Dear Subject Editor,

On behalf of all authors, I want to thank you for the opportunity to resubmit this manuscript, “\_\_\_” for consideration in Global Change Biology. We have carefully reviewed all of the feedback on our first submission and have integrated such feedback into this new version of the manuscript for review. The majority of the reviewer comments focused on clarity in the statistical approaches used in this study as well as the necessity for more background information about expected dynamics in this high-latitude butterfly system. We have now significantly overhauled the introduction and methodology section of this work to address these concerns. We have also addressed the minor comments that the reviewers left during the initial review. Overall, we believe that the reviewer comments have strengthened this work which concerns the first ever documented trends for high latitude butterfly communities in North America. We hope that this work will inspire future investigation into the unique threats faced by cold-adapted butterflies in the face of accelerating climate change.

You can find our detailed responses to reviewer comments appended to the end of this document as well as line-by-line changes to the original manuscript in additional files attached to this submission.

Once again, many thanks for your time and consideration,

Vaughn Shirey, Ph.D.

David H. Smith Postdoctoral Conservation Research Fellow

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**RESPONSE TO REVIEWERS**

**Reviewer One**

The study investigates the impact of global climate change on insect populations, focusing on butterflies in poleward regions where the effects of warming are most pronounced but biodiversity data is lacking. The study analyzed 50 years of butterfly population data, revealing that cold-adapted species are declining more than warm-adapted. Species' average annual temperature range and wingspan size emerged as consistent predictors of these trends. I like the study, and it was a pleasure to review it. However, there are a few areas that the authors could clarify or expand upon to strengthen their manuscript. Below are my specific comments.

**Thank you for your time in reviewing our work. We are glad you found it of interest. We have worked to address your specific concerns below in a new version of the work.**

The title suggests that minimum temperatures (rather than for example summer temperatures) are responsible for the shifting butterfly communities. Also, the discussion starts with a lengthy discussion of why winter temperatures may influence butterflies. I find this a bit misleading, as the authors did not test different types of temperatures (minimum vs summer temperatures, for instance). As different types of temperature are usually correlated, the results might have been very similar for summer temperatures as the presented results for minimum temperatures.

**Thank you for this feedback, we agree that there may be confusion here and have since reorganized our introduction to reflect that we only model “minimum temperature” broadly rather than season specific temperatures (and as you mention, seasonal trends are likely highly correlated which would be difficult to account for in our modeling framework). We still think the discussion of winter temperatures as a limiting factor to poleward range expansion is relevant to the patterns we found but have now moved a majority of this to the discussion where we comment further on why we see the occupancy patterns we do.**

The first sentence of the abstract: I am not quite sure that the current literature submits that climate change is a major driver of observed insect declines. This statement needs justification in the introduction. I think that many studies confirm climate change as a major driver of shifts in insect communities, with climate winners and losers. This becomes important as the authors suggest a more nuanced perspective of insect biodiversity decline in favour of a scenario of climate “winners” and “losers” (see l. 451454). A suggestion that is already there.

**Thank you for pointing this out. We have reorganized our discussion and language here to point out how climate change can drive shifts in communities (e.g., “winners and losers”) but not necessarily all-out declines in insects broadly (although it may be compounded with other more deleterious processes like invasive species proliferation, disease, etc.). We have added additional cited literature to support this scenario.**

The main assumption of occupancy modelling is probably not met: one of the critical assumptions of occupancy-detection models is the closed population assumption, that is, that the occupancy of a species at a site remains stable between multiple visits. In this study, the five years within a five-year period are used as repeated visits. However, it seems very unrealistic that no change within the five-year periods does occur while there are clear changes between five-year periods. Thus, the closed population assumption is likely to be violated. I am not sure if and how this will affect the results, but I propose that it needs to be addressed more explicitly.

**This is true, we do violate the closure assumption in this work; however, we note that simulation studies indicate that, when applied to opportunistic data, occupancy-detection models can still robustly estimate trends in occupancy despite this violation (see Shirey *et al.* 2023 in Methods in Ecology + Evolution where we break simulated data into various time bins and still reclaim trends). Further, empirical work with opportunistic data that has examined how these visits and closure-periods are determined has little to no effect on inference (Jönsson *et al.* 2021). Since interannual variability in insect populations can be quite high, we view our binned time intervals as smoothed (*i.e.,* averaged) estimates of occupancy probability over a discrete time frame. We have made these citations and rationale more prominent in the methodology section where we discuss the occupancy-detection framework.**

The description of the occupancy-detection modelling could be improved. For example, a link-function seems necessary to constrain the values of Psi and p between 0 and 1. Such a link function is, however, not mentioned in the formulas or the text. Also, it is not quite clear to me how the random effects were specified. The authors also mentioned that they specified priors proposing that they applied a Bayesian framework (which was however not mentioned in the text).

**Thank you for pointing this out. We agree and have now modified the description of our occupancy-detection model to include the use of the logit-link function (this was an error of omission on our part). We have also clarified more details about the random effects structure in our model. With respect to priors, we have clarified that all mean values in our model are drawn from normal distributions with a mean of zero and a variance that was drawn from a uniform (flat) distribution. This means that we are assuming no-effect of our covariates on occupancy or detection.**

- abstract: it is mentioned four times in the abstract that the study cover a region with sparse biodiversity data (l. 13, 15, 21, 25). Please avoid redundancy. **Done, we have now removed some of these references and kept them only where we make the sparsity of records a core focus of the sentence.**

- l. 162: remove "make" **Done.**

- l. 179: .remove "were found to" **Done.**

- l. 307-309: I do not understand. This sentence seems to describe the specification of the prior (apparently you aim to estimate the parameter values using a Bayesian approach)? It seems that the normal distribution is describing several parameters, as the variance of the Normal distribution has also a prior. **We have clarified this following your larger comment, but the parameters are being drawn from their own normal and uniform (in the case of variance) distributions (not one universal distribution).**

- l. 321: what is on a linear scale? **We have removed this language from the manuscript since we have now clarified that the logit-link function is necessary for our calculations.**

**Reviewer Two**

This is an interesting and very promising manuscript on the effects of climate change on butterfly communities and what the role of recent warming has had on population decline.  
  
With some revision and more clarity on the background, methods and results it has potential to be an interesting and influential paper within global change ecology and more specifically in the subfield of identifying and predicting species responses to climate change. Using predictors describing the climatic conditions across the geographic range is a valuable part of the study since this is something that is being shown in more and more studies to have explanatory power in explaining species responses to climate change.  
  
The manuscript still needs some work though (see major comments 1-3). Not least is there some additional editing needed. There are many unfinished sentences and relatively many typos, long and complicated sentences, and missing words. It feels like the last few editing rounds were left undone before submitting. Please revise thoroughly so that the reviewers in the next round can concentrate on the content instead of guessing what you are trying to say. That being said, the results are cool and interesting, and the methods seems robust, but they need to be backed up with statistics in the main text (both in text and in tables). If the results and methods hold after clarifications and adding statistical information I would love to read and cite this paper when it is published!

**Thank you! We appreciate your feedback (and are excited that you are enthused about using it in your own work). We have significantly expanded upon the statistical component of our research in both the main text and supplemental materials. Please see our comments below addressing your specific concerns.**

1. Recent studies have also looked into the range-wide thermal conditions of species and found that this may in fact be a better predictor than many often used traits. This is one of the most interesting parts of the study and I think you would benefit from looking up similar studies where similar patterns have been found to place your expectations and findings in context. In this study, mean temperature across the range of the species is used, although thermal variability across the species range could be more proximately connected with species ability to persist since this affects how specialized vs. generalized they are to climatic conditions. Therefore, I would suggest (although not require) that you explore this trait too. See e.g.:

Day, PB, Stuart-Smith, RD, Edgar, GJ, Bates, AE. (2018) Species’ thermal ranges predict changes in reef fish community structure during 8 years of extreme temperature variation. Divers Distrib. 24: 1036– 1046. https://doi.org/10.1111/ddi.12753

Herrera, J.M., Ploquin, E.F., Rasmont, P. & Obeso, J.R. (2018) Climatic niche breadth determines the response of bumblebees (Bombus spp.) to climate warming in mountain areas of the Northern Iberian Peninsula. Journal of Insect Conservation, 22, 771–779

Hoffmann, A.A., Hallas, R.J., Dean, J.A. & Schiffer, M. (2003) Low Potential for Climatic Stress Adaptation in a Rainforest Drosophila Species. Science, 301, 100–102.

Scridel, D., Bogliani, G., Pedrini, P., Iemma, A., Hardenberg, A. von & Brambilla, M. (2017) Thermal niche predicts recent changes in range size for bird species. Climate Research, 73, 207–216.

**Thanks for this suggestion – this is something that has been on our mind since our original submission, and we are glad that you brought it up. We have now included a new post-hoc analysis where we use the difference in minimum and maximum average annual temperature across a species range as a predictor of decline. In doing so, we found that the difference in minimum and maximum temperature across the range is not a strong predictor of occupancy trends, suggesting that average range wide temperature is still the only consistent predictor of occupancy trend in the region. This was a new, unexpected result, and we have now added content to the discussion to discuss the ramifications of this new model. We have also included the citations you listed above for context.**

2. As the manuscript stands now, there is quite an imbalance between the introduction and the rest of the content. The introduction sets the manuscript up to deal with the methodology of occupancy modeling, although the abstract, results, and discussion deal with the effects of climate change on butterfly occupancy. Thus, the intro does not introduce the theory behind why species would shift according to some common rules, e.g. based on traits or climatic niches. And vice versa, the results and discussion does not deal with the pros and cons of the method e.g. through a comparison of this method to some other method or using other data (except as anecdotal comparison to previously observed patterns – see my third comment concerning this). This gap needs to be addressed and my suggestion is that you reduce the methodology part in the intro (you can move some of it to the methods) and instead concentrate on the ecological theory or your research question in the intro. What were your hypotheses and why? What are the theories behind the proposed hypotheses and are there previous empirical evidence to support them? How do they align with your aims a-c?

**Thank you for making this point, we have now modified the introduction to give further detail on our hypotheses and the literature that supports those assumptions. The authors and I have discussed the issue of having a methodology-forward introduction and ultimately decided to retain much of the language from the original manuscript. We feel that this is important since we are one of the first groups to use this approach with presence only data and that, in doing so, we need to make a strong case approach as well as provide context for why we are not using more traditional approaches (e.g., MaxEnt modeling, etc.). We are happy to reconsider this since we do feel like the introduction is quite long; however, would likely move much of this text to a supplemental file. In short, because of the nature of our dataset, we feel like the issues with presence only data and our approach deserve comprehensive recognition in the introduction.**

3. The weakest points, I find, are a) the way in which you present the results using only figures, and b) how you motivate the robustness of your chosen method.

3a. You do not present any statistical results in the main text, only refer to figures, while making rather bold statements on the results. The results section needs to be backed up with numbers referring to mean estimates and confidence intervals for the reader to be able to evaluate the reliability of your results and how your inference aligns with these.

3b. Just because the inference of your study aligns in broad sense with some previous results from other systems and regions it doesn’t mean that the method itself is robust. I wonder, is this line of motivation even needed? You refer to other work that have shown the strength and weaknesses of occupancy models, so could you not just fall back on that instead of trying to make this a methods paper?

**Thank you for making these points. We have now included statistical results in the main text to highlight trends for select species.**

The title is imprecise. 50 years of shifting OF WHAT? It is also somewhat long and I that you revise the title to be more succinct and exact. **We have now workshopped a new title which highlights the core message we want to convey, the decline of cold-adapted species, due to climate change. The title ended up being about the same length, but it is now more results oriented.**

Lines 10- 12. Should the sentence end with “change” instead just “responding to climate”. **Done.**

Line 15. What continent? **Changed to “North America.”**

Line 71 regionS, I believe. Leading to what kind of profound impacts for insect development? **Fixed, we have also now added some examples of impacts to development.**

Line 72 you say to PERSIST, but one of the alternatives on line 74 is to face risk of decline. **We have now modified this sentence to address other reviewer feedback.**

Line 81 “Than” instead of “that” + logic not clear in the end of this sentence (how high human population in the south is connected to the sapling pattern). **Fixed**.

Line 85 tautology, platform twice, can one be removed/exchanged? **Changed the first instance to “programs.”**

Line 99 “with” instead of “of” **Fixed.**

Line 108 something is not right in the middle of this line

Line 110 “had” instead of “having”. And explain why this did not make sense. **Fixed.**

Line 111-114 simplify sentence, perhaps split **We have now condensed this sentence.**

Line 119 is there a word missing between presence-only and for? **Fixed.**

Line 120-124 long and complicated sentence, please revise. **Fixed.**

Line 128-129 something not right in this sentence. **Fixed.**

Line 139 why minimum temperature? **We have now linked this decision to previous paragraphs that discuss the importance of minimum temperature in our study system.**

Lines 143-146 are less likely refers to northern range edges here. I cannot make sense of this sentence. Sentence ends with “much lower “, but where? Even adding “much lower there” would make it clear that you mean in the south, which I’m now guessing you do. **Thanks for pointing this out, we have now added “there” to the end of this sentence.**

Line 147 but climate is change in the north too, why do the northern species not move too? **Our thinking here is that the northernmost species are running out of space to move (*e.g.,* hitting the Arctic Ocean). We think the phrasing “in contrast” was confusing here and have removed it.**

Line 152 remove excess “been” . **Fixed.**

Linea 173 “on” instead of “about” **Fixed.**

Line 178-79 remove excess “found”. What does “sufficient granularity to detect trends” mean? **Fixed. By sufficient granularity, we mean enough discrete time bins to estimate a trend.**

Line 183-186 unclear what is meant in this sentence. **We have clarified by giving and example here.**

Line 189 why minimum temperature and precipitation? And is that annual minimum temperature and annual precipitation sum? **The values reflect the annual average minimum temperature and average annual precipitation. We have added clarification on why we chose these to the introduction.**

Line 195-199 messy sentence and where does the first bracket close? **We have added the missing bracket for clarity.**

Line 199-201 and 203-204 belongs to Results. **We have moved these sentences to the beginning of our results section.**

Line 239 more details on these traits please. What is disturbance tolerance? What kind of variable is the host plant family breadth? How many and which levels in overwintering stage? **We have now added clarification on the meaning of these traits and refer to the data paper for how they were calculated to save space.**

Line 247 how were duplicates removed? **This was incorrect, we actually don’t need to filter for duplicates since all of the records are collapsed to a binary value of detection/non-detection data. We have removed this from our workflow description.**

Line 265 what is meant with community data here? Data on several species? **We mean that observers/collections went out with the intention of recording communities over target taxa. We have now clarified this in the text.**

Line 267 remove “/” and write complete sentences instead. Also elsewhere in the text. **Fixed.**

Line 268-269 I do not understand what is meant in this sentence. **We believe that our prior fixes have now clarified this sentence, but please let us know if it is still unclear.**

Line 275-276 please revise. Do you mean: “We used the percentage of points that fell within community clusters as a proxy the probability that the site had been visited by observers with a community focus.”? **Yes, we have now included this language instead.**

Line 277 “has been found” instead of “was found” **Fixed.**

Line 284 “in” instead of “at” **Fixed**

Line 285-288 and 291-295 examples of long, complicated and hard-to-understand sentences. I’m not going to point out more of them after this, but please revise the whole text for clarity. **Thank you for pointing this out, we have now edited it for clarity as well as checked the rest of the text to eliminate long sentences.**

Line 344. When have you introduced these hypotheses? This should be introduced already in the introduction. **We have now edited the introduction to do so. We also motivate our hypotheses in the table describing the trait models.**

Line 348 trait or trait value? **We have kept this language as we mean “trait” broadly rather than specific trait values.**

Line 355 does this now relate to the occupancy models or the trait models? **We have clarified that we mean the trait models here.**

Line 372 what response? Range shifts, occurrence or what? The other “to” should be “in” **Fixed.**

Lines 374-376 can you refer to model statistics in addition to only figures. It is impossible for the reader to interpret whether your rather bold statements are robustly underpinned. **We have added more data from our models to the results in order to support our conclusions. We hope that these new metrics are sufficient, but please let us know if there is something specific you have in mind so that we can support our claims.**

Lines 390-392 I cannot see that in the figure and there are no statistics to refer to. This feels like an overstatement based on what I’m seeing but I trust number and would need to see the model statistics presented.

Line 398 top model based on what? **Based on our model selection approach. We have now clarified this in the sentence.**

Line 449 you had one climatic variable

Line 445 Rather a non-statement. Couldn't you instead argue why predictions are needed, it’s not just something scientists do for fun right? **Due to new inference, we have removed this paragraph from our work.**

Line 487-488 opportunities and challenges for what? **We have now clarified for butterfly survival.**

Line 504 Notice how you only now come back to the topic that almost your entire Introduction revolved around? **We agree that it takes awhile to circle back to the topic of modeling but still think that it is important to highlight the capacity of these tools for future study.**

Line 533 why do the cold-adapted species not benefit? **We did not test this explicitly here and now provide recommendations for further research in the conclusion.**

Figure 2. What is the coldest quartile of ranges? Is it species that are grouped into the quartiles or species distribution areas and the trends in those different areas? **The species are grouped into quartiles based on their range-wide temperature trait. We have now clarified this in the caption.**

**Reviewer Three**

I enjoyed reading this manuscript. It addresses a very important methodological topic in ecology, the desire to assess changes in species' distributions and abundances from haphazardly collected presence-only distribution data. It also evaluates species' range shifts in relation to climate change, an ecologically important and interesting question.

If indeed these methods are generally robust, the paper does provide a "holy grail" for inferring about species' distributions in the past.  I am excited to explore similar methods.

**Thank you for your time in reviewing this work, we are glad you enjoyed it! Please reach out, I would love to hear about your work and chat about occupancy-detection approaches at any time.**

Something is not right with the confidence bands in Figure 4, and supplemental figures S5-7. Credible intervals around a regression represent the range of credible lines based on the data. For almost any regression model (regardless of whether it is Bayesian or likelihood-based) the distribution around a line is wider at the limits of the data than in the center (as opposed to the parallel lines in Figure 4). This shape occurs because the slope-intercept probability distribution (or likelihood surface) is bivariate normal, with a negative correlation of the slope and intercept parameters. So, based on my knowledge, these credible bands were either computed incorrectly, or they are described incorrectly and are actually prediction intervals derived from a Bayesian model, which are not credible intervals. (If I am wrong about the definition of a credible interval, I am open to being educated by the authors of this paper.)

**Thank you for pointing this out, you are correct that this was mislabeled. The original bands in Figure 4 (and corresponding supplemental figures) represent the prediction intervals. We have now properly labeled these intervals both in the figures and the text.**

My other comments and suggestions are quite minor. In general, it is a solid and interesting paper.

1. I would like to see a correlation matrix among traits used as predictors (or some kind of analysis of how they are associated).

**Done, it is now in the supplemental as Supplemental Figure 8. We note a highly negative correlation between range-wide average temperature and human disturbance avoidance which is to be expected due to lower human population densities in northern extremes.**

2. I wish the discussion were a little bit less rosy about presence-only data, and spent more time discussing potential pitfalls – For example, what assumptions were made in the simulations that were the basis of this analysis? How likely is real data to meet these assumptions? It is the Holy Grail, and maybe we have achieved this point. But I am only 85% convinced.

**We love presence-only data but also want to be realistic about the potential pitfalls in our analysis. We have now clarified the results of the cited simulation studies and how actual presence-only data may deviate from these simulations. We have also increased our discussion around previous work in the use of occupancy-detection models with presence-only data in order to provide greater support for this approach. The new discussion presents a more balanced interpretation of our results in the context of past research while still maintaining the utility of occupancy-detection models for reconstructing trends.**

3. Table 1 is almost impossible to read in manuscript form. It would need some other format for publication (Smaller fonts? Landscape formatted pages?). **We agree that this table is too large. To alleviate this, we have now converted the table to landscape format. We hope this facilitates interpretation of the table and are open to further suggestions in order to increase readability.**