nature portfolio

Peer Review File

Insect diversity can predict biodiversity status in Africa

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

27th Jan 23

Dear Dr Landmann,

Your manuscript titled "Measuring insect-based biodiversity status in Africa" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to:

1) Provide compelling evidence of the novelty, relevance and added value of the proposed indicator compared to others available in the literature to capture the biological status and changes of species (insects) across the world (and in particular in Africa)

2) Ensure that your methods are fully explained and justified and that uncertainties and limitations of the study are assessed and discussed, including those related to iNaturalist data and the species diversity model

3) Provide clearer guidance on data and model requirements to apply the metric in future research

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Alienor Lavergne, PhD

Associate Editor

Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In the study it proposes an index to estimating biodiversity status metric across microhabitats in two agro-ecological zones in Africa using spatially explicit earth observation and insect.

In my opinion the paper fits the scope of the "Communications earth & environment" journal. Firstly, I would like to congratulate the authors for the work done. The topic is very interesting and the study is a very high quality study. Methods are well described and justified. Discussion is completed and well wrote. Despite that, the manuscript needs some revisions to be published.

I provide some key points to complete/improve:

Major comments

1.Introduction

1.1.The introduction should be strengthened by presenting in more detail the state of the art. While the need for action is described, recent and current research as well as research gaps are not sufficiently presented.

Particularly it would be necessary provide a sinthesis of similar indicators to measure/estimate biodiversity status in order to justify the novelty and relevance of the proposed indicator iBS. Moreover, it would be interesting mention to composite indicators to measure the achievement in terms of biodiversity conservation and emphasizing the role of “local indicators” to providing a complete framework about the worldwide biodiversity status should be provided. As example of composite indicators related that issue, you can see/cite: de Castro-Pardo, M., Martín, J. M. M., & Azevedo, J. C.

(2022). A new composite indicator to assess and monitor performance and drawbacks of the implementation of Aichi Biodiversity Targets. Ecological Economics, 201, 107553.

Although in the Discussion section some indicators are discussed, a brief relation of the state of the art should be presented in the beginning of the manuscript to present the reader with a complete contextual framework and emphasizes the relevance of the proposed indicator (which, in my opinion, is relevant). In the Discussion section, you could maintain the comparison between the results using these indicators and yours and analyzing the merits and advantages of your proposal.

I suggest to adding this information as a separate (sub) section after Introduction.

1.2.The aim/s of the study should be clearly defined. I presume, at least, two objectives, the proposal of the "new" (if the authors could justify the novelty) indicator capable to involve the local particularities and the aims related to the application of this metric. I'm not sure if the application is an objective by itself or only the validation of the proposed index. This point should be well clarified in the manuscript and maybe in the abstract too.

2.Results

Spatial patterns at localized sites

How analyzed micro-habitats were selected? Randomly? This should be clearly described.

3.Discussion

As the conclusions of this study have been described in this section, the title should be "Discussion and conclusion".

Minor comments

L62: Change "indictors" by "indicators"

Reviewer #2 (Remarks to the Author):

The authors present a biodiversity metric (iBS) that integrates biological information (here, insect species richness) with data on human impacts (here, landscape naturalness). The metric can make use of high resolution data and can be applied to different contexts and questions (e.g., biodiversity status of protected areas). The metric has some similarities to the Biodiversity Intactness Index, as well as the Biodiversity Habitat Index (https://www.bipindicators.net/indicators/biodiversity-habitat-index), which focuses on composition, through the use of Generalized Dissimilarity Models, rather than richness. In my

opinion, the metric could be a useful addition to the literature, as its calculation is relatively simple and it can make use of high resolution Earth Observation data. Plus, it goes beyond the regularly used Human Footprint Index and similar, which do not include biological information (see Mammola et al., 2023 [full references included below], which highlights the issue of many papers talking about biodiversity without sampling biodiversity data). The manuscript is generally well written and clear to the reader. However, I do have a few major comments, and quite a few minor comments, which I have listed below (line number in brackets).

My first general comment is that I am, as yet, unconvinced that the biodiversity information included in the iBS is capturing the intended attributes it seeks to. This doesn't mean that I think the metric is not working, just that I would like more evidence and clarity about how it is capturing the biological aspects. How do the D and N components influence the iBS metric? Fig. 2A shows that iBS is highly correlated with land cover (here, satellite imagery), and although this is less the case for Fig. 2B, I'm still concerned that the maps of iBS generally reflect a layer of human impacts, rather than a true biodiversity metric. This concern is also raised by the fact that the results from the, very different regions and taxonomic groups, included in Fig. 3 appear to have similar values for iBS. Does N often have a larger variance than D and therefore more influence on iBS? Does D have a more skewed distribution and therefore less influence on iBS? I think there are a few things that could be done to answer these questions and potentially reduce these concerns for the reader. First it would be informative (if not essential) to show the contributing layers for each of the iBS outputs in the Supplementary. Maps of D and N (and maybe plots of their distributions) would help the reader to understand the patterns in the data. Correlations between the inputs (D and N) and the final metric (iBS) would also be informative to see how the inputs influence the results. Simulations would be an even better step, to show how different data structures affect the iBS metric. For instance how does the mean and sd of the input data, or the distribution (normal, skewed) of the input data, affect the influence/weight of the D and N components. Basically, I want more evidence that this metric is capturing the properties it sets out to (i.e., insect biodiversity status), rather than attributes that may (or may not) be correlated with biodiversity status (i.e., cumulative human impacts).

In addition, although frequently used, species richness is a very blunt measure of biodiversity, it does not capture many other aspects of biodiversity that may be particularly important across a landscape (e.g., species representativeness, species composition [beta diversity], complementarity). I think this raises the point that it needs to be clear how this approach differs from other work that better captures these aspects (e.g., Generalized Dissimilarity Modelling - Biodiversity Habitat Index). Is the intention that this is a more computationally feasible metric? Is this metric only to be interpreted at local scales (i.e., local biodiversity status not landscape biodiversity status)? I'm concerned that when the iBS metric is averaged it may become a poor indicator of biodiversity status. E.g., you could have a landscape with the same 100 species in every pixel, or a landscape with a different 80 species in each pixel – which is more biodiverse? Is this metric only informative at local scales, if so this should be stated. What is the USP of the metric? It needs to be clear how the metric can be appropriately used.

Following on from concerns about the influence of D and N, I also think that it is unclear how sensitive the metric is to the structure of the biodiversity data (see Isaac & Pocock 2015 for concerns about potential bias in biological data). Species richness in particular is very sensitive to sampling effort (Roswell et al., 2021), so how does the iBS metric deal with this? There are many potential issues and risk of biases when using biological data, especially resources such as iNaturalist. Risk-of-bias tools (e.g.,

Boyd et al., 2022) could be useful for assessing these risks. Perhaps the burden shouldn't be on the metric developer, but I think these kinds of issues and concerns and risks need to be clearly communicated to the reader, so that the metric does not get misused. Again, simulations could help more clearly layout how the structure of the biodiversity data (e.g., sample completeness, geographic representativeness, data density, temporal aspects [e.g., is one area sampled much earlier than another area]) affects the iBS metric.

Moreover, the data requirements for a valid estimate of iBS need to be better communicated. Currently, it is stated in the methods (line 386): 'The species diversity model (D) that is multiplied with a complementary index on the naturalness of land surfaces (N) is critical since it hinges on the quality and evenness of the species data used.' What do you mean by quality? How even does the data need to be? What years of data did you use in the case study (e.g., old data may not be linked to the current environment)? At what scale can you make inferences from iBS? What biodiversity metrics could be included (beyond species richness)?

I think these questions and aspects need to be addressed throughout the manuscript so that it is clear to the reader how best to use (and when to not use) the iBS metric approach. What the data requirements are. How the user can test the validity and adequacy of their iBS results. And what are the potential uses (e.g., what other questions could be asked, what other biodiversity aspects could be included) and limitations of the iBS approach.

N.B. I think Mokany et al., 2022 does a good job of explaining the potential uses and potential limitations of the GDM approach that could be useful as a template.

Minor comments:

(24) 'Many insects are indicators and "passengers" of global change' – unclear what passengers refers to? Could this be made explicit for the reader?

(39) 'Specifically, in Africa, losses in insect biodiversity are linked to declines in other species that feed on insects and crop yield losses due to the scarcity of pollinators3,4.' – this sounds like insect declines are due to losses in species that feed on insects. Perhaps something like this would work better: 'Moreover, losses in insect biodiversity can lead to declines in other species that feed on insects, as well as to crop yield losses due to the scarcity of pollinators3,4.'

(42) 'Overall, biodiversity in African ecosystems is broadly predicted to undergo greater declines than in any other region7.' – this seems like an unnecessarily definitive claim. Especially as the analysis referenced does not include climate change and is forecasting potential future scenarios, not predicting the future. Perhaps something like this would be fairer: 'Overall, biodiversity in African ecosystems is increasingly imperilled now (REF) and likely into the future7.'

(46) 'To varying degrees, depending on the species, order, or group, insect diversity and abundance can be used to estimate overall ecosystem-level biodiversity and environmental integrity …' – what do you mean by species, order, group? Do you mean species, family, order? Or maybe using 'taxonomic group' would work better: 'To varying degrees, depending on the taxonomic group, insect diversity and abundance can be used to estimate overall ecosystem-level biodiversity and environmental integrity …'

(53) ‘In some cases, insect habitat shifts can be used as early warning indicators of ecosystem-level environmental change14 …’ – what do you mean by insect habitat shifts? Would ‘shifts in insect habitat’ be clearer for the reader?

(56) ‘Trophic responses to human transformations that affect species distributions and richness are more rapid at the micro-habitat or local scale (i.e., insect mapping unit) than at national or regional scales15’ – I think a clearer definition of what is being referred to as ‘micro-habitat’ throughout this manuscript is needed. What is an insect mapping unit? What sort of scale is micro? Potentially for insects micro-habitat would refer to <1m (e.g., small patches of bare ground, dead-wood). Micro-habitat is also a very taxon-specific term (i.e., what is micro for one species may not be micro for another). I assume micro is being used due to the high resolution of the EO data, so maybe local-scale is a better term to use throughout?

(65) ‘The Essential Biodiversity Variables 2020 (EBV2020) Initiative …’ – this could benefit from a reference so that the reader can see more about it, as it might not be that widely known.

(81) ‘The metric uniquely considers functional diversity and structural and compositional biodiversity aspects23’ – I can’t see how the metric considers functional diversity? This should either be explained explicitly here or dropped. The metric does not incorporate traits, for example, or the functional contributions of species. I guess there is potential for the metric to do so, so maybe it should be phrased in this way instead?

(96) ‘human impact manifestations’ – strange turn of phrase, would ‘human impacts’ be clearer for the reader?

(114) ‘D denotes habitat diversity (richness), predicted from 0 to 1, where 1 is the most species rich (diverse) space.’ – confusing use of habitat diversity. Surely, D, as used here, denotes species diversity not habitat diversity. Perhaps: ‘D denotes species diversity (here, species richness), predicted from 0 to 1, where 1 is the most species rich location.’ would work better?

(117) ‘Landscape naturalness (N), which denotes various human impacts in relation to functional biodiversity loss, is scaled from 0 to 1, where 1 refers the highest naturalness.’ – I don’t see how N relates to functional biodiversity loss (also see comment on line 81 above). I would stick to the direct relationships of the metrics: ‘Landscape naturalness (N), which denotes various human impacts, is scaled from 0 to 1, where 1 refers the highest naturalness (lowest human impact).’

(143) ‘Ten-meter resolution EO variable data on per pixel “greenness”, wetness”, brightness”’ – I think these terms (greenness, wetness, brightness) need explaining to the reader. Perhaps a definition of what they refer to could be provided in brackets after each word. Also check the use of speech marks in this sentence.

(174) ‘Areas with higher fractional coverage of agroforestry are thus important to maintain local biodiversity status.’ – I’m not sure this assertion is supported by the results. I think the results show that agroforestry has higher levels of biodiversity than non-protected areas, but this does not mean that you need agroforestry to maintain biodiversity. Biodiversity would be maintained better if all agroforestry was converted to protected areas in this case (obviously, this is hypothetical and would impact other factors such as food security). Potentially delete this sentence?

(203) Figure 2 is not colour-blind friendly. Is an alternative colour scheme available?

(243) 'Distributions for SB_FI (n = 50-500, random samples) for Kakamega are illustrated for P, NP, and AF in coral color.' – None of the points are coral coloured?

(256) 'The consistencies of the iBS across the two agro-ecological zones, despite disaggregating the metric along various taxonomic levels and using various human influence variables (i.e., FI and the Naturalness Index) shows the sensitivity of the metric to localized micro-scale biodiversity status.' – I don't see how those results confirm that the metric is sensitive to localized micro-scale biodiversity status? I think more explanation of your thinking here would be beneficial for the reader.

(308) 'meaningfulness' should be 'meaningfully'

(384) 'responds' –should be 'responses'

(422) 'The data were combined using a heuristic data fusion approach described in58.' – I think it would be helpful for the reader to provide a short (i.e., ~1 sentence) explanation here as well.

(431) 'indictors' – should be 'indicators'

(432) 'dragon flies' – should be 'dragonflies'. This needs correcting throughout the manuscript.

(453) 'Use and a machine learning' – delete 'and'

(462) 'computed calculated' – duplication, choose one word.

(500) It's great that the processing time has been included 👍

(542) 'based on based on' – duplication.

(581) 'provided' – 'accessed' instead?

(582) 'http://dmmg.icipe.org/dataportal' – it be much better if this link took the reader to the actual data. I can't seem to find it from searching for iBS.

(583) 'https://github.com/icipe-official' – again this link just takes the reader to the ICIPE GitHub account, not to the repo with the code for this paper.

I hope this review proves useful!

References

Mammola, S., Fukushima, C.S., Biondo, G., Bongiorni, L., Cianferoni, F., Domenici, P., Fruciano, C., Giudice, A.L., Macías-Hernández, N., Malumbres-Olarte, J. and Miličić, M., 2023. How much biodiversity is concealed in the word 'biodiversity'?. Current Biology, 33(2), pp.R59-R60. https://www.sciencedirect.com/science/article/pii/S0960982222019030

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Boyd, R.J., Powney, G.D., Burns, F., Danet, A., Duchenne, F., Grainger, M.J., Jarvis, S.G., Martin, G., Nilsen, E.B., Porcher, E. and Stewart, G.B., 2022. ROBITT: A tool for assessing the risk-of-bias in studies of temporal trends in ecology. Methods in Ecology and Evolution, 13(7), pp.1497-1507. https://besjournals.onlinelibrary.wiley.com/doi/full/10.1111/2041-210X.13857

Mokany, K., Ware, C., Woolley, S.N., Ferrier, S. and Fitzpatrick, M.C., 2022. A working guide to harnessing generalized dissimilarity modelling for biodiversity analysis and conservation assessment. Global Ecology and Biogeography, 31(4), pp.802-821. https://onlinelibrary.wiley.com/doi/10.1111/geb.13459

Reviewer #3 (Remarks to the Author):

In this study the authors set out to develop a new metric of insect biodiversity change, which they apply in the context of insect biodiversity in Africa. When I read the title of this manuscript I was really excited. The topic of understanding insect biodiversity change in Africa is highly important, and a significant gap in the peer-reviewed literature. As the authors mention, lots of crops grown in Africa tend to have a higher pollination dependence, making insect pollinator biodiversity of high importance. Having now read the whole manuscript however, although the manuscript is well written I think on the whole, I don't think the title is an accurate description of what the study itself achieves. I don't think this is necessarily too much of a problem, but I think the authors either need to change their title, or make it really clear why their metric is preferable to others for understanding insect biodiversity change in Africa (or elsewhere). Below I have made a set of major comments, as well as some minor comments for the authors to address.

Major comments:

I'm unfortunately not convinced that the author's metric is altogether different from the BII, which also measures change relative to a natural baseline state. The authors state that the BII is not spatially or pixel explicit, and that the BII does not incorporate actual manifestations of landscape disturbance. The first of these points I don't think is true (for example see de Palma et al 2021), and the second I think is actually technically true of both the BII and the author's iBS. In other words, studies calculating a BII and the author's iBS both use rasters derived from remotely sensed data, in which variables such as reflectance and absorbance are used to infer landscape structure.

It's not completely clear how the authors built their model of insect biodiversity change, but it appears that insect species occurrence from iNaturalist is used to infer species richness. As the authors suggest on L476, I think this is problematic, given the sampling biases inherent in occurrence data such as this. High species richness predicted in very dense urban areas is I think exactly what I would expect if the

model is being heavily influenced by a sampling bias in iNaturalist. The authors present a solution to "select another site within the same overall area with more data density", but it's not clear to me what this means or whether the authors have implemented this approach here.

Minor comments:

Title: I love the title of this manuscript, but I don't think it well describes the content of the paper. The paper really is about the author's metric of insect biodiversity, which is then applied in the context of insect biodiversity in Africa.

L24: where is "passengers" a quote from? If it's not from a particular source I don't think you need the quote marks, and regardless it's not clear to me what you mean by "passengers".

L26: note that for the acronym iBS many people will think "Irritable bowel syndrome". This will also be the case for Googling the term I think. Perhaps something to consider for the name/acronym.

L41: I think this needs to be more specific to be true. Do you mean 50% of crops? 50% of the calorific intake? 50% of total production? From the citation it looks like of the countries censused only Ethiopia makes 50% of its sales from pollinator dependent crops.

L42: I think for some crops (e.g. cocoa) the yield gap in the absence of pollination will be greater than 59%.

L44: more specifically could say continent.

L62: "several indicators"

L88: I think this section would be better described as "Methods" to distinguish from the Introduction

Figure 1 and 3. I would consider a violin plot rather than a boxplot and overlaid dots. With a violin plot you can put across more information about the distribution without overplotting points. Also in the legend you need to say what NP, AF, P, a, b, and c are, and you need to state what each point is.

Figure 2. perhaps note that green and red are not colour blind friendly. What do you mean by 2000km2 here in the legend? Also it's not clear to me what the relevance of describing the climate is in the figure legend.

L278: what do you mean by the 'Natural Index'? I think you need a citation here

L282: it's not clear to me what you mean when you say that the BII is not spatially explicit or pixel explicit. De Palma et al (2018) uses a model to predict total abundance as a function of a number of anthropogenic spatial variables, and then projects this total abundance globally at the pixel level according to a number of anthropogenic rasters.

L283: more specifically the BII is typically calculated as change relative to primary vegetation at a minimal level of land-use intensity. It's not clear to me how this differs conceptually from what the authors present here.

L285: what do you mean by "actual"? Tree height from remotely sensed data will be estimated via a model.

We thank the reviewers for their excellent comments and suggestions, and meticulous observations that will undoubtedly improve the manuscript and contribute to the practical implementation of the metric. Please see our answers (A) to each reviewers' comments below.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

In the study it proposes an index to estimating biodiversity status metric across microhabitats in two agro-ecological zones in Africa using spatially explicit earth observation and insect.

In my opinion the paper fits the scope of the "Communications earth & environment" journal. Firstly, I would like to congratulate the authors for the work done. The topic is very interesting and the study is a very high quality study. Methods are well described and justified. Discussion is completed and well wrote. Despite that, the manuscript needs some revisions to be published.

A: We thank reviewer 1 for these positive and prolific comments, and the below suggestions for improvements.

I provide some key points to complete/improve:

Major comments

1.Introduction

1.1.The introduction should be strengthened by presenting in more detail the state of the art. While the need for action is described, recent and current research as well as research gaps are not sufficiently presented.

A: We agree that the state-of-art should be better emphasized, and a synthesis of current indicators be added to underline the uniqueness of the metric. We added a section at the end of the introduction accordingly, from: "Most biodiversity measures use parametric species richness estimates for various ecosystem or land use types[7,20]. They often disregard…".

Particularly it would be necessary provide a sinthesis of similar indicators to measure/estimate biodiversity status in order to justify the novelty and relevance of the proposed indicator iBS.

A: In the new state-of-art section, we included a synthesis paragraph that alludes to the added value of this metric and justifies its novelty. We would like to highlight the following main novelties;

- Unlike other indicators, the iBS is not affected by broad taxonomic categories/data, but specific insect indicator species.
- Unlike most "other" indicators and metrics investigated, that use generalized and published data often extracted from scientific papers or reports with assumptions about the coordinates, our metrics uses pixel-based models outputs related to species occurrence/richness.
- Unlike many assessments that rely on the Human Footprint Index as a proxy measure for biodiversity, our metric uses biological data (i.e., Mammola et al., 2023).
- The data sets used in this study are readily available, and given sufficient insect occurrence data, our metric can be straightforwardly computed and replicated.

Moreover, it would be interesting mention to composite indicators to measure the achievement in terms of biodiversity conservation and emphasizing the role of "local indicators" to providing a complete framework about the worldwide biodiversity status should be provided. As example of

composite indicators related that issue, you can see/cite: de Castro-Pardo, M., Martín, J. M. M., & Azevedo, J. C. (2022). A new composite indicator to assess and monitor performance and drawbacks of the implementation of Aichi Biodiversity Targets. Ecological Economics, 201, 107553.

A: Agreed that it makes sense to compare the iBS to composite indicators. In the new section on state-or-the -art, we included a paragraph to explain the differentiation to of the iBS to composite indicators:" Lastly, the iBS is uniquely tied to taxonomic insect orders (i.e, Lepidoptera) that function as known indicators of micro-habitats biodiversity status. This differentiates it from composite indicators that use broad taxonomic groups with various interactions with human impacts, thereby reducing the appropriateness of the indicator as a measure of overall biodiversity (de Palmer et al., 2021)…".
In many sections of the manuscript, we alluded to the relevance of the iBS to measure local biodiversity, e.g. in the introduction, we state: "Trophic responses to human transformations that affect species distributions and richness are more rapid at the local scale (i.e., insect mapping unit) than at national or regional scales[15]. Localized and/or sudden land transformation often abruptly affect insect life cycles, nesting (i.e., oviposition sites), and foraging behavior16,17". Furthermore, also in the introduction (newly added section), we state: ". Many biodiversity measures render different trends at local and regional levels. This leads to a hindrance in the adoption of relevant policies. In part, inconsistent biodiversity measures are due to the exclusion of localized biotic data, the effects of landscape patterns and measures not being sensitive (or "over sensitive") to biodiversity status over a range of human impacts[22].".

Although in the Discussion section some indicators are discussed, a brief relation of the state of the art should be presented in the beginning of the manuscript to present the reader with a complete contextual framework and emphasizes the relevance of the proposed indicator (which, in my opinion, is relevant). In the Discussion section, you could maintain the comparison between the results using these indicators and yours and analyzing the merits and advantages of your proposal.

I suggest to adding this information as a separate (sub) section after Introduction.

A. Agreed. Please see our answer to comment 1 above.

1.2.The aim/s of the study should be clearly defined. I presume, at least, two objectives, the proposal of the "new" (if the authors could justify the novelty) indicator capable to involve the local particularities and the aims related to the application of this metric. I'm not sure if the application is an objective by itself or only the validation of the proposed index. This point should be well clarified in the manuscript and maybe in the abstract too.

A. Agreed. At the end of the new state-of-art section, we added a paragraph to better explain the objective of the study. Specifically, we mention that the study does not aim to validate the metric over a range of ecological conditions, but rather develop it and show its applicability to measure insect-based biodiversity. We stipulate that the metric provides a baseline measure on the actual local biodiversity status and is updatable whenever data becomes available in future.

2.Results

Spatial patterns at localized sites

How analyzed micro-habitats were selected? Randomly? This should be clearly described.

A: We added a new section in the "method protocol", titled: "Guidance on data and model requirements, and uncertainties of the approach" to explain and discuss in detail how insect

occurrence data (density of the points from the iNaturalist), agro-ecological zones, that need to be suitable for the ecology of the insects used (see also new implementation suitability map for Africa in Figure 4), and the tree heights data sets (Table 5) were used to select the two study sites used in this work. Using these criteria, the two sites were selected.

3.Discussion

As the conclusions of this study have been described in this section, the title should be “Discussion and conclusion”.

A: We agree, and renamed the section “Discussion and Conclusion”

Minor comments

L62: Change “indictors” by “indicators”

A: Changed accordingly

Reviewer #2 (Remarks to the Author):

The authors present a biodiversity metric (iBS) that integrates biological information (here, insect species richness) with data on human impacts (here, landscape naturalness). The metric can make use of high resolution data and can be applied to different contexts and questions (e.g., biodiversity status of protected areas). The metric has some similarities to the Biodiversity Intactness Index, as well as the Biodiversity Habitat Index (https://www.bipindicators.net/indicators/biodiversity-habitat-index), which focuses on composition, using Generalized Dissimilarity Models, rather than richness. In my opinion, the metric could be a useful addition to the literature, as its calculation is relatively simple and it can make use of high resolution Earth Observation data. Plus, it goes beyond the regularly used Human Footprint Index and similar, which do not include biological information (see Mammola et al., 2023 [full references included below], which highlights the issue of many papers talking about biodiversity without sampling biodiversity data). The manuscript is generally well written and clear to the reader. However, I do have a few major comments, and quite a few minor comments, which I have listed below (line number in brackets).

A: Thank you to reviewer 2 for the meticulous comments and suggestions. We appreciate the above positive analyses of the writing style and the overall uniqueness of the approach presented.

My first general comment is that I am, yet, unconvinced that the biodiversity information included in the iBS is capturing the intended attributes it seeks to. This doesn’t mean that I think the metric is not working, just that I would like more evidence and clarity about how it is capturing the biological aspects. How do the D and N components influence the iBS metric? Fig. 2A shows that iBS is highly correlated with land cover (here, satellite imagery), and although this is less the case for Fig. 2B, I’m still concerned that the maps of iBS generally reflect a layer of human impacts, rather than a true biodiversity metric. This concern is also raised by the fact that the results from the, very different regions and taxonomic groups, included in Fig. 3 appear to have similar values for iBS. Does N often have a larger variance than D and therefore more influence on iBS? Does D have a more skewed distribution and therefore less influence on iBS?

A: Thank you for these prolific comments. We produced a text document as supplementary material, titled.;” Contributions and sensitivity of each of the input variables (*N* and *D*) to compile the iBS metric”. In this document, the variance, skewness, variance, Standard Deviation, Means and Medians for the *N* and *D* layers were computed. Histograms were also produced. For Kakamega in Kenya, it can be seen (Table 1), that the variance values for *N* and *D* are 0,021 and 0,025,

respectively. St. Lucia, the variance values are 0,101 and 0,045 for the *N* and *D* inputs, respectively (Table 2). Both inputs to the iBS share similar variances values for *N* and *D* for Kakamega, while for St. Lucia, N has a larger variance and thus more influence. From the Pearson's correlations in Figure 3 and 4, however, it becomes apparent that *D* accounts for more data variation than N. This is because the correlation between the iBS and *D* is higher than the correlation between the iBS and N. There is also a histogram overlap between the iBS and *D*, as opposed to the overlap between iBS and N. Regarding skewness, the values for Kakamega showed a stronger skewness for *D* (3,229) than for N (-1,353), while for St. Lucia the skewness for N was larger. By comparing both sites, it becomes apparent that neither *D* nor *N* have greater influence over one another. Further, we can deduce, that *N* does not have a larger variance and land cover influence than *D*, and that both variables hold each other fixed, depending on the underlying biodiversity (from *D*) and landscape patterns (ergo both variables were found to be equally important in forming the iBS).

I think there are a few things that could be done to answer these questions and potentially reduce these concerns for the reader. First it would be informative (if not essential) to show the contributing layers for each of the iBS outputs in the Supplementary. Maps of D and N (and maybe plots of their distributions) would help the reader to understand the patterns in the data.

A: Agreed. In the new supplementary document, we show both input variables and the computed iBS as spatial data layers (maps) for both sites, respectively (Figures 1 and 2). Value distributions for both variables are illustrated in the histogram graphs (Figure 5 and 6, respectively). Spatial patterns, correlations and amplifications for/between both input variables are explained in supplementary materials text.

Correlations between the inputs (D and N) and the final metric (iBS) would also be informative to see how the inputs influence the results. Simulations would be an even better step, to show how different data structures affect the iBS metric. For instance how does the mean and sd of the input data, or the distribution (normal, skewed) of the input data, affect the influence/weight of the D and N components.

A: Partly agreed. Essentially, the Pearson's correlations illustrated in both Figures 3 and 4 show a higher correlation between *D* and the iBS, than between *N* and the iBS. However, skewness and variances vary for *D* and *N* when comparing the two sites (see explanation above). In the supplementary section we now also state: "For both study areas, the Pearson's correlation between *N* and *D* are the lowest among all correlations, showing the distinctness and complementary features of individual input data when creating iBS". So, having both input data with the lowest correlation, it makes sense to think that they are non-residual and provide a different set of information in forming the iBS metric. This, and the above-mentioned variability in skewness and variance, means that weighting of one variable over the other one, cannot be recommended. Rather, data densities for *D* should be considered when fine tuning site selection, as shown in Figure 4 in the new method protocol.

Basically, I want more evidence that this metric is capturing the properties it sets out to (i.e., insect biodiversity status), rather than attributes that may (or may not) be correlated with biodiversity status (i.e., cumulative human impacts).

A: We again thank reviewer 2 for the concerns about the attribution of the metric and suggestions on how to better illustrate the influence of the various components as supplementary materials. We agree that biodiversity metrics must be related to the objectives of measurements. In our case, we aimed to show the possibilities of using local scale data and biotic (*D*) and spatially explicit human

impact *(N*) as a unique way to capture biodiversity status (not biodiversity per se). We addressed the need for explicit baseline data using species that are explicitly indicators for biodiversity status. In the introduction we added a paragraph to explain the uniqueness of the metric in this regard.

In addition, although frequently used, species richness is a very blunt measure of biodiversity, it does not capture many other aspects of biodiversity that may be particularly important across a landscape (e.g., species representativeness, species composition [beta diversity], complementarity). I think this raises the point that it needs to be clear how this approach differs from other work that better captures these aspects (e.g., Generalized Dissimilarity Modelling - Biodiversity Habitat Index). Is the intention that this is a more computationally feasible metric?

A: Agreed that the differentiation of both needs to be made and explained. In the introduction a paragraph is added accordingly; "The iBS metric does not consider the composition of species and their changes in time and space (as considered in beta biodiversity dissimilarly approaches), nor aspects related to interspecies interactions, and population structure as determined by genetic processes and structure [27,26]. Unlike the case for dissimilarity/similarity model approaches [28], the iBS does not aim to compare different species or species communities and quantify these differences between two sites and their ecological conditions. Rather, the metric aims to provide an earth observation (pixel-based) and straightforward measure (i.e., computationally easy) for local biodiversity based on insect -assemblages as collective orders that represent indicator species for intact landscapes".
Furthermore, we pointed out in several sections throughout the manuscript that the iBS would not work when implementing for an area that encompasses various agro-ecological or climate conditions. In the introduction, for instance, it is stated; "When using our metric approach, the prerequisite is that the area of interest lies within a common climate zone". The iBS provides a biodiversity status baseline (valid now and updatable later) for a specific agro-ecological zone.

Is this metric only to be interpreted at local scales (i.e., local biodiversity status not landscape biodiversity status)? I'm concerned that when the iBS metric is averaged it may become a poor indicator of biodiversity status. E.g., you could have a landscape with the same 100 species in every pixel, or a landscape with a different 80 species in each pixel – which is more biodiverse? Is this metric only informative at local scales, if so this should be stated. What is the USP of the metric? It needs to be clear how the metric can be appropriately used.

A: Thank you for this comment. In line 8 of the abstract, we refer to "micro-habitats" and, furthermore, in the introduction we allude to the need for local or micro-scale applicability of metrics: "Spatially explicit indicators and metrics that make use of biotic inputs are needed to quantify the status of biodiversity at local levels in Africa...". In the "method protocol", we mention at several places that this is a local scale metric, and confined to a common climate or agro-ecological zone; e.g., "Our metric, however, is not entirely scale independent, as it is implemented with spatially explicit satellite data at 10-25-meter resolution for site -specific analysis. However, given that both inputs or model variables are accurate (i.e., model AUC >93 for *D*), the metric can be aggregated with great confidence within a given climate zone, or used complimentarily to existing regional scale biodiversity status assessment". We also included a new practical guideline section on the criteria of how the metric can and should be used. This includes ascertaining the data densities from the iNaturalist, and suitable agro-ecological zoning for the taxa being used (see new section "Guidance on data and model requirements, and uncertainties of the approach" in the "method protocol"). Furthermore, the aspect of diversity and number of different species are present versus how evenly they are represented (*i.e.,* 1,000 elephants and 1 lion is less diverse than 500 elephants and 500 lions). In this context, we added a new section in the new subsection "Other biases

associated with the iNaturalist data and site selection that users should consider" in the method protocol. Herein, we assessed the species diversity scores and bootstrapped confidence limits for the used insect order lepidoptera and the Polyphaga sub-order. The Brillouin Index diversity method was used, which is an improvement of the Shannon Index. The results showed that the diversity scores for both insect taxa orders and sites lie within the 95% confidence intervals, respectively, insinuating high precision to measure diversity when using these insect orders (see new Table 7). This essentially addresses uneven detectability where some species are highly recorded while others like rare species are less recorded.
Furthermore, the *D* variable is predicted from a species diversity model, and this is one input to the metric. Species diversity models predict distribution of species across larger areas, using localized information. But, they are only indicative of local species richness and diversity, since other factors also contribute to local biodiversity. By including tree heights (3D habitat structure) and 2-dimensinal explicit data in vegetation vigor (from spatially explicit satellite data), we aimed to predict local insect biodiversity with *D* as best as possible.

Following on from concerns about the influence of D and N, I also think that it is unclear how sensitive the metric is to the structure of the biodiversity data (see Isaac & Pocock 2015 for concerns about potential bias in biological data). Species richness is very sensitive to sampling effort (Roswell et al., 2021), so how does the iBS metric deal with this? There are many potential issues and risk of biases when using biological data, especially resources such as iNaturalist. Risk-of-bias tools (e.g., Boyd et al., 2022) could be useful for assessing these risks. Perhaps the burden shouldn't be on the metric developer, but I think these kinds of issues and concerns and risks need to be clearly communicated to the reader, so that the metric does not get misused. Again, simulations could help more clearly layout how the structure of the biodiversity data (e.g., sample completeness, geographic representativeness, data density, temporal aspects [e.g., is one area sampled much earlier than another area]) affects the iBS metric.

A: Thank you for these prolific comments. As stated above, we compiled a practical guideline in the "method protocol", titled "Guidance on data and model requirements, and uncertainties of the approach". The new section illustrates how data density from the iNaturalists, and tree height component (from 25-meter GEDI data), and agro-ecological zones are used to guide the selection of areas for implementation and illustrate risks and data requirements associated with implementing the method in Africa. Areas not suitable/suitable for implementation are mapped (Figure 4), and users should also consider the accuracies for D, that are related to data density and number of occurrence data sets used (Figure 4). In Figure 4 (B), we simulate, using current data densities on the used insect taxa from the iNaturalist, how the metric could be implemented in future (current year + x). We assume that according to Mammola et al. (2023) more citizen science data on insects will be collected in future in areas with political stability and along the fringes of the current areas well sampled (left figure). It was shown that the area suitable for implementing the metric increases from around 5% of the total African land mass (currently) to about 8% of the total mass. In how many years this is would be achievable is unclear. On the timing of the samples, we assumed that human transformation, measured by *N*, and diversity/richness, measured by *D*, does not change over a period of 5 years, the time frame used for extracting data from the iNaturalists. The new section states: "To overcome temporal bias, only the most recent iNaturalist insect occurrence data (from 2017-2022) were used for diversity modelling *(D* variable), for both sites…". The risk of unevenness of the occurrence data and *D* variable being related to diversity was probed in using a diversity index score that we newly computed. This is explained in the new section in the method protocol; "To further probe possible sampling bias in the iNaturalist insect data for both study sites, we calculated

the species diversity scores and bootstrapped confidence limits for the used insect order Lepidoptera and the Polyphaga sub-order…".

Moreover, the data requirements for a valid estimate of iBS need to be better communicated. Currently, it is stated in the methods (line 386): 'The species diversity model (D) that is multiplied with a complementary index on the naturalness of land surfaces (N) is critical since it hinges on the quality and evenness of the species data used.' What do you mean by quality? How even does the data need to be? What years of data did you use in the case study (e.g., old data may not be linked to the current environment)? At what scale can you make inferences from iBS? What biodiversity metrics could be included (beyond species richness)?

A: Thank you for these comments. In the above addresses to the comments, we already explained that we produced a new section in the "method protocol", titled "Guidance on data and model requirements, and uncertainties of the approach", and in Figure 4 to explain the data ramification (tree cover needs to be above 3 meter for the insect orders used) and how the occurrence data density for *D* can be considered to implement the metric in Africa. The sentence referring to the quality of the inputs (specifically *D)* is unclear, we agree. We, thus, re-wrote the sentence to better emphasis and explain the data requirements; "The species diversity model *(D*) that is multiplied with a complementary index on the naturalness of land surfaces *(N)* critically hinges on the evenness of the species data used". In the new section, we alluded to the timeline assumptions of the occurrence data, linked to the timing of the satellite observations. We also already refer to the limits of the scalability of the metric, and that the metric is used for localized conditions, due to the small-scale characteristics of insect habitats. The metric includes species richness/abundance only partly, through D. As the metric is influenced by *D* and *N*, the metric is an indicator for biodiversity status, site-level intactness, and integrity (i.e., Scholes and Biggs, 2005) but with more spatial explicitness and without a time-series component. In the introduction, we included a new section that explains the uniqueness of the metric and more clearly explains what biodiversity-relevant information the metric measures: "This metric exhibits various specific novelties, differences, and complementarities over existing composite biodiversity indicators…". The *D* variable is related to insect abundance and diversity (essentially "richness") since the assemblages include various species groupings (also see comment above). The variable *N* is a good proxy for urbanization since the open street layer was one input and can be a proxy for land use and land cover. However, *D* is a weak proxy for overall species biodiversity. The result, iBS, is related to the conservation status of a site and likewise represents local ecosystem intactness. High values iBS were found to correlate well with local ecosystem "intactness", areas under protection (protected areas, PA) and a good proxy for vegetation density. High iBS were found for the coastal forests in St. Lucia, and the Kakamega protected forest in Kenya. Both ecosystems are mapped as intact protected forests by the recent Biodiversity Intactness Index (BII) (de Palmer et al., 2021).

I think these questions and aspects need to be addressed throughout the manuscript so that it is clear to the reader how best to use (and when to not use) the iBS metric approach. What the data requirements are. How the user can test the validity and adequacy of their iBS results.

A: Thank you again. We agree and, as already stated, included a new section in the "method protocol" titled; "Guidance on data and model requirements, and risks and validity of the approach". This section outlines the accuracies of the various D model outputs as a function of the number of references data points used and the occurrence data density. The section also assesses risks related to field sampling when using the iNaturalist samples/data and other area selection criteria.

And what are the potential uses (e.g., what other questions could be asked, what other biodiversity aspects could be included) and limitations of the iBS approach. N.B. I think Mokany et al., 2022 does a good job of explaining the potential uses and potential limitations of the GDM approach that could be useful as a template.

A: Once again thank you for the comment and referring to existing approaches regarding assessing potential limitations. As already noted in the above responses, the new section in the "method protocol" on data requirements also alludes to risks inherent in the iNaturalists, assumptions made in this study and limitations regarding specifically site selection. Above we also already emphasized the characteristics of diversity/richness modelling and how biases were addressed for the variable D in this context. We already also explained in the new section in the introduction how this approach differs from GDM. Essentially, for our approach to be implemented, we recommended to use data density measures (spread), the use of agro-ecological zones corresponding to the ecology of the insect (or other species/taxa), and other ecological criteria (such as tree height) to probe suitability of the approach of a given area of interest.

Minor comments:

(24) 'Many insects are indicators and "passengers" of global change' – unclear what passengers refers to? Could this be made explicit for the reader?

A: Thank you for this comment. We agree that this phrase creates confusion. We deleted the phrase in the abstract. The phrase on "insects being indicators of global change" effects is retained and already encompasses what we intended to refer to.

(39) 'Specifically, in Africa, losses in insect biodiversity are linked to declines in other species that feed on insects and crop yield losses due to the scarcity of pollinators3,4.' – this sounds like insect declines are due to losses in species that feed on insects. Perhaps something like this would work better: 'Moreover, losses in insect biodiversity can lead to declines in other species that feed on insects, as well as to crop yield losses due to the scarcity of pollinators3,4.'

A: Agreed, we re-wrote this sentence according to your suggestion.

(42) 'Overall, biodiversity in African ecosystems is broadly predicted to undergo greater declines than in any other region7.' – this seems like an unnecessarily definitive claim. Especially as the analysis referenced does not include climate change and is forecasting potential future scenarios, not predicting the future. Perhaps something like this would be fairer: 'Overall, biodiversity in African ecosystems is increasingly imperiled now (REF) and likely into the future7.'

A: Agreed. This is a very good point, and your suggestions sound less like a definitive claim. We thus took up your suggestion and re-wrote this sentence accordingly.

(46) 'To varying degrees, depending on the species, order, or group, insect diversity and abundance can be used to estimate overall ecosystem-level biodiversity and environmental integrity …' – what do you mean by species, order, group? Do you mean species, family, order? Or maybe using 'taxonomic group' would work better: 'To varying degrees, depending on the taxonomic group, insect diversity and abundance can be used to estimate overall ecosystem-level biodiversity and environmental integrity …'

A: We agree somewhat, and re-formulated the sentence as such; "To varying degrees, depending on the species, order, or taxonomic rank, insect diversity and abundance can be used to estimate overall ecosystem-level biodiversity and environmental integrity…"

(53) ‘In some cases, insect habitat shifts can be used as early warning indicators of ecosystem-level environmental change14 …’ – what do you mean by insect habitat shifts? Would ‘shifts in insect habitat’ be clearer for the reader?

A: Agreed, and apologies for the mistake and incorrect sentence. We re-arranged the sentence as suggested.

(56) ‘Trophic responses to human transformations that affect species distributions and richness are more rapid at the micro-habitat or local scale (i.e., insect mapping unit) than at national or regional scales15’ – I think a clearer definition of what is being referred to as ‘micro-habitat’ throughout this manuscript is needed. What is an insect mapping unit? What sort of scale is micro? Potentially for insects micro-habitat would refer to <1m (e.g., small patches of bare ground, dead-wood). Micro-habitat is also a very taxon-specific term (i.e., what is micro for one species may not be micro for another). I assume micro is being used due to the high resolution of the EO data, so maybe local-scale is a better term to use throughout?

A: Thank you for this comment. Local scale is also fine, but microhabitat is a commonly used term for earth observation-based insect studies, and we suggest to nonetheless use “micro-scale” but explain it in the text. In the introduction we added the following sentence: “A micro-scale for a species is characterized by a very specific vegetation structure as well as abiotic conditions (e.g., presence of raw soil, deadwood) and by a specific microclimate, whereby the spatial scale of a microhabitat is relative and species-specific (Habel et al., 2022)”.

(65) ‘The Essential Biodiversity Variables 2020 (EBV2020) Initiative …’ – this could benefit from a reference so that the reader can see more about it, as it might not be that widely known.

A: Agreed, we inserted a reference on the EBVs: “The Essential Biodiversity Variables 2020 (EBV2020)..”.

(81) ‘The metric uniquely considers functional diversity and structural and compositional biodiversity aspects23’ – I can’t see how the metric considers functional diversity? This should either be explained explicitly here or dropped. The metric does not incorporate traits, for example, or the functional contributions of species. I guess there is potential for the metric to do so, so maybe it should be phrased in this way instead?

A: Fully agree with the reviewer. This section i.e., term of functional biodiversity was completely dropped and re-written for clarity and to place the metric into perspective, see new section in the introduction.

(96) ‘human impact manifestations’ – strange turn of phrase, would ‘human impacts’ be clearer for the reader?

A: We fully agree and re-wrote the phrase accordingly

(114) ‘D denotes habitat diversity (richness), predicted from 0 to 1, where 1 is the most species rich (diverse) space.’ – confusing use of habitat diversity. Surely, D, as used here, denotes species diversity not habitat diversity. Perhaps: ‘D denotes species diversity (here, species richness), predicted from 0 to 1, where 1 is the most species rich location.’ would work better?

A: Good point. The phrase was changed accordingly and now reads: “where iBS is the actual (current) insect-based biodiversity status and D denotes habitat species diversity (here, species richness),…”

(117) ‘Landscape naturalness (N), which denotes various human impacts in relation to functional biodiversity loss, is scaled from 0 to 1, where 1 refers the highest naturalness.’ – I don’t see how N relates to functional biodiversity loss (also see comment on line 81 above). I would stick to the direct relationships of the metrics: ‘Landscape naturalness (N), which denotes various human impacts, is scaled from 0 to 1, where 1 refers the highest naturalness (lowest human impact).’

A: Thanks for this comment. The phrase referring to “functional biodiversity loss” was deleted.

(143) ‘Ten-meter resolution EO variable data on per pixel “greenness”, wetness”, brightness”’ – I think these terms (greenness, wetness, brightness) need explaining to the reader. Perhaps a definition of what they refer to could be provided in brackets after each word. Also check the use of speech marks in this sentence.

A: We agree and inserted a short explanation in parathesis, the phrase now reads; “greenness ”(measurement value for chlorophyll active of vegetation), wetness”(measurement value of surface water), brightness”(measurement value of interactions between soil and canopy moisture, usually sensitive to bare soil surfaces)…”

(174) ‘Areas with higher fractional coverage of agroforestry are thus important to maintain local biodiversity status.’ – I’m not sure this assertion is supported by the results. I think the results show that agroforestry has higher levels of biodiversity than non-protected areas, but this does not mean that you need agroforestry to maintain biodiversity. Biodiversity would be maintained better if all agroforestry was converted to protected areas in this case (obviously, this is hypothetical and would impact other factors such as food security). Potentially delete this sentence?

A: Agreed that this is confusing. To prevent ambiguities, the whole sentence was deleted.

(203) Figure 2 is not colour-blind friendly. Is an alternative colour scheme available?

A: This is a good point, we changed Figure 2 accordingly, made it compatible for color blind persons.

(243) ‘Distributions for SB_FI (n = 50-500, random samples) for Kakamega are illustrated for P, NP, and AF in coral color.’ – None of the points are coral coloured?

A: See above, the figure was changed

(256) ‘The consistencies of the iBS across the two agro-ecological zones, despite disaggregating the metric along various taxonomic levels and using various human influence variables (i.e., FI and the Naturalness Index) shows the sensitivity of the metric to localized micro-scale biodiversity status.’ – I don’t see how those results confirm that the metric is sensitive to localized micro-scale biodiversity status? I think more explanation of your thinking here would be beneficial for the reader.

A: Agreed that this is wrongly worded. We changed the sentence and added an additional sentence to further explain this. The new sentence reads: “…shows the relevance sensitivity of the metric as a reliable proxy for micro-scale biodiversity status. The metric values are comparable between taxa and sites despite different underlying landscape patterns. As stipulated, indicator insects can be used as a proxy for overall site-specific biodiversity status”.

(308) ‘meaningfulness’ should be ‘meaningfully’

A: Changed accordingly.

(384) ‘responds’ –should be ‘responses’

A: Changed accordingly

(422) ‘The data were combined using a heuristic data fusion approach described in58.’ – I think it would be helpful for the reader to provide a short (i.e., ~1 sentence) explanation here as well.

A: Thanks for this and agreed. We added more detail to this section and re-wrote the paragraph as follows; “The Naturalness Index (*N*) consists of four human influence proxies namely, population density, land transformation, accessibility, and electrical power infrastructure that are measured with the geodata Gridded Population of the World v4.11, WorldCover V100, OpenStreetMap, and VIIRS Stray Light Corrected Nighttime Day/Night Band Composites Version 1 [64], respectively. While the GPW, WorldCover and Nighttime Lights data sets are available globally in the Google Earth Engine platform, OSM data were extracted manually from www.openstreetmap.org. The data were then combined using a heuristic data fusion approach described in[65]. The input geodata were converted to weights by applying preprocessing steps and summed up on a pixel level. The resulting product is then inversely normalized to the range [0;100] to form the Naturalness Index”.

(431) ‘indictors’ – should be ‘indicators’

A: Changed accordingly.

(432) ‘dragon flies’ – should be ‘dragonflies’. This needs correcting throughout the manuscript.

(453) ‘Use and a machine learning’ – delete ‘and’

A: Changed accordingly

(462) ‘computed calculated’ – duplication, choose one word.

A: Corrected accordingly

(500) It’s great that the processing time has been included 👍

A: Thank you

(542) ‘based on based on’ – duplication.

A: Corrected

(581) ‘provided’ – ‘accessed’ instead?

A: Changed accordingly

(582) ‘http://dmmg.icipe.org/dataportal’ – it be much better if this link took the reader to the actual data. I can’t seem to find it from searching for iBS.

A: A new link was given that now includes a short description, the original input data, the script used for the tasseled cap computation, and results from the study areas; https://dmmg.icipe.org/dataportal/dataset/measuring-insect-based-biodiversity-status-in-africa

(583) ‘https://github.com/icipe-official’ – again this link just takes the reader to the ICIPE GitHub account, not to the repo with the code for this paper.

A: A new link was given that now includes a short description, the original input data, the script used for the tasseled cap computation, and results from the study areas; https://dmmg.icipe.org/dataportal/dataset/measuring-insect-based-biodiversity-status-in-africa

I hope this review proves useful!

A: yes, the review comments are very useful

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Reviewer #3 (Remarks to the Author):

In this study the authors set out to develop a new metric of insect biodiversity change, which they apply in the context of insect biodiversity in Africa. When I read the title of this manuscript I was really excited. The topic of understanding insect biodiversity change in Africa is highly important, and a significant gap in the peer-reviewed literature. As the authors mention, lots of crops grown in Africa tend to have a higher pollination dependence, making insect pollinator biodiversity of high importance. Having now read the whole manuscript however, although the manuscript is well written I think on the whole, I don't think the title is an accurate description of what the study itself achieves. I don't think this is necessarily too much of a problem, but I think the authors either need to change their title, or make it really clear why their metric is preferable to others for understanding insect biodiversity change in Africa (or elsewhere). Below I have made a set of major comments, as well as some minor comments for the authors to address.

A: We are grateful for this comment. Somewhat agreed. With the title we aim to draw the focus to Africa as a data scarce region in this regard. In clearly explaining the data requirements (e.g., data density from the iNaturalist), and risks (unreliable results if implemented over various climate zones) and sampling biases, in the new section in the method protocol, we provide context to the possibilities that exist for Africa-wide implementation. In the "Introduction" we also included a new section on state of the art, that clearly explains the differences between this approach and others,

i.e., why this metric is preferable for biodiversity status baseline mapping (see also answers to reviewer 2 above). As such, we do not prefer to change the manuscript title.

Major comments:

I'm unfortunately not convinced that the author's metric is altogether different from the BII, which also measures change relative to a natural baseline state. The authors state that the BII is not spatially or pixel explicit, and that the BII does not incorporate actual manifestations of landscape disturbance. The first of these points I don't think is true (for example see de Palma et al 2021), and the second I think is technically true of both the BII and the author's iBS. In other words, studies calculating a BII and the author's iBS both use rasters derived from remotely sensed data, in which variables such as reflectance and absorbance are used to infer landscape structure.

A: We agree that the differentiation between the BII and the iBS is somewhat not clearly communicated. We thus re-wrote the whole paragraph that compares the iBS to the BII, specifically, and highlighted some differences more clearly. The new paragraph now reads; "Both the BII and the use biome-specific species abundances and relate this to average changes in biodiversity using high resolution land use data to predict biodiversity changes over time. As opposed to these species fraction estimates for specific land units (i.e., parametrically), the iBS has the advantage that it uses non-parametric per pixel spectral features (i.e., abundances of greenness and wetness in each 20-meter pixel) and high-resolution tree heights data (from the Global Ecosystem Dynamics Investigation mission) to predict explicit insect species abundance and diversity patterns. This is then weighed by the human impact component using the 10-meter high-resolution naturalness index to measure, and not indicate, biodiversity status 41. Our metric is suitable for characterizing the current localized biodiversity status baseline, without making use of an intact baseline reference site. The use of insect indicator species is that they are specific to the overall biodiversity status of a localized site".
Moreover, a new section in the method protocol, titled "Guidance on data and model requirements, and uncertainties of the approach", was included to explain suitability of implementing the metric in Africa, currently and in the future (Figure 4). Herein, we also allude to data ramification (tree cover needs to be above 3 meters for the insect orders used) and how the occurrence data density for diversity modelling can be considered to implement the metric in Africa. Given these data characteristics and dependencies, the iBS is less suitable for seamless and continental biodiversity modelling, compared to the BII.

It's not completely clear how the authors built their model of insect biodiversity change, but it appears that insect species occurrence from iNaturalist is used to infer species richness. As the authors suggest on L476, I think this is problematic, given the sampling biases inherent in occurrence data such as this. High species richness predicted in very dense urban areas is I think exactly what I would expect if the model is being heavily influenced by a sampling bias in iNaturalist. The authors present a solution to "select another site within the same overall area with more data density", but it's not clear to me what this means or whether the authors have implemented this approach here.

A: Thank you for this very good comment. Indeed, the number of points and their density determines the variable D, which is one of the input variables. A new section tiled; "Guidance on data and model requirements, and uncertainties of the approach" was added with sub-section on "Main data and model requirement criteria", and "Other biases associated with the iNaturalist data

and site selection that users should consider". In this section, we explain how biases through preferred sampling, low number of data points within a given area, and chosen an unappropriated site selection, my influence the accuracy of the method, specifically the variable *D*. We computed area weighted point data densities (Table 2) further to confirm the suitability of the two study sites used. Furthermore, the influence the total number of occurrence points had on the data density was assessed, and the criteria tree canopy height and water availability in each site. Furthermore, uneven detectability of data was herein addressed by selecting a wide range of species within the order Lepidoptera and sub-order Polyphaga, and computing the species diversity scores and bootstrapped confidence limits for the used insect order lepidoptera and the Polyphaga sub-order. All these measures confirmed the suitability of the chosen wo study sites, regarding biasness of the iNaturalist data.
The ambiguous sentence "select another site within the same overall area with more data density" is unclear and is now deleted; area selection and data density is already dealt with in the new data requirements section.

Minor comments:

Title: I love the title of this manuscript, but I don't think it well describes the content of the paper. The paper really is about the author's metric of insect biodiversity, which is then applied in the context of insect biodiversity in Africa.

A: Thanks again for the comment. As already explained in the above answers, we added a section to contextualize the data needs, risks, and possibilities of the metric. We already mentioned in many sections of the manuscript, that the metrics is localized and dependent on insect occurrence data (i.e., density) and showed how this limits its use over much of Africa (Figure 4). As the focus is Africa, and insects and we are developing a measure that can be used complementary to others, we suggest retaining the title as is.

L24: where is "passengers" a quote from? If it's not from a particular source I don't think you need the quote marks, and regardless it's not clear to me what you mean by "passengers".

A: Agreed that this is confusion. The wording "passenger" was deleted.

L26: note that for the acronym iBS many people will think "Irritable bowel syndrome". This will also be the case for Googling the term I think. Perhaps something to consider for the name/acronym.

A: Thank you for this comment. We tend to disagree. We put forward the acronym since it is short and straightforward.

L41: I think this needs to be more specific to be true. Do you mean 50% of crops? 50% of the calorific intake? 50% of total production? From the citation it looks like of the countries censused only Ethiopia makes 50% of its sales from pollinator dependent crops.

A: Thank you for this meticulous observation. We have corrected the sentence, that now reads: "In some African countries, between 15 to 40% of all calories, protein, and iron nutrient intakes come from are pollinator dependent crops5, Insect declines can thus result in cause yield gaps, that can be estimated at 59% or more for some seed crops[6]".

L42: I think for some crops (e.g. cocoa) the yield gap in the absence of pollination will be greater than 59%.

A: Thanks again. We have corrected this by the above newly formulated sentence.

L44: more specifically could say continent.

A: Agreed. The word “throughout” was added to emphasize that local sites site is missing in all of Africa.

L62: “several indicators”

A: Corrected

L88: I think this section would be better described as “Methods” to distinguish from the Introduction

A: Here we disagree. There is already the method protocol section that details the methods, data needs and background information on how to replicate the approach (methodologically).

Figure 1 and 3. I would consider a violin plot rather than a boxplot and overlaid dots. With a violin plot you can put across more information about the distribution without overplotting points. Also in the legend you need to say what NP, AF, P, a, b, and c are, and you need to state what each point is.

A: Thanks you for this prolific suggestion. The figure and legend was changed accordingly.

Figure 2. perhaps note that green and red are not colour blind friendly. What do you mean by 2000km2 here in the legend? Also it’s not clear to me what the relevance of describing the climate is in the figure legend.

A: Agreed that the km2 and climate zone may be irrelevant. We deleted the climate reference and described that the 2,000 km2 refers to the area sizes. The Figures were re-coded for color blind purposes (see new Figure 2 in manuscript)

L278: what do you mean by the ‘Natural Index’? I think you need a citation here

A: Agreed, the relevant reference was added

L282: it’s not clear to me what you mean when you say that the BII is not spatially explicit or pixel explicit. De Palma et al (2018) uses a model to predict total abundance as a function of a number of anthropogenic spatial variables, and then projects this total abundance globally at the pixel level according to a number of anthropogenic rasters.

A: Agreed, see comments above. We amended this paragraph and alluded to the differences, “Both the BII and the use biome-specific species abundances and relate this to average changes in biodiversity using high resolution land use data to predict biodiversity changes over time. As opposed to these species fraction estimates for specific land units (i.e., parametrically), the iBS has the advantage that it uses non-parametric per pixel spectral features (i.e., abundances of greenness and wetness in each 20-meter pixel) and high-resolution tree heights data (from the Global Ecosystem Dynamics Investigation mission) to predict explicit insect species abundance and diversity patterns. This is then weighed by the human impact component using the 10-meter high-resolution naturalness index to measure, and not indicate, biodiversity status 41. Our metric is suitable for characterizing the current localized biodiversity status baseline, without making use of an intact baseline reference site. The use of insect indicator species is that they are specific to the overall biodiversity status of a localized site”.

L283: more specifically the BII is typically calculated as change relative to primary vegetation at a minimal level of land-use intensity. It's not clear to me how this differs conceptually from what the authors present here.

A: Agreed that this needs to be differentiated. Please see our comments above, in which we explain that we do not use a reference site and a dissimilarity approach (as taken in beta or meta biodiversity assessments). Rather we assess localized patterns (alpha diversity), largely depending on occurrence data availability for indicators insect species.

L285: what do you mean by "actual"? Tree height from remotely sensed data will be estimated via a model.

A: Thank you for this remark. Agreed. We deleted "actual" accordingly.

17th May 23

Dear Dr Landmann,

Your manuscript titled "Measuring insect-based biodiversity status in Africa" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alienor Lavergne, PhD

Associate Editor

Communications Earth & Environment

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear authors,

Congrats for the work done. In my opinion, the requested changes have been covered. You have provide a high quality manuscript.

Well done.

Reviewer #2 (Remarks to the Author):

I previously reviewed this manuscript and believe the authors have done a good job at revising the manuscript. The new analysis helps reassure me (and hopefully the readers) that the iBS metric captures the dynamics it sets out to (although I'm still apprehensive of its sensitivity to data availability, echoing Reviewer 3's concerns). Still, I believe the updated manuscript is much improved and only have a few remaining minor comments.

(1) "Measuring insect-based biodiversity status in Africa" – I share some concerns with Reviewer 3 that the title does not capture the exact objectives of the paper. It seems to imply that an assessment of insect biodiversity across the whole of Africa has been made. Potentially more representative titles would be something like: "Developing an insect-based biodiversity metric for Africa" or "Developing a local-scale insect-based biodiversity metric for Africa" or "The potential of measuring insect-based biodiversity status in Africa".

(101) "Unlike most other indicators investigated, the iBS is not affected by broad taxonomic datasets that are ecologically restricted (not covering larger landscapes or ecologically diverse areas.22." – unclosed bracket. Two full stops.

(207) "Fig.1" – check spacing after 'Fig.' throughout manuscript

(252) Figure 2 – panels A and B use different colour schemes (the 'very high' category is different shades of green). It would be better if these were consistent. I also find the chosen colour scheme difficult to read and understand, perhaps something like this would be colour-blind friendly and more intuitive: https://colorbrewer2.org/#type=sequential&scheme=YlGnBu&n=5 or a palette from here https://cran.r-project.org/web/packages/viridis/vignettes/intro-to-viridis.html?

(Supplementary Material) The maps in the Supplementary are not colour-blind friendly.

Reviewer #3 (Remarks to the Author):

Thanks to the authors for a revision of their manuscript. I do however still have a slight reservation.

Primarily I still find it hard to follow how the authors have calculated their iBS metric. The methods are distributed between the sections “Introduction”, “Characteristics and implementation of the metric”, “Methods Protocol”, “Guidance on data and model requirements, and uncertainties of the approach”, and "Other troubleshooting”. I would really like to have seen the latter part of the Introduction switched to a Methods section, and then any content that needs to be seen at that stage brought within the Methods. At the very least the authors need to provide more of the code used to calculate the iBS, since the code made available appears to just be the random forest predicting presence and pseudo absence on the basis of greenness, wetness, brightness, and tree height.

We thank the reviewers again for their excellent comments and suggestions. Please see our answers (A) to each reviewers' comments below.

Reviewer #1 (Remarks to the Author):
Dear authors,
Congrats for the work done. In my opinion, the requested changes have been covered. You have provide a high quality manuscript.
Well done.
A: We thank reviewer 1 for these positive and prolific comments.

Reviewer #2 (Remarks to the Author):
I previously reviewed this manuscript and believe the authors have done a good job at revising the manuscript. The new analysis helps reassure me (and hopefully the readers) that the iBS metric captures the dynamics it sets out to (although I'm still apprehensive of its sensitivity to data availability, echoing Reviewer 3's concerns). Still, I believe the updated manuscript is much improved and only have a few remaining minor comments.
A: We thank reviewer 2 for the complements and are thankful for further comments and suggestions to further improve the manuscript.

(1) "Measuring insect-based biodiversity status in Africa" – I share some concerns with Reviewer 3 that the title does not capture the exact objectives of the paper. It seems to imply that an assessment of insect biodiversity across the whole of Africa has been made. Potentially more representative titles would be something like: "Developing an insect-based biodiversity metric for Africa" or "Developing a local-scale insect-based biodiversity metric for Africa" or "The potential of measuring insect-based biodiversity status in Africa".
A: Thank you for this comment. We agree that the title is too topic based and overzealous and does not most accurately reflect the content. We disagree that "developing" is most suitable word in the title. We thus changed the title to: "Insect diversity can predict biodiversity status in Africa".

(101) "Unlike most other indicators investigated, the iBS is not affected by broad taxonomic datasets that are ecologically restricted (not covering larger landscapes or ecologically diverse areas.22." – unclosed bracket. Two full stops.
A: Thank you for this comment. We eliminated the opening bracket and the unnecessary full stop .

(207) "Fig.1" – check spacing after 'Fig.' throughout manuscript
A: Thank you for this comment. We double-checked the spacing of all figure names in the paper.

(252) Figure 2 – panels A and B use different colour schemes (the 'very high' category is different shades of green). It would be better if these were consistent. I also find the chosen colour scheme difficult to read and understand, perhaps something like this would be colour-blind friendly and more intuitive: https://colorbrewer2.org/#type=sequential&scheme=YlGnBu&n=5 or a palette from here https://cran.r-project.org/web/packages/viridis/vignettes/intro-to-viridis.html?
(Supplementary Material) The maps in the Supplementary are not colour-blind friendly.
A: Thank you for this comment. We used the palettes from the source you recommended to modify the colors to be colorblind friendly. We also made certain that the colors in both maps of figure 2

were consistent across all classes.

Reviewer #3 (Remarks to the Author):

Thanks to the authors for a revision of their manuscript. I do however still have a slight reservation.

Primarily I still find it hard to follow how the authors have calculated their iBS metric. The methods are distributed between the sections “Introduction”, “Characteristics and implementation of the metric”, “Methods Protocol”, “Guidance on data and model requirements, and uncertainties of the approach”, and "Other troubleshooting”. I would really like to have seen the latter part of the Introduction switched to a Methods section, and then any content that needs to be seen at that stage brought within the Methods.
A: Thank you for this constructive feedback. We agree and placed the latter part of the section “Characteristics and implementation of the metric” in the “method protocol”. The text was placed in a new section in the “method protocol” that specifically describes the methodology and overall approach. Some of the text redundancies referring to the methods in the “Anticipated results” were deleted.

At the very least the authors need to provide more of the code used to calculate the iBS, since the code made available appears to just be the random forest predicting presence and pseudo absence on the basis of greenness, wetness, brightness, and tree height.
A: Thank you for this comment. An R code for computing the metric is now accessible in the repository.