24th June 2014

Dear Dr Rowe,

Your Letter entitled "Spatially heterogeneous impact of climate change on small mammals of montane California" has now been seen by reviewers, whose comments can be found below this letter. In the light of their advice, we have decided that we cannot offer to publish your manuscript in Nature Climate Change.

Although all three reviewers find your work of interest, reviewers 1 and 2., in particular, raise concerns about the advance your findings represent over earlier work and the strength of the novel conclusions that can be drawn at this stage. We feel that these issues are sufficiently important as to preclude publication in Nature Climate Change.

I am sorry that we cannot be more positive on this occasion but hope that you find the reviewers' comments helpful when preparing your paper for resubmission elsewhere.

Yours sincerely,

Dr Rory Howlett

Chief Editor

Nature Climate Change

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Reviewers Comments:

Reviewer #1 (Remarks to the Author):

The ranges of species sampled over the past century in mountain ranges in California represent one of the most important case studies for the effects of environmental change on the elevational distributions of species. This ms tests for changes to the range limits of small mammals in three mountain ranges in Southern, Central, and Northern California. The strong point of the ms is the high level of replication of taxa in different regions, enabling testing of whether species responses are consistent depending on region (S, C and N), range limit (upper or lower) and one species trait (low or high elevation species).

Whilst the ms presents a nice summary of changes in this important system across the three mountain ranges in California, the presentation of spatially-replicated resurveys (lines 49-50) has some precursors for other taxa or regions that should be cited in the ms (e.g. Gottfried et al 2012 Nature Climate Change 2, 111-115 for European alpine plants; Menendez et al 2014 Global Ecol Biogeogr 23, 646-657 for dung beetles in Europe). For a modelling study of responses by alpine plants in different regions, see Engler et al (2011) Global Change Biology 17, 2330-2341.

I also feel that the argument or hypotheses and text generally could be tightened up. The establishment of "contrasting" hypotheses for responses to global versus local climate change (lines 79-86) seems like something of a straw man. The results are presented to some extent in this context, but it does not seem surprising that species respond more directly to local changes, and indeed it could be argued that the authors find evidence for all four of the hypotheses as presented. For example, lines 111-114 suggest support for hypotheses 1 and 2 - and the result of comparable levels of range contraction to expansion by low elevation species is not really "contrary to our expectation" (lines 113-114) as presented in the hypothesis on line 81, where the explicit comparison is between low and high elevation species. The authors note that "none of the 22 species found in all three regions shifted both their upper and lower limits in the same direction in all three regions" (lines

101-103) - but rather than being presented as evidence against generalised responses (would we really expect such a generalised response?) the heterogeneity in responses seems more interesting as a basis for understanding drivers of change. Although this idea is implicit in the work, it should be brought more to the fore.

Overall, the results suggest a relatively high level of consistency in species responses (particularly by high elevation species showing range contractions). The interesting advance in terms of understanding community level changes may be the observations of heterogeneity in leading edge expansions by low elevation species. An important next step, of which this ms could play a valuable role, is in establishing the reasons for these heterogeneous responses, and the authors might couch the article more explicitly in this context. A few possible reasons for this heterogeneity are presented on lines 137-139, and might be elaborated / explained / tested in more detail.

The authors do test whether four temperature variables or one precipitation variable are most closely related to observed range shifts, finding greater evidence for effects of temperature than precipitation, and greater evidence for these climate effects in the high elevation species (many of which are showing contractions). They state (lines 185-186), "While no-analogue climates are typically considered when predicting future species' ranges, ours is the first study to examine how such disappearing climates can affect inference and interpretability of observed range shifts" - I don't know if this sentence is very helpful. It sounds like an introductory sentence, setting up an approach that will be taken, and it certainly may need greater explanation. The discussion and Figure (1c) regarding different expected directions of change depending on temperature or precipitation trends is interesting, although presumably the two variables generally interact with each other: can an

approach be taken that incorporates effects of both variables into a composite measure of climate?

It should be possible to shorten some areas of the text, particularly those referring to responses of individual species, for example lines 96-100, 120-128; and the concluding paragraph (lines 189-203) could also be stronger and more concise. The recent reference by Pearson et al (2014; Nature Climate Change 4, 217-221) on species traits and vulnerability may be relevant.

Minor comments:

Line 41: reference 9 doesn't seem the most apposite or important here, and can be dropped.

Lines 220-223: check grammar of this sentence.

Fig 2 legend: you should note if species with no bars shown were not present (above the 10% criterion, or at all?) in the system shown.

Reviewer #2 (Remarks to the Author):

The manuscript by Rowe et al. compares extensions and contractions of the elevation ranges of small mammals in California across three replicate sampling transects. It builds on prior work by these authors (in fact, using some of the same data) that employs detailed historical surveys, modern resurveys, and occupancy models to account for detectability when calculating range changes. The advance in this manuscript is to compare previously documented patterns in the central Sierra Nevada Mtns to 2 other transects, one to the north in the southern Cascades and one to the south in the southern Sierras. They find that range changes are not usually similar in direction across transects, but that range changes are predicted by variation in temperature among the transects. The analytical framework, which has been vetted in prior publications, is sound (although I have one major complaint - see below), and the manuscript is fairly well written, but my opinion is that the advance over

previous studies is rather incremental.

Analytical framework:

The manuscript relies solely on the Akaike Information Criterion to assess model performance. This is incomplete. The "best" model in terms of AIC could still be very poor in terms of explanatory power, but right now readers of this manuscript have no way to judge that. Some consideration of variance explained should be given, for example by presenting R2 or some analog along with AIC throughout the manuscript and supplement.

Minor comments:

L53: Implies that this study will be about both birds and mammals - please rephrase.

L193: Suggest using "contraction" for consistency

L264: What is the rationale for excluding widespread species?

Figure 4: The percentages for low elevation species are not that much lower than the percentages for the high elevation species. Is this a power/sample size issue, or is there a meaningful biological difference between these groups of species?

Reviewer #3 (Remarks to the Author):

General comments:

This manuscript summarises ranges shifts observed in small mammal communities in three different landscapes/areas (Lassen Volcanic-Northern, Yosemite- central and Sequoia/Kings Canyon N.P.- Southern) in California. This manuscript consists of an extended follow up from a previous manuscript published in Science in 2008 by some of the authors (e.g. Steven R. Beissinger, Craig Moritz). The paper in Science includes part of the data and focuses on the impact of a century of climate change on small-mammal communities in Yosemite National park, USA.

This manuscript focuses on the spatially replicated effects and assesses if responses of small mammal species are replicated across areas. The three areas have different elevation (2800m Northern area, 3200m Central and 3800m Southern), temperature and precipitation limits. 34 species were detected (13 low elevation and 19 high elevation) in both the historical and modern surveys in at least one area and it was possible to model the detectability and occupancy for 28 species. The methods used to model the detectability and occupancy were appropriate.

The main conclusion is that there are idiosyncratic responses to changing climatic variables and that the responses in one area are not consistent with the responses in a different area. This is an interesting result, presenting a less clear message than previous research, thus it would have been good to see other factors included in the analyses. Are there habitat associations or biotic interactions that could explain the patterns observed? It would have been good to see a model with habitat associations and accounting for habitat availability in the three areas, using the modern data (I assume this information is not available for the historical data).

This manuscript needs an extensive revision of the abstract to better convey the messages that distinguish this manuscript from previously published research.

Specific comments:

Abstract

- needs to be improved to present more quantitative figures. It is not clear how many species were assessed. Saying that 2/3 of species sowed responses is not meaningful if the total sample is not described.

- Clarify, add precision with numerical figures to the sentence "few species shifted limits in the same direction in all regions"

The main conclusion of the study is not new: "Local change in temperature was a better predictor of shift direction than precipitation, suggesting the heterogeneous responses of small mammals were influenced by local temperature change consistent with 20th century warming." It would be better to focus on the conclusions that can be drawn from the multi-landscape comparison.

Line 110 replace "as" by "than"

Figures

Why are species 12 and 13, on figure 2, classified as low elevation species? Clarify the classification of species.

Figure 3 is not well labelled and the legend is not good at describing it. Proportions of species that expanded, contracted or remained stable are presented but the numbers refer to the total number of responses in low and high elevation areas across the 3 sites. Legend needs improving.

Figure 4 the n refers to number of range shifts not species. As it is it misleads the readers to think that the sample size provided refers to the number of low elevation and high elevation species.