Some very minor final suggestions

The authors have done an excellent job of responding to the previous round of reviewer comments and the article now reads very well overall. I have a few very minor comments, suggestions and queries before I write my recommendation.

For the sections on sub-sampling:

Comment 1: It is not immediately clear why the intuitive explanation for results from female and juvenile networks is a combination of power and sampling (L446-448) – this has not been set out very well. The results would also seem to suggest that the relationship between social centrality and EPG is different for females and juveniles. For females there is a statistically significant association with both their position in the full network and that in the female only network, while for juveniles it is neither suggesting a biological difference here (less variation in juvenile EPGs? Juvenile EPGs depending on status of mother? Something else unrelated to the network?). Another curiosity here is that the results for females persist in both networks despite them being uncorrelated (at least according to Spearman rank). It could be this means that both overall connections and connections to other adult females are important? [or is it that the centrality measures are correlated when ranks aren't used – I can't remember the results before this was changed?]

Answer:

We now provided clearer description about what the different result from different way of sub-sampling network means in the Methods part (lines 372-376).

We also changed a bit of our description of the results from the data reduction models, hoping they will be clearer in the current version (lines 450-456).

We now provided further discussion on the possible reason why we fail to observe relationship in juveniles (lines 544-547).

For the adult female network measures, yes, the strength centrality is correlated based on Pearson's correlation, while they are not correlated based on Spearman's correlation. But we agree, these results shown that the social position of females are comparatively close in whole-group networks and adult female only networks. It is not surprising as females are known to be the core of Japanese macaque groups (Yamagiwa and Hill, 1998).

Comment 2: I am also intrigued by the results on random sub-sampling and strength. It seems very striking that there is no (apparent) decline with an increasing number of removed individuals in Table 6 (one is apparent for eigenvector centrality albeit only just). Can I just check how the comparisons were made – was it a% of statistically significant effects in the same direction as the observed effect or any statistically significant effects? If only the latter can I suggest that you provide both options in the table? It might help explain this slightly odd pattern a little more clearly if reductions in statistically significant effects in the same direction are being compensated for by increases in statistically significant effects in the opposite direction (the results level out at the ~5% error rate expected). It's really striking that just removing ~2 individuals from the analysis has such a big effect and it would be good to see this explored even just a little more. Following this suggestion might also help point out that with smaller sub-samples it is possible (albeit rare) to have results that are statistically significant effects in the wrong direction.

Answer: The comparisons were made in the former way you described: % of statistically significant effects in the same direction.

We now provided some more discussion on this topic (lines 585-589). However, limited by our ability of accessing what specifically were changed during random sub-sampling of the network, we still might not be able to explore this topic in depth.

We now provided also % of results that are significant in the opposite direction, and show an increasing tendency with the increase of individuals removed.

Now just a few quick suggestions on writing/typos that I noted when carefully reading the article.

L35-36: Suggest "partly an effect of sampling the incomplete network"

Answer: We applied this comment to the manuscript (lines 36).

L324: Would be good to clarify/clearly state that the zero-inflated part of the model was intercept only

Answer: We now added (intercept-only) words when describing the zero-inflated model (lines 324).

Figure 2: is it worth considering a log scale for the y axis given the distribution of the data. It may not be clearer but it may be worth checking?

Answer: We tested the log scale and it did increase the visibility of Figure 2. However, as log scale changes the y axis of the graph, we are worried that it might be unclear for the readers and that the results presented in models and in graphs are not equivalent. We therefore decided to keep our current approach.

L444: Suggest changing "than" to "to"

Answer: We applied this comment to the manuscript (lines 448)

L530-531: These results are not clear from the main text because of the unusual choices for the presentation of the Tables – might be worth stating explicitly in the relevant section as is done for adult females already

Answer: We decided to stay with our current presentation of results, that only statistically meaningful results should be included in the result section. We now provided explanation in the discussion to make it clearer (lines 535-539).

L543-544: This statement currently has the potential to be misleading. I would suggest changing "in general" to "can"

Answer: We applied this comment to the manuscript (lines 553)

SI Tables (where relevant): In the captions it would be good to integrate the two separate sentences about which entries are highlighted in bold as this is quite confusing currently.

Answer: we now re-phrase the sentences (SI Tables 4,5,6,7,8).