This manuscript presented by Yitbarek and Philpott presents a very comprehensive and novel statistical analysis of the dominance hierarchies of arboreal twig-nesting ants over nesting resources. The most prominent shortcoming that I find in this manuscript is the lack of focus towards emphasizing the significance of the findings of the study and their contribution for filling the identified research gap. The effort displayed by the authors in working out a comprehensive and novel statistical method for quantitatively analysing the dominance hierarchies of arboreal twig-nesting ants is highly commendable. However, I observe that the authors have not pitched the manuscript in a way that emphasizes the significance of these findings and their contribution for filling the identified research gap. Provided that these minor revisions will be addressed upon submission, I recommend this manuscript for publication.

As an immediate revision, I suggest the title to be changed as “Arboreal twig-nesting ants form dominance hierarchies over nesting resource”, since this study has focused only on twig-nesting ant species. The existing title, in my opinion, covers a wider range which includes all sorts of arboreal ants, not just twig-nesting ants.

While appreciating the effort of authors for identifying the significant research questions existing, I suggest supporting the existence of these research gaps with appropriate citations. For example, Stuble et al., 2017 provides a good insight of the existing knowledge and limitations involved with this research question (Stuble, K.L., Jurić, I., Cerda, X. and Sanders, N.J., 2017. Dominance hierarchies are a dominant paradigm in ant ecology (Hymenoptera: Formicidae), but should they be? And what is a dominance hierarchy anyways. *Myrmecological News*, *24*, pp.71-81.).

The results that the authors have obtained for dominance hierarchy uncertainty/steepness are quite an interesting and significant finding. To enhance the weight of this finding I suggest including a more detailed description of steepness and how it contributes to community structure and species coexistence in the discussion section. For example, Sánchez-Tójar et al., 2017 provides a descriptive insight of steepness as a source of uncertainty of dominance hierarchy (Sánchez‐Tójar, A., Schroeder, J. and Farine, D.R., 2018. A practical guide for inferring reliable dominance hierarchies and estimating their uncertainty. *journal of animal ecology*, *87*(3), pp.594-608).

Since the beginning of the manuscript the authors’ main objective had been to present novel statistical analyses to explain dominance hierarchies of arboreal twig-nesting ants over nesting species. Hence, the paragraph coming under discussion section (lines 228-251), elaborating existing knowledge on dominance hierarchies over food resources in ant communities fails to make a direct connection with the results obtained by the authors and the ultimate research question addressed by the manuscript. I suggest further elaboration on resource utilization in nesting sites and connection of that information with the results obtained on competition shown by different species over nesting sites. For example, Adams et al., 2019 provides some insight into how resource availability in nesting sites contribute to community structure. (Adams, B.J., Schnitzer, S.A. and Yanoviak, S.P., 2019. Connectivity explains local ant community structure in

a Neotropical forest canopy: a large‐scale experimental approach. *Ecology*, *100*(6), p.e02673.) In fact, the information included in lines 290-303, on how nest site factors such as canopy structure, tree size and nest entrance size, gives a comprehensive explanation on how dominance hierarchies over nesting sites affect the species coexistence, based on existing knowledge and findings as well as existing limitations and knowledge gaps. Hence, I suggest combining these two sections. It will better establish the relationship between dominance hierarchy and variability observed in the results in resource utilization in nesting sites, shown by coexisting species arboreal twig-nesting ant species. This relationship will add more weight to the answers provided by this study when addressing the major research question of how dominance hierarchies contribute to species coexistence.

I agree with the authors' point presented in lines 285-290, that there can be factors other than dominance hierarchy, influencing distribution and coexistence of arboreal ant communities. I suggest that this statement will carry more weight in the manuscript if a suitable regression analysis can be conducted to determine to what percentage does ant distribution and coexistence depend on dominance hierarchies. According to the information given in 116-118 I presume that the data collected from surveys on ant abundance/distribution and the dominance hierarchy rankings stated in Table 1 will serve this purpose.

The authors have presented a very interesting and novel approach for quantifying dominance hierarchies in arboreal ant communities and have made a good attempt to explain species coexistence in ant communities through it. However, in the conclusion section, I fail to see the weight and significance of those findings and their contribution to answering the main research question stated at the beginning. Rather, I see background information and suggestions for followup research included in the conclusion, which are content that could be moved to the discussion section. Hence, I suggest re-writing the conclusion in a way that the findings of this study are highlighted and emphasizes its contribution for filling the existing research gap.

I suggest moving the sentence, in lines 168-169, "The nodes in the network represent…" to the figure caption to improve the comprehensibility of the figure.

I commend the authors for their successful attempt of analyzing dominance hierarchies of arboreal twig-nesting ant species using a novel and comprehensive statistical approach, based on extensive field and lab work added to thorough literature review provided. Also I appreciate the manuscript written in unambiguous English. So I recommend this manuscript for publication, given that the above mentioned minor revisions are addressed and the manuscript is pitched in a manner that the significance and contribution of this study is emphasized to make a bigger impact.