



Peer Community In Ecology

How empirical sciences may improve livestock welfare and help their management

Marie Charpentier based on peer reviews by **Alecia CARTER** and 1
anonymous reviewer

Sebastian Sosa, Marie Pelé, Elise Debergue, Cedric Kuntz, Blandine Keller, Florian Robic, Flora Siegwalt-Baudin, Camille Richer, Amandine Ramos, Cédric Sueur (2018) Impact of group management and transfer on individual sociality in Highland cattle (*Bos Taurus*). arXiv, ver. 4, peer-reviewed and recommended by Peer Community in Ecology.

<https://doi.org/10.48550/arXiv.1805.11553>

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Understanding how livestock management is a source of social stress and disturbances for cattle is an important question with potential applications for animal welfare programs and sustainable development. In their article, Sosa and colleagues [1] first propose to evaluate the effects of individual characteristics on dyadic social relationships and on the social dynamics of four groups of cattle. Using network analyses, the authors provide an interesting and complete picture of dyadic interactions among groupmates. Although shown elsewhere, the authors demonstrate that individuals that are close in age and close in rank form stronger dyadic associations than other pairs. Second, the authors take advantage of some transfers of animals between groups -for management purposes- to assess how these transfers affect the social dynamics of groupmates. Their central finding is that the identity of transferred animals is a key-point. In particular, removing offspring strongly destabilizes the social relationships of mothers while adding a bull into a group also profoundly impacts female-female social relationships, as social networks before and after transfer of these key-animals are completely different. In addition, individuals, especially the young ones, that are transferred without familiar conspecifics take more time to socialize with their new group members than individuals transferred with familiar groupmates, generating a potential source of stress. Interestingly, the authors end up their article with some thoughts on the implications of their findings for animal welfare and ethics. This study provides additional evidence that empirical science has a major role to play in providing recommendations regarding

societal questions such as livestock management and animal wellbeing.

References:

[1] Sosa, S., Pelé, M., Debergue, E., Kuntz, C., Keller, B., Robic, F., Siegwalt-Baudin, F., Richer, C., Ramos, A., & Sueur C. (2018). Impact of group management and transfer on individual sociality in Highland cattle (*Bos Taurus*). arXiv:1805.11553v4 [q-bio.PE] peer-reviewed and recommended by PCI Ecol. [<https://arxiv.org/abs/1805.11553v4>](<https://dx.doi.org/https://arxiv.org/abs/1805.11553v4>)

Reviews

Evaluation round #2

DOI or URL of the preprint: <https://arxiv.org/abs/1805.11553v2>

Version of the preprint: 2

Authors' reply, 07 September 2018

We cannot directly compare groups as they are not same size or same group composition. However we can compare corrected strengths of associations or centralities of individuals as we did. We also added the following sentence in the MS to be clearer: "Changes of enclosures without adding or removing individuals were included as 0 changes in the analyses. This allows to compare networks with transfer of individuals to networks without transfers."

Decision by Marie Charpentier, posted 07 September 2018

Revisions asked.

I only have minor comments regarding this revised version (I'm sorry for the time it took). Alecia Carter proposed an additional analysis ("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") that authors should definitively follow. This would improve greatly the article. However, if the authors cannot run these analyses, they should explain it clearly in their next response. Otherwise, I'm happy with the responses the authors provided. The only very last point, but I'm not sure how PCI deals with these questions, the article probably needs to be carefully edited for the English, although I'm, myself, not an English-native speaker!

Alecia's minor additional (related) comment: "One little point: I seemed to miss that the manuscript compared the networks after the transfer to networks that didn't have a transfer (as a control); the authors said that this was already done. If so, I think this needs to be made clearer as I really didn't understand that from the manuscript. If I wasn't clear and the authors didn't understand my point, then I still think this could improve the manuscript."

Reviewed by Alecia CARTER, 06 September 2018

One little point: I seemed to miss that the manuscript compared the networks after the transfer to networks that didn't have a transfer (as a control); the authors said that this was already done. If so, I think this needs to be made clearer as I really didn't understand that from the manuscript. If I wasn't clear and the authors didn't understand my point, then I still think this could improve the manuscript.

Reviewed by anonymous reviewer 1, 21 August 2018

The authors responded convincingly to all of my suggestions/comments. I do not detect new corrections to make except one in supp info, they could homogenize the shape of their titles (even interline, same font size and with or without "full stop" at the end of titles). Nice work!

Evaluation round #1

DOI or URL of the preprint: <https://arxiv.org/abs/1805.11553v1>

Version of the preprint: 1

Authors' reply, 19 July 2018

[Download author's reply](#)

Decision by Marie Charpentier, posted 19 July 2018

Revision needed

Both reviewers and I agree that the article submitted by Sosa et al. is a nice piece of work. Reviewers have, however, suggested interesting ways to strengthen authors' findings. In particular, reviewer 1 proposes an alternative analysis (compare changes in network's metrics with and without exogenous changes) that I suggest to explore. This reviewer also asks for a better presentation of the predictions. Finally, both reviewers provide useful corrections that should help authors to improve the reading. I also suggest to follow these corrections carefully. Pending these changes, I would be glad to read a new improved version of the manuscript to eventually recommend it.

Reviewed by Alecia CARTER, 04 July 2018

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.

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.

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

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"

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.

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

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.

It would be easier to follow the manuscript if 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.

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.

Tables and Figures need titles as well as descriptive legends.

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

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.

The legends of figures 3 and 4 seem reversed.

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

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