To The Editor,

Specific issues raised by our reviewers (shown in bold font), and our responses (in plain text), are listed below. Line numbers associated with reviewer comments refer to the original manuscript, while line numbers in our responses refer to the current version of the manuscript.

*Comments from the handling editor*

**Differences in survival between age classes and subordinates vs. dominants**

The analyses have been re-run with four age-classes instead of two. This change had essentially no effect on model selection, and on the strength of evidence for climate vs. social effects. However, our revised results are somewhat different as a result of other changes, specifically the method of estimating the number of subordinate adults (see below).

**Survival cannot be estimated for non-juveniles because of dispersal bias**

It is true that, as with any mark-recapture study, we could not distinguish between permanent emigration by dispersing individuals and true mortality. To reflect this difference, we have replaced the term ‘survival’ with ‘apparent survival’ (sensu Lebreton et al. 1992) in the results section. However, we do not believe dispersal is a significant confounding factor. Radio telemetry data from our study population suggest that only male individuals >3 years old permanently disperse. Apparent survival should therefore reflect true survival for all female marmots, and for all but the oldest male age-class. Since we did not find strong evidence for age\*sex interactions, we infer that this bias in apparent survival due to dispersal did not influence our results. Furthermore, in a sample of radio-tagged individuals, all permanent dispersal events occurred in mid-late June, several weeks after the onset of trapping and resighting. It is therefore unlikely that dispersing males were not captured or resighted prior to dispersal. Text has been added to the methods section to clarify this justification (lines 136-142).

**Inadequate global model specification**

Our global model did not include social group as a covariate of survival because the number of marmots in individual age\*social group\*sex\*year combinations was often quite small (mean = 0.72), despite the fact that most social groups were completely censused annually. Rather than conduct our analyses with such small sample sizes, we chose to pool data across social groups, and incorporate social group-specific differences via biologically plausible linear covariates representing social group size and group territory-specific estimates of food availability (Ngroup, SUB, and VEG respectively).

**Inadequate goodness-of-fit testing**

The GOF test we conducted for our original manuscript, using program MARK, suggested our initial general global model had only minor overdispersion (c-hat =1.25), and that social group-specific differences did not cause a major systematic lack of fit. We corrected for this overdispersion in our initial analyses by using QAICc rather than AICc.

Per our reviewers’ suggestions, we re-ran our GOF analyses using program U-CARE. The global test in UCARE indicated no significant lack of fit in our original global model (age\*sex\*time; see revised Methods), implying that our original analysis was the more conservative of the two (lines 157-159).

**Insufficient literature review**

We thank the reviewers for their suggestions regarding the literature, and have incorporated the articles they suggested.

**Methods for social group assignment and home-range estimation**

We have expanded and clarified the text related to this issue (lines 228-239). Rather than rely on one method for social group assignment, we used a combination of methods, which are described in more detail in Kyle et al. 2007. Social group ID was based on home range overlap, burrow sharing, and social interactions. We could not rely on shared hibernacula use alone because we did not observe all individuals emerging from their burrows in all years, and summer burrow sharing was not a reliable method.75% overlap was used because it was the lowest percentage of overlap that correctly identified social group affiliation by 100% for the fraction of marmots that could be confidently assigned to social groups by other methods.

**Missing references**

We have updated our reference list.

*Reviewer 1 comments*

**Social group size during hibernation remains unknown, because spring dispersal is not accounted for.**

This critique seems to be based in part on a misunderstanding of our methods. We used the number of marmots per social group in the previous summer, not the following summer, as a covariate in our analyses, so that marmots lost to overwinter mortality would not affect our results. However, we agree that changes in group size during the summer could still be important. We cannot perfectly resolve this issue, because we did not trap marmots immediately prior to winter immergence. We have attempted to reduce this source of error by using the number of marmots per group during the previous July and August (several weeks after dispersal occurs, based on radio-telemetry data) as a covariate in our revised analyses, instead of the total number observed during the summer. As the revised results show, calculating the SUB covariate this way did result in stronger evidence for a subordinate effect, although there was considerable uncertainty associated with this effect, and our results still indicate that climate as indexed by PDO was the strongest, most consistent influence on survival, at least in the juvenile age-class. A revised decription of our methods related to social group size can be found beginning on line 217.

**Use of annual survival estimates to infer patterns of winter mortality**

The revised Discussion includes a paragraph in which we lay out our arguments for interpreting our annual survival estimates as primarily reflecting survival during hibernation (beginning on line 332). These include a post-hoc analysis of juvenile survival during the summer (June-August), versus the winter (August-June). Survival probabilities were estimated for each period based on a model where Phi = AGE \* TIME and p = TIME. We found that summer apparent survival rates were equal or close to 1 throughout the study period, while winter survival estimates closely tracked the model-averaged annual survival probabilities in our original analyses (Appendix S6 in ESM).

*In-line comments:*

**73-74 The range of M. monax is huge, including areas with severe winter conditions in the North.**

We have clarified the text in this section to reflect the fact that other marmot species are ‘obligately associated’ with alpine habitat (line 63-64).

**137-142 I cannot see the relevance of this information for the question investigated.**

Information on vegetation type and productivity was included because we felt that habitat type and quality were important considerations when discussing differences in survival between marmot species. However, the study site description has been edited with a greater focus on the winter environment, which is more directly relevant to our analyses (106-111).

**164-167 Which fraction of trapped individuals could not have been sexed properly? Why was this the case? Sex of marmots can be determined reliably from the distance between the genital and anal opening.**

The mean fraction of unknown-sex individuals in a given year was 0.10 (SE = 0.02). A-G distance cannot be used for sexing hoary marmots (T.J. Karels, pers comm). Sexing must be done by visually inspecting the genitals, and some animals contract the surrounding muscles too tightly to allow their genitals to be clearly seen. A sentence has been added that states this problem (180-184).

**150 In this section, I miss more detailed information about the exact trapping period and its relation to snow conditions.**

As discussed in our methods, we do not have exact dates of autumn snowfall or spring snowmelt for our site in all years, which was part of our motivation for using PDO as a proxy for winter conditions. We have added more detailed information on the start and end-dates of trapping to the Methods (114-115), and included a range of snowmelt dates from a previous study at our site (109-110)

**237 Here and throughout the paper: Does the term “colony” refer to social group? I recommend not using this term because it is in studies of ground squirrels also used for describing assemblies of solitary individuals in favourable habitat.**

The word 'colony' has been replaced with 'social group' throughout the text.

**334-335 But see Arnold W. (1993) Energetics of social hibernation. In: Life in the Cold: Ecological, Physiological, and Molecular Mechanisms , Carey C., Florant G. L., Wunder B. A., Horwitz B. (eds.) Westview Press, Boulder, pages 65-80.**

In the revised manuscript, we devote several paragraphs to discussing arousal synchrony and other behavioral and physiological explanations for why group thermoregulation may be more effective in alpine marmot social groups than in our study (347-354).

**306 I assume the authors mean “emigration” and not “immigration”.**

Yes. This error has been fixed.

*Reviewer 2 comments:*

**Short study duration**

The Discussion section now explicitly acknowledges the limitations of the size of our dataset, and more clearly lays out the assumptions made during our analysis (332-344).

**Differential effects of male vs. female subordinates were not addressed**

We have incorporated subordinate sex-ratios into our most recent analyses, and the revised manuscript includes a discussion of the Implications of subordinate adult sex-ratios on social thermoregulation (358-371).

**Although the effect of group thermoregulation on survival is weak it may be qualitatively important.**

We have revised the presentation of our results to more clearly explain the biological significance of PDO, the number of subordinates and interaction between the two, in terms of effects size and uncertainty regarding those effects. (258-280in Results).

**The relative role of temperature and snow depth is still unclear, & The timing of low temperature (freezing) compared to snow falls may also be important**

We cannot evaluate the importance of the timing of frost relative to autumn snowfall, because we do not have adequate snow data. We did incorporate an autumn frost intensity variable similar to the one used by Farand et al. (2002) into our revised candidate model set, but it was not a good predictor of survival (257-258).

*Specific comments*

**P5 L94: I am not sure that the relative importance of density was considered in this analysis.**

'Density' has been changed to 'abundance' in this section.

**P7 L151: Do you have an idea about the proportion of individuals caught in the population?**

The proportion of Individuals captured should be very close to 1 in all but the first year, based on field observations of tagged vs. untagged animals (Kyle et al. 2007) as well as the high estimated detection probability (243-248).

**P11 L237: what is a colony? Does it refer to a family group?**

This language has been changed to ‘social group’ throughout the manuscript.

**P13 L282-286: it is written that authors do not have weather data but in the following sentence, it is written that PDO well correlated with winter climate at their site. This looks like contradictory. Please clarify.**

This section has been clarified. The correlation between PDO and snowmelt was based on data from an earlier, but partially overlapping time period (Morrison and Hik 2007), and from a weather station located 30 km away (Burwash Landing) We chose to use PDO rather than snow data from the Burwash station, even though the two were correlated (202), because of substantial differences in elevation and topography between that station and our site, and because a literature review suggested that PDO was more likely to be a better indicator of the synoptic weather patterns influencing snow at our site (sources cited between lines 192-197). This assumption was borne out by the fact that snow depth at Burwash showed correlations with juvenile survival that, while weak, were consistent with our results using PDO (262-265), albeit weaker.

**P14 L306: ‘emigration’ instead of ‘immigration’**

This has been fixed.

**P16 L355: ‘balance’ is not clear for me (see my ‘general comments’).**

The discussion section has been substantially rewritten. The term 'balance', which referred to the presumed simultaneous influence of temperature and snowpack on survival, has been replaced with 'interaction' throughout. We have also clarified why the negative correlations between PDO and survival that we observed specifically imply that deep snowpack compensates for cold temperatures (negative PDO values are correlated with deeper-than-average snowpack, but colder-than-average temperatures in the Interior of the Yukon Territory (198-205).

**P18 L404-406: This sentence is not clear for me. Does this mean that there is no trend in the decline in survival or does this mean that the trend exists but other factors may explain this trend?**

This sentence was intended to convey the fact that a correlation between annual variation in PDO and survival should not be taken as evidence that hoary marmot survival is being affected by a long-term warming trend in the Yukon (although such a trend is evident in climate data from the region). It was removed during revisions, because we decided that a discussion of the effects of a long-term warming trend were outside the scope of the analysis.