Sosa et al. provide an analysis of the social networks of highland cattle, using management decisions to investigate the effect of social and environmental changes on the social networks of the cattle and individuals' positions in those networks. The sample and sampling of groups is commendable given the constraints of such a system and the analyses are appropriate for the data (I have just one comment below). The authors combine lines of evidence to understand the effect of individuals' traits on their social centrality and use their findings to provide recommendations for welfare during transfers.

Thank you very much for your help and your comments.

We replaced the article in arxiv. The replacement on the site is scheduled to be announced at Fri, 20 Jul 2018 00:00:00 GMT.

I have one concern regarding the analyses of the second aim of the study i.e. the impact of group change on the positions of individuals in the networks. It could be that the social networks are always changing, and the observed changes in individuals' positions in the groups with social/enclosure change is just an artefact of ongoing dynamics in the networks of highland cattle. I feel that these analyses would be stronger if the authors compared the changes in metrics of the individuals in the groups with "exogenous" change to the changes (or not) of the metrics of the individuals in the groups without the "exogenous" change as a "control". This would better show that the observed change in network position is due to the change in group composition and not due to "background" change in individuals' positions through time. For example, a direct comparison could be made between Rob group's 2nd observation periods and Rol group if Rol group's data were broken into similarly-sized time periods. The same could be done for Rob group's second and third changes (enclosure change) with Nie and Stu groups in the same period. Since the authors suggest there is no difference between seasons in the cattle's behaviour, the "control" periods wouldn't necessarily have to be directly comparable with regards to the period of study.

This is already done as:

- we compared the changes in the group composition (from 0 transfer to x transfers).

- we used permutations to reinforce analyses power and to check whether the number of transfers is linked to a real change in social networks or if it is only due to an artefact. These permutations allowed to compare 0 transfer to 0 transfer, 0 transfer to 1 transfer, 1 to 2, 0 to 2 and so on

Some minor comments (see attached pdf for more specific comments):

We corrected directly on the MS the changes asked in the pdf.

Could the authors elaborate on the predictions? Some of them seem to be stated without an explanation as to why the particular prediction was made.

We added more details and references in the predictions.

Was the following prediction (from p5) tested?: "Resident individuals, i.e. those who experienced the arrival of a newly transferred individual in their group, should be less impacted than those being transferred"

This was tested in the point b. of the results.

Were rates of aggression quantified after transfers? If so, could the authors use these data to "test" their recommendation from Discussion section c. that transferring juveniles with an adult would result in "smoother" transitions / integration of individuals? The sample may be small, but it could provide anecdotal support for this recommendation.

We do have aggressions, but we have only one transfer with females + juveniles (in). Other in-transfers are not with juveniles or are out-transfers. So we do not think that we could make conclusions with only that, even if it would be nice to have this information.

P4. Females are not the philopatric sex in all primates (e.g. chimpanzees). Consider rephrasing this sentence.

Done

P7. This merits of the nearest neighbour or gambit of the group approaches depend on the question being asked: if one is interested in disease / information transmission, it is irrelevant if individuals A and C are in proximity because they are associated with each other or not, but that they can share disease / information because of their mutual association with B. It may be worth mentioning that the nearest neighbour approach is appropriate for this kind of study that aims to determine individuals' preferred associates rather than dismissing the gambit of the group approach, which can be a useful rule.

We changed the paragraph. We hope it sounds more appropriate now.

It would be easier to follow the manuscript it the numeration of the subsections in the Discussion matched the analyses that were done or were not enumerated in a similar format as that in the Methods section i.e. a, b, c, etc.

Done

Figure 1 is very useful. It requires a legend (e.g. do the forward, back and vertical strokes indicate a different type of change? Why are these different for the different groups? c.f. vertical strokes for enclosure changes for Rob group but bull departure for Nie group). The dots were not red in my version of the manuscript—either the figure or the legend needs to be updated. Finally, I think the solid line of the Rob group in the white, non-observation period should be a dashed line, if I've understood correctly.

We updated the figure 1 according to your comments. Thanks a lot!

Tables and Figures need titles as well as descriptive legends.

We gave more description of the figures and tables.

Table 3: can remove "(just one sex)" after "NA" throughout the table because this is explained in the legend.

Done

Figure 2 may be more useful if a change in at least one group is shown (preferably 2), in addition to the network exemplars shown.

Done

The legends of figures 3 and 4 seem reversed.

We corrected it.

Figures S1-5 could appear as 1 supplementary figure with S1 at the top (new panel "a") and S2-5 (panels "b-e") in a grid below. Table S1 is not a Table. These could also appear as one graph with panels a-d

Done

Download the review (PDF file)
Reviewed by anonymous reviewer, 2018-06-25 13:03
Download the review (PDF file)

We corrected directly the changes asked on the pdf file.

In the paper the authors aim to test (1) how familiarity, dominance rank, age and sex can affect sociality (social centrality) in *Bos taurus,* and how transfer(s) (linked to farmer management) of individuals in the studied groups can affect this sociality according to the socio-demographic parameters previously cited. I find the paper really interesting and well writing. I have just some minor corrections to suggest and some propositions for improving the reading of the manuscript.

Thank you very much for your comments

Abstract:
- the repetition line 13 needs to be corrected.

Done

Introduction:
- page 3 lines 27-29, can you reword the sentence?

We simplified the sentence.

- page 4 and 5: the paragraph lines 19-29 is a detailed description of your "material and methods" part and the last following paragraph (lines 30-end of introduction) lists your expected results. Is it really necessary to detail your material and methods so much in this

introduction? Maybe go to the line (line 20) and do an only one paragraph with a short part on what you have done (more synthetically) and then make your assumptions/expectations. As for these expectations, could you classify them to make them easier to read? Perhaps in the same form as your "material and methods" part with 1. effect of socio-demographic factors on associations/sociability and 2. the effect of changes.

> We preferred to keep the explanations of the Mat&Met as it was asked by a previous reviewer. This reviewer did not understand the two steps of analyses, so we had to explain. We added paragraphs for the expectations.

Material and methods:
- you have (in table 1) surfaces very variable between your observation sites. Can it have an effect on the groups? Could you add this information in your paper?

> We added the following sentence: "Enclosure size did not have an effect on aggression and cohesion of group members (correlation test with permutations between the enclosure size and the mean number of aggressions per day per individual: N=11, rho=-0.30, pperm=0.317; correlation test with permutations between the enclosure size and the mean number of 3m proximity per scan per individual: N=11, rho=-0.37, pperm=0.214)"

In this table 1, in the column "obs. time", can you add "period 1" and "period 2" before the dates?

> Done

- page 5 b. lines 24-25 when you cite the supp info, could you add the names of the files (S1 to S5)

> Done

- page 6 line 11: about the 90% of groundcover, how have you estimate this area? Add the information please (GIS?)

> We added this detail: "surface estimation with GIMP 2.9"

- page 6 line 16: can you add fig. 1 to complete the Table 1.

> Done

- page 6 part c.
For the group "Rob", notably rob 5 (line 27), I count 11 and not 12 females (14+7-13+3=11), isn't it? Correct this point in the text and in fig 1 and table 2.

> There was a mistake with "and one 1yo individual". It was not the case.

- page 7 line 2: can you add the age of the juvenile female as in the other localities?

> Done

- page 8 The abbreviation MDS can appear the first time when " the Modified David Score" is cited (line 10 I think) and after you can use "MDS" line 14 and line 16, or you can use the "Modified David Score" without abbreviation in all the text. Please homogeneise.

Done

- page 9 part f. "we" and not "We" line 25.

Done

- Page 10: line 4, add the R version please. You give the information page 12 but it could be done before.

Done

- page 11 line 19: I am not familiar with the average of statistics of different models using ΔAICc<10, could you add reference(s) on this point or explain why and how you do this (and why you have not take the best model (the best AIC)? I think it is important for understanding your result part.

References are Barton (2013) and Burnham and Anderson (2004) as included in the paper. We added this sentence: "Burnham and Anderson (2004) emphasized that information-theoretic approaches (AIC) allow formal inference to be based on more than only one best model (lowest AIC) and leading to more robust conclusions."

-page 12 line 5 Table S1 represents more figures than tables, please correct in the text and in supp info.

Done

Results:
- you have an inversion of legends between fig 3 and 4. Please correct.

We corrected it

- concerning the tables 4 to 7, please homogeneise the typo of "PPerm" or "Pperm".

Done

- Table 3, why don't you give the global results by locality (in addition to the global results for any locality)?

We did in this way because our purpose was more to give a p-value for the population (highland cattle) than for each group composition.

-Page 13 part c.: line 16-20, we don't understand why you do not add sex in the variables. Perhaps, it is linked to the previous comment in mat and meth on the average of statistics, but it is important to clarify this point.

There was a mistake. The table S1 was the same than the table S2... Sorry. We corrected this mistake.

Perhaps, the tables S2 to S5 in supp info can be used to clarify this point, by putting in bold the models and the variables retained for example?

Done

In these tables, why sex has not numerical value, what means the +? Please clarify.

Effect of sex is indicated by + because it is a factor (categorical). We added it to the legend.

- page 14 line 1: "RIV" or "rvi"? Please homogenize in the manuscript.

Done

Discussion:
- page 14 line 29, please remove "in figure 2".

Done

- page 16 line 10, please remove the point before citations.

Done

- page 16 lines 10 to 12: do you consider that you have tested the correlation between landscape and sociality? I don't think. Could you modulate this point?

We changed to "correlation between strength of associations and dominance."

References:
- page 21 Farine, D.R. 'n.d.', is it the references of 2017? Please correct.

Indeed, we added 2017.

- page 23 Villaret and Ron: please correct the typo (not all names in capital).

Done

Supplementary information:
- figures S1 to S5: on the maps, it lacks north orientation, the legend (dotted or solid lines) and the map source. You say in mat and meth that there is forest in all sites, could you add a legend about this? This could complete the landscape point previously suggested.

We added details to the figure S1 (now from a. to e.)

- as said previously, table S1 is not table, please correct.

We corrected it.

- For the tables S2 to S5, their names will change (S1 to S4, also in the text of manuscript), as previously said, you could put in bold the model lines and the variables retained finally in your results

Done.

and clarify in the text why sex has not numerical value (what means the +).

Effect of sex is indicated by + because it is a factor (categorical). We added it to the legend.