# MS. NO. PSRM-OA-2019-0126.R1 ENTITLED "TAKING DYADS SERIOUSLY"

Dear Editors,

Thank you for the opportunity to revise and resubmit our manuscript once more. We believe the manuscript has benefited from the Reviewers' helpful and thoughtful comments. We have revised the manuscript, taking seriously each individual point raised by the Reviewers. Our comments and responses are shown in *BLUE* below each of the points made by the Reviewers.

We hope you agree that the manuscript has improved through this process and we are looking forward to your response.

*Date*: January 4, 2021 Version 0.02.

## Reviewer 1

(1) In my previous review I pointed out some problems with vague language, mostly in the opening sections. Honestly, I still find the introduction to be problematic. Maybe it could be cut and we just open with the part of section 2 where the paper begins to discuss processes that lead to dyadic and higher order dependencies? The reason is that the intro tries to strike a very general tone. But in reality, the paper proposes a specific estimation strategy for parameters in a specific parametric model of networked interactions. I think the paper would be more useful to readers if it started with the specific applications, and then on the basis of that, motivated the statistical specification on substantive grounds. There are some swipes at other literature are neither necessary nor clear enough to be very compelling.

- *something, something*

(2) "Most of these studies use a generalized linear model (GLM). However, this approach to studying dyadic data increases the chance of faulty inferences by assuming data are conditionally independent and identically distributed (iid)." It should really read, "Most of these studies use a generalized linear model (GLM). However, this approach to studying dyadic data increases the chance of faulty inferences **if the estimation and inference assume** data are conditionally independent and identically distributed (iid)." But then if this is all just about mistakenly assuming iid, just say it quickly and move on.

- *something, something.*

(3) Why would the mean estimate be biased downward? And, **biased with respect to what target of inference**? A problem with the opening discussion is that it does not establish a target of inference. Without doing so, "bias" is undefined. It seems like they mean the coefficient for the linear relationship between some regressor and the latent outcome variable. But what if one is using a nonparametric model or a semiparametric model that operates on a different latent space—is bias even defined? It seems that for bias to be defined we have to be restricting ourselves to a class of models that propose the same latent space, but that makes this much less general that the discussion leads on. Moreover, that paper is not committing to talking about bias relative to causal effects under effect heterogeneity, but isn't that usually what people are after? Or, maybe the paper is really concerned with bias with respect to a forecast prediction? If so, then we need to set up a forecast prediction error analysis.

- *something, something.*

(4) Moving to section 2, the extended discussion of specifying a likelihood under iid is overkill. Again, I would just move on to the discussion of the application. The point that iid is inappropriate will receive due attention in the course of developing the networked interaction model.

- *something, something.*

(5) Explain how inference is done for the parameters from the AME model. How are intervals produced exactly? (Need this because you examine these in the simulation and make many statements about intervals throughout the text.) What are the operating characteristics, in terms of convergence properties, etc.?

- *something, something.*

(6) The DGP is simple enough that the paper needs to include the analytical calculation of the bias from the simple probit regression of Y on X. We get no insight by just

having monte carlo simulation results. For the simulations, clarify the "standard international relations approach" as just Y regressed on X, when you introduce it. Then explain how confidence intervals were produced. Are these just normal-approximation Wald intervals with non-robust standard errors? Explain exactly how the confidence intervals are produced for the AME model. They were never characterized in the section that discussion the model.

- *something, something.*

## Reviewer 2

(1) I would have liked to see a justification for the choice of papers used in the replications. It is still unclear. Are these cases where the theory fail to characterize the direct and indirect effects, and implicitly assume that the latent dependencies are nuisances?
   - *something, something*

(2) The presentation of the AME model clearly states that the Xs are exogenous; it is likely that they are not in replications. Still, the AME estimates (which in the simulations do not seem to outperform the standard method for estimating the betas when correlation between X and W is high) suggest that the models in the original papers are misspecified, and that these differences are likely driven by a better modeling of the DGP. This is a very important take-home point.
   - *something, something*