23-Sep-2014

Dear Dr Rowe:

Your manuscript has now been peer reviewed and the reviews have been assessed by an Associate Editor.  The reviewers’ comments (not including confidential comments to the Editor) and the comments from the Editors are included at the end of this email for your reference. As you will see, the reviewers and the Editors have raised some concerns with your manuscript and we would like to invite you to revise your manuscript to address them.

We do not allow multiple rounds of revision so we urge you to make every effort to fully address all of the comments at this stage. If deemed necessary by the Associate Editor, your manuscript will be sent back to one or more of the original reviewers for assessment. If the original reviewers are not available we may invite new reviewers. Please note that we cannot guarantee eventual acceptance of your manuscript at this stage.

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When submitting your revision please upload a file under "Response to Referees" in "Section 6 - File Upload". This should document, point by point, how you have responded to the reviewers’ and Editors’ comments, and the adjustments you have made to the manuscript. It will expedite the processing of your revision if you are as specific as possible.

Your main manuscript should be submitted as a text file (doc, txt, rtf or tex), not a PDF. Your figures should be submitted as separate files and not included within the main manuscript file.

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Thank you for submitting your manuscript to Proceedings B; we look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Best wishes,

Professor Innes Cuthill

Editor, Proceedings B

mailto: [proceedingsb@royalsociety.org](mailto:proceedingsb@royalsociety.org)

Associate Editor, Dr Marcel Cardillo

Comments to Author:

Both reviewers agree that the spatially replicated resurveys of century-old mammal surveys have provided the opportunity for a powerful test of the effects of climate change on elevational distributions of mammal species. Both reviewers have some important concerns, however, especially about the clarity of explanation and rationale for some of the methodological decisions that you made - please be careful to address all of these. I would also like to see a little bit more discussion of the biology involved in mediating the link between climatic changes along elevations and the distributions of species. On a related note, it might be worthwhile exploring the phylogenetic signal in the various responses of different species - this might help clarify some of the heterogeneous results.

Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author(s)

This is a really great paper.  It's a fantastic data set, and the analyses are really interesting and well-presented.  I only have a few minor comments, listed by line number.

132-137: I'm not sure why, but something about this bit confuses me - are you saying you have that much data for all observations, or some subset of that data for each?

“Of the 15,277 historical mammal records used in this study, 8,688 are backed by voucher specimens in the MVZ”

223: This seems like an odd title for this section - if I read it correctly, you're not testing the predictions of range shifts, although you may be testing the predictORS of range shifts.  The title as written suggests that this section will discuss field validation of predicted range shifts, which doesn't seem like what you're doing.  If that IS what you're doing, it's not clear to me.

## “*Testing predictions of range shifts”* Agree. Change to “predictors”

224-237: I think it might be good to just step back and say "here's what we're doing and why" in a topic sentence.  Make it clear that these are multi-species models right off the bat, and say what you'd like to get from them.

I will add

258: Euclidean

corrected

264: I'd like to see the rationale for this model spelled out specifically.  My interpretation is that you're comparing the model with different local conditions to this model in order to demonstrate that fine-scale climate differences are important.  If that's right, it would be good to say so here.  If that's not right, then you should definitely spell out the actual rationale more clearly.

“We compared these climate-data derived models to an “overall warming model” that assumes an increased temperature at all grid cells over the same time period, which always predicted upslope movements.”

I will add clarification

311-312: Again regarding the overall warming model: it's good to see that it's not a great predictor, but it seems kind of obvious given the fact that it's at odds with reality.

Not always at odds with reality. Need to clarify.

315: This might be a good place to bring up the fact that these predictors are spatially correlated, and that makes it difficult to pick out which one is actually driving differences.  In particular I'm guessing minimum, mean, and maximum annual temperature are highly correlated.  Can you present a biological justification for preferring one over the other?  I don't know much about montane mammals in California, but it's easier for me to understand how "too hot" would be more of a problem for a lot of them than "not cold enough".

I will add an explanation of the biological impacts of min and max in the methds.

Referee: 2

Comments to the Author(s)

This paper examines the elevational shifts of small mammals in three mountain areas in California. This replicated approach is of particular interest in the context of climate change because rarely historical distributional data are available in a so wide extent.

However, the paper is difficult to follow, with quite dense sections as well as poorly detailed passages. I have some major concerns and some minor suggestions on the manuscript.

Major concerns

1. Some hypotheses formulated in the introduction are not biologically relevant. The authors refer to "overall climate warming" and "local changes in temperature and precipitation" as two potential different drivers of elevational shifts. In fact, they tested which one explained better the observed distributional changes of study species. However, using "overall climate warming" values instead of "local changes in temperature and precipitation" is a matter of the data available for the region under study. Ultimately, individuals will experiment the local climate in their habitats , not a "overall climate warming". Unfortunately, long-term climate data are available for a few sparse localities in a region and "overall" temperature values have to be used to evaluate climate change. I might understand to compare the effects derived from "overall climate warming" vs. "local changes in climate" from a methodological point of view to evaluate if differently grained data sources may drive to similar conclusions in terms of distributional shifts.
   1. Not sure how to respond to this. Do we just need to improve the clarity of what we did?
2. The main strength of the manuscript is the replicated resurvey of elevational distributions to test whether species responses are spatially consistent. However, this point is not sufficiently highlighted throughout the other sections of the paper, say, which are the potential expected differences in species responses to climate change between regions given their different climates? This is unknown., or which analyses are relevant to test this hypothesis? Which hypothesis

Minor suggestions

Material and methods

Page 6, lines 106-109. The differences in temperature (in precipitation are ok) between regions are not evident from Fig. 1b. Some further details, as mean temperatures for a given elevation, could be helpful to show these differences.

Violin plots are not enough? Mean temp at multiple elevations?

Page 7, line 125. 134 historical localities, but the sum (34+47+32) gives 113.

Corrected. Should have been 34+47+53 = 134

Page 7, lines 138-144. The total number of sites included in the study is unclear.

We include all 134 historical and 166 modern sites. Need to add a clear sentence stating this.

Page 8, line 166. Apparently, there are 33 species in Table S4.

Correct Table S4 to 34 species, split out N. fuscipes and N. macrotis

Page 9, lines 173-174: "...with the exception of sites within the Yellow Pine...". Why this change was made?

Page 9, lines 175-177. At this point of the ms, it is not clear why species were classified into categories. "Widespread" category is missing here.

Defined “widespread”. Added clarification.

Page 9, lines 185-186. The number of sites used in the analyses is not clear here either. Repeated sites should not be included, given modelling was under an "unpaired-site" framework.

Why not? They are in different eras.

Page 10, line 195. There are 16 models in Table 5.

Need to correct text. Full models are not shown, only the 16 best models based on preliminary model selection as stated in the text.

Page 10, line 196. Does "trend" correspond to "time" in Table S5?

Yes. Correct S5

Page 10, line 201. "The full candidate model set...". Only 16 models are listed.

Correct. Need to clarify in text and state only the top 16 models are in the table.

Page 11, line 228. "Species identity was included as a random effect...". How was tested the effect of the random factor (random intercept, random slope)?

Morgan?

Page 11, line 232. "We defined 12 models...". Apparently, there are more than 12 potential models based on the 3 categorical explanatory variables including one-way interactions only.

Check

Page 12, line 256. "...and the 5% of historical cells...". What the historical cells were used for?

Clarified as: “*For each historical site, we identified the 5% of modern cells that were nearest climatically. To test for availability of similar environments within the historical era we also identified the 5% of historical cells that were nearest climatically (see supplementary methods for details)*”

Page 12, lines 263-265. What is the magnitude of the overall warming here?

“We compared these climate-data derived models to an “overall warming model” that assumes an increased temperature at all grid cells over the same time period, which always predicted upslope movements.”

Need to clarify “overall warming model” perhaps we should just call this the “upslope model”. There is no magnitude to the “overall warming model”

Results

Page 13, lines 289-295. This paragraph could fit better after the results presented in lines 296-307.

I am not sure why this order would be preferable.

Page 14, lines 299-300. The results corresponding to Table S3 are hardly explained.

The reviewer wants more detail on AIC model selection from Table S3

Page 14, line 310. It is unclear why Fig. 1c is referred here.

“Nearest climatic neighbour analyses revealed that both upslope and downslope shifts were predicted by temperature change at range limits (Fig. 1c).”

Suspect issue is with “range limits”. Figure 1c shows that historical sites would predict upslope and downslope shifts, but does not indicate species range limits.

Page 14, lines 310-320. These analyses are not clearly detailed in the Material and methods and they are difficult to follow here.

Need to clarify.

References

Page 22, line 494. This paper is from 2010.

Corrected

Supplementary material

Please, check latin names, in several sections they appear as single words.

Need to correct

Table S3. There is a gap within the table. The analyses referring to "Shift up vs. down" require some further details for better understanding.

Correct and clarify

Fig. S3. There are many legends that overlap occupancy curves. Some curves, as that for Neotoma cinerea (particularly in the southern region), are difficult to understand based on a quadratic relationship with elevation and effects of categorical variables. Given the huge length of the Supplementary material, I have checked only a sample of the occupancy curves. In the case of Tamias quadrimaculatus, which occurs in the central region only (e.g. Fig. 2), it appears as present in the three regions according to the curves. This suggests that all supplementary material requires careful checking.

Check all SM figures.

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Abstract: Resurveys of historical collecting localities have revealed range shifts, primarily leading edge expansions, which have been attributed to global warming. However, there have been few spatially replicated community-scale resurveys testing whether species’ responses are spatially consistent. Here we repeated early 20th century surveys of small mammals along elevational gradients in northern, central and southern regions of montane California. Of the 34 species we analysed, 25 shifted their ranges upslope or downslope in at least one region. However, two-thirds of ranges in the three regions remained stable at one or both elevational limits and not one of the 22 species found in all three regions shifted both their upper and lower limits in the same direction in all regions. When shifts occurred, high elevation species typically contracted their lower limits upslope, whereas low elevation species had heterogeneous responses. For high elevation species, site-specific change in temperature better predicted the direction of shifts than change in precipitation, whereas the direction of shifts by low elevation species was unpredictable by temperature or precipitation. While our results support previous findings of primarily upslope shifts in montane species, they also highlight the degree to which the responses of individual species vary across geographically replicated landscapes.

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