We want to thank the reviewers for the thoughtful and constructive comments. They were insightful and made the manuscript stronger. We also want to thank the editor for facilitating the review process. One thing we would like to note is that we are still awaiting formal approval from the USGS. We hope we can accommodate any of the minimal changes in the proof stage, should this be accepted.

**Reviewer 1**

***Comment:***

The authors cite Arnold’s (2010) paper about informative parameters in AIC-based model selection, but then perform interpretation on models that contain uninformative parameters. My understanding is that the premise of Arnold (2010) is that if a nested model is within 2 AIC units of a more complex model, then the parameters in the more complex models are uninformative. Here, the authors identify parameters as “informative” when I believe the should not be; for example the top-ranked model in Appendix C contains no informative parameters, since the null model (with no covariates) is within 2 AIC units.

***Response:***

Because our more complex model differed from the null model by more than 1 covariate (and was therefore penalized by more than 2 AIC units), our understanding is that it should still be considered competitive. (From Burnham and Anderson 2002, "Models having Di[DAIC] within about 0–2 units of the best model should be examined to see whether they differ from the best model by 1 parameter and have essentially the same values of the maximized log-likelihood as the best model."). We define informative parameters as those in models considered competitive based on AIC results and for which the 85% CI of the beta coefficient does not overlap zero. We have clarified by adding this definition to the methods; we also added parameter CI ranges to Appendix S3.

***Comment:***

The most consistent variable appearing in the models was herd migratory diversity. This variable must be correlated with switching to some extent, because switches can only occur in herds where multiple migration strategies are present. This correlation complicates interpretation of the variable. My suggestion would be to build a null model with which to compare the observed data, so that the authors could quantify the degree to which herd migratory diversity is related to switching, just as a function of the data structure. (For example, simulate migratory switching at equal rates in hypothetical herds with different numbers of strategies, then quantify the relationship between switching and herd migratory diversity in this synthetic data).

***Response:***

The correlation between switching prevalence and herd migratory diversity was only 0.21, which is quite low. Nearly all (90%) of the herds we studied had more than 1 migratory strategy. In our opinion, if behavioral switching is an important driver of migratory changes, then an elk should be similarly likely to switch between between 2 tactics as between 4 - i.e., the number of possible behavioral tactics matters less than the propensity to change between any tactic. Additionally, the diversity metric is not just a measure of the number of tactics, but the distribution within the herd as well, adding increased nuance to the relationship. With all the above factors considered, we believe there is still significant variation in the data that warrants the modeling we performed.

***Comment:***

Variables: I would be interested to see if time-lagged environmental variables have any effect on switching behavior. Are elk learning from their experience in a prior year, or responding to current conditions?

***Response:***

The variables are essentially already time lagged since all variables are from the year before the switch occurs. It would certainly be interesting to see if years further back have impact on switching indicating a potential impact of memory, however that is out of scope of this analysis.

***Comment:***

Finally, I wonder if it would also be worth analyzing an individual’s migratory strategy as a function of these same variables, or (possibly equivalently) model switching “to” a strategy in additional to “from” a strategy. This direction is more intuitive to me, since it could indicate which strategies are likely to become more common under which conditions.

***Response:***

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***Comment:***

Line 239-240: How was space use measured for residents, where BBMMs are not appropriate?

***Response:***

The process referred to here only applied to non-resident elk. Clarified in the text in line 254-259: "For each non-resident elk-year, we visually identified the timing of spring and fall migrations based on departure from or arrival to clustered GPS points on the map coupled with a change in slope of the NSD or elevation change curves. We measured migration distance as the diameter of the smallest circle encompassing the 99% isopleth of a Brownian Bridge movement model (BBMM) for the movement data during the spring migration for each non-resident elk-year (Sawyer et al. 2009)."

***Comment*:**

Table 1: Can the predictions be defined in the same direction or relative to the same migration strategy? As written, some refer to residency and some to migration, some “towards” and some “away from.” A common frame of reference would have helped me interpret the predictions more easily.

***Response*:**

The prediction column in Table 1 only uses the away from framing, saving for the social theme, in which we predict increased switching rates in all directions

***Comment*:**

The prediction column in Table 1 only uses the away from framing, saving for the social theme, in which we predict increased switching rates in all directions

***Response*:**

Good point, we used this term a lot without explicitly definining it. Added definition in text in lines 361 - 366: "Nearly all switches were to or from intermediate tactics (defined as EM/SDM, as opposed to R/LDM which lie on the ends of the behavioral continuum)."

***Comment:***

Figure 2: Can the migration strategies be placed “in order”? The text refers to SDM and EM as “intermediate” strategies, so intuitively they should be in the middle of the axes.

***Response:***

Thank you for making this suggestion, we have reordered the figure accordingly.

***Comment:***

Line 383-395: This paragraph is really interesting. Do you have any indication that the prevalence of migration in this sample/study is representative of the prevalence of migration in the region?

***Response:***

Thank you for pointing out that we missed this--it is a key point. We added a sentence in the methods section clarifying the herd level representation covered by the dataset used in this report in lines 239 -242: "This dataset is a subset of movement data from the 26 known GYE herds for which GPS data have been collected (Gigliotti et al. 2022), using data only from those individuals with at least two consecutive years of movement data (20 of the 26 herds)." We also added a sentence in the discussion section about this as well in lines 532 -535: "Although this is the most comprehensive sample of multiyear elk movement data from the GYE, it is possible our results under- or over-estimated switching rates given that we lacked multiple years of consecutive data from 6 of the 26 known herds in the ecosystem (Gigliotti et al. 2022)."

**Reviewer 2**

***Comment:***

Defining switching requires individual seasonal movements to be categorized into discreet ‘tactics’ based on the geographic and elevational differences between seasonal ranges in consecutive years. Cluster analysis is used to define the tactics within the two-dimensional continuum that describes the seasonal movement behavior. As is readily apparent by viewing the data in the inset of figure 1 or the centered and scaled data in Appendix A, a relatively small change in distance and/or elevation between summer and winter ranges of an animal near the boundaries defining the various migratory tactics could result in an animal’s seasonal movements in consecutive years to cross a boundary and thus be defined as a ‘switch’. This leads me to wonder what proportion of the switches defined in the analysis might be due to relatively minor differences in movement behaviors in consecutive years that are perhaps biologically not that significant versus more substantial changes in movement behaviors.  It seems like it would be insightful to explore this a bit to give the authors and readers more insight regarding the switching. One way to visually accomplish this would be to produce a graph similar to Appendix A of just the pairs of data points for consecutive years of individuals that were identified as switching migratory tactics and connecting those two points.

***Response:***

Thank you for this key point of feedback. We certainly agree that our automated leaves room for non-biologically consequential switches. Despite the possibility of relatively small changes in behavior being classified as switching, we feel our approach is justified as it is an objective, reproducible methodology that does not rely upon predetermined behaviors. Again, we appreciate you noting this concern. We acknowledge this limitation in our discussion section, adding several sentences in lines 535-540: "Similarly, it is possible that relatively minor shifts in distances traversed and/or elevation changes were denoted as switches in migratory tactics due to the automated classification of migratory tactics. While this could result in changes in tactic that do not have considerable biological consequences, our approach is justified as it is objective, reproducible, and does not rely on predefined behaviors (Lowrey et al. 2020). "

***Comment:***

My second general comment relates to interpretations of the data and analyses of the study with respect to potential temporal trends in migratory behaviors. While the data set used in this work is impressive I do not think it provides insight into potential shifts in migratory behaviors.  While the data were collected over a 14 yr period, the studies of the individual herds generally spanned only 2-4 years with the 20 short-term herd studies distributed over the 14 yr span of the data. Individual herds had variable migratory portfolios, as nicely summarized in Appendix B. An finally, most of the switching behavior identified came from data on individual animals with only 2 consecutive years of movement data providing information to identify a single potential switch in movement tactics. Thus most of the documented switching does not provide insight on whether a switch in seasonal movement tactics represents a permanent change in movement behaviors of the individual animals.  The modest amount of longer-term movement data for individual animals presented seems to indicate that seasonal movement tactics at the individual animal level can be plastic with animals switching back and forth between tactics.  Given all these dynamics I don’t think that this study can provide much insight on the contribution of switching behavior to potential long-term trends in migration behavior of elk herds in the GYA or at broader spatial scales. It does seem to me that the modest information available from this study that demonstrates individuals switch back and forth among tracts, as opposed to a permanent change in migratory tactic after a switch, suggests that the documented shifts in the proportion of individuals in a herd that are resident versus migratory is most likely primarily driven by differences in the fitness of the strategies and not individual animals changing movement tactics.  More data would be needed to make such an assertion convincing but this study certainly sheds new insights on the issue.  I realize my rambling thoughts in this paragraph does not provide any explicit suggestions for improving the manuscript but perhaps it can provide a bit of food-for-thought when you revisit the discussion.

***Response:***

We agree that our ability to speak about long-term trends of migratory behavior is limited. We mention this limitation in the second to last paragraph in the Discussion section, but have added to it in accordance with this comment in lines 543 - 548: "Additionally, due to the lack of long-term data for individuals, we were unable to evaluate the permanency of shifts in tactics and therefore cannot make predictions about long-term trends in migratory behavior. Regardless, our findings provide considerable support for the idea that diverse migratory portfolios, and the cultural knowledge transmission they enable, lead to increased migratory propensity that can make ungulates more resilient and adaptable to changing conditions over the short term." We have also made sure to specify that the trends we see are during our study period, and may or may not be reflected outside of it (e.g. lines 427-428). If we do comment on the long term, we make sure to clearly state it is speculation rather than inference from the results (e.g. 476-477).