

Revision manuscript #1195

Reviewer #A and Handling Editor

1. Title.

Authors – We have changed the title of this manuscript as suggested.

From: Climate-driven variation in the use of space of the neotropical marsupial *Metachirus nudicaudatus*

To: Climate-driven variation in space use by the neotropical marsupial *Metachirus nudicaudatus*

2. Not finding population size effect does not suggest this statement.

Authors – This statement is based on previous studies on population dynamics with this species in the study area. A more extensive explanation can be found in the "discussion section" of the manuscript. Thus, we opted to maintain the statement in the abstract.

3. Line 40: (Didelphimorphia, Didelphidae)

Authors – We have added it as suggested.

4. Is the home range size of males and females similar just because one study found it? What others studies found? Is there a general pattern for marsupials?

Authors – In general, for didelphid marsupials, the larger the animal, the larger will be its home range. Indeed, our hypothesis was based on one study. We agree that we have to test the hypothesis regarding the general pattern for the group. Thus, we have changed our hypothesis.

Line 54 - "(1) Movement areas differ between males and females. In general, males of didelphid marsupials have larger home ranges than females (Cáceres et al. 2012)".

5. Line 43: Or space?

Authors – The correct term is habitat.

6. Besides the fact that high population size can increase the intensity of space use (IU), the home range size (HRD) of mammals has an inverse relationship with the population density.

Authors – We agree with the reviewer statement and we have rephrased our hypothesis.

Line 55-58 - "(2) High population sizes increase the intensity of habitat use and decrease the daily home-range areas. Previous studies have shown a more intensive use of areas (Almeida et al. 2015) and smaller home ranges (Pires & Fernandez 1999) at high population sizes."

7. Which season has more food resources at the study area? Many studies points that it may affect the size and intensity of home range and space use.

Authors – We do agree and differences between resource availability between season are explored in our study. We have made it clearer in our hypotheses.

Line 58-60 - "(3) Daily home range areas are larger in the dry season. Home ranges are usually larger at periods with low resource availability (Cáceres et al. 2012); in the study area this period is the dry season (Ferreira et al. 2016b)."

8. Define home range. Maybe Space use, Movement areas, Movement distances, or daily movement are better

Authors – We defined home range properly.

Line 39-40 - "... home-range size (the space used to obtain the necessary resources for its survival and reproduction; Burt 1943)..."

9. It's a little confusing. I think the hypotheses should be generated based on general patterns for the group, not for what was found in a single previous study at the same area. General patterns should be cited at the introduction, before presenting hypothesis.

Authors – We agree and we have based most of our hypothesis on general patterns of the group. Cáceres and collaborators have made a review on the use of space by marsupials and we have based our hypothesis on their findings. We did not presented these general patterns in the introduction because we do not have enough space for it; this is a short communication and space is also a limiting factor for us!

10. Seasonal? But it occurs almost throughout the whole year, except in the two driest months (May and June).

Authors – We agree with the reviewer and we have removed the word seasonal.

11. Line 58-59: it starts in the middle of the dry season (July) and cease in the beginning of the next dry season (April) (Citation).

Authors – We have added a citation to this information.

12. What was done about the first few meters after the animal was released, was it discarded?

Authors – Yes, we have discarded the first ten meters. We have added this new information in the manuscript.

Line 85-86: *"To avoid potential adverse effects of researchers, we did not analyze the first ten meters of thread."*

13. It's not clear. Was this variable estimated by the period of the year in which the species is supposed to reproduce, or by the reproductive condition of the monitored animals?

Authors – As stated in the same paragraph, the reproductive season was determined by the condition of the females.

14. Estimated population size. Define the period for the abundance estimative.

Authors – We stated in the manuscript that we wanted to test if the *current population size* affects the space use of this marsupial population. Thus, the period was the same month the animal was tracked with the spool-and-line device.

15. Which months are considered to be at dry or rainy seasons?

Authors – As stated in the manuscript we evaluated each year and month separately with the Setzer index. This index uses data on precipitation and temperature of the month to estimate the effective monthly precipitation. Based on this information we obtained the monthly cutoff points for dry ($\pi_m \geq 10.8$) or wet ($\pi_m < 10.8$) periods. For example, besides 2003, August was classified as a wet period in years. If necessary, we can add an appendix with all the months the study was conducted and the classification each one received based on this index.

16. It's important to separate the presence / absence of young individuals from adult reproductive activity.

Authors – We did not understand this suggestion, but we will state what we have done. The presence of young individuals with female marsupials is an important clue of the reproductive activity. In neotropical marsupials pregnancy is short and it is almost impossible to detect in the field. Thus, researchers that study marsupial populations use this clue to infer the reproductive activity of the study population.

17. Only 10 models are presented at the table.

Authors – We agree with the reviewer and we apologize for our mistake. We modeled 10 models for each response variable, not 11.

18. What was the global model? Why don't you have a model including all the four tested variables?

Authors – We do not have a global model with all variables included because we did not generate a set of models with combinations (subsets) of the terms in the global model. Our models were based on hypotheses.

19. Were there two separate analyzes? Each one with 11 models? If these tests are different, they should not be added together.

Authors – Yes, there are two separate analyzes. We did not separate the explanation because we are aware that we are submitting this manuscript as a *short communication* and, consequently, the space is reduced. Since the hypotheses are the same for both response variables, there is no need to write twice the same procedure.

20. Line 97: "...in R environment (version 2.13.0) (Citation)."

Authors – We have added the citation.

21. Present these results into graphs and include differences between current population size, climatic, and reproductive seasons, as well as size sample for each variable.

Authors – As stated above, this is a short communication. We do not have enough space to include three figures. That is why we opted to give the same information in this paragraph. We have added the sample size for each variable in the beginning of the paragraph, except for population size. We have added it now.

Line 120 - "*During the study period, population size varied from 1 to 8 individuals (4.11± 1.73).*"

22. Table should be inserted after being quoted in the text.

Authors – We have changed it.

23. Which hypothesis? Differences between sexes? Or climate?

Authors – Initially, we had four hypotheses and we confirmed two. That is why we state that our hypotheses were partially confirmed.

24. Analysis results. But what is the real differences between dry and rainy seasons? You should make a table comparing the model results to the size of the estimated areas.

Authors – The table comparing the results for both dry and wet season is the table 2. In this table, we give the estimates of the intercept and curvature of the top-ranked model. Daily home range in the wet season is smaller (-0.431 ± 0.141) than in the dry season. We stated the difference between DHR in the dry and wet season in the following sentence: "*DHR varied between climatic seasons, with individuals using larger areas in the dry season (Table 2)*".

25. Less plausible, but important as well as population size.

Authors – We did not state in the manuscript that we only considered the models with a $\Delta AICc \leq 2$, following the paper of Richards (2005). We added this information to the text and it will be clear now.

Line 106-107: "*Models were considered equally plausible with $\Delta AICc \leq 2$ (Richards 2005)*."

26. Besides of “not differ” it is better to say that sex and season were less important to explain the IU differences.

Authors – We think that if the *null model* (or the intercept-only model) was selected as the top-ranked model, this means that no analyzed factor was able to explain the variation in the intensity of habitat use. Thus, we can affirm that there are no differences between sexes and seasons regarding this variable.

27. The 2nd, 3rd and 4th models all look plausible. You should cite the other plausible models. The model average is able to do this.

Authors – We have explained why we did not considered these models in #25 and #26.

28. This sentence should also be at the introduction.

Authors – We did not understand why we should include a possible explanation to our results in the introduction. We were trying to explain, based on evidences from previous studies, the results we found. We opted to not include this information in the "*introduction section*".

29. Line 135: Standardize

Authors – We have changed it.

30. Not only experimentally. Conduct natural resources availability data collection could also support, or refuse the tested hypothesis.

Authors – We have added the suggestion.

Line 153-155: *"We suggest that further studies should test experimentally, or with the natural resources available in the area, the effect of food availability on the space use of marsupials in an attempt to confirm or rebut the supported hypothesis."*

31. Line 167: This reference is not cited in the text.

Authors – We have removed it.

Table 1

32. I suggest present a table with daily home range and intensity of habitat use results.

Authors – Since the manuscript is a short communication we showed the most important results in tables (we have two); all other results are in the text format, which also inform the reader the differences without occupying space.

33. Where and when this study was conducted?

Authors – We have added this information.

34. Again, it is confuse how the reproductive season was estimated.

Authors – We did not tried to explain in the table legend how reproductive season was estimated. Details can be found in the text.

35. Relative Abundance?

Authors – No, population size.

36. Where are the AICc values presented?

Authors – There is no need to present the AICc values. We defined AICc because the reader has to understand what it means to understand the $\Delta AICc$.

37. Headings should be more complete. Always inform were and when the work was conducted.

Authors – We have accepted the suggestion and have changed it.

38. Was does “Clim”, “Line”, etc., means? It should be specified at table’s heading.

Authors – We have accepted the suggestion and have added it.

Suggestions on writing style:

Line 13: We have included the term *habitat*, not *space*, as suggested, because we have estimated the intensity of habitat use.

Line 14: The reviewer suggested a change in the writing style that we do not agree. Although, we have improved the sentence. "*...and if it differs between males and females.*"

Line 20: The verb to rebut is correct. There is no need to change to refute.

Line 43: We have removed the term "of areas".

Line 49: The name of the species is now complete.

Line 87: We corrected to Non-reproductive.

Line 106: The verb to confirm is correct. There is no need to change to corroborate.

Line 110: We have accepted the suggestion.

Line 121: We have corrected the name of the species.

Reviewer #B

1. Although the spool and line isn't a recent technique to mapping the small mammals track, only few studies has dedicated to apply it and create new records about the movement of these animals in Brazilian tropical area.

Authors – We agree with the reviewer. Few studies with small mammals in Brazil applied this simple and relatively cheap technique in the study of animal movements. Here we have shown how informative results about movement ecology and population dynamics can be generated when this technique is applied.

2. I suggest the use of another reference to compare the data: ALMEIDA, A.J., TORQUETTI, C.G. & TALAMONI, S.A. 2008. Use of space by Neotropical marsupial *Didelphis albiventris* (Didelphimorphia) in an urban forest fragment. Rev. Bras. Zool. 25:214-219.

Authors – We have accepted your suggestion.

3. As a suggestion, I think that the research should dispend more efforts to amount the data allowing the use of more robust analysis. Maybe in the future, another scientific communication can be write.

Authors – We thank you for the suggestion.

4. I suggest the use of expression “Spool-and-line” between the keywords

Authors – We have added the expression to the keyword list.

5. Very good note!

Authors – Thank you.

Reviewer #C

1. Line 13: "... intensity of habitat use ".

Authors – We have added the term in the sentence.

2. I do not agree that the variable measured is spatial arrangement. DHR and IU may be related to it, but not necessarily.

Authors – We agree with the reviewer and have corrected the sentence to "*Population size had no effect on their movements...*".

3. Why a negative result should show an ecological factor regulating the population? Why the lack of a population size effect on the species space use indicates intraspecific competition?

Authors – As stated for the reviewer #A (#2), this statement is based on previous studies on population dynamics of this species in the study area (Ferreira *et al.* 2016a, b). Both long-term studies (13 and 16 years, respectively) have shown that *M. nudicaudatus* is regulated by direct density dependence, i.e., this marsupial has a self-regulating population (Ferreira *et al.* 2016a), and regulation is stronger in the dry season (Ferreira *et al.* 2016b). We suggested four possible mechanisms that can result in the population regulation based on previous knowledge of this species: 1) Intraspecific competition for food resources; (2) Intraspecific competition for space; 3) Predation by a generalist predator; and 4) Detrimental effects of parasites. In here, we tested if population size in the current month affects DHR and IU. We found no effect of population size on *M. nudicaudatus* movement, which strength our hypotheses of intraspecific competition for food.

We understand that this may not be clear and we have rewritten the discussion section that we correlate the results of this manuscript with previous results.

Ferreira MS, Kajin M, Cerqueira R, Vieira MV (2016a) Marsupial population dynamics in a tropical rainforest: intraspecific competition and nonlinear effect of rainfall. *J Mammal* 97:121–127. doi:10.1093/jmammal/gyv161

Ferreira MS, Vieira MV, Cerqueira R, Dickman CR (2016b) Seasonal dynamics with compensatory effects regulate populations of tropical forest marsupials: a 16-year study. *Oecologia* 182:1095-1106 doi 10.1007/s00442-016-3735-x

4. All hypotheses should be supported by logical assumptions first, then authors could use data from other studies. I suggest the authors expand this paragraph. Why females and males should or should not differ in their DHR? Why large populations result in increased intensity of use? Why DHR are larger in the dry season? The reasons why these hypotheses are tested should be stated prior to results found for the same species or other species of marsupials.

Authors – We agree and, as stated for reviewer #1, we have changed the third paragraph and we have included general patterns for the group and we have tried to explain our hypotheses better.

5. There are relevant information in the literature to support the opposite direction for this hypothesis. For instance, females may become more territorial during the breeding season and reduce their DHR (see an example with a spiny rat in Almeida et al. 2013. Use of space by the Neotropical caviomorph rodent *Thrichomys apereoides* (Rodentia: Echimyidae). *Zoologia* v. 30, 35-42). Also, sex differences may be expected when females are more territorial than males, by investing in acquiring and defending resources while males invest in finding females. This seems to be the case for *D. aurita*: (1) Cáceres 2003. Use of the space by the opossum *Didelphis aurita* Wied-Newied (Mammalia, Marsupialia) in a mixed forest fragment of southern Brazil. *Revista Brasileira de Zoologia* v. 20, 315-322; 2) Loretto and Vieira 2005. THE EFFECTS OF REPRODUCTIVE AND CLIMATIC SEASONS ON MOVEMENTS IN THE BLACK-EARED OPOSSUM (*DIDELPHIS AURITA* WIED-NEUWIED, 1826). *Journal of Mammalogy*, v. 86, 287-293.; 3) Cerboncini et al. 2011. Use of space by the black-eared opossum *Didelphis aurita* in a rural area in southeastern Brazil. *Mammalia* v. 75, 287-290). This may the reason why more males were sampled during the reproductive season.

Authors – We agree that several studies support differences between sexes. We have changed the first hypothesis. However, it is important to note that several studies are showing a different trend, with no differences in DHR between males and females, as we did found.

6. This can only result in similar DHR if HR size is correlated to DHR size. This is usually true, but there are some important information in the literature that may be used to support it. The idea that DHR size usually correlates with HR size

should be added in the text (not necessarily here). For results related to the effect of thread tracked on space use variables, see:

1) Cerboncini, R.A.S.; Passamani, M.; Braga, T.V. 2011. Use of space by the black-eared opossum *Didelphis aurita* in a rural area in southeastern Brazil. *Mammalia*, 75: 287-290.

2) Almeida et al. 2013. Use of space by the Neotropical caviomorph rodent *Thrichomys apereoides* (Rodentia: Echimyidae). *Zoologia* v. 30, 35-42.

3) Loretto and Vieira 2005. THE EFFECTS OF REPRODUCTIVE AND CLIMATIC SEASONS ON MOVEMENTS IN THE BLACK-EARED OPOSSUM (*DIDELPHIS AURITA* WIED-NEUWIED, 1826). *Journal of Mammalogy*, v. 86, 287-293.

Authors – We agree and we removed that information from that part of the manuscript. We have added the general pattern for didelphid marsupials as a support for our hypothesis. A review of space use of didelphid marsupials can be found in Cáceres *et al.* 2012.

Cáceres, N. C., Prevedello, J. A., & Loretto, D. 2012 Uso do espaço por marsupiais: fatores influentes sobre área de vida, seleção de habitat e movimentos. In: N.C. Cáceres, & E.L.A. Monteiro-Filho (Eds.), *Os marsupiais do Brasil: biologia, ecologia e evolução*. pp. 327-346. Campo Grande: Mato Grosso do Sul Federal University Press.

7. Since expectations are similar for males and females, this should be treated as a single hypothesis.

Authors – We agree and we have changed it.

8. Did the authors collect this information or was it collected from another study?

Authors – This data comes from a study developed by the Laboratório de Vertebrados which sought to understand factors affecting the movement of marsupials. Professor Marcus Vinícius Vieira and Professor Rui Cerqueira delineated the study and students from the laboratory collected the data.

9. The species reproductive season extends for 10 months. Is this really seasonal?

Authors – We agree with the reviewer and we have removed the word seasonal.

10. How often and for how long?

Authors – Every other month. We have included this information in the manuscript.

Line 75-77: *"Three 0.64 ha grids, each with 25 trap stations spaced 20 m apart in a 5 × 5 design, were sampled every other month from June 1999 to February 2005 (details of the study area and capture-recapture methods in Ferreira et al. 2016a,b)."*

11. Spool length?

Authors – We have added the spool length detail.

Line 80-82: *"Juvenile individuals received spools of approximately 1.7 g (175 m), while subadult and adult individuals, of 4.5 g (480 m)."*

12. Why were they followed? Was this procedure taken every month? What does trapping session mean (when a trapping session starts and when it ends)? How likely researchers may have affected the animals movements?

Authors – We have clarified all the doubts in the methods the reviewer have addressed.

Line 86: *"Animal paths, i.e. the thread released by the spool-and-line device, were tracked..."*

Line 75-77: *"Three 0.64 ha grids, each with 25 trap stations spaced 20 m apart in a 5 × 5 design, were sampled every other month from June 1999 to February 2005 (details of the study area and capture-recapture methods in Ferreira et al. 2016a,b)."*

To avoid misunderstanding we have removed the term trapping session, and replaced by capture.

Line 83-84: *"The exception was the first day of capture when individuals were followed four hours after release."*

This is a plausible concern. We have added the procedure we adopted during the study and also pointed that we did not analyze the first meters of thread to remove any potential effects of the researchers.

Line : *"After release, the researcher stood still until the animal moved away to avoid forcing it to run in any particular direction."*

Line 85-86: *"To avoid potential adverse effects of researchers, we did not analyze the first ten meters of thread."*

13. This procedure is similar to generating a null (simplest) model. Thus, I do not believe the results for IU indicate lack of effect of the explanatory variables. When selecting models based on a null model, similarity in the amount of variation explained by a more complex model can be an indicative of statistical significance. Thus, if the null model explains data better, this is not necessarily an indicative of lack of effect. I suggest the authors rethink their results, or even provide a different

approach to model selection. Since most models are built upon simpler models, the authors could even use a log-likelihood ratio test for determining statistical significance of the explanatory variables.

Authors – We have interpreted the model selection procedure differently. The intercept-only (null) model was included in both analyzes to compare the explanatory power of independent variable relative to **other unaccounted sources of variation in data**. Thus, if the null model is selected, it means that the variables analyzed are not able to explain the variation in the data better than the model with only the amount of thread tracked. It is important to highlight that the model select approach **do not test for statistical significance**. We do not think it is right to change our approach to model selection, or rethink our results, just to find a relationship between IU and any variable. We simply did not find any relation between variables, which is already an important result!

To clarify our procedure regarding the model selection approach, we added the following sentence: "*Models were considered equally plausible with $\Delta AICc \leq 2$ (Richards 2005).*"(line 106-107)".

14. This part needs some clarification. Your results indicate effects of seasonality, but not your hypotheses.

Authors – We agree and we have rewritten our hypothesis to make it clearer.

15. The effects of the amount of thread tracked seem obvious, since the amount of thread probably subestimated the distance travelled by the species in one night. Most studies with spool-and-line found similar results. Thus, I believe the amount of thread tracked is a kind of a null model in your analyses, and most of the variation in DHR and IU should be related to it. However, models with sex and seasonal differences provided a good explanation to your data, even with a larger number of factors in the models. I do not believe your results show a lack of effect of these variables.

Authors – Yes, the model with only the amount of thread tracked can be viewed as a null model. We know this variable can affect our results, thus we added it in all models to control its effect. Regarding DHR, the best-ranked model had the effect of climatic seasons, while the second -ranked models had the effect of climatic seasons and sexes. As explained before, we did not state in the manuscript that we only considered the models with a $\Delta AICc \leq 2$, following the paper of Richards (2005). We added this information to the text and it will be clear now.

Line 106-107: "*Models were considered equally plausible with $\Delta AICc \leq 2$ (Richards 2005).*"

For this reason, we only considered models that followed that rule. Besides, the second-ranked model have the same effect, climatic season, plus the effect of sex. This means that the variable sex has little contribution to data variability and the model was selected because climatic season has a strong effect.

Regarding IU, we think that if the *null model* (or the intercept-only model) was selected as the top-ranked model, this means that no analyzed factor was able to explain the variation in the intensity of habitat use. Thus, we can affirm that there are no differences between sexes and seasons regarding this variable.

16. Could it be also related to the beginning of the reproductive season? How territoriality may affect DHR and IU? Are females more territorial than males, as in other neotropical marsupials?

Authors – We do not think so. The beginning of the reproductive season for this species is mid-july. Territoriality in neotropical marsupials was never really tested. Lack of home range overlap indicate but do not confirm territoriality. DHR areas do overlap between males and females which may indicate lack of territoriality in *M. nudicaudatus*.

17. The hypotheses should be clearly stated here. Which two hypotheses were supported? Is the lack of effect of population size an indicative of an ecological factor? Is there any reason to consider that climatic seasonality could possibly not affect animal movement? I believe the authors should strengthen their hypothesis for these results to be meaningful. Please see comments on the objectives section.

Authors – We agree and we have strengthen our hypotheses based on general patterns of the group. We also have changed the result section and have made it clearer as requested.

18. An interesting result is that three (more complex) models provided better explanatory results than the simplest (null) model. I do agree with the authors that climate appears to be the only factor responsible for changes in DHR. However, some of the explanatory variables are not fully independent. If the species is territorial (as many small mammal species in the neotropics), territoriality may affect the way animals use space, but it would also affect capture probability. Thus, seasonality affects may not be independent of sex and population effects (males can

be more prone to be detected during the breeding season, for instance, if they are less territorial than females). This seems to be the case based on the number of males and females sampled with spools, but it would also result in an increased population size during specific periods.

Authors – Females of some didelphid marsupials have shown trends to stay in some sites probably to ensure access to food resources, an indication of a possible territorial behavior. However, territoriality was never really tested. Some authors also suggest that a lack of home range overlap also indicate territoriality, but it do not confirm. In *M. nudicaudatus*, DHR areas do overlap between males and females which may indicate lack of territoriality if we follow this line of thought.

Of course some variables are not independent, it is almost impossible to have in a field study variables that are completely independent - everything is related and only few variables can be controlled for. In here, we were aware that the amount of thread tracked could affect our results and that is why we have controlled it for.

19. Based on the results, it seems that seasonality, population size and reproductive stage affected IU, at some extent at least. The way the authors arranged the analyses is based on the length of line tracked as a a null model (simplest one). Since this is a vagile species and spool length may represent only a small portion of the distance the animals travel in one night, most of the variation is expected to be related to line length. However, models with other explanatory variables resulted in similar AIC values, which can indicate they may be related to a part of the variation found in estimates of IU.

Authors – If of the variation was expected to be related to line length we would not have detected the effect of climatic seasonality on DHR. As stated in #15, we think that if the *null model* (or the intercept-only model) was selected as the top-ranked model, this means that no analyzed factor was able to explain the variation in the intensity of habitat use. Maybe climate and population size can have an effect on IU, however we did not detect it in the analyses.

Suggestions on writing style:

Line 10: We have corrected it.

Line 101: We have removed it.