nature portfolio

Peer Review File

Predicted decline in common bird and butterfly species even under conservation policy scenarios in Europe

Corresponding Author: Dr Stanislas Rigal

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

Version 0:

Decision Letter:

15th December 2025

*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 Rigal,

Your manuscript entitled "Predicted decline in common bird and butterfly species despite conservation policies in Europe" has now been seen by three reviewers, whose comments are attached. My apologies again for the delay in gathering these reviews.

The reviewers have raised a number of concerns which will need to be addressed before we can offer publication in Nature Ecology & Evolution. We will therefore need to see your responses to the criticisms raised and to some editorial concerns, along with a revised manuscript, before we can reach a final decision regarding publication.

We therefore invite you to revise your manuscript taking into account all reviewer and editor comments. Please highlight all changes in the manuscript text file.

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:

* 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.

* Extended Data Figures - please ensure that any supplementary figures and tables that are crucial to the manuscript’s conclusions are converted into Extended Data figures and tables to increase visibility of these data. Extended Data figures and tables are online-only (present in the online PDF and full-text HTML versions of the paper), peer-reviewed display items that provide essential background to the article but are not included in the main article due to space constraints. A maximum of ten Extended Data display items (figures and tables) is permitted.

Please use the link below to submit your revised manuscript and related files:

Link Redacted

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 four to eight weeks. If you cannot send it within this time, 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 please visit 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.

Yours sincerely,
[redacted]

[redacted]

Reviewer comments:

Reviewer #1 (Remarks to the Author):

General comments

The manuscript entitled Predicted decline in common bird and butterfly species despite conservation policies in Europe by Rigal et al. presents a comprehensive analysis that relates environmental pressures to abundance trends in common bird and butterfly species. It further projects changes in these trends under BAU conditions, one SSP scenario, and three NFF scenarios and analyzes these changes at the European scale and across 24 distinct biophysical regions.
Based on their findings, the authors conclude that current EU conservation policies may be insufficient to halt biodiversity decline. As I am not an ecologist/statistician by training, my review primarily focuses on the data and projection aspects of the manuscript. From this perspective, the methodological approach appears sound and appropriate for addressing the question of future trends in common bird and butterfly species.
While similar studies may exist, I am not aware of any that have assessed changes in common species trends in such a comprehensive and multi-scenario manner. The main conclusion - that existing conservation policies are likely inadequate to meet biodiversity targets, even under nature-positive futures – represents a valuable contribution to both the scientific literature and the broader public discourse. However, the title may be somewhat misleading, as it suggests that the policies assessed have already been implemented and evaluated post hoc. Apart from that, the manuscript is well-written, though some methodological clarifications and adjustments in the presentation of results are needed. Additionally, a more nuanced discussion of methodological choices and limitations would strengthen the manuscript. Please find some suggestions below.

Data/Methods

It is clearly evident that the authors have made considerable efforts to present their methods as transparently as possible. However, given the complexity of the analysis—with numerous individual processing and modeling steps—I recommend beginning the data and methods section with an 'overall approach' subsection that outlines the major steps without going into detail. Additionally, the authors might consider including a workflow figure to visually represent the individual steps of the analysis. Moreover, the methods section would benefit from a careful review to eliminate duplications and unnecessary details, while ensuring that all key information is clearly captured (see examples in the specific comments). For instance, it is currently difficult to follow which time periods are being referred to, and terms such as 'the past', 'the initial time period', and 'present day' are used inconsistently. A small box or table summarizing the time periods and their roles in the analysis would greatly improve clarity for the reader.

Results

I would recommend a few changes to improve the presentation of the results. First, the color scales used in the figures are not color-blind friendly. Please consider switching to alternative color schemes that are accessible to all readers. Second, for the spatial results, if changes in the pressures are not shown directly in the figures but are instead placed in the supplementary information, it becomes difficult to follow the explanations in the main text—especially since these explanations often refer to how changes in specific pressures influence spatial patterns. I strongly recommend presenting

these elements side by side to improve clarity. Third, I was somewhat puzzled by the relatively large decrease in agricultural surface area in the SSP1 scenario compared to other scenarios. This extent of change is not reflected in the original publication by Dou et al. (2023), nor in other implementations of SSP1 (e.g., Brown et al. 2019). Depending on the specific implementation and quantification within the model, SSP1 could even lead to an increase in total agricultural area, as agriculture becomes less intensive but potentially more widespread. This discrepancy might be related to the reclassification of land systems – particularly mosaic landscapes – into either agricultural or other land categories. I suggest revisiting this aspect and, if the results are confirmed, including a comparison with other representations of SSP1 land-use projections in the discussion, along with potential implications for biodiversity trends. Lastly, I recommend avoiding the phrasing 'most species are impacted in a positive/negative way' as this implies a strong majority. Instead, I would suggest using 'a majority of species' in cases where the trend is more moderate.

Discussion

Building on my comment above regarding agricultural area in SSP1, I find it somewhat inconsistent that the authors conduct such a nuanced analysis - highlighting the complex relationships between pressures and biodiversity - yet conclude that 'even the environmentally-friendliest scenario is not good enough'. It is unclear what 'environmentally-friendly' means in this context. Reducing agricultural inputs and restoring some land can certainly be considered environmentally beneficial. However, these measures may also result in increased land consumption for agriculture or forestry, as yields tend to be lower in less intensive production systems (see the whole land sparing vs. land sharing debate). Perhaps, the authors could compare SSP1 outcomes to other SSP scenarios (even if only qualitatively with respect to the indicators they use in their analysis) before making definitive claims about its environmental friendliness. Additionally, I suggest downplaying the emphasis on the 'conservation-targeted scenarios'. These scenarios represent relatively simplistic implementations of certain EU and global biodiversity targets, and are by no means holistic. For example, pesticide use is not considered at all, and restoration efforts are implemented in a relatively simplified manner. This is understandable, given that such scenario frameworks are still relatively new, but their limitations should be clearly acknowledged in impact assessments like this one.

Specific comments

l111 I would suggest to explicitly mention agriculture and forestry here, instead of the more general 'resource extraction'.

l115 Add 'expansion of agricultural land' after 'urban sprawl'.

l130 Replace 'scenarisation' by 'scenario framework'

ll132ff. While basing Nature Futures Framework (NFF) scenarios on SSP projections may be one valid approach, it is not the only one. The NFF framework is, in principle, independent of the SSPs, and this should be acknowledged here.

l173ff. Could the underlying research question(s) and/or hypothesis be explicitly stated here?

ll196-199 It's good that the authors are transparent about the differences here, but this level of detail is not necessary at this point in the manuscript. I recommend moving it to the supplementary information.

ll199-201 Since there are only three different schemes, I would recommend explicitly stating which are considered the "best estimators" here.

l201 Replace 'In other' by 'In order'

l202 Either include the full set of guidelines or remove the example ('such as…') and just provide a reference.

l203 And you're also not targeting rare species, so simply stating "To extract common bird species, we…" would be sufficient.

l205 Remove 'In addition'

ll208-210 It's not entirely clear to me how this aligns with the 2000–2005 and 2016–2021 time periods used later in the manuscript. Perhaps some more explanation needed?

ll214-218
&
ll239-242 In this section, the writing shifts from the active voice ('we used…') to the passive voice ('xy are used'). I recommend maintaining a consistent voice throughout the methods section

Figure 1b The butterfly data appear to be spatially biased. Have any steps been taken to address this issue?

l254 Replace 'This' by 'it'

l257 Remove 'planned'

l258 The authors express concern that their analysis may be confounded if bioregional classifications include habitat and/or land-cover, given that these variables are also used as predictors in their model. However, isn't the same issue applicable to climate variables? Wouldn't it be better to group similar responses to pressures using cluster analysis or a comparable method?

l264 The labels in the figure in the supplementary information also include "25." I haven't checked whether any numbers are missing in between, but it might be worth verifying this.

l270 Could the authors provide a justification for why these two specific climate variables were selected?

l275 Could the authors provide a justification for why these two time periods and lengths were selected?

ll293ff. I'm a bit confused by the use of different spatial scales (e.g., 1 km × 1 km grid, biophysical regions, etc.). Could the authors clarify which analyses were conducted at which spatial level?

l296 Unclear. Is this 'temperature variability' analogous to the 'temperature variance' in the past data?

ll319-324 To my knowledge, the High Resolution Layers (imperviousness, tree cover density) and CORINE are not necessarily consistent—there may be areas where they disagree. How did the authors address this issue?

l329 Standard CORINE products are not necessarily consistent over time, meaning that deriving changes or annual change rates from them is problematic. Specific "accounting layers" are available that allow for such analyses. Did the authors use these? If so, please clarify; if not, consider adding a note on this in the methods section.

ll331-334 At some point, the authors should address the fact that the land system map is not representative of either the earlier period (2000–2005) or the later period (2016–2021). Additionally, the map includes data that are not necessarily representative of 2015, which could be problematic when attempting to detect changes over such a short time frame.

l336-337 These categories overlap with the CORINE-derived land cover/use classifications. Why were both used? Are they consistent?
Additionally, how was the classification done - specifically, how were mosaic systems treated? This is particularly important, as the projected biodiversity results probably depend heavily on this classification.

l350 Include reference to NFF framework paper (Pereira et al., 2020)

l351 Add 'in this implementation' after 'The NFF scenarios'.
They don't need to do so.

l370 Please note that this is already an aggregated classification. The standard CORINE dataset includes more than 15 land cover classes (see https://www.eea.europa.eu/data-and-maps/figures/corine-land-cover-1990-by-country/legend)

l393ff. I wonder why the actual projected protected areas were not used. If I understand the Dou et al. paper correctly, these should be provided as part of the NFF projections. Could the authors clarify this?

l422 Which year or time period were the counts modeled for?

l461 What does 'extending observed values' mean? Is it referring to linear extrapolation? Could the authors clarify this?
Figure 2 Please use a color-blind safe color scale in the figures to ensure accessibility for all readers.

It also feels counterintuitive if more intense colors translate to less intense land-use (f-g).

l509 Remove 'Concerning the effects of climate change'.
All results section Please replace 'most of..' by 'a majority of..' throughout this section.

ll582-583 Wouldn't it be strange if it weren't? After all, it's the pressures that drive the changes in the indices.

l586 That's true for North-Eastern Europe, but South-Western regions mostly show no change or even a slight decrease - yet they still score bad in the change in indices..

ll591-592 I think it's not ideal that such central material - namely, the changes in pressures – can only be found in the supplements. Are there any options to bring it into the main text?
Figure 5 Is there a reason why only NfN is shown here?

ll666-679 Somewhat repetitive of the results—consider shortening.

l666 Rephrase to 'some of the objectives'. Not all objectives of the different strategies are considered in the scenarios.

l700 It’s important to clarify here that the ‘beneficial effect’ refers exclusively to using SSP1 as the baseline—no other scenarios were tested.

ll715-717 Please add that this is likely the case for land-use related pressures as well. Agricultural inputs and/or intensity levels may change in the future in ways not reflected in historical data.

l722 Unclear how the multi-species indices help ‘reduce prediction uncertainty’. Could the authors clarify?

l724 Which ‘important questions’ are raised?

ll728ff. I think it's a bit too simplistic to label SSP1 as the 'most environmentally friendly'. In what terms? When it comes to agriculture, SSP1 likely involves significantly more land use—albeit potentially less intensive—which could still have negative impacts on biodiversity. It may well be that another SSP, although not typically considered 'environmentally friendly', might perform better in the same analysis simply because it requires less land for agriculture and/or forestry.

References

Brown, C., Seo, B., & Rounsevell, M. (2019). Societal breakdown as an emergent property of large-scale behavioural models of land use change. Earth System Dynamics, 10(4), 809–845. https://doi.org/10.5194/esd-10-809-2019

Dou, Y., Zagaria, C., O’Connor, L., Thuiller, W., & Verburg, P. H. (2023). Using the Nature Futures Framework as a lens for developing plural land use scenarios for Europe for 2050. Global Environmental Change, 83, 102766. https://doi.org/10.1016/j.gloenvcha.2023.102766

Pereira, L. M., Davies, K. K., den Belder, E., Ferrier, S., Karlsson-Vinkhuyzen, S., Kim, H., Kuiper, J. J., Okayasu, S., Palomo, M. G., Pereira, H. M., Peterson, G., Sathyapalan, J., Schoolenberg, M., Alkemade, R., Carvalho Ribeiro, S., Greenaway, A., Hauck, J., King, N., Lazarova, T., … Lundquist, C. J. (2020). Developing multiscale and integrative nature–people scenarios using the Nature Futures Framework. People and Nature, 2(4), 1172–1195. https://doi.org/10.1002/pan3.10146

Reviewer #2 (Remarks to the Author):

In this manuscript, the authors use long-term monitoring data of birds and butterflies to model effects of main pressures (incl. climate change and land-use/land-management change) across 26 countries in Europe, and to model future trends under different climate and land-use scenarios. Their findings, namely that no future scenario will result in positive projected trends for birds and butterflies, are highly relevant for conservationists and policymakers, and point to the fact that a systemic change is necessary to reverse this pattern. I found the methodological approach in the manuscript to be robust and the results to be original and of immediate interest to the readership of Nature Ecology and Evolution. The authors used some of the best long-term biodiversity datasets available for Europe, and they uploaded a subset of this data to a Zenodo repository together with the R codes to ensure reproducibility of their analyses.
I enjoyed reading this manuscript, it is well-written and well-referenced. My main comments are the following:
1. Representativeness of the data for the modelled areas: the butterfly data have important spatial biases, with most data being collected in the United Kingdom, Netherlands, Germany, Catalonia and southern Sweden. However, figure 6 shows projected butterfly trends also for eastern European countries where no butterfly survey was done (Fig. 1). I understand that these areas were modelled as part of larger biophysical regions (e.g. 2, 13, 19) for which some data exists – yet I wonder how confidently can model results be transferred across different and spatially separated parts of the same biophysical region?
2. Could you provide confidence intervals for the projected future trends in bird and butterfly counts presented in Fig. 4? Some measure of uncertainty is important to evaluate whether these projected trends, under the different scenarios, deviate significantly from each other and from the trend observed in the past.

Some specific comments:
L200: in order to harmonise abundance
Figure S1b and S1d: please add to the legend that the map of biophysical regions is in Fig. S1e (and please explain the colour code in Fig. S1e). It would be nice to have more descriptive names, not only numbers, to differentiate the different biophysical regions, if those are available.
L207 “we focused on sites visited at least five years between 2000 and 2021”, but in your analyses only data from 2000-2005 and 2015-2021 are used, is this correct?
L278 is the circular buffer 2.5km in diameter or in radius?
Figure 1: I think it would be helpful to also show, e.g. by using different colours, which type of monitoring scheme was used at each site (e.g. line transects, point counts, etc.)
Supplementary material 2: I find it odd to have the metadata pertaining the csv. tables in a separate file; could you add a metadata sheet to the bird and butterfly data table (and save as .xlsx files) or alternatively add a README file in the Zenodo repository where the csv. files are saved? This would make this information much easier to find! Similarly, the description of the files SM3_bird_model_check.pdf and SM3_butterfly_model_check.pdf are at the end of supplementary material 3 but not within the files..

L279 Please specify which figure within supplementary material 3 you are referring to, as this also includes graphs for model checking unrelated to the effect of buffer size. Also, supplementary materials should be numbered in the order in which they appear in the main text, so this should be supplementary material 2 instead of 3.
L317 I guess you meant “imperviousness data” and not “imperviousness density data”?
L425-429 “The pressure variables included …. percentage of protected areas” perhaps there is a more fitting term than “pressures” to describe these covariates, as not all used variables are pressures (e.g. protected areas, landscape diversity)
L427 why were low and medium production forest types merged together?
L431 Was siteID treated as a random effect term? Was VIF of covariates always low?
Supplementary material 2: please explain all acronyms (e.g. FaBI, FoBI, GBI or WBI). Also, are “Effect of covariate trends on species trend” the coefficients of the model in Eq.6?
Eq.3 is actually two equations..
Figure S3b (butterfly temporal slope comparison) what do the different colours mean? Note that the following figure (boxplots) is also labelled S3b, please correct this.
Figure S3b (boxplots) why are boxplots for certain biophysical regions (e.g. 8, 15, 22, 24) missing?
L452 please indicate clearly which item within supplementary material 4 you are referring to
L450-452 you write here that you focused only on significant model parameters to estimate projected temporal trend, but in Eq. 6 all parameters are included – am I missing something?
L507 are these significant positive and negative effects, or not?
L564 (Fig. 2 c, f) instead of (Fig. 2 c, g)
L568 also grassland butterflies, besides farmland birds, seem to benefit from climate change according to your results
Fig.5b the 1 is missing in SSP1
Fig. S4n: why is there no grassland butterfly species modelled? Please add a short explanation to the figure legend.
Fig. S4z: is this figure ever referred to in the main text? What does LDA stand for? I suggest authors change the colour code as red and green might be hard to discriminate for color-blind people.

Reviewer #2 (Remarks on code availability):

The R code needs minor revisions (and could benefit from some more comments to guide the user). For example, lines 18-19 in the “main_code.R” have some typos ("packages_publi.R" instead of "package_publi.R", and "function_publi" instead of "function_publi.R") that prevent the source codes to be loaded properly. The lines 127-145 and 206-220 return this error message: “No shared levels found between `names(values)` of the manual scale and the data's values.”

Reviewer #3 (Remarks to the Author):

Rigal and colleagues have used monitoring data on common bird and butterfly species to project their abundance in different sustainability scenarios. More specifically, the authors used the projected changes in climate, land use and land-use intensity of the Nature Futures Framework to assess their impact on 263 common bird and 144 common butterfly species as well as on selected indicator species for farmland and forest. Based on their results the authors conclude that sustainability scenarios need to be adjusted to improve biodiversity conservation while simultaneously allowing the use of natural resources.

Such modelling approaches are relevant and an important component to project potential outcomes of internationally agreed area-based conservation targets. This study adds to this field of research, in that it shows that these agreed targets seem not to halt the current biodiversity loss. Hence, the study calls for reconsidering the demand for natural resources to be able to bend the curve of biodiversity loss.

In general, the paper is written well. Still, some aspects of the manuscript could be enhanced:
1) The introduction is not always clearly structured e.g. in the first and third paragraph you deal with human activities, drivers and pressures on common and rare species that could be better integrated. The same holds true for integrating the current international agreements and the forward-looking approaches that aim to integrate societal needs with conservation targets. It would be helpful to reorder parts of these thoughts to streamline the arguments. The aspect of indices and indicators comes a bit out of the blue given the focus on common species, i.e. try to better integrate. Most importantly, I am missing clear expectation/predictions on how these different scenarios might impact common biodiversity in terms of birds and butterflies. This would also help to structure methods, results and discussion more clearly.
2) Some aspects in the method section need some more explanation. While the monitoring was well explained, Fig. 1 summarising the data should also provide the number of monitoring sites and countries available for birds and butterflies. Next you explain the environmental and climate data that are needed to project abundances based on different scenarios. I would be helpful to add scenario input data or something alike to guide readers through the methods (see also my comment on expectations). This is also obvious in Fig. 2 which lacks e.g. explanations in the legend for the different scenarios, i.e. BAU, SSP1 etc., same holds true for y-axis labelling. As the general change in % of total surface is rather low, this needs to be addressed throughout the whole manuscript most clearly.
3) The description of your findings could be enhanced, i.e. using structuring expectations that would help guide the reader through your findings. Currently, you are presenting the effects of climate, land use and land-use intensity somehow not embedded within the scenarios which are the main topic of your manuscript, e.g. you mention only numbers of species that are projected to respond positively or negatively but not proportions of all common/farmland/forest species; likewise the effects of climate, land use and land-use intensity could be ordered rather than presenting them with dark and light green

colors interchangeably. Fig. 4 projecting the abundance trends of all common, farmland and forest species according to the different scenarios could be improved, i.e. add information that y-axis scaling differs and try to optimize further so that the number of the axes are comprehensible (explain why axis for all species is limited to c. 100 while for farmland and forest it ranges from 200 to 1000). Fig. 5 and 6 are somehow had to grasp, e.g. it is difficult to relate the trends to numbers.
4) While the discussion is a nice piece to read. Some more details could be added to enhance the overall contribution of your work to this field of research. Most importantly, the relatively small change in % of surface of land use across scenarios should be highlighted. How does this relate to the continuous decline of common species, i.e. are agreed targets to modest, e.g. % of surface change not very large. Please add some thoughts on that as well as spatial configurations in the different scenarios and how this might impact your projected abundances. Also, the final section on implications of your work could be enhanced, e.g. what exactly do your mean with bold, integrated policy strategies and who can scenario projections contribute to this. Please specify!

**********END**********

Version 1:

Decision Letter:

11th May 2026

Dear Dr. Rigal,

Thank you for submitting your revised manuscript "Predicted decline in common bird and butterfly species despite conservation policy scenarios in Europe" (NATECOLEVOL-25082960A). It has now been seen again by the original reviewers and their comments are below. The reviewers find that the paper has improved in revision, and therefore we'll be happy in principle to publish it in Nature Ecology & Evolution, pending minor revisions to satisfy the reviewers' final suggestions and to comply with our editorial and formatting guidelines. Please do not revise in response to the reviewers' current suggestions until you receive further information on these formatting points from us.

If the current version of your manuscript is in a PDF format, please email us a copy of the file in an editable format (Microsoft Word or LaTex)-- we can not proceed with PDFs at this stage.

If you have not done so already, please ensure that you also email us a completed copy of the Reporting summary :

Reporting summary: https://www.nature.com/documents/nr-reporting-summary.pdf

We are now performing detailed checks on your paper and will send you a checklist detailing our editorial and formatting requirements in about a week. Please do not upload the final materials and 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.

Best regards
[redacted]

[redacted]

Reviewer #1 (Remarks to the Author):

The authors have substantially improved the manuscript, particularly with respect to clarity in the methods section and the presentation of results. The revisions reflect careful consideration of reviewer feedback, and the paper is clearly stronger than the original submission. However, some of my comments have not been fully addressed.

One point that still requires clarification concerns the relationship between the Shared Socioeconomic Pathways (SSPs) and the Nature Futures Framework (NFF). The manuscript does not yet sufficiently emphasize that SSPs and NFFs are conceptually independent scenario frameworks. In its current form, the text gives the impression that the NFF framework is primarily there to 'nuance' SSP worlds, whereas in fact NFF scenarios can be implemented independently of SSPs and could equally draw on societal drivers derived from alternative scenario narratives or extrapolations (e.g., ll112 ff.)

Furthermore, I think methodological simplifications arising from the aggregation of land-use categories, the (potential)

inconsistencies between historic, present, and future data and the use of partly non-overlapping time periods should be explicitly mentioned in the discussion. I think this could be done by just adding one sentence acknowledging such limitations. Relatedly, the land-use outcomes, and in consequence all conclusions drawn in the paper, depend on the specific implementation and parameterization of SSP1, as well as on how policy targets are translated into model parameters within the different NFF value perspectives (as the authors now also acknowledge in the discussion). Previous LU model intercomparison studies have demonstrated substantial variation in land-use outcomes across models, scenarios, and implementations (e.g. Popp et al. https://doi.org/10.1016/j.gloenvcha.2016.10.002 ; Alexander et al. https://doi.org/10.1111/gcb.13447). While the comparison to a SSP3 scenario is very insightful, I think it would be the SSP1 (and/or NFFs) implemented in another LU model that would be required to test the robustness of the results. I am not asking for this, as I think it goes far beyond this study, but a concise statement noting that results (and even conclusions) may vary considerably depending on how (in this case) SSP1 is parameterized in a given land-use model would help contextualize the findings and avoid overgeneralization.

In addition, the explanatory details provided in the authors' response to the comment made on line 275 of the original submission should be added directly to the manuscript.

Finally, replacing the term 'pressure' with 'driver' is not always appropriate, especially in contexts where it leads to formulations suggesting that 'drivers increase or decrease'.

Reviewer #2 (Remarks to the Author):

I thank the authors for carefully addressing all my comments, and I found the manuscript to be much improved since the first submission. I especially appreciated their effort in making their results more accessible by developing a Shiny app, which allows readers to interactively browse through their many outputs. This app is currently only mentioned under the Data availability section of the main text; I think readers should be directed to it more explicitly – could this be mentioned in the results section, and/or in the figure legends of the related results? I also found some of the newly added figures, particularly the analytical workflow in figure 1 and the figure S1a in the appendix, very useful in improving the manuscript's clarity and interpretation. Since the last review, the authors added a comparison with the scenario SSP3 in (some of) their results. I also found this to be a useful addition, but this additional analysis and related results are touched upon only in the discussion section. I would recommend the authors to add information about this analysis in the methods and results. Besides this, I only have a few minor comments:
Figure S1c-d panels a-c show proportions (between 0 and 1) rather than percentages (0-100%).
Figures 2 and S1b: please add to the legend what the vertical solid and dashed lines represent – is the solid line the average coefficient across all species?
L401 could you add (here or earlier on in the methods section) a few words on why SSP3 was chosen as comparison, among all SSP scenarios?
L403 Compared
L730 which was always below 2
Figure S3b and S3e: The legends state "See the map of biophysical regions in Fig. S1f." but the map of biophysical regions is actually Fig. S3f.
In general, I find the numbering of the figures in the supplementary material a bit confusing, particularly the use of letters, which may refer either to different figures (Fig. S1a, Fig. S1b) or different panels within the figures – but I imagine this will be formatted according to the journal's guidelines upon acceptance.

Reviewer #2 (Remarks on code availability):

The authors improved the code following my previous comments and added a README file.

Reviewer #3 (Remarks to the Author):

I would like to congratulate the authors on the constructive revisions they have made to their text, including the visuals. Overall, my comments have been satisfactorily addressed. The only point that, in my opinion, has not yet been implemented is concluding the introduction with clear expectations/hypotheses regarding the different scenarios. This should be possible based on the published literature, especially since you referenced to it in the discussion. As previously noted, these expectations would serve as a structural framework throughout the manuscript.

Best Nina Farwig

Reviewer #1:

General comments

The manuscript entitled Predicted decline in common bird and butterfly species despite conservation policies in Europe by Rigal et al. presents a comprehensive analysis that relates environmental pressures to abundance trends in common bird and butterfly species. It further projects changes in these trends under BAU conditions, one SSP scenario, and three NFF scenarios and analyzes these changes at the European scale and across 24 distinct biophysical regions.

Based on their findings, the authors conclude that current EU conservation policies may be insufficient to halt biodiversity decline. As I am not an ecologist/statistician by training, my review primarily focuses on the data and projection aspects of the manuscript. From this

perspective, the methodological approach appears sound and appropriate for addressing the question of future trends in common bird and butterfly species.

While similar studies may exist, I am not aware of any that have assessed changes in common species trends in such a comprehensive and multi-scenario manner. The main conclusion - that existing conservation policies are likely inadequate to meet biodiversity targets, even under nature-positive futures – represents a valuable contribution to both the scientific literature and the broader public discourse. However, the title may be somewhat misleading, as it suggests that the policies assessed have already been implemented and evaluated post hoc. Apart from that, the manuscript is well-written, though some methodological clarifications and adjustments in the presentation of results are needed. Additionally, a more nuanced discussion of methodological choices and limitations would strengthen the manuscript. Please find some suggestions below.

We thank the reviewer for the constructive comments that helped us to strengthen the analysis and clarify the manuscript. We provide below detailed answers to your suggestions. We agree that the title could be misleading and we modified it as follows:

"Predicted decline in common bird and butterfly species despite conservation policy scenarios in Europe"

Data/Methods

It is clearly evident that the authors have made considerable efforts to present their methods as transparently as possible. However, given the complexity of the analysis—with numerous individual processing and modeling steps—I recommend beginning the data and methods section with an 'overall approach' subsection that outlines the major steps without going into detail. Additionally, the authors might consider including a workflow figure to visually represent the individual steps of the analysis.

We thank the reviewer for this helpful suggestion. In line with this recommendation, we have revised the beginning of the Data and Methods section to improve clarity and readability. We also included a brief "Overall approach" subsection and a workflow figure (Figure 1) at the beginning of the Results section that outlines the main steps of the analysis without going into technical detail, thereby providing readers with a clear roadmap before presenting the full methodology.

In addition, we have added a workflow figure in the supplementary (Figure S3g) to visually summarize the different stages of data processing and modeling at the different spatial levels (Europe and biophysical regions). We believe that these changes make the structure of the analysis more transparent and easier to follow, particularly given its complexity.

Figure 1: Workflow of the analyses. a) We combined European-scale monitoring data for common birds and butterflies with 1 × 1 km data on land use, land-use intensity and climate change. b) We estimated effects of land-use, land-use intensity and climate change on individual species trends. c) We combined land-use, land-use intensity and climate scenarios with estimated effects of direct drivers on each species to d) produce trends in multi-species indices based on birds and butterflies in each scenario.

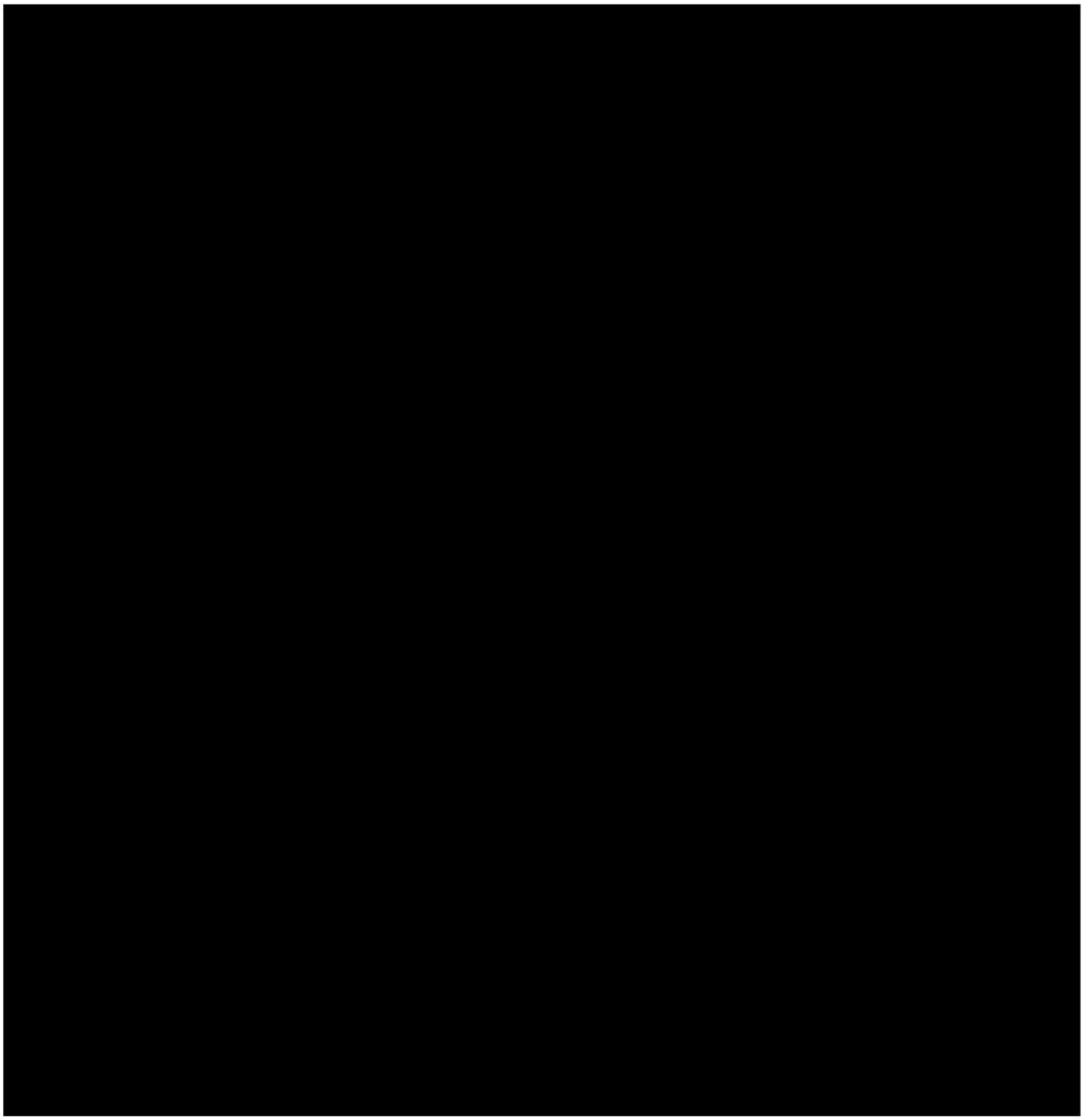

Figure S3g: Workflow across spatial levels. a) Biodiversity data (PECBMS bird data in this example) are analysed with b) environmental and driver data available at a 1 × 1 km average in a 2.5km buffer around each bird monitoring site. c) This analysis provides an estimate for the effect of each driver on each species at the European level, which is used together with driver scenarios at the European level to produce d) scenarios of bird relative abundance. e-g) A similar framework is applied for each biophysical region (region 25 in this example).

Moreover, the methods section would benefit from a careful review to eliminate duplications and unnecessary details, while ensuring that all key information is clearly captured (see examples in the specific comments). For instance, it is currently difficult to follow which time periods are being referred to, and terms such as 'the past', 'the initial time period', and 'present day' are used inconsistently. A small box or table summarizing the time periods and their roles in the analysis would greatly improve clarity for the reader.

We thank the reviewer for this constructive comment and agree that the Methods section required further clarification. In response, we have carefully revised the section to improve consistency and readability. In particular, we standardized the terminology used to refer to time periods throughout the manuscript.

Following the reviewer's suggestion, we have added a summary table (Extended Table 1) that clearly presents the different time periods and their roles across variables in the analysis. We believe this addition substantially improves the transparency of the temporal framework.

	Past		
Data	Historical	Recent	Future
Birds	2000 – 2021 (yearly)		
Butterflies	2000 – 2021 (yearly)		
Climate	2000 – 2005 (average)	2016 – 2021 (average)	
Imperviousness	2006	2018	
Tree cover density	2012	2018	2050
Open land cover	2000	2018	
Land-use intensity	2015		
Landscape diversity	2000	2018	
Protected areas	2024		
Ecosystem productivity	2000 – 2019 (average)		

Extended Table 1: Details on the time period used for biodiversity, environmental and driver data

In addition, we have streamlined the Methods section by focusing on the key elements of the analysis, while transferring more detailed or technical information to Supplementary Material. This restructuring helps reduce redundancy and enhances the overall clarity of the section.

Results

I would recommend a few changes to improve the presentation of the results. First, the color scales used in the figures are not color-blind friendly. Please consider switching to alternative color schemes that are accessible to all readers.

This was an important point that is now corrected. We used a color code based on the NFF triangle and complementary colors for additional scenarios, which formed a color-blind friendly combination. We also now use different line types in Figure 4 to even more clarify the differences between scenarios. In addition, we developed an Shiny application (https://press-com-bio.sk8.inrae.fr/) allowing to explore and zoom-in the different figures and datasets.

Second, for the spatial results, if changes in the pressures are not shown directly in the figures but are instead placed in the supplementary information, it becomes difficult to follow the explanations in the main text—especially since these explanations often refer to how changes in specific pressures influence spatial patterns. I strongly recommend presenting these elements side by side to improve clarity.

We have moved most of the figures illustrating regional changes in pressure (now we use the term drivers in line with the IPBES terminology) to the Extended Data section (Extended Figure 2-7).

Due to limitations on the number of figures allowed in this section, some figures remain in Supplementary Material (Figure S1k-m). Here again, the Shiny application (https://press-com-bio.sk8.inrae.fr) facilitates access to and visualization of the detailed results.

Third, I was somewhat puzzled by the relatively large decrease in agricultural surface area in the SSP1 scenario compared to other scenarios. This extent of change is not reflected in the original publication by Dou et al. (2023), nor in other implementations of SSP1 (e.g., Brown et al. 2019). Depending on the specific implementation and quantification within the model, SSP1 could even lead to an increase in total agricultural area, as agriculture becomes less intensive but potentially more widespread. This discrepancy might be related to the reclassification of land systems – particularly mosaic landscapes – into either agricultural or other land categories. I suggest revisiting this aspect and, if the results are confirmed, including a comparison with other representations of SSP1 land-use projections in the discussion, along with potential implications for biodiversity trends.

We thank the reviewer for highlighting this important point. As shown in Dou et al. (2023), SSP1 exhibits absolute changes in cropland and mosaic systems that are similar to those observed under NAC, NFS, and NFN. However, in contrast to these scenarios, SSP1 shows a decline in grassland area, whereas grassland increases under NAC, NFS, and NFN.

As a result, in Dou et al. (2023) the overall absolute change in agricultural land (defined as the sum of cropland, grassland, and mosaic systems) is more negative under SSP1 than under the other scenarios at the European scale, (in line with other SSP1 implementations, see for instance Riahi et al. (2017) https://doi.org/10.1016/j.gloenvcha.2016.05.009),

Furthermore, in this implementation of SSP1, the relative share of high-intensity agriculture increases. This reflects an absolute increase in high-intensity permanent croplands and high-intensity agricultural mosaics, combined with a decline in low-intensity arable lands and low-intensity agricultural mosaics. Unlike in NFN, NFS, and NAC, this decline is not offset by an increase in low-intensity grasslands.

However, we agree with the reviewer that SSP implementation may vary and the results in our study must be interpreted for a specific implementation of SSP1. This clarification has now been explicitly incorporated into the Discussion section as follows (l. 395-404):

"*The scenarios analysed in this study are based on the shared socio-economic pathway (SSP1) that is designed to take the most account of environmental issues, compared to other SSPs*[78]*. The implementation of SSP1 analysed in this study relies however on specific choices, in particular adopting a more land-sparing approach, illustrated by a strong decline in agricultural areas and a higher share of high intensive farmland compared to the current situation. In contrast, an alternative SSP3 which is expected to lead to highest removal of natural land at the global level*[79]*, would mostly reduce landscape diversity in Europe, with less consequences on the agricultural surface. Compare to SSP3, the implementation of SSP1 held more positive trends in birds and butterflies (except forest birds and grassland butterflies, see comparison between SSP1 and SSP3 in Fig. S1u, v) in line with the a priori expectation.*"

Lastly, I recommend avoiding the phrasing 'most species are impacted in a positive/negative way' as this implies a strong majority. Instead, I would suggest using 'a majority of species' in cases where the trend is more moderate.

We thank the reviewer for this suggestion, and we modified the wording accordingly.

Discussion

Building on my comment above regarding agricultural areas in SSP1, I find it somewhat inconsistent that the authors conduct such a nuanced analysis - highlighting the complex relationships between pressures and biodiversity - yet conclude that 'even the environmentally-friendliest scenario is not good enough'. It is unclear what 'environmentally-friendly' means in this context. Reducing agricultural inputs and restoring some land can certainly be considered environmentally beneficial. However, these measures may also result in increased land consumption for agriculture or forestry, as yields tend to be lower in less intensive production systems (see the whole land sparing vs. land sharing debate). Perhaps, the authors could compare SSP1 outcomes to other SSP scenarios (even if only qualitatively with respect to the indicators they use in their analysis) before making definitive claims about its environmental friendliness.

We thank the reviewer for this important remark and agree that, although SSP1 is generally considered the sustainability-oriented pathway among the five SSPs, it cannot be unambiguously described necessarily as the most environmentally friendly scenario.

To provide a more appropriate point of comparison, we have added a contrasting, less environmentally oriented scenario in the Supplementary Material: SSP3 ("Regional Rivalry"), in which energy and food security concerns take precedence over environmental objectives in a more competitive international context. The corresponding land-use change maps were derived from the additional material of Dou et al. (2023).

In addition, we have revised this section of the Discussion to better reflect the limitations of this particular implementation of SSP1 and to avoid overstating its environmental performance (l. 395-404, see the citation in the response to the previous comment).

Additionally, I suggest downplaying the emphasis on the 'conservation-targeted scenarios'. These scenarios represent relatively simplistic implementations of certain EU and global biodiversity targets, and are by no means holistic. For example, pesticide use is not considered at all, and restoration efforts are implemented in a relatively simplified manner. This is understandable, given that such scenario frameworks are still relatively new, but their limitations should be clearly acknowledged in impact assessments like this one.

We thank the reviewer for this suggestion. The limited set of conservation policies implemented in the scenarios analysed in the study is now clearly stated in the discussion and we now use "scenarios with additional conservation targets" instead of "conservation-targeted scenarios".

Specific comments

l111 I would suggest to explicitly mention agriculture and forestry here, instead of the more general 'resource extraction'.

We modified the text as suggested (l. 99-102):

"*Yet, they are often directly impacted by human activity for instance by agriculture, forestry or urbanisation*[13] *and the depletion of populations observed in several taxa is a first step towards the status of threatened species*[14,15]*.*"

l115 Add ‘expansion of agricultural land’ after ‘urban sprawl’.

We modified the text as suggested.

l130 Replace ‘scenarisation’ by ‘scenario framework’

We modified the text as suggested.

ll132ff. While basing Nature Futures Framework (NFF) scenarios on SSP projections may be one valid approach, it is not the only one. The NFF framework is, in principle, independent of the SSPs, and this should be acknowledged here.

We now explicitly state this point in the text as follows (l. 114-118):

“The shared socio-economic pathways (SSPs) are a common scenario framework to depict changes impulsed by society under different assumptions on socio-political choices[23]. These different assumptions may be nuanced by making societal preferences and values more explicit. The IPBES Nature Future Framework (NFF) provides an operational tool allowing biodiversity positive futures under different value settings to be identified[24].”

l173ff. Could the underlying research question(s) and/or hypothesis be explicitly stated here?

We rewrote the introduction and in particular the last paragraph to make the question more explicit, as follows (l. 149-158):

“To analyse whether socio-economic scenarios designed with specific conservation objectives and sustainability policies may benefit common biodiversity, we assess in this study the predicted effects on common bird and butterfly species of Europe-wide sustainability scenarios. We use data from the monitoring of common breeding birds and butterflies in Europe between 2000 and 2021 at over 20,000 sites in 26 European countries to estimate the correlative observed effect of climate change, land use change and land use intensity change on 265 species of common birds and 144 species of common butterflies. We use spatial scenarios of changes in drivers under business-as-usual and scenarios reaching several international and EU conservation objectives, in order to anticipate the effect of these changes on common biodiversity and more specifically on indicator species for forest and open habitats.”

ll196-199 It's good that the authors are transparent about the differences here, but this level of detail is not necessary at this point in the manuscript. I recommend moving it to the supplementary information.

Following the main comment on the methodological section, we focused on the key element and moved the additional details, including this point, to supplementary material 3.

ll199-201 Since there are only three different schemes, I would recommend explicitly stating which are considered the “best estimators” here.

We remove this sentence as abundance value selection (e.g. max of several monitoring sessions per year) had been done by PECBMS prior to sending the data.

l201 Replace ‘In other’ by ‘In order’

This typo is corrected.

l202 Either include the full set of guidelines or remove the example ('such as…') and just provide a reference.

We remove "such as", as it is already the full set of guidelines.

l203 And you're also not targeting rare species, so simply stating "To extract common bird species, we…" would be sufficient.

We modified the text as suggested.

l205 Remove 'In addition'

We modified the text as suggested.

ll208-210 It's not entirely clear to me how this aligns with the 2000–2005 and 2016–2021 time periods used later in the manuscript. Perhaps some more explanation needed?

Following the main comment on the methodological section, we now provide a table (Extended Table 1) to clarify the use of the different periods by variables.

Ll214-218 & ll239-242 In this section, the writing shifts from the active voice ('we used…') to the passive voice ('xy are used'). I recommend maintaining a consistent voice throughout the methods section

We thank the reviewer for noting this inconsistency. We have carefully checked the Methods section to ensure a consistent use of voice throughout. In addition, we have moved and rewritten the two paragraphs in question within the Statistical Analysis subsection, where we describe the calculation of multi-species indices, to improve both consistency and clarity.

Figure 1b The butterfly data appear to be spatially biased. Have any steps been taken to address this issue?

The potential spatial bias is addressed at several steps in our analysis. First, in the GAM, we explicitly model spatial autocorrelation and include transect identity and scheme identity, which prevent the model estimate from being driven by neighbour transects and/or belonging to the same scheme. In addition to the model run at the European level, we also compute, by biophysical regions with enough data, the model estimating the driver effect on butterflies and the model estimating the future trends in the different scenarios. In the presentation of the results, we now project the multi-species indices only in continuous polygons of the biophysical regions which effectively encompass the transects.

l254 Replace 'This' by 'it'

We modified the text as suggested.

l257 Remove 'planned'

We modified the text as suggested.

l258 The authors express concern that their analysis may be confounded if bioregional classifications include habitat and/or land-cover, given that these variables are also used as

predictors in their model. However, isn't the same issue applicable to climate variables? Wouldn't it be better to group similar responses to pressures using cluster analysis or a comparable method?

We thank the reviewer for pointing out this question. The biophysical regions partly rely on climatic data that represent the climate at the start of this century. As we used the trend in climatic data to assess the climate effect on species trend, the only potential confounded in our model would be the climatic data used as a control variable for species abundance (Fig. S1b), with no effect on the estimation of species trend.

l264 The labels in the figure in the supplementary information also include "25." I haven't checked whether any numbers are missing in between, but it might be worth verifying this.

The biophysical region #20 corresponds to Malta and is not represented in our analysis. We choose to keep the numbering from the original publication for comparison purposes. We now explain this in the caption of the figure.

l270 Could the authors provide a justification for why these two specific climate variables were selected?

We added references to support our choice. Based on the annual frequency of the data, we cannot analyze the effect of seasonality or extreme events. But an increase in mean temperature, a change in precipitation, and in temperature variability have been found to influence population dynamics of bird and butterfly species. This is now justified as follows (l.556-558):

"*Increases in mean temperature, changes in precipitation, and in temperature variability have indeed been found to influence population dynamics of bird and butterfly species*[100–102]*.*"

l275 Could the authors provide a justification for why these two time periods and lengths were selected?

Climate variables can show relatively strong annual variability. It is therefore recommended to use a shifting temporal window to buffer this variability in order to compare different periods of time. By using six years, we ensure having a robust estimate of the climate variable at the beginning and the end of the period.

ll293ff. I'm a bit confused by the use of different spatial scales (e.g., 1 km × 1 km grid, biophysical regions, etc.). Could the authors clarify which analyses were conducted at which spatial level?

We shortened the methods to make this section more focused on the key element and gain clarity. We also added a supplementary figure (Figure S3g) to make the use of the different spatial scales clearer.

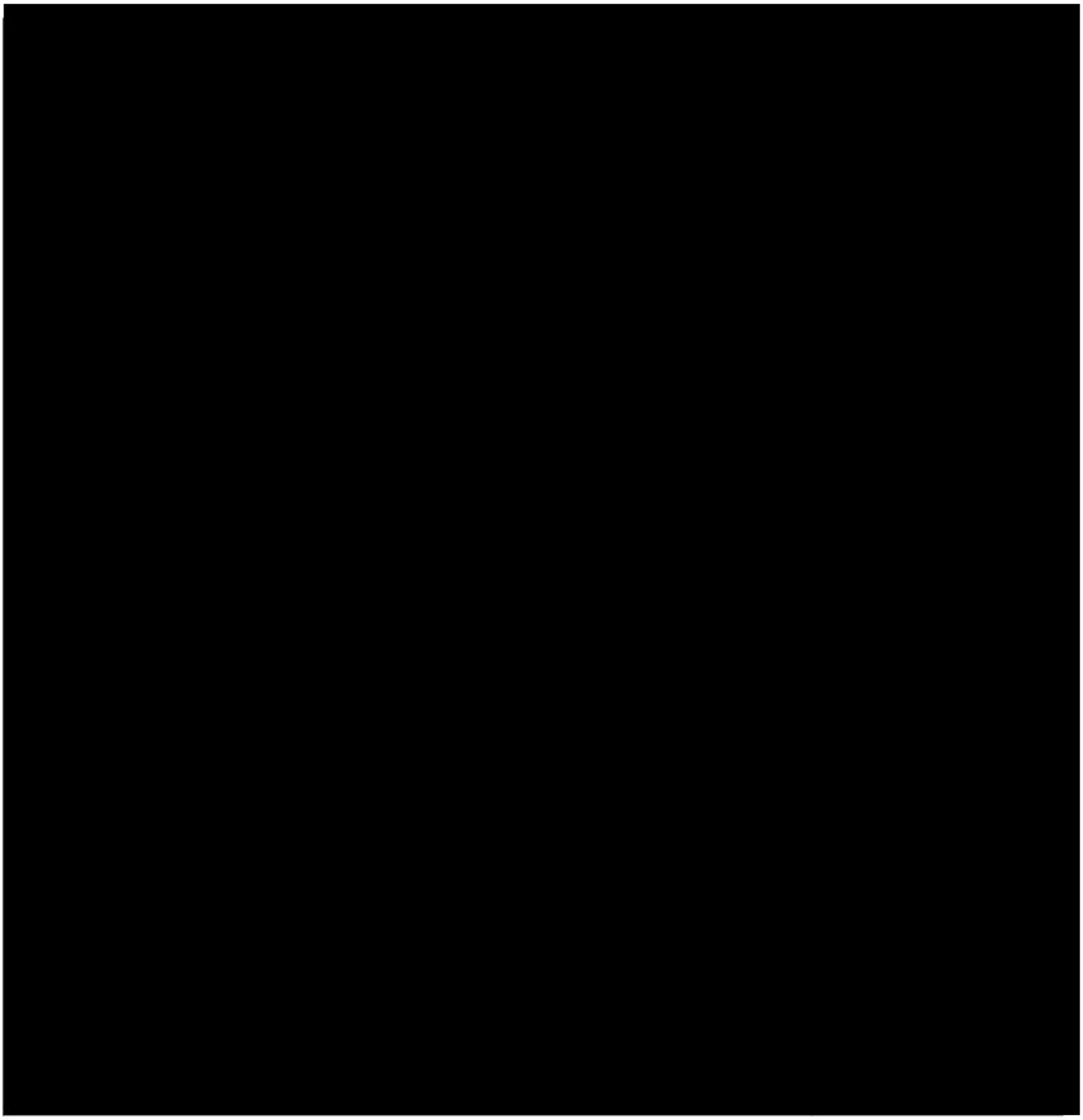

Figure S3g: Workflow across spatial levels. a) Biodiversity data (PECBMS bird data in this example) are analysed with b) environmental and driver data available at a 1 × 1 km average in a 2.5km buffer around each bird monitoring site. c) This analysis provides an estimate for the effect of each driver on each species at the European level, which is used together with driver scenarios at the European level to produce d) scenarios of bird relative abundance. e-g) A similar framework is applied for each biophysical region (region 25 in this example).

l296 Unclear. Is this 'temperature variability' analogous to the 'temperature variance' in the past data?

This is right and is now corrected accordingly.

ll319-324 To my knowledge, the High Resolution Layers (imperviousness, tree cover density) and CORINE are not necessarily consistent—there may be areas where they disagree. How did the authors address this issue?

We provided a comparison of the classification according to the HRL and CLC in the supplementary (Figure S4b-e). The use of a 2.5 km buffer average limits the influence of local mismatches between layers. In addition, High Resolution Layers are used here to provide continuous values, not classification, which also limits the potential influence of mismatches.

l329 Standard CORINE products are not necessarily consistent over time, meaning that deriving changes or annual change rates from them is problematic. Specific "accounting layers" are available that allow for such analyses. Did the authors use these? If so, please clarify; if not, consider adding a note on this in the methods section.

We thank the reviewer for highlighting this point. Following the suggestion, we now use the accounting layers and specify them in the text. The main consequence of this change in the methods is that the change in agricultural surface in the past and thus in BAU is the lowest.

ll331-334 At some point, the authors should address the fact that the land system map is not representative of either the earlier period (2000–2005) or the later period (2016–2021). Additionally, the map includes data that are not necessarily representative of 2015, which could be problematic when attempting to detect changes over such a short time frame.

We thank the reviewer for raising this important point. We agree that the land-system map is not strictly representative of a single reference year. While it is primarily based on datasets from around 2015 (e.g., High Resolution Layer thematic maps for imperviousness, forests, grasslands, water, and wetness) and 2018 (CORINE Land Cover), it also incorporates data from earlier (e.g., 2009) and more recent (e.g., 2020) sources.

As in Dou et al. (2023), this dataset is therefore considered broadly representative of conditions around 2015, and we have used it accordingly in our analysis. We acknowledge that this temporal heterogeneity may limit the precision of detecting short-term changes.

We have now explicitly stated this limitation in the Methods section as follows (l.631-632):

"*It is representative on average for the year 2015, even though the year of the various datasets may vary around this date.*"

l336-337 These categories overlap with the CORINE-derived land cover/use classifications. Why were both used? Are they consistent? Additionally, how was the classification done - specifically, how were mosaic systems treated? This is particularly important, as the projected biodiversity results probably depend heavily on this classification.

We now use CORINE-derived land cover/use classifications.

l350 Include reference to NFF framework paper (Pereira et al., 2020)

This reference was cited in the introduction and is now also included here.

l351 Add 'in this implementation' after 'The NFF scenarios'.They don't need to do so.

We modified the text as suggested.

l370 Please note that this is already an aggregated classification. The standard CORINE dataset includes more than 15 land cover classes (see https://www.eea.europa.eu/data-and-

maps/figures/corine-land-cover-1990-by-country/legend)

We thank the reviewer for pointing this out. We recalculated the landscape diversity using 28 classes belonging to agricultural areas, forest, and semi-natural areas, and wetland out of the 44 available classes. We did so to avoid different urban or water classes from influencing this landscape diversity, which is designed to estimate the diversity in the landscape mosaic used by most of the species. This is explained as follows (l. 665-670):

"To compute the landscape diversity, we focused on the 28 land-use classes from the 100 × 100 m CORINE Land Cover accounting layers in 2000 and 2018 belonging to agricultural areas, forest and semi-natural areas and wetland. The value of the Shannon diversity index for each monitoring site/transect was derived by averaging the index values within a 2.5km buffer. This index ranges from 0, in case of total homogeneity in the cell, to ln(28) which represents the maximum diversity when all the land-use classes are equally present."

l393ff. I wonder why the actual projected protected areas were not used. If I understand the Dou et al. paper correctly, these should be provided as part of the NFF projections. Could the authors clarify this?

Dou et al. (2023) did not provide data for projected protected areas, but the data they used comes directly from O'Connor (2021), which we used in our analysis.

l422 Which year or time period were the counts modeled for?

This information was missing in this paragraph and we added it as follows (l. 716-717):

"To do so, we modelled the observed abundance between 2000 and 2021 (C counts)"

l461 What does 'extending observed values' mean? Is it referring to linear extrapolation? Could the authors clarify this?

This sentence was not clear and we changed it as follows (l. 754-757):

"the expected temporal trend between the recent period and 2050 under business as usual scenario (BAU) by assuming that the observed values of changes in land-use and land-use intensity changes remain the same and using the expected changes in climate variables (i.e. BAU corresponds to a climate reference scenario)"

Figure 2 Please use a color-blind safe color scale in the figures to ensure accessibility for all readers. It also feels counterintuitive if more intense colors translate to less intense land-use (f-g).

This was an important point that is now corrected. As explained above, we used a color code based on the NFF triangle and complementary colors for additional scenarios, which formed a color-blind friendly combination. As suggested, we also used more intense colors to represent more intense land-use.

l509 Remove 'Concerning the effects of climate change'. All results section Please replace 'most of..' by 'a majority of..' throughout this section.

Following the reviewer's suggestion, we now use "a majority of" through the results.

ll582-583 Wouldn't it be strange if it weren't? After all, it's the pressures that drive the changes in the indices.

This sentence was not clear and we modified it as follows (l. 318-320):

"The pattern of observed and expected changes in bird and butterfly indices is spatially heterogeneous (Fig. 5 and 6 and Fig. S1n-s), following the spatial distribution of direct drivers (Extended Fig. 2-7 and Fig. S1k-m)."

l586 That's true for North-Eastern Europe, but South-Western regions mostly show no change or even a slight decrease - yet they still score bad in the change in indices.

We agree with the reviewer and rewrote this part of the result section with more precise examples to illustrate the results by biophysical regions.

ll591-592 I think it's not ideal that such central material - namely, the changes in pressures – can only be found in the supplements. Are there any options to bring it into the main text?

We agree with the reviewer and most of this material is now in the extended data.

Figure 5 Is there a reason why only NfN is shown here?

We choose to represent in the figure and focus in the text on the scenario with additional conservation targets which is expected to lead to the most positive trend at the European level, NFN for birds and NAC for butterflies. This is now explicitly stated as follows (l. 320-339):

"In the scenario with the most positive trend for Farmland birds at the European level (NFN), Farmland Bird Index is expected to show more negative trends in biophysical regions with an increase in forest cover (e.g. Western part of France, North-western Spain), while more positive trends are expected in biophysical regions with an increase in low intensity farmland, protected areas and temperature (e.g. Northern UK, Scandinavia) (Fig. 5c and Fig. S1t).

In the scenario with the most positive trend for FoBI at the European level (NFN), forest birds are expected to show more positive trends in biophysical regions with positive forest cover change, in particular with low to medium management intensity (e.g. North-western Europe). In contrast, forest birds are expected to exhibit lower trends under NFN in biophysical regions with an increase in high intensive farmland, precipitation increase and rising urbanisation (e.g. Eastern Europe and Southern Europe) (Fig. 5f and Fig S1t).

For butterflies, the scenario with the most positive trend at the European level is NAC. Grassland butterfly species are expected to show more negative trends in this scenario in biophysical regions with increasing high intensive farmland and higher share of forests under low to medium intensity management (e.g. Central Europe). In contrast, grassland butterflies are expected to show more positive trends in NAC in biophysical regions with increasing temperature and higher share of intensive forests (e.g. Southern Sweden and Finland) (Fig. 6c and Fig. S1t)."

ll666-679 Somewhat repetitive of the results—consider shortening.

This part of the discussion was rewritten to respond to other comments.

l666 Rephrase to 'some of the objectives'. Not all objectives of the different strategies are considered in the scenarios.

This was modified as suggested.

l700 It's important to clarify here that the 'beneficial effect' refers exclusively to using SSP1 as the baseline—no other scenarios were tested.

We now clearly state the baselines in the text, as follows (l. 449-451):

"Furthermore, the beneficial effect for common species, observed on a continental scale, of scenarios with additional conservation targets compared to SSP1 and BAU (but also SSP3, Fig. S1u, v), is not evenly distributed."

ll715-717 Please add that this is likely the case for land-use related pressures as well. Agricultural inputs and/or intensity levels may change in the future in ways not reflected in historical data.

We add a sentence to reflect this point, as follows (l. 468):

"*This may also be the case for other drivers, such as land-use intensity."*

l722 Unclear how the multi-species indices help 'reduce prediction uncertainty'. Could the authors clarify?

We agree with the reviewer that this point needed clarification, we modified the sentence as follows (l. 472-475):

"Nevertheless, using common species helps to reduce prediction uncertainty and robust multi-species indices derived from these species facilitate the interpretation of expected change per habitat, highlighting differences between scenarios."

l724 Which 'important questions' are raised?

We were introducing the next sentences, this was clarified by adding a colon mark, as follows (l. 475-493):

"Notably, the continued decline of common biodiversity—even under scenarios incorporating a range of conservation measures or production changes—raises important questions: although the range of conservation targets implemented in the scenarios used in this study is limited, current measures are already being scrutinised and challenged[93]*, prompting reflection on the potential impact of more transformative conservation actions. This issue underscores both the societal acceptability of the drastic changes needed to halt biodiversity loss in Western societies*[2,94]*, and the limitations inherent in the SSP framework. If even SSP1, designed to focus on environmentally-friendly priorities*[78,79]*, enhanced by additional conservation measures in the three variant scenarios explored here, is insufficient to stop declines in common species, more ambitious regulations and alternative anticipatory frameworks may be required—ones that reconsider assumptions about the ongoing demand for natural resources to support food, energy, and economic growth*[20,95]*. These*

findings highlight the need for bold, integrated policy strategies that balance resource use with biodiversity conservation to achieve meaningful outcomes for common biodiversity, in particular by enhancing the landscape diversity and the transition of the agricultural model. Global scenarios can provide an insight into this trajectory, characterised by low demand for energy and raw materials, coupled with a food transition and a reduction in pollution from the agricultural system[96]; however, spatially detailed scenarios aimed at reversing the decline of common species and identifying compatible socio-economic pathways remain an area for further research."

ll728ff. I think it's a bit too simplistic to label SSP1 as the 'most environmentally friendly'. In what terms? When it comes to agriculture, SSP1 likely involves significantly more land use—albeit potentially less intensive—which could still have negative impacts on biodiversity. It may well be that another SSP, although not typically considered 'environmentally friendly', might perform better in the same analysis simply because it requires less land for agriculture and/or forestry.

We thank the reviewer for this insightful comment and agree that labeling SSP1 as the "most environmentally friendly" scenario is an oversimplification. As rightly pointed out, environmental performance depends on the dimension considered, and trade-offs exist between land-use extent and intensity. In particular, SSP1 may involve lower-intensity practices but also greater land demand for agriculture, which can have negative implications for biodiversity.

In response, we have revised the text to avoid this overly general characterization and instead clarify that SSP1 represents a sustainability-oriented pathway with specific assumptions (e.g., reduced intensification, changes in consumption patterns), but does not systematically minimize biodiversity impacts in our analysis.

To further address this point, we have also introduced a contrasting scenario (SSP3 – Regional Rivalry) in the Supplementary Material, allowing for a more nuanced comparison across pathways with differing land-use drivers and priorities. This helps illustrate that scenarios not primarily designed as "environmentally friendly" may, under certain metrics (e.g. total land-use demand), perform differently or even more favorably.

The revised discussion now explicitly acknowledges these trade-offs and avoids normative labeling (see citations in responses above, l. 395-404 and 475-493).

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Pereira, L. M., Davies, K. K., den Belder, E., Ferrier, S., Karlsson-Vinkhuyzen, S., Kim, H., Kuiper, J. J., Okayasu, S., Palomo, M. G., Pereira, H. M., Peterson, G., Sathyapalan, J., Schoolenberg, M., Alkemade, R., Carvalho Ribeiro, S., Greenaway, A., Hauck, J., King, N., Lazarova, T., … Lundquist, C. J. (2020). Developing multiscale and integrative nature–people scenarios using the Nature Futures Framework. People and Nature, 2(4), 1172–1195. https://doi.org/10.1002/pan3.10146

Reviewer #2:

In this manuscript, the authors use long-term monitoring data of birds and butterflies to model effects of main pressures (incl. climate change and land-use/land-management change) across 26 countries in Europe, and to model future trends under different climate and land-use scenarios. Their findings, namely that no future scenario will result in positive projected trends for birds and butterflies, are highly relevant for conservationists and policymakers, and point to the fact that a systemic change is necessary to reverse this pattern. I found the methodological approach in the manuscript to be robust and the results to be original and of immediate interest to the readership of Nature Ecology and Evolution. The authors used some of the best long-term biodiversity datasets available for Europe, and they uploaded a subset of this data to a Zenodo repository together with the R codes to ensure reproducibility of their analyses.

We thank the reviewer for the constructive comments that helped us to strengthen the analysis and clarify the manuscript and in particular for checking the reproducibility of the code. We provide below a point-by-point response to the comments.

I enjoyed reading this manuscript, it is well-written and well-referenced. My main comments are the following:

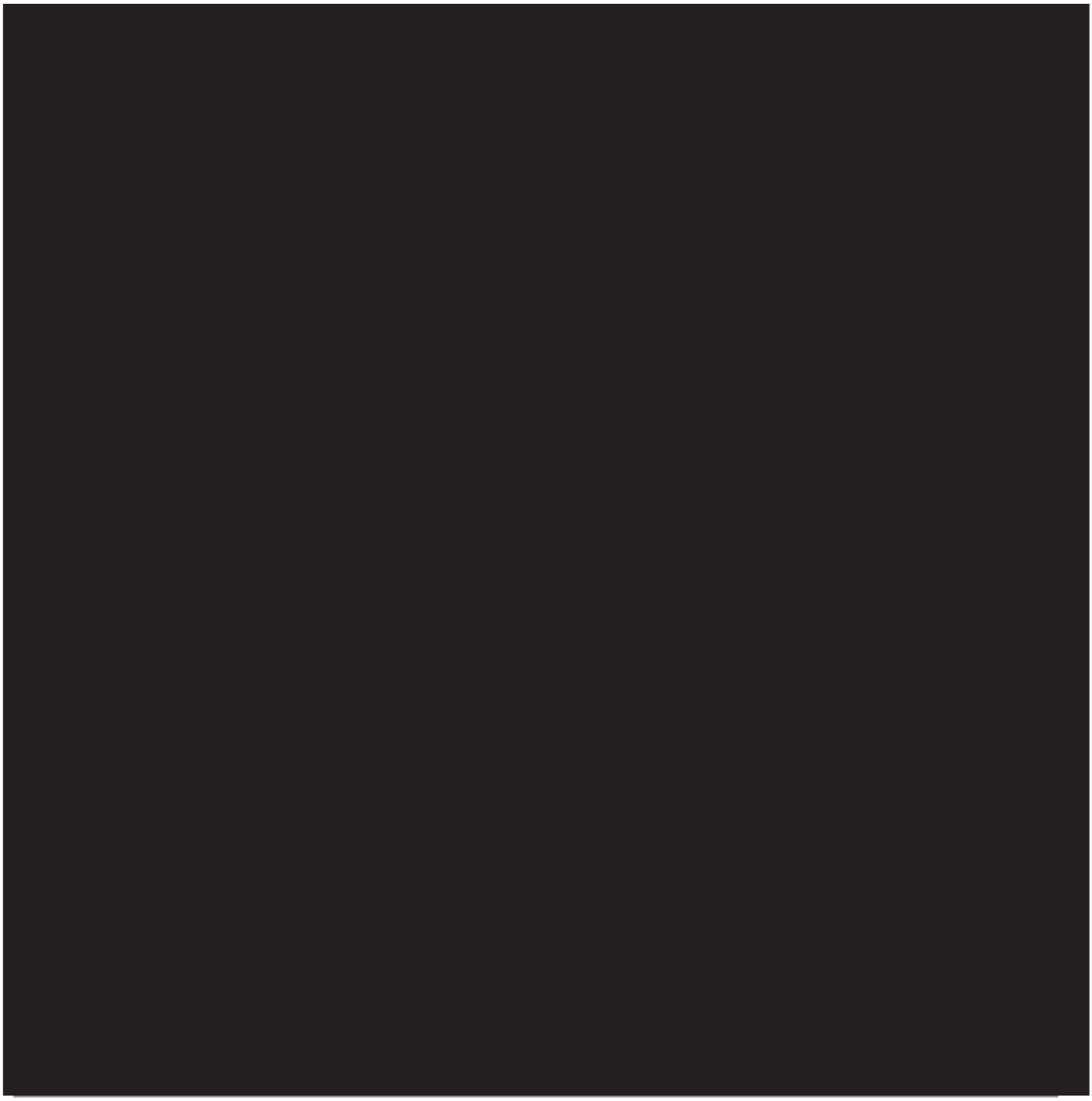
1. Representativeness of the data for the modelled areas: the butterfly data have important spatial biases, with most data being collected in the United Kingdom, Netherlands, Germany, Catalonia and southern Sweden. However, figure 6 shows projected butterfly trends also for eastern European countries where no butterfly survey was done (Fig. 1). I understand that these areas were modelled as part of larger biophysical regions (e.g. 2, 13, 19) for which some data exists – yet I wonder how confidently can model results be transferred across different and spatially separated parts of the same biophysical region?

We thank the reviewer for this important comment. We now present in Figure 6 the projected trends only in parts of biophysical regions (i.e. continuous polygons) where the monitoring transects are located.

Figure 6: Mean annual trend in abundance by biophysical region for a) grassland butterfly species between 2000 and 2021, b) difference between mean trends in SSP1 and BAU, c) difference between mean trends in NAC and SSP1, and d) for woodland butterfly species between 2000 and 2021, e) difference between mean trends in SSP1 and BAU and f) difference between mean trends in NAC and SSP1. Regions in grey have either not enough data for trend estimate or trend models with a low fit.

2. Could you provide confidence intervals for the projected future trends in bird and butterfly counts presented in Fig. 4? Some measure of uncertainty is important to evaluate whether these projected trends, under the different scenarios, deviate significantly from each other and from the trend observed in the past.

We agree with the reviewer that the confidence intervals are crucial to correctly interpret the projected trends. The figure presenting them was previously in supplementary material, it is now in the extended data (Extended Figure 1) to facilitate access to this information.



Extended Figure 1: Geometric mean of species temporal trend calculated with significant and non-significant parameters between 2000 and 2021 (Past) and in the different scenarios (BAU, SSP1, NFN, NFS, NAC), for a) All Birds index, b) Farmland Bird index, c) Forest Bird index, d) All Butterfly index, e) Grassland Butterfly index and d) Woodland Butterfly index. 95% confidence intervals from geometric standard error and the significant differences between scenarios are displayed (* p-value < 0.05, ** p-value < 0.01, *** p-value < 0.001).

Some specific comments:

L200: in order to harmonise abundance

This was modified as suggested.

Figure S1b and S1d: please add to the legend that the map of biophysical regions is in Fig. S1e (and please explain the colour code in Fig. S1e). It would be nice to have more descriptive names, not only numbers, to differentiate the different biophysical regions, if those are available.

We modified the legends as suggested. We agree with the reviewer that using names for biophysical regions would increase the clarity but unfortunately, the original publication (Renetzeder et al. 2008) only uses numbers. We prefer to keep these numbers to facilitate the comparison between this publication and our analysis.

L207 "we focused on sites visited at least five years between 2000 and 2021", but in your analyses only data from 2000-2005 and 2015-2021 are used, is this correct?

We thank the reviewer for pointing out this lack of clarity in the method. We used bird and butterfly monitoring data between 2000 and 2021. And the minimum number of 5 visits was set to avoid having only spatial replicated (i.e. sites monitoring once) and maximise the number of spatio-temporal replicates for our analysis based on species temporal trend. To improve the clarity of the period used for biodiversity and driver data, we added a specific table (Extended Table 1).

L278 is the circular buffer 2.5km in diameter or in radius?

This information was indeed missing, we used the radius and added this information to the text.

Figure 1: I think it would be helpful to also show, e.g. by using different colours, which type of monitoring scheme was used at each site (e.g. line transects, point counts, etc.)

We added a supplementary figure with the type of monitoring scheme (Fig. S3c).

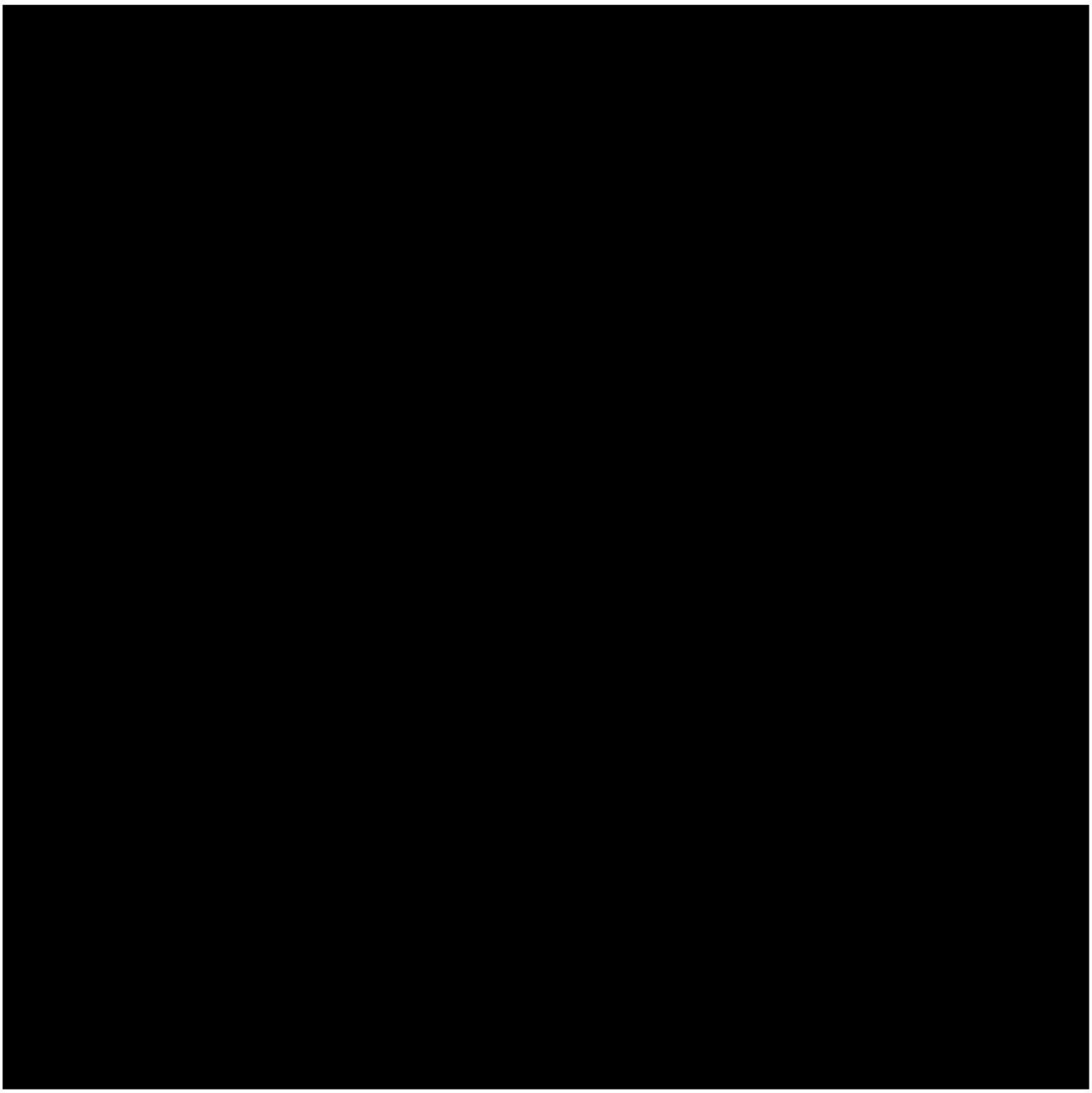


Figure S3c: Selected sites from bird monitoring schemes differentiated by scheme methods. Most countries typically use one scheme, with some exceptions: for Czechia and Latvia, we used respectively point count data and transect count data; for Finland, we included both point count data and line transect data that have a longer temporal and larger spatial coverage; for Spain we included the regional line transect scheme in Catalonia; and for Belgium we used schemes for Brussels and Wallonia.

Supplementary material 2: I find it odd to have the metadata pertaining the csv. tables in a separate file; could you add a metadata sheet to the bird and butterfly data table (and save as .xlsx files) or alternatively add a README file in the Zenodo repository where the csv. files are saved? This would make this information much easier to find! Similarly, the description of the files SM3_bird_model_check.pdf and SM3_butterfly_model_check.pdf are at the end of supplementary material 3 but not within the files.

As suggested we included a README file in the Zenodo repository with the metadata and added the description of S4h_bird_model_check.pdf and S4i_butterfly_model_check.pdf files at the top of the files.

L279 Please specify which figure within supplementary material 3 you are referring to, as this also includes graphs for model checking unrelated to the effect of buffer size. Also, supplementary materials should be numbered in the order in which they appear in the main text, so this should be supplementary material 2 instead of 3.

We carefully checked the text and now specify each supplementary figure when referring to supplementary material. We also changed the numbering accordingly.

L317 I guess you meant "imperviousness data" and not "imperviousness density data"?

This is right and now corrected.

L425-429 "The pressure variables included …. percentage of protected areas" perhaps there is a more fitting term than "pressures" to describe these covariates, as not all used variables are pressures (e.g. protected areas, landscape diversity)

We agree with the reviewer and use the more generic term "covariate" instead of "pressure" in this sentence.

L427 why were low and medium production forest types merged together?

The justification was missing and is now provided as follows (L. 632-634):

"For forest intensity, we merged low to medium management intensity as low intensity would correspond to primary forests that are not well represented in Europe."

L431 Was siteID treated as a random effect term? Was VIF of covariates always low?

siteID is treated as a random effect nested in the scheme id. The VIF of covariate was always below 2 as now explicitly stated in the text.

Supplementary material 2: please explain all acronyms (e.g. FaBI, FoBI, GBI or WBI). Also, are "Effect of covariate trends on species trend" the coefficients of the model in Eq.6?

We thank the reviewer for pointing this out. We now explain acronyms and link the "Effect of covariate" to the alpha and beta coefficient in Eq.6

Eq.3 is actually two equations.

We simplified these equations to reduce them to a single one.

Figure S3b (butterfly temporal slope comparison) what do the different colours mean? Note that the following figure (boxplots) is also labelled S3b, please correct this.

This information was missing and is now included as follows (and figure numbering was checked):

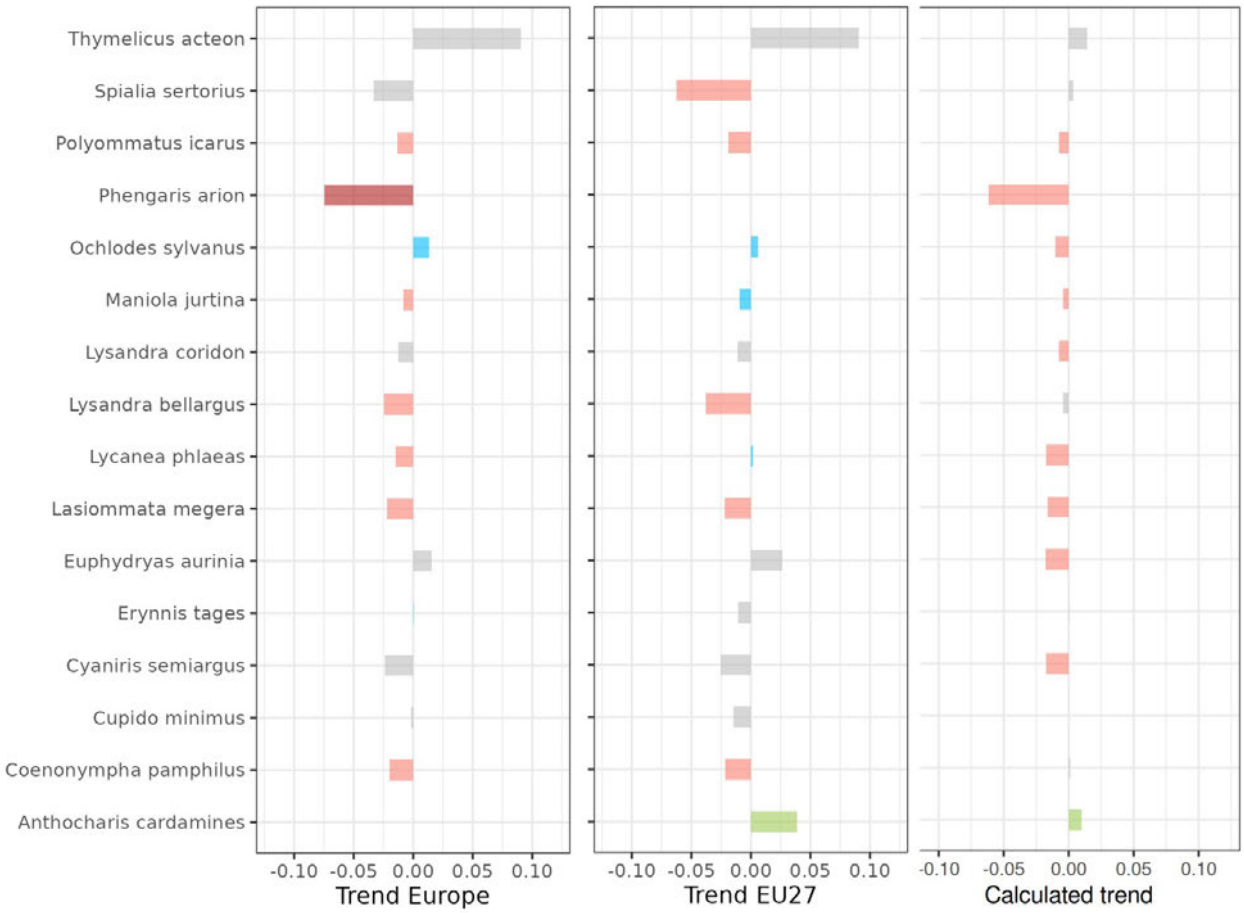


Figure S4g: Comparison between the temporal slope (i.e. multiplicative trend depicting an average percentage change per year) for 16 butterfly species calculated by our model with the slope provided by the *European Grassland Butterfly Indicator 1990-2020: Technical Report* (see this report for the detail of periods)[2]. Dark red indicate strong decline, light red indicates decline (moderate in Trend Europe and Trend EU27, significant in Calculated trend), green indicates increase (moderate in Trend Europe and Trend EU27, significant in Calculated trend), grey indicates uncertain trends (uncertain in Trend Europe and Trend EU27, non-significant in Calculated trend) and blue indicates stable trends.

Figure S3b (boxplots) why are boxplots for certain biophysical regions (e.g. 8, 15, 22, 24) missing?

This is the case for biophysical regions with not enough data to run the analysis (8,15,22) and Malta (20) for birds and more for butterflies. This is now clearly stated in the caption of figures S4j-m.

L452 please indicate clearly which item within supplementary material 4 you are referring to

We carefully checked the text and now specify each supplementary figure when referring to supplementary material.

L450-452 you write here that you focused only on significant model parameters to estimate projected temporal trend, but in Eq. 6 all parameters are included – am I missing something?

This was corrected, we now use all parameters and also provide trend estimation calculated with significant parameters only in Figure S4f. The results are very similar, except for woodland butterflies, where SSP1 becomes positive in the version that includes only significant results, due to the greater relative weight of the sharp reduction in agricultural land cover, the increase in which has a significant negative impact on the trend for the majority of these species.

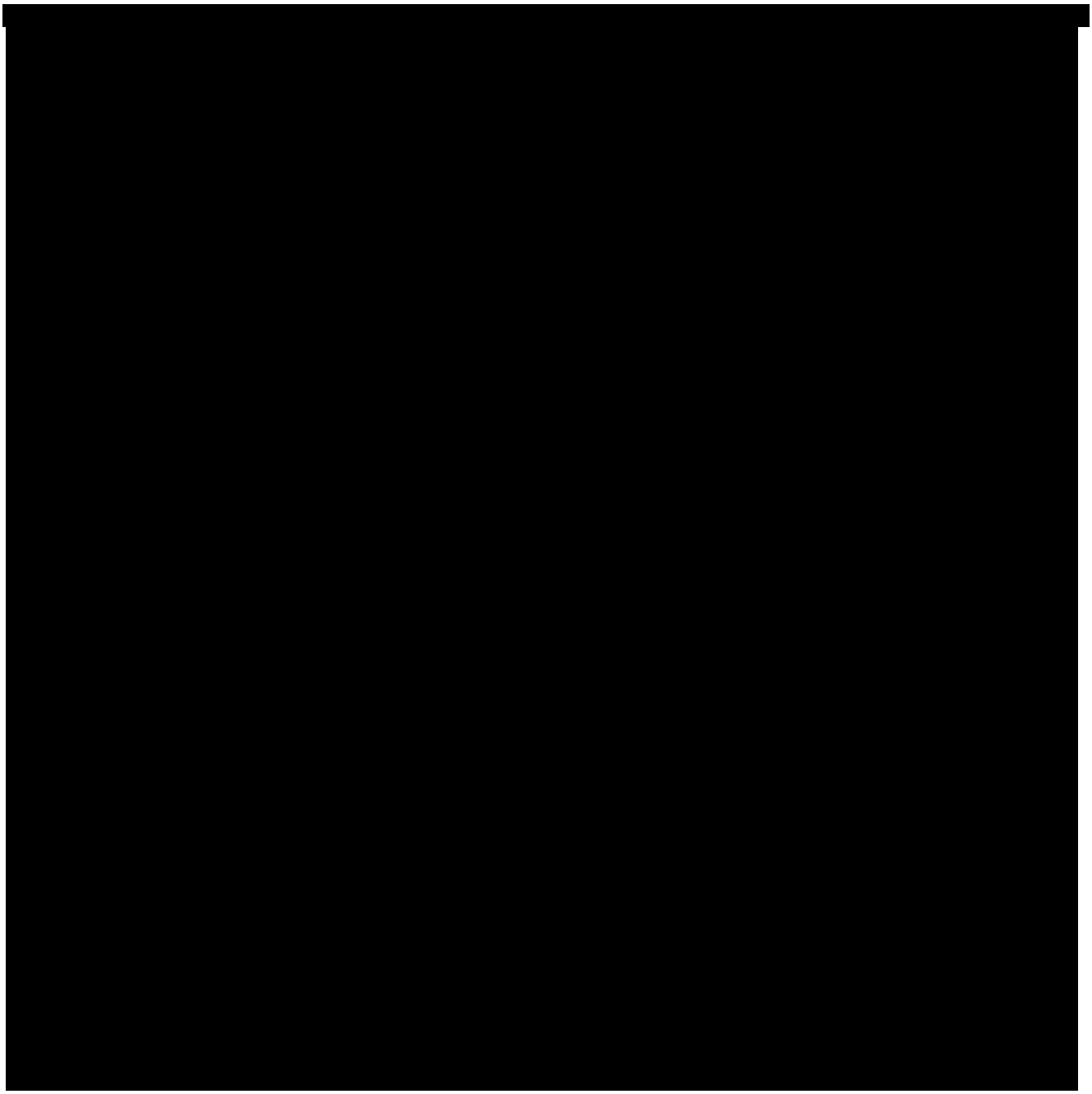


Figure S1y: Geometric mean of species temporal trend calculated with significant parameters only between 2000 and 2021 (past) and in the different scenarios (BAU, SSP1, NFN, NFS, NAC), for a) All Birds index, b) Farmland Bird index, c) Forest Bird index, d) All Butterfly index, e) Grassland Butterfly index and d) Woodland Butterfly index. 95% confidence intervals from geometric

standard error and the significant differences between scenarios are displayed (* p-value < 0.05, ** p-value < 0.01, *** p-value < 0.001).

L507 are these significant positive and negative effects, or not?

In the text, we now mention all effects (significant and non-significant) and present them also in Figure 2 and Figure S1a and S1b.

L564 (Fig. 2 c, f) instead of (Fig. 2 c, g)

This part of the text was modified but we carefully checked the references to figures in the text.

L568 also grassland butterflies, besides farmland birds, seem to benefit from climate change according to your results

For grassland butterflies, BAU was indeed slightly more positive than the past trend in the previous version but not significantly different. In the current version, due to the changes in some driver estimates (Land-cover and Shannon diversity, see the response to reviewer 1 comments l329 and l370), the trend in BAU and the past trend for grassland butterflies are even more similar. The comparison between trends was moved from the supplementary to the Extended data (Extended Figure 1) to facilitate the access to this important result.

Fig.5b the 1 is missing in SSP1

This is now corrected.

Fig. S4n: why is there no grassland butterfly species modelled? Please add a short explanation to the figure legend.

There is only one species in the grassland specialist butterfly class, so we do not model the multi-species index for this class. This is now explicitly stated in the caption.

Fig. S4z: is this figure ever referred to in the main text? What does LDA stand for? I suggest authors change the colour code as red and green might be hard to discriminate for color-blind people.

We thank the reviewer for this suggestion. The figure is now clearly referred to in the text and we modified the colors accordingly.

The R code needs minor revisions (and could benefit from some more comments to guide the user). For example, lines 18-19 in the "main_code.R" have some typos ("packages_publi.R" instead of "package_publi.R", and "function_publi" instead of "function_publi.R") that prevent the source codes to be loaded properly. The lines 127-145 and 206-220 return this error message: "No shared levels found between `names(values)` of the manual scale and the data's values."

We thank the reviewer for checking the reproducibility of the code and we made the necessary modifications.

Reviewer #3:

Rigal and colleagues have used monitoring data on common bird and butterfly species to

project their abundance in different sustainability scenarios. More specifically, the authors used the projected changes in climate, land use and land-use intensity of the Nature Futures Framework to assess their impact on 263 common bird and 144 common butterfly species as well as on selected indicator species for farmland and forest. Based on their results the authors conclude that sustainability scenarios need to be adjusted to improve biodiversity conservation while simultaneously allowing the use of natural resources.

Such modelling approaches are relevant and an important component to project potential outcomes of internationally agreed area-based conservation targets. This study adds to this field of research, in that it shows that these agreed targets seem not to halt the current biodiversity loss. Hence, the study calls for reconsidering the demand for natural resources to be able to bend the curve of biodiversity loss.

We thank the reviewer for the constructive comments that helped us to strengthen the analysis and clarify the manuscript. We provide below detailed answers to your comments.

In general, the paper is written well. Still, some aspects of the manuscript could be enhanced: 1) The introduction is not always clearly structured e.g. in the first and third paragraph you deal with human activities, drivers and pressures on common and rare species that could be better integrated. The same holds true for integrating the current international agreements and the forward-looking approaches that aim to integrate societal needs with conservation targets. It would be helpful to reorder parts of these thoughts to streamline the arguments. The aspect of indices and indicators comes a bit out of the blue given the focus on common species, i.e. try to better integrate. Most importantly, I am missing clear expectation/predictions on how these different scenarios might impact common biodiversity in terms of birds and butterflies. This would also help to structure methods, results and discussion more clearly.

We thank the reviewer for this comment on the framing of the introduction. We rewrote and shortened the introduction to match the journal requirements and we believe the clarity of this section has improved. We also clearly state expectations on the impact on common biodiversity, as follows (l. 118-124):

"*The wide range in expected land-use and land-use intensity changes across scenarios*[19,25] *is therefore likely to influence the future dynamics of common biodiversity. In particular, the trade-off between an increase in natural lands combined with higher intensive practices in productive areas versus landscapes where human activities and non-human dynamics coexist is expected to result in contrasted dynamics for species*[26]*. The majority of common birds, for instance, depend on open land vs. forests*[27] *or are sensitive to agricultural practices*[22]*."*

2) Some aspects in the method section need some more explanation. While the monitoring was well explained, Fig. 1 summarising the data should also provide the number of monitoring sites and countries available for birds and butterflies. Next you explain the environmental and climate data that are needed to project abundances based on different scenarios. It would be helpful to add scenario input data or something alike to guide readers through the methods (see also my comment on expectations). This is also obvious in Fig. 2 which lacks e.g. explanations in the legend for the different scenarios, i.e. BAU, SSP1 etc., same holds true for y-axis labelling. As the general change in % of total surface is rather low, this needs to be addressed throughout the whole manuscript most clearly.

Following this suggestion and similar comments on the clarity of the method section from the other

reviewers, we also rewrote the method section. We added the number of sites and countries in the legend of Extended Figure 8. We also add the explanation for the name of the different scenarios in the legend of Figure 3 and make the y-axis labelling clearer in Figure 3. However, if the values of changes may seem low, it should be noticed that it is an annual rate of changes, i.e. an annual change of 0.3% represents 9% of change over 30 years.

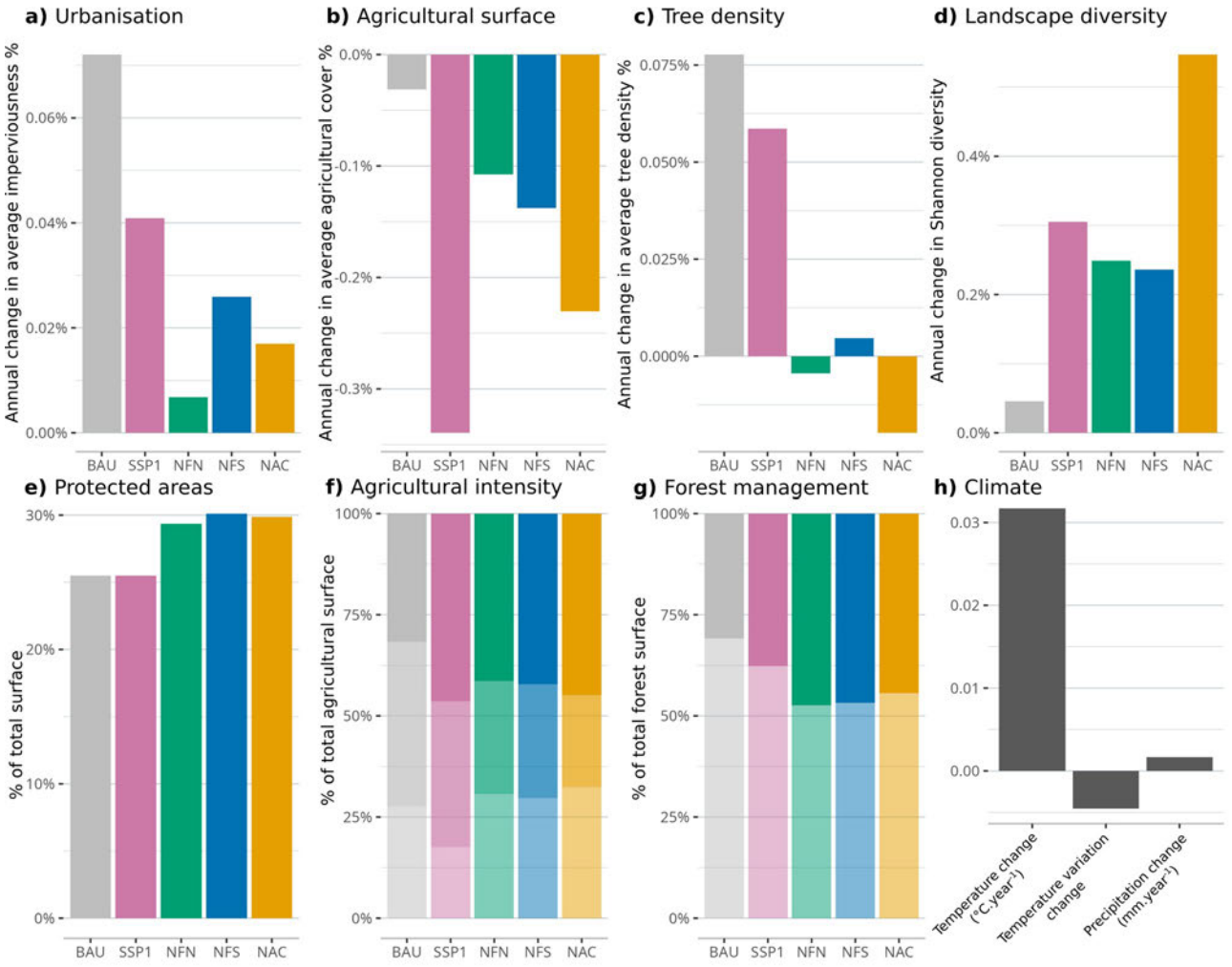


Figure 3: Predicted changes in scenarios in 2050 at the European scale in land use: a) urbanisation (annual rate of change in the average percentage of imperviousness), b) agricultural surface (annual rate of change in the the average percentage of agricultural cover), c) tree density (annual rate of change in the average percentage of tree density), d) landscape diversity (annual rate of change in the average landscape diversity); in land-use intensity: e) protected areas (average percentage of protected areas), f) production intensity in agricultural areas (low intensive in shaded colour, medium intensive in intermediate colour and high intensive in full colour), g) production intensity in forest areas (low/medium intensive in shaded colour, high intensive in full colour), and h) in climate (temperature change in °C.year^{-1}, and precipitation change in mm.year^{-1}). BAU: Business-As-Usual, SSP1: Shared Socioeconomic Pathway 1, NFN: Nature For Nature, NFS: Nature For Society, NAC: Nature As Culture. Note that in addition to the quantitative differences between scenarios, the location of the changes varies (Extended Fig. 3-7).

3) The description of your findings could be enhanced, i.e. using structuring expectations that would help guide the reader through your findings. Currently, you are presenting the effects of climate, land use and land-use intensity somehow not embedded within the scenarios which are the main topic of your manuscript, e.g. you mention only numbers of species that are

projected to respond positively or negatively but not proportions of all common/farmland/forest species; likewise the effects of climate, land use and land-use intensity could be ordered rather than presenting them with dark and light green colors interchangeably. Fig. 4 projecting the abundance trends of all common, farmland and forest species according to the different scenarios could be improved, i.e. add information that y-axis scaling differs and try to optimize further so that the number of the axes are comprehensible (explain why axis for all species is limited to c. 100 while for farmland and forest it ranges from 200 to 1000). Fig. 5 and 6 are somehow hard to grasp, e.g. it is difficult to relate the trends to numbers.

We thank the reviewer for these suggestions. We enhanced the structuring of the description of the result by linking more directly driver effects and scenarios and reporting the proportion of species negatively and positively impacted specifically with an additional supplementary figure (Figure S1a).

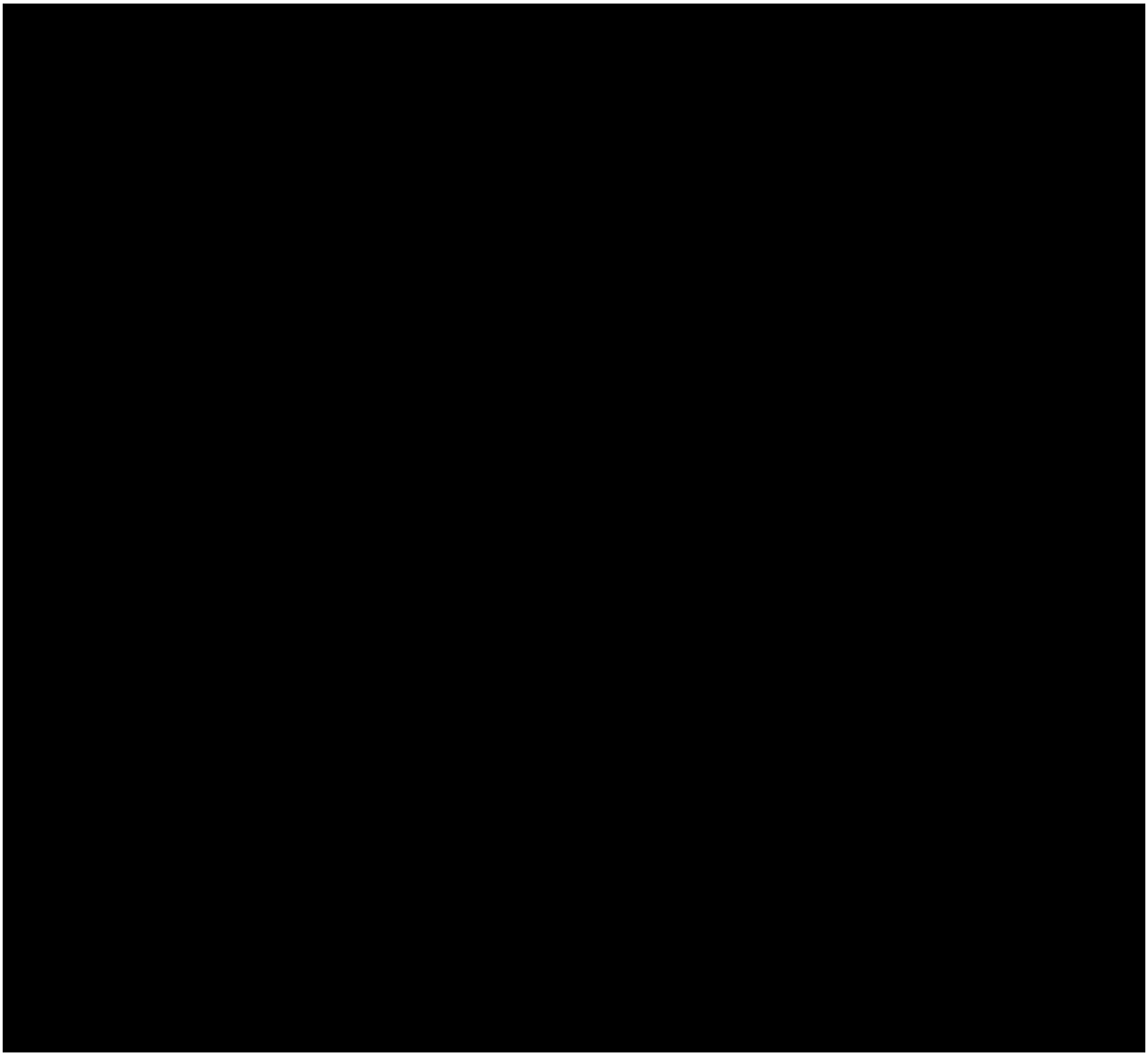


Figure S1a: Distribution of positive and negative effects of drivers and environmental variables on temporal trends at the European level for all bird species (n = 223), b) farmland bird species (n = 34), c) forest bird species (n = 29), d) all butterfly species (n = 135), e) grassland butterfly species

(n = 12) and f) woodland butterfly species (n = 28). Non-significant negative effects in orange, significant negative effects in dark red (p-value < 0.05), significant positive effects in dark blue (p-value < 0.05), non-significant positive effects in light blue.

As the order of driver effects varies across species type, we kept the same ordering for the drivers in Figure 2, to facilitate the comparison between species types. We followed the reviewer' suggestion to improve Figure 4 by using the same y-axis and optimizing the number of axes. The changes in the y-axis value for grassland and woodland butterflies are linked to the refinement of the model for estimating trends that we propose in this revision. Rather than excluding the top-percentile site abundances (aim: to limit potential bias due to very high population sizes), we replace the highest values with the top-percentile value (same aim, but this avoids disregarding years/sites where the species is very abundant). Consequently, we now use a negative binomial distribution rather than a quasi-Poisson distribution, so that sites with high abundance do not overwhelm the influence of sites with lower abundance. Furthermore, the use of a GAM for large datasets (bam function) has allowed us to include the site effect as a random variable (previously done only at the bioregion level) and to increase the number of dimensions of the smoothing term for spatial autocorrelation. Finally, to ensure greater reproducibility, rather than limiting trends by species to the 10th and 90th quantiles of the observed trends, we use the 'official' PECBMS trend values and limit the calculated trends to the range between the minimum and maximum of the PECBMS values, with geometric trends ranging from 0.93 to 1.07, which corresponds to an annual decrease or increase of 7% (and is classified as very strong by the PECBMS). This yields past trends that are closer to those reported in reference material (Figure S4f and g) and robust to species removal (Figure S1u).

Figure 4: Predicted abundance trends of a) Common Bird Index, b) Farmland Bird Index, c) Forest Bird Index, d) Common Butterfly Index, e) Grassland Butterfly Index and f) Woodland Butterfly Index. Observed trend (2000-2021) is displayed in black, Business As Usual (BAU) in dotted grey, the "Taking the Green Road" Shared Socioeconomic Pathway (SSP1) in purple, Nature For Nature (NFN) in green, Nature For Society (NFS) in blue and Nature As Culture (NAC) in yellow (see geometric standard errors in Extended Fig. 1). We use 2021 as a pivotal year, i.e. a reference to express the relative population abundance with a value of 100 in 2021.

We did not find a satisfactory way to facilitate the readability of Figure 5 and 6 but we believe that the Shiny application (https://press-com-bio.sk8.inrae.fr) provided will help the interested reader to look at the detailed results.

4) While the discussion is a nice piece to read. Some more details could be added to enhance the overall contribution of your work to this field of research. Most importantly, the relatively small change in % of surface of land use across scenarios should be highlighted. How does this relate to the continuous decline of common species, i.e. are agreed targets to modest, e.g. % of surface change not very large. Please add some thoughts on that as well as spatial configurations in the different scenarios and how this might impact your projected abundances. Also, the final section on implications of your work could be enhanced, e.g. what exactly do you mean with bold, integrated policy strategies and who can scenario projections contribute to this. Please specify!

We thank the reviewer for this important comment. Concerning the rate of change in land-use, as we explain above, the values of annual changes may seem low per se but the total amount of change over the period (2021-2050) is significant. We agree with the reviewer that specification was needed in the final section and we completed it as follows (l. 486-493):

"These findings highlight the need for bold, integrated policy strategies that balance resource use with biodiversity conservation to achieve meaningful outcomes for common biodiversity, in particular by enhancing the landscape diversity and the transition of the agricultural model. Global scenarios can provide an insight into this trajectory, characterised by low demand for energy and raw materials, coupled with a food transition and a reduction in pollution from the agricultural system[96]; however, spatially detailed scenarios aimed at reversing the decline of common species and identifying compatible socio-economic pathways remain an area for further research."

Thank you for submitting your revised manuscript "Predicted decline in common bird and butterfly species despite conservation policy scenarios in Europe" (NATECOLEVOL-25082960A). It has now been seen again by the original reviewers and their comments are below. The reviewers find that the paper has improved in revision, and therefore we'll be happy in principle to publish it in Nature Ecology & Evolution, pending minor revisions to satisfy the reviewers' final suggestions and to comply with our editorial and formatting guidelines. Please do not revise in response to the reviewers' current suggestions until you receive further information on these formatting points from us.

We thank the Editor and the three reviewers for their positive feedbacks and the additional comments, to which we respond point-by-point below.

Reviewer #1:

The authors have substantially improved the manuscript, particularly with respect to clarity in the methods section and the presentation of results. The revisions reflect careful consideration of reviewer feedback, and the paper is clearly stronger than the original submission. However, some of my comments have not been fully addressed.

We thank the reviewer for the positive feedback and we hope the changes provided in the new version now addressed his/her comments.

One point that still requires clarification concerns the relationship between the Shared Socioeconomic Pathways (SSPs) and the Nature Futures Framework (NFF). The manuscript does not yet sufficiently emphasize that SSPs and NFFs are conceptually independent scenario frameworks. In its current form, the text gives the impression that the NFF framework is primarily there to 'nuance' SSP worlds, whereas in fact NFF scenarios can be implemented independently of SSPs and could equally draw on societal drivers derived from alternative scenario narratives or extrapolations (e.g., ll112 ff.)

As suggested, we now clearly state that the two frameworks are independent (l. 141-144):

"The IPBES Nature Future Framework (NFF), a scenario framework independent of SSPs, provides an operational tool allowing biodiversity positive futures under different value settings to be identified[24]."

Furthermore, I think methodological simplifications arising from the aggregation of land-use categories, the (potential) inconsistencies between historic, present, and future data and the use of partly non-overlapping time periods should be explicitly mentioned in the discussion. I think this could be done by just adding one sentence acknowledging such limitations.

We agree with the reviewer that these limitations should be explicitly mentioned and we included them in the discussion, as follows (l. 445-453):

"The work of forecasting remains limited notably by uncertainty related to the choice of conservation strategies, their translation into spatial modifications of direct drivers, the limited set of drivers available with partly non-overlapping time periods, the link to responses of common biodiversity and the absence of stochastic processes[90]. Using observed effects of climate change to anticipate the effects of future climate change, in particular, has to be interpreted with caution, as this driver is likely to reach levels of change that go beyond the ones observed in the past decades (i.e. the period for which biodiversity data are available)[91]. This may also be the case for other drivers, such as land

use and land-use intensity, whose estimated impacts could also be limited by the aggregation of land-use categories and by potential inconsistencies between these categories in past and future data."

Relatedly, the land-use outcomes, and in consequence all conclusions drawn in the paper, depend on the specific implementation and parameterization of SSP1, as well as on how policy targets are translated into model parameters within the different NFF value perspectives (as the authors now also acknowledge in the discussion). Previous LU model intercomparison studies have demonstrated substantial variation in land-use outcomes across models, scenarios, and implementations (e.g. Popp et al.https://doi.org/10.1016/j.gloenvcha.2016.10.002 ; Alexander et al. https://doi.org/10.1111/gcb.13447). While the comparison to a SSP3 scenario is very insightful, I think it would be the SSP1 (and/or NFFs) implemented in another LU model that would be required to test the robustness of the results. I am not asking for this, as I think it goes far beyond this study, but a concise statement noting that results (and even conclusions) may vary considerably depending on how (in this case) SSP1 is parameterized in a given land-use model would help contextualize the findings and avoid overgeneralization.

As suggested, we added a concise sentence in the discussion highlighting this point (l. 380-382):

"Given that land-use outcomes can vary from one implementation to another[79,80], the effects on biodiversity in other SSP1 parametrisations may differ."

In addition, the explanatory details provided in the authors' response to the comment made on line 275 of the original submission should be added directly to the manuscript.

This information is now provided in the relevant paragraph of the methods (l. 553-554):

"By using a six-year average, we ensure that we obtain a reliable estimate of the climatic variables at the start and end of the period, despite annual variability."

Finally, replacing the term 'pressure' with 'driver' is not always appropriate, especially in contexts where it leads to formulations suggesting that 'drivers increase or decrease'.

We checked the manuscript to ensure that the wording was appropriate.

Reviewer #2:

I thank the authors for carefully addressing all my comments, and I found the manuscript to be much improved since the first submission. I especially appreciated their effort in making their results more accessible by developing a Shiny app, which allows readers to interactively browse through their many outputs. This app is currently only mentioned under the Data availability section of the main text; I think readers should be directed to it more explicitly – could this be mentioned in the results section, and/or in the figure legends of the related results?

We thank the reviewer for the positive feedback and the additional comments provided. As suggested, we added the link to the Shiny app in the result section as a note.

"The results presented below and in the supplementary documents are also available in an interactive format on https://press-com-bio.sk8.inrae.fr/."

I also found some of the newly added figures, particularly the analytical workflow in figure 1 and the figure S1a in the appendix, very useful in improving the manuscript's clarity and interpretation. Since the last review, the authors added a comparison with the scenario SSP3 in (some of) their results. I also found this to be a useful addition, but this additional analysis and related results are touched upon only in the discussion section. I would recommend the authors to add information about this analysis in the methods and results.

We now introduce the SSP3 in the method section (l. 636-638)**:**

"In addition to these four scenarios, an implementation of SSP3 "A Rocky Road", based on future regional rivalry is available for comparison and results based on this SSP3 scenario are presented in supplementary material (Fig. S1u, v)."

Besides this, I only have a few minor comments:

Figure S1c-d panels a-c show proportions (between 0 and 1) rather than percentages (0-100%).

This is now corrected.

Figures 2 and S1b: please add to the legend what the vertical solid and dashed lines represent – is the solid line the average coefficient across all species?

We added the following information to the captions of these figures.

"In each panel, 0 is indicated by a dashed line and the median of the distribution by a solid line."

L401 could you add (here or earlier on in the methods section) a few words on why SSP3 was chosen as comparison, among all SSP scenarios?

See the response to reviewer's second comment.

L403 Compared

This is now corrected.

L730 which was always below 2

This is now corrected.

Figure S3b and S3e: The legends state "See the map of biophysical regions in Fig. S1f." but the map of biophysical regions is actually Fig. S3f.

This is now corrected.

In general, I find the numbering of the figures in the supplementary material a bit confusing, particularly the use of letters, which may refer either to different figures (Fig. S1a, Fig. S1b) or different panels within the figures – but I imagine this will be formatted according to the journal's guidelines upon acceptance.

We did not change the numbering at that step.

Reviewer #3:

I would like to congratulate the authors on the constructive revisions they have made to their text, including the visuals. Overall, my comments have been satisfactorily addressed. The only point that, in my opinion, has not yet been implemented is concluding the introduction with clear expectations/hypotheses regarding the different scenarios. This should be possible based on the published literature, especially since you referenced to it in the discussion. As previously noted, these expectations would serve as a structural framework throughout the manuscript.

We thank the reviewer for the positive feedback. We added the following sentence at the end of the introduction to clarify the different hypotheses (l. 176-180):

"While the achievement of conservation targets is expected to benefit biodiversity by mitigating several direct drivers of degradation, the need to meet competing societal demands may exacerbate other pressures, particularly those associated with increased land-use intensity, resulting in uncertainty regarding the net biodiversity outcomes."