earlier in the manuscript what constitutes the higher protection status. Also line 322.

- We addressed this comment in our new WDPA/KBA analysis. In our new version of this analysis, we completely distinguish between protected areas and key biodiversity areas, rather than assigning cells to a protection status.

(y) Extended data 4 - please clarify why 'climate','community change','defaunation' used on axes in this figure whereas elsewhere it is 'climate change', 'defaunation' and 'disruption' (as per the phrasing in the main ms). Note that the global triangle has 'climate', 'community change' and 'disruption' - Is this a mistake in labelling as community change and disruption are presumably synonymous given the other labels?

- This is a labelling mistake and has been corrected to match the in-text descriptions.

(z) Methods (line 448 on). The first sentence (line 450 on) needs rephrasing - it is very confusing.

- We removed some of this sentence, as the metric clarification is described later on in the same paragraph.

(aa) Line 463 - how did you consider correlations between temperature and precipitation (see earlier remark).

- We added a sentence addressing the roles of temperature and precipitation, as well as how they might correlate, and included a reference. (Line 261-264)

(bb) Line 472 - Is this phrasing the correct way around - if ice was earlier in time than the novelty condition, then presumably the novelty condition was closer to the present day and should remain within the analysis?

- We reworded this so it is clear, as this is a minor wording error – we take ice to be a novel condition, and therefore the time is taking as the closer time to the present of the two. (Lines 694-696)

(cc) Line 475 - I am unfamiliar with these databases - how robust are their predictions of modern ranges in the absence of human pressures - and how far back does this absence of human pressure go?

- The present natural ranges are meant to represent the range a species would have today without human influence – independent of the time of human pressure. The robustness of the ranges varies between species depending on the method used to estimate present natural ranges. We added some explanation about the methods in Faurby et al to our methods part:
 - “Present natural ranges represent the hypothesized ranges of species today without human influence and are derived from the current IUCN ranges for all extant species and species extinct after 1500 aD and modified to correct for anthropogenic range restrictions under consideration of species’ dispersal constraints, biotic constraints and abiotic limiting factors. The accuracy of the present natural ranges varies hereby between species. Present natural ranges for species extinct prior 1500 aD were estimates based on the present natural ranges of extant species which have been found at similar fossil site as the extinct species. See methods and supplementary information in Faurby et al44 for details on range modifications and quality checks.”

(dd) Line 491 - I found this an interesting decision given introductions are a key part of novelty? I.e. is it justifiable to consider only defaunation as leading to novelty? Or is this another reason why your results are conservative (see comment (r)).

- The “current” data used for calculating this does not take introductions into account very well, so we chose to only focus on loss for this particular novelty process. We added some text into the main text to clarify this, in addition to what we added in the discussion section on conservative estimation.

(ee) Line 500 - how did you account for Botanic Gardens? I was involved in some work where it became apparent that some species occurrences were related to this - and not non-native introductions per se.

- We cleaned the data using a cleaner package (CoordinateCleaner) to remove such records, which was accidentally not reported in the methods. It has been added, with the relevant citation.

(ff) Line 527 - how does this method account for e.g. National Parks in the UK - which are considered protected by some, but are likely not in the frame of this work?

- As we only included areas specifically marked WDPAs, regardless of what level they were, many UK national parks are included (eg Yorkshire Dales, which are IUCN category V). Selecting PAs this way avoids any debate on cutoffs and gives us a minimum standard. For KBAs which lack protection, many who do not make the WDPa cutoff are included. Either way, we included a disclaimer that these databases may not be comprehensive in terms of levels of local protection.

(gg) Line 540 - In the main text I would make the decision explicit and also outline what constitutes a 'small' (or larger) effect size in relation to Hedges G.

- We added a sentence explicitly outlines the effect size cut offs.

Reviewer #2 (Remarks to the Author):

The authors map novelty in the terrestrial biosphere and at a global scale based on novel climatic conditions (abiotic component), defaunation and floristic disruption (abiotic component) representing three dimensions of novelty. Further analyses comprise a closer look at novelty in protected areas and key biodiversity areas. The topic of the manuscript is a valuable contribution to current debates about novel ecosystems and ongoing discussions in restoration ecology and biogeography about biodiversity and conservation baselines. While the concept of the paper is mostly convincing and the manuscript is well-structured and written I would like the following issues to be addressed by the authors, mostly concerning the methodology and terminology.

- Thank you to Reviewer 2 for their constructive feedback, especially for taking the time to review our code. We agree there are important improvements to be made in these areas and have addressed all specific feedback in full.

The terms “drivers of novelty”, “indicator of novelty” and “dimensions of novelty” are used in the manuscript, which seems confusing. The authors should be more explicit if they consider climatic novelty, defaunation and floristic disruption to be drivers of novelty or rather indicators/dimensions? I would opt for these metrics to be indicators/dimensions of novelty as the underlying drivers are rather land use change, human activity etc. Please consistently use one of the terms and explain how you perceive the link between novelty on the one hand and climatic novelty, defaunation and floristic disruption on the other hand. This requires changes throughout the manuscript.

- We have harmonised the use of terminology in the manuscript to ensure we consistently use the term “driver” when referring to our three broad processes and the term “metric” to refer to our individual measurements. We then clarify the use of the term “driver” in

the paragraph describing the mechanism by which they could form novel ecosystems.

The summary paragraph and the introductory paragraph of the main text mention global transformation of the Earth by human activities and human land use. These sentences are not backed up by literature. Here are two inspirational papers:

Gordon, J.D., Fagan, B., Milner, N. et al. Floristic diversity and its relationships with human land use varied regionally during the Holocene. Nat Ecol Evol 8, 1459–1471 (2024).

<https://doi.org/10.1038/s41559-024-02457-x>

Venter, O., Sanderson, E., Magrath, A. et al. Global terrestrial Human Footprint maps for 1993 and 2009. Sci Data 3, 160067 (2016). <https://doi.org/10.1038/sdata.2016.67>

- We have added three references to the first sentence, including one provided by the reviewer. (Line 25)

You mention that novel ecosystems have become widespread in natural and semi-natural landscapes. It seems, however, contradictory to the reader how a landscape can still be natural while novel ecosystems are widespread? Can this contradiction be explained to the reader in the main text?

- We modified the language in the introduction where this is first mentioned. Although novel ecosystems are by definition only present in unmanaged landscapes, the terminology was unclear in our text. We shifted this to instead talk about our three drivers and how they would likely result in the emergence of novel biosphere states.
- In addition, we agree with the reviewer here – and the word “wild” and “natural” have been changed throughout the manuscript to reflect our opinion that novel landscapes are widespread.

The third paragraph of the main text mentions the research gap concerning novelty at local and regional scales. The fourth paragraph explicitly mentions that the aim of the manuscript is to quantify novelty and the underlying drivers locally. But can the manuscript actually achieve this? The analysis is rather a global one and your data has a resolution of 10 km x 10 km. Therefore, I would delete the word “local” from the manuscript and rather state that your analysis is global.

- We clarified the definition of “local novelty” earlier in the manuscript to avoid any confusion – when we use the term “local novelty” we are instead referring to the spatial reference of the novelty calculation. We made sure to explicitly state this, including the reference from which we get our definition (Ordonez and Gill 2024). (Line 73-74)

In the main text protected areas and key biodiversity areas are not mentioned until the presentation of the results. As these are an essential part of the manuscript these need to be mentioned early in the main text, for example in paragraph four of the main text where the aims of the manuscript are clarified.

- We added a reference to the PA/KBA analysis where the aims are stated. (Lines 155-159)

What does the focus on only wild and semi-wild systems, as stated in paragraph four, imply? All maps and subsequent analysis are global. Not even urban and agricultural areas are excluded from your analysis. If this statement only leads to the zoom-ins in figure 1 please rather delete it as it evokes different expectation.

- We removed the reference to wild and semi-wild areas, as it didn't accurately convey what we meant by our approach. As we are not directly measuring the presence of novel ecosystems, and only focus on the extent to which novelty processes are unprecedented, this needed to be made clearer. We have made some changes throughout to specify that the focus on these areas is only true for the protected/key biodiversity area discussion. (See specifically lines 80-84, 86)
- We also clarified in the introduction that our goal is to create maps of how local conditions have changed, focusing on three drivers. We instead focus our discussion and consequences on wild and semi-wild areas, based on and around our WDPA and KBA analyses.

You measure climatic novelty based on mean annual temperature and annual precipitation sum. Firstly, in the fourth paragraph the precise term "annual precipitation sum" is missing and only "precipitation" is mentioned. Secondly, why did you decide to use these metrics as, for example, annual precipitation sum can stay the same with ongoing climate change while temporal precipitation patterns can change in a region?

- We added the precise term. (Line 90)
- We added in our justification for the two variables – specifically that these are the most robust outputs from the Earth System Models, so are the most trustworthy to use at the temporal scale and resolution we are interested in (with annual precipitation sum being the only precipitation output in the core CHELSA dataset). (Line 91-93)
- We additionally added text to discuss the interaction between the variables, both in response to this and other reviewer comments (such as below). (Lines 124-130, 262-264)

Did you consider the interrelatedness of climatic novelty, defaunation and floristic disruption? Floristic disruption can, for example, be caused partly by changes in climate.

- We agree with the reviewer that the interrelatedness of our three drivers is a key component of understanding the whole picture of novelty – we don't measure this directly as to focus our study on the exposure to the drivers and not the consequences. We added a few sentences in the introduction, specifically at the end of the paragraph discussion the ways our drivers impact novelty, to address this. (Lines 124-130, 262-264)

Please consistently use either the term "alien species" or "nonnative species" (also written "non-native" in the manuscript) throughout the entire text.

- We harmonised the terminology to consistently use the term "alien species" after McGeoch et al 2024.

The terms "wild and semi-wild" as well as "natural and semi-natural" are used in the text. Are they refereeing to the same? If yes, please consistently use only one term.

- We harmonised the language and have changed some references to these terms throughout the text (in response to other comments made by both reviewers).

At the end of the introductory main text you present your novelty scale that ranges between 0-1 and how you divided it into three categories. Firstly, I was wondering why you decided on three categories and secondly, it is confusing why this scale is developed but in the proceeding

paragraph you start presenting your results using a novelty score of 0.2. I would advise to stick to using your three categories when presenting your results.

- We clarified the text here to distinguish between the different novelty measurements. (Lines 131-148)
- We removed the reference to 0.2 – this was intended as a 1st percentile reference but is not needed.

It seems obvious to expect differences of climatic novelty when dividing your result by climatic region. Have you considered using another categories for your subanalysis such as biome classifications that are not mainly based on climate (see Fischer et al., 2022 for inspiration: <https://onlinelibrary.wiley.com/doi/full/10.1111/geb.13574>)?

- Thank you for the interesting suggestion. We have now included some additional analyses in the Extended Data which analyses data within biome classes. We left this as extended data because we want to focus on very broad, regional patterns rather than assume ecological responses. We have also added text in the main text, as well as the methods, regarding this new analysis.

You state that your results concerning floristic distribution might depend on data availability for nonnative species. If that is highly likely, why did you include regions of low data availability in your analysis and not blank out those regions? The global novelty maps may be misleading because this explanation is not given in the figure or its subheader. To make your global analysis and maps reflect true patterns and not data availability I would advise to reduce it to areas with sufficient data availability for all three metrics. There are packages available to evaluate sampling bias in biodiversity collections, e.g., the R package sampbias (<https://cran.r-project.org/web/packages/sampbias/sampbias.pdf>).

- This is an important caveat in this work (and any work which uses biodiversity data at global scales). We have clarified how our approach is intentionally conservative in the main text (Lines 171-184)
- To further address this important point, we included two additional Extended Data figures. First, we repeated our overall novelty results only using cells which met our minimum requirement for GBIF data. This resulted in a smaller global area covered but ensured reliability. Second, we produced a map which only used the five metrics which have full global coverage. This skews the representation of the three dimensions but removes bias from biodiversity data. We present these as Extended Data and include references to them (and the reason they exist) within the text and as Extended Data Figure 5

When presenting your results you write: “When directly compared, protected areas are equally exposed to novelty as unprotected areas at the global scale ($G = 0.03$, Figure 3), confirming our hypothesis.” I could not find that hypothesis in the main text.

- Currently this hypothesis is stated at the end of the intro in that section. We have moved it to the end of the introduction, so it is clearer and in a more natural location for the reader. (Lines 155-159)

Your analysis is based on raster data. Therefore, I was wondering how you converted the data on protected areas (provided as vectors) into raster data. Did you consider a pixel to be a protected area only when it was covered 100% by an according polygon declared as a protected area? Did you consider using different thresholds of coverage (e.g., 100%, 75%, 50%)? This might change your results concerning novelty in protected areas. Furthermore, did you exclude protected areas that have the status “proposed” and which are not actually implemented? This information is not given in your methods description.

- We have added this information into the methods – we assumed a >50% coverage as our cut-off.
- We originally included proposed protected areas, but have now updated the analysis to remove these. We have also made our analysis stricter by only included protected areas which have an IUCN category and therefore have some level of strict protection (although this varies).
- Finally, we updated our methods for doing the randomisation of selecting unprotected space. As a result of all of these changes, much of the “Vulnerable Nature” results and Figure 3 have changed, without affecting the overall result of the section. We have summarised these changes in more detail at the top of this response letter.

You state that your results likely underestimate local novelty. Can you elaborate why you come to this conclusion?

- We extended this part of the text greatly to include more details, specific examples, and elaboration on the different ways novelty could be underestimated. (Lines 260-288)
- We also included a reference here to a previous comment on dealing with missing data in the alien species metric. (Lines 275-276)

In figure 2a the legend is missing, colours are not explained. Only at a second view it becomes clear that the climatic regions are named in b. Significant differences should be indicated in the figures, with for example letters above the boxplots. Additionally, the n (sample size) for each category would be helpful to assess the figure. If you decide not to present p-values in your figures an extra figure summarising the effect sizes might be a valuable addition as those numbers are hidden in the SI and in the text but are not visually represented.

- The colours in Figure 2a are just for visual purposes only, we removed them to avoid confusion.
- We changed the figure so that the axis titles are on both panels, and included the sample size for each region in the figure caption (we also did this for other figures too).
-

Why not stick to the exact same colours in figure 2b as in figure 1? The „climate novelty“ colour looks different.

- We fixed the colours in Figure 2b to broadly match figure 1 – the exact corner colours are much too bright to be in a boxplot so we muted them slightly.

It might be a valuable addition to represent global values in figures 2 a and b as you did in figure 3.

- We added the global values for this figure to match figure 3.

In the methods description please use a new paragraph for describing floristic disruption. Is it now combined with the defaunation metric in one paragraph which makes it hard to find.

- We split floristic disruption into its own paragraph.

For the calculation of the Biodiversity Intactness Index you mention the use of abundance data. Do you really use abundance data and if yes, which data source did you use?

- We clarified the calculation of the BII, which is using the global data layer from Newbold et al.

Please check the reproducibility of your code, as the source of the reference raster you used (HYDE data ?) is not clear and therefore getting the data and reproducing your results is not possible. For reproducibility it would help to indicate data sources also in the R Code.

- Good catch! We have answered these concerns below. In short, we included links to each data source within the code and updated many parts which were not clear.

Reviewer #2 (Remarks on code availability):

I could not easily reproduce the code.

- Thank you for taking the time to review our code. In addition to the two changes made below, we made a number of changes in part 1 of the code to make the analytical steps clearer.
- We have also made changes in part 2 of the code, both to increase the clarity of the code but also updating the randomisation protocol for the protected area results.
- Finally, we moved the figure making code into part 3.

The source of the reference raster used (HYDE data ?) is not clear and therefore getting the data and reproducing your results is not possible. For reproducibility it would help to indicate data sources also in the R Code.

- This is a good catch – a HYDE layer was being used to standardise some aspects of the pipeline early on and was left in without reference. We have now included this in the code with an explanation and link.
- We have also added comments with links to the data sources in the R code.

Why is the raster package still being used in the code as it is not supported anymore and not mentioned to be relevant for the data analysis in the methods section?

- Sorry for that! We updated part 1 and 2 of the code to only use *terra*. We also removed any reference to *sp* (in favour of *sf*).

Response to reviewers:

Reviewer #1 (Remarks to the Author):

SUMMARY

I enjoyed reading this revised manuscript, and believe the authors have addressed many of the comments raised by previous reviews. This paper remains an important step in understanding exposure to novel conditions across the globe - and will encourage more questions to be addressed. Indeed, in some ways, it raises more questions than answers (e.g. what would happen if interactions were considered, what are the wider biodiversity and functional consequences), but I believe that raising of more questions to be a strength.

I have a few remaining comments that will hopefully further clarify the authors' approach; in particular, I think some further justification of looking at both protected areas and KBAs is worth bringing out - and there also needs to be some clarification around the comparisons as there are discrepancies between what is put in the main text and what is shown in figures (see detailed comments below). Getting a colleague, unfamiliar with the subject area, to read through any revised submission may also be useful to make sure the messages are clear and supported throughout.

- We thank Reviewer 1 for their feedback on our revised manuscript, and for taking the time to review our changes carefully. We agree with them that this manuscript raises a lot of additional questions, and we are glad they consider it a positive step towards understanding global novel conditions.
- We have addressed all their comments, which mostly centred around clearing up some details with the protected area and KBA analysis. These comments have improved the clarity of the manuscript greatly.

DETAILED COMMENTS (in order of the manuscript, not of importance)

Line 41-42: Can something be pre-Homo sapiens if stated as anthropogenic novel states? Doesn't anthropogenic imply Homo sapiens, or can it comprise other members of the genus?

- We changed the wording both here and in the preceding paragraph to be clearer on our framing of the impacts. It isn't straightforward to say "pre-anthropogenic", and this comes with complex and undesirable connotations. We additionally removed the word "anthropogenic" in the second paragraph to just read "relevant baselines" to address this further.

Line 93 - "...additionally due to their impacts on biodiversity patterns at large spatial scales" needs a reference.

- We added two references here, one new and one existing in the manuscript, to highlight the importance of these two variables.

Seems odd that the first floristic disruption metric is presented in one paragraph (line 109 on) together with other metrics, and intactness (the other floristic disruption metric) in its own paragraph (line 115 on). It appears more explanation is required for intactness (and this also addresses a previous review point) but I think it worth re arranging these sections to be more

Commented [MK1]: [FR: here my suggestion for a changed sentence: "These novel ecosystems – systems with biotic and abiotic compositions without historical precedent, that have crossed a threshold precluding their return to a baseline state prior to gross anthropogenic modification, and which autonomously maintain and self-develop their novel characteristics..." – it's sort of touches on my pet peeve. Not all humans, and not all Homo sapiens have the same ecological impacts, so it's really not straightforward to say 'pre-anthropogenic' in the sense that many ecosystems have to some (totally acceptable) degree been modified by humans for a really long time. It wouldn't be useful nor desirable to argue for a return to states prior to ANY human impact as that would fix very likely negatively impact indigenous communities.]

logical. In addition, I would suggest line 121 on should be a separate paragraph too, as this zooms back out to consideration of all three measures of exposure.

- Good point - we amended this paragraph to be more logical. Specifically, we added the proposed split at line 121 and combined the paragraphs concerning the metrics.

Line 124: decades to centuries (??).

- Good catch - we swapped the ordering so it was logical.

Line 131: "...across the three evaluated dimensions."

- We added in the omitted word.

Line 151: Suggest useful to include description/definition of terrestrial protected areas and key biodiversity areas within this opening section. It remains unclear why it is useful to look at both e.g. what additional information do you get by looking at e.g. KBAs rather than protected areas? And how are KBAs and/or protected areas defined? [I realise more of a definition is provided around lines 206 on; it remains unclear why both metrics are useful to investigate though].

- We added extra text here to clarify further the definition of both, and in order to further clarify why testing both would be useful.

The explanation of how this comparison was carried out is much improved - although see point below about line 230. As well as the already included explanation, it may help address my previous point by discussing/presenting to what extent protected areas and KBAs spatially overlap - for instance, do all KBAs exist within protected areas (hence why the comparison is with areas outside of formal protection for both**) or can KBAs be outside of formal protection?

- We also agree with the reviewer that the degree of overlap would be useful to specify, and so we added this detail.

****NOTE on line 230 you state the comparison for KBAs is with 'non-Key Biodiversity Areas' but in the earlier methods section you state it is with areas outside of protection for both analyses i.e. line 215-216 "...tested KBAs against areas not marked as having formal protection". Please clarify. [Having read more of the paper and e.g. supplementary figures such as Extended Data Figure 8, I assume that 215-216 is a typographical error, and the comparison is with areas that are not KBAs as opposed to unprotected areas, which would make more intuitive sense. Explaining the spatial overlap or otherwise of these different areas (globally or within the different climate regions) may help understand why both]**

- The reviewer is correct – line 215-216 is a typographical error and the intuition is correct. We altered the wording of this line to amend this.

Line 237/238: Capitalise "key biodiversity areas" to be consistent with elsewhere.

- We capitalised the term, and double checked each other appearance of the term to ensure consistency.

The results from line 217 to 218 seem to suggest that KBAs have lower novelty than elsewhere (44% compared 58%) but line 252 states that "KBAs are just as exposed to novelty". Please can

you explain this - as the percentage difference actually seems quite large in this case? See also my remarks on Figure 3, which seem to suggest that all pairwise comparisons are significant, which suggests that there is a slightly lower exposure to novelty (in both protected areas and KBAs) rather than the take home that they are equally exposed - but you can highlight the relatively low effect size? [As you do in the Methods where you point to how you are assess pairwise comparisons]

- We added some extra text in this area as to ensure the correct conclusions are being drawn. Specifically, we made sure that in each case the comparison was clear, and we split the sentence at lines 217-218 so that it didn't sound like we were comparing PAs and KBAs.
- With the new analysis, the number inside KBAs needed correcting.
- We opted to highlight the low effect size in each case.

Line 292: As well as understanding the biodiversity dynamics, it is likely important to understand the functional dynamics of current and future novel ecosystems (which I think is mentioned after, but could be explicit here too?).

- We added an explicit reference to function here.

Line 330: Suggest 'less' as opposed to 'decreasingly'.

- We reworded this section slightly to be clearer, and therefore changed this wording to be more precise.

Figure 1 e and h highlight potentially important remarks about the data - is there really such a dramatic difference in proportional contribution of drivers of novelty in parts of Sardinia but not others (Figure 1 e - north vs south of the island; see also other Spanish islands in Med) and/or such different drivers of novelty in New Zealand compared to south eastern Australia (Figure 1h) (although this latter comparison may be more realistic?). It may also be useful, on Figure 1, to provide an example point in the ternary plot showing how it is x% defaunation, y% climate and z% disruption? [I note which axis is which is more clearly presented on Extended Data Figure 4]

- These dramatic shifts are largely due to limitations in the defaunation data. We bring this up in the discussion, so we added some additional signposting to this figure to make it clear.
- The difference between Aus and NZ is a similar but distinct reason – in that we only measure mammal defaunation so it is bound to be clearer. We also raise this in the discussion.

On Figure 3 - if all pairwise comparisons are significant, how can areas be considered equally exposed? I realise in some cases effects sizes are small but even so...? See also comment around line 217-218 vs line 252.

- Here we only focus on the pairwise effect sizes as our measure (see methods). We added in some extra text on each figure where p values are mentioned so this is clear. We additionally added text to the results to make this clear.

Line 674: Would be easier to understand this if the three novelty dimensions were placed immediately after stating 'three novelty dimensions'. Otherwise it looks as though climate change, defaunation and floristic disruption are 'two conceptually related metrics'. This phrasing is confusing.

- This is a good point. We rearranged the wording so that this was clearer.

Reviewer #2 (Remarks to the Author):

The authors created global maps of ecosystem novelty and their three proposed drivers which are climate change, defaunation and floristic disruption. Based on the first round of reviews, the authors added clarifying information to the main text, improved their methods, and changed results accordingly. Additionally, the R code was revised to enable reproducibility. As the revision of the manuscript was mostly thorough, I would like to ask the authors to make only a few minor changes to their manuscript.

- We thank Reviewer 2 for looking at our manuscript again, for the positive and constructive feedback, and in particular taking the time to review our code to ensure everything is transparent. We have made all the changes to the manuscript, which have vastly improved the clarity of our message. In particular, our justification for our metrics being robust has been amended to be much stronger and compelling and we thank them for suggesting this change.

l.52-54: Please simplify the sentence and check if the word “ecosystem” or “ecosystems” is required here. You could write: “However, global assessments of what drives the emergence of ecosystem novelty is, with few exceptions, missing.

- We changed the wording to use “ecosystem” instead of “ecosystems”.

l.75: The term “direct anthropogenic interventions” might be more suitable as climate change, mentioned in the second half of the sentence, is also an anthropogenic intervention, although indirect.

- The reviewer is right here, and we added the word “direct” to be more precise.

l.82: Why “but”? What about using another connector here?

- We removed the connectors here as they weren't needed in the sentence structure.

l. 123: Here, you are still using the term “nonnative” instead of “alien”.

- Thanks for spotting this, both here and a few other places. We went through and checked every instance, which included two additional places not spotted, to ensure the terminology was consistent.

l. 125: ...“they interact”... . Please delete “will” as they are already interacting.

- We change the language here to reflect this fact.

l. 155-159: Please strengthen the argumentation behind your second hypotheses. By adding "...although..." to your hypothesis it seems quite weak.

- We reworded this whole section to first explain the reasoning behind the hypothesis (that our metrics are regional phenomena) and then counter the expected rebuttal (that protected areas in particular could reflect a lower biotic novelty).

l. 175: Here, you use the term "nonnative" again. Please consistently use one term, i.e. alien.

- We corrected the term, in addition to other places as described above

l. 237-238: Please change to "Key Biodiversity Areas". Please check where the term needs to be changed to capital letters in the manuscript. It can still be found written differently in other paragraphs as well.

- We corrected the capitalisation here and confirmed other appearances match.

l. 285-288: Instead of stating that you think your results are robust please give arguments.

- We expanded and modified this section so that we focus on the arguments for why our metric should be trusted, rather than keeping it vague.

l. 287: Please remove "certainly" and replace it with "likely".

- We changed the term to the suggested phrasing.

l. 314: Please use the term "invasive alien species".

- We corrected the term.

l. 325-327: The first concluding sentence can be written in a more convincing and correct way, e.g.: "In conclusion, our results show that today's biosphere is majorly exposed to multiple degrees of novel conditions. Protected areas and Key Biodiversity Areas are equally exposed to novelty as unprotected areas and areas of low biodiversity importance."

- We agree, and think this change would help the conclusion be more easily read. We changed this sentence by lightly adapting the wording the reviewer used above, keeping the proposed two-sentence structure.

Commented [MK2]: [@Jens-Christian Svenning](#) [@Alejandro Ordoñez Gloria](#) [@Felix Riede](#) this is the only other bit that might expand beyond some wording changes -

For easy searching, this section is currently starting with the phrase "Despite these sources of novelty underestimation..."

Commented [JS3R2]: I've added some suggestions

Commented [FR4R2]: Me too.