RSBL-2024-0458 entitled "Passive plasticity and a sex difference in the predictability of mobility"

Dear Dr. Johar,

I thank you for the opportunity to revise and resubmit my manuscript (RSBL-2024-0458) entitled “Passive plasticity and a sex difference in the predictability of mobility”.

The reviewers offered many excellent and helpful comments, suggestions, and ideas for future studies. I can’t thank them enough for their efforts. I have done my best to address and incorporate their suggestion into the revised version of our manuscript, with an eye to keeping manuscript length in check.

I hope my revisions are satisfactory.

Clint Kelly

Referee: 1  
  
Comments to the Author(s)  
General comments  
  
Although the topics of the manuscript (hereafter ms) are interesting and have the potential to bring novelty to insect movement ecology in the future, they remained slightly preliminary due to the dimension of the study and the short analyses reported. These issues restrict the MS from delivering any broad and straightforward message expected by the journal. Therefore, I may consider this MS as a partial fit into the journal's mission. The concept of the ms is excellent, but this superb idea is only partly opened and slightly supported by the analyses from the point of view of the potential reader. The manuscript is excellent in grammar and format; only a minor adjustment might be considered for an update.

Thank you for your kind words.  
  
  
Specific comments  
  
Lines 31-34 – the justification of behavioural predictability, to a large extent, is quite short and mostly relies on the excessive knowledge of the cited references. I may add more detail here in a long(er) sentence.

Excellent point. Broader context and detail has been added to this statement.  
  
Lines 68-82 – Although the sampling design is well-implemented, I missed some details about the data collection, such as how often the individuals were tracked per day(night). Some descriptive information might be added, such as the number of data records per individual, total distance taken per individual, and average distance taken per individual.; S.D. for distance taken per individual.

I have added information about daily relocation frequency and inserted a frequency histogram of nightly travel distance per sex in the supplemental material. The total number of times observed and average number of times observed per individual are now given for each sex.  
  
Lines 93-106 – The applied statistical methods seem well-implemented. I have missed the presentation and analysis of the individual trajectories. In addition, the hidden Markov models are becoming popular nowadays in movement ecology, and they can estimate the transition probabilities between various movement stages. This technique might directly support the current analytical approach in the MS.

I’m not sure what you mean by “individual trajectories” but the focus of this analysis is on the difference between the sexes, not individuals. Hidden Markov models are indeed powerful statistical tools, however, the data presented here do not lend themselves to their use, particularly because I do not have transition states (e.g. stationary to walking to running). This would be an interesting avenue to pursue if the technology evolves wherein transmitter small enough to fit an insect can record different behavioural/movement states.  
  
Line 150 – period is missing after the word “nights”

The period has been added.  
  
Lines 156-170 – Large vertebrates, as an example, were illustrated in the ms for presenting behavioural predictability. Although the examples are correct, I may argue their legacy due to the higher cognitive function/role in animal behaviour in vertebrates compared to invertebrates/insects. It can be a good option to discuss the lack (or rather low number of publications) of these types of studies among invertebrates and suggest a potential pathway for future research.

My point in this section of the manuscript is not the number of studies investigating predictability in vertebrates vs. invertebrates; my point is that few studies have investigated sex differences in predictability. The final sentence of the manuscript states that we need more investigation of sex differences in behavioural predictability, particularly regarding movement behaviour.  
  
Lines 184 – I agree with the author that the major missing element from this neat MS of the suggested laboratory experiment for confirming the hypothesis on passive plasticity. without this experiment, there is no reference point for the calibration of these results presented in this MS.  
  
  
  
Referee: 2  
  
Comments to the Author(s)  
  
This study investigates whether male and females weta differ in their predictability of nightly travel distance. The question and setup are clear and results as expected, and I agree with the broad conclusion that this is likely ‘passive’ rather than the result of any kind of adaptive process.  
  
The data and model seem reasonable (although see a couple of notes below) for the main hypothesis test. There is little information given on the mating success data, and with a relatively small sample size then perhaps it is not surprising that this does not produce meaningful patterns.  
  
I think the author could perhaps improve the visuals with this paper to provide some more information to the reader, eg distributions of the raw data, picking out some representative individuals that are relatively high / low predictability in each sex, etc.

I include two figures in the online supplemental material that address this suggestion. First is a correlation between predictability and behavioural type with individuals colour-coded by sex. This allows you to see the predictability for individuals of each sex. Second, is a frequency histogram of nightly travel distance for each sex.  
  
Finally, I am not sure I agree that the paper’s final conclusion that “we need to redress the paucity of published studies on sex differences in behavioural predictability, particularly regarding mobility” is warranted from what comes before. See below for more on distributions, but the difference in residual variation across the sexes when there is a large difference in mean values and the measurement variable has a strict lower bound does not seem surprising to me. If this is to be the closing statement then I think a bit more work needs to be done in the preceding discussion to make a case for why this might be an interesting and useful line of research.

This is an interesting and fruitful line of research simply because we have approximately three studies investigating it. Once we have more data in hand then we can address whether this is a biologically important phenomenon or one that is restricted to a handful of taxa. Regardless, if there is a sex difference it behooves us to figure out why and the reason simply can’t be due to a “strict lower bound” because this applies to both sexes, yet they differ.  
  
The modelling process seems reasonable although one thing I would consider is that this model assumes that males and females are drawn from the same distribution in terms of among-individual variation in mean (and observation-related slope) travel distance. Given the results state a previous report on this population found very different repeatabilities for males and females (although the variance components are not provided here), it is possible that this influences the results and should be considered.

The repeatabilities do not differ between the sexes and sex explains very little (approx. 8%) of the among-individual variation in travel distance. I added this information to the Results and supplemental material.  
  
I am also unfamiliar with the inverse hyperbolic sine transformation or the R package bestNormalize, but I did wonder the extent to which the model results could be overinterpreted given the fact that (i) there is a lower bound in the data (movement distance cannot be <0) and (ii) females have a lower mean and thus by default there is less space in the distribution for variation to occur? However, I think this is usually more of an issue when correlations between individual mean and predictability are overinterpreted; here it seems clear that females are able to travel short and predictable distances because food is close by and plentiful, while males may travel longer and less predictable distances because mating opportunities are more patchily distributed. I suppose largely conforms to the author’s conclusion that the differences in predictability are driven by ‘passive’ factors, with the caveat that to some extent it may be generated by the process of males searching for females over long distances. It would be quite interesting to know how distributions of male movements are affected by female density, if it were possible to test this at some point!

The inverse hyperbolic sine transformation was introduced by Johnson in 1949. One of its aims is to transform data having zeroes to avoid adding an arbitrary number to the data and log-transforming. It really took off in econometrics in the 1980s and has been used quite frequently by evolutionary ecologists over the past 2 decades (more so now since the publication of the R package bestNormalize).

I’m not sure what you mean by “overinterpreted” but I’m sure it would apply to all analyses of animal movement because every creature’s movement is bounded by zero.

Average movement by females might be less than that exhibited by males but it is not zero i.e. there is lots of opportunity for females to vary in their movement. In fact, some females walk as far as males. What’s important in this analysis is how consistent each individual is in their movement.

I agree that testing male movement in relation to female density or availability would be interesting. I doubt it could be satisfactorily done in the field and lab tests present challenges given these animals are so large and walk considerable distances. We tried a small-scale test of this in Kelly and Gwynne (2016, BEAS) but could only test 2 individuals at a time. This idea should be revisited, however.  
  
8: not sure about the placement of the parenthetical statement here – I’m not sure I would define predictability as ‘residual within-individual variation’ (this seems at odds with the definition in the introductory paragraph as well?), and behaviour hasn’t been mentioned at this point.

I’ve re-worded the first line of the Abstract to make my meaning clearer. However, residual within-individual variation is the field-standard definition of predictability (see Hertel et al. 2020; Cleasby et al. 2015; Stamps et al. 2012; Biro & Adriaenssens

013).

24: While plasticity can be termed generally within-individual variation, it might be best to describe it as within-individual variation in relation to some other variable (it’s easy to get bogged down in the definitions here, but it’s a bit confusing to discuss residual variation that’s effectively left over after controlling for within-individual variation…)

I agree. I have added that individual plasticity is context-dependent change in behaviour and is represented by a reaction norm slope.

31: Don’t understand use of ‘conversely’ here

I am comparing non-biological sources of variation, such as statistical sampling error, with biological sources of variation, such as selection.

80-81: Is there any data to support this assumption? It is not unreasonable but would be good if there was a stronger basis to it. I am unsure of how weta detect one another, so if one weta travelled a long distance in a straight line while another travelled the same overall distance in a more thorough exploration of the local area these would be quite different strategies… yet in this setup a thorough local search would give the same ‘distance from start’ phenotype as a short straight line distance. Is there anything known about differences in strategies among males in this or related species? Also, the methods state that weta seek refuge during the day by hiding under vegetation or other objects – how well distributed are such refuges in the pasture? E.g., would those that travel some distance have to make their way back to find refuge?

We can only assume that this is the minimum distance travelled. The sinuosity of the weta’s nightly travel route is unknown and would be nearly impossible to quantify in situ. Perhaps future technology will allow us to combat this challenge, but I’m afraid this will remain an issue in many animal tracking studies.

Nothing has been reported in this or other species regarding males possessing or specialising in different search strategies; however, that would be an interesting line of research.

Any clump of roots or tuft of grass can act as a refuge. So when you look out onto a hectare of pasture or grassland, the number of refuges approaches infinity. Therefore, females likely have little need to return to a refuge since they can hunker down nearly anywhere.

128-131: consider rewording second part such that “nor was mating success in either sex correlated with behavioural…” comes before the results information – currently we are given correlations and p-values etc before knowing what the variables being correlated actually are.

“…mating success correlated with…” was placed before “…behavioural predictability…”