We would like to thank the editor and reviewers for the constructive criticisms and insightful suggestions. We believe this paper to be significantly better because of it. We have organized the comments and suggestions from the editor and reviewers below in broad sections. In each section, we included the specific comments from the editor/reviewers followed by how we have addressed them in the manuscript and our rationale (in italics).

**DEFINING AND EXPANDING THE DISCUSSON OF CONTAGION**

Editor: Reviewer 1 notes that you never define ‘contagion’ explicitly. The literature on contagion is extensive and very diverse across the social and natural sciences. Clearly, a comprehensive review of social contagion is beyond the scope of the paper. However, it is important that you link explicitly to the literature in which the concept of contagion you use is rooted. Your revised paper will greatly benefit from an additional effort to clarify how you chose to use this term and why. For a recent review in the context of network models you may find it useful to consult the paper by Parker, Lomi and Pallotti, (“New Network models for the analysis of social contagion…,” Organizational Research Methods).

R1: “The paper makes an explicit reference to ‘contagion’, but this concept is never explicitly discussed nor defined (there is a 3-line discussion in passim in the last sentence of the paper). If this is the angle taken, it would be good to have some more detail about what the Author(s) mean by contagion and how exactly they are operationalising it.”

RESPONSE: *We have restructured the front end of the paper to clarify what we mean by contagion. We also included a short section that refers to the literature from which our concept of contagion comes from. We have clarified throughout the text and also with our research questions, that we are interested in examining two basic elements that would make gang violence contagious: its replication (violence leading to more violence) and its diffusion to third parties.*

**RESTRUCTURING THE PAPER AROUND SPECIFIC RESEARCH QUESTIONS**

Editor: Reviewer 1 also expresses the view that the paper could be stronger if redeveloped around a set of specific research questions. I concur with this view. I believe that a set of clearly specified research questions will make your contribution clearer for criminologists and, at the same time, help readers who are not necessarily conversant with criminology research understand the more general relevance of your work. Successful papers published in interdisciplinary journals such as Network Science typically demonstrate awareness that readers represent diverse audiences and multiple scientific communities.

R1: “I feel the paper lacks a set of explicitly defined research questions. It would have helped to have some clarity on this – particularly since the literature review is very dense and not always easy to follow (it points to many different directions…). E.g., on page 6, there is a discussion on the ‘collective’ vs. ‘normative’ features of violence, but this is then not followed up in the remainder of the work. “ (R1-line 238)

RESPONSE: *We have included specific research questions organized around the two elements of contagion we focus on: the replication of gang violence and its diffusion to third parties. We hope that the reorganization of the literature also helps with clarity.*

**ISSUES WITH THE USE AND DEFINITION OF THE TERM RETALIATION**

Editor: Finally, Reviewer 1 finds issues in the way you use ‘retaliation.’ (S)he finds that in some cases (e.g., imperfect retaliation), what you call ‘retaliation’ may not be retaliation at all. I will have more to say about this point in my comments below.

R1: “the terminology is not consistent with the one included in the model (Table 1), e.g., is “delayed” the same as “deferred” retaliation? Is “reflexive” the same as “immediate”?”

R1: ““imperfect retaliation”, as described in Table 1, may not be ‘retaliation’ at all. It might be an unconnected attack. How do we know that B -> Y was a retaliation for A -> B and not an independent decision taken by B for whatever reason? In order to be a retaliation, there should be a previous link (e.g., a non-violent one) between Y and A. If this is not the case, or if we don’t have enough evidence, I am afraid I don’t see how this effect can be termed retaliation. As this effect is key in the overall architecture of the paper, it definitively warrant more thinking – or some deep restructuring of the paper.”

RESPONSE: *The reviewer has a good point. Our use of “imperfect retaliation” was simply reflecting of Jacobs’ retaliation typology but we now use the term displaced violence for the term used in the model, which is one of the ways Jacobs argues “imperfect retaliation” might occur. Overall, we have reduced our discussion of other kinds of retaliation from Jacobs’ typology since many of them cannot clearly be modeled or distinguish in the REM. Instead, we focus on the difference between reciprocity and immediate retaliation (see pages 7-8).*

**MORE BACKGROUND/DETAILS ABOUT THE SPECIFICATION OF REM**

Editor: Reviewer 2 finds that the relational event modeling framework needs to be better explained given its novelty in the specific domain of applications. While in the context of a special issue on REMs is unnecessary to explain the model in great detail, the current draft errs in the opposite direction. Note that there is more than one way to specify relational event models. You need to be more explicit about what specification you adopt and what alternative you have considered. It is important, in other words, that you provide information on the model building process so that your paper may provide an exemplar application of REMs that criminologists will be able to replicate and emulate.

R1: “REM models should be described in more detail, including some formal description of the model used (formulas).”

R2: “The main concern I have is that given this is one of the first papers to introduce relational event models to gang violence, it would be useful to provide additional information about how these models were specified.”

RESPONSE: *We have added additional details about the specification of the REM and added the formula for the model on pages (pp. 13-14; pp. 16-18). We clarified that the specification of our model was guided by theory, and we added details in the interpretation of the results to clarify this. In cases where the literature was limited, we describe terms we considered including but ultimately excluded from the model based on fit (e.g., pp.25-26).*

**THE EFFECT OF TYPE OF CRIME**

Editor: The contextual effects of the type of crime (reviewer 2 comment 4) are obviously important and need further discussion – with specific reference to the broad range of criminal activity that is being investigated in the paper

R2: " The author(s) spend considerable time discussing the finding that “small gangs are victimized at a lower rate than larger gangs, but they attack other gangs at a rate similar to medium and very large gangs.” They offer various explanations for this finding, but I’d be curious about the extent to which these results may be related to the type of violence perpetrated. […] Could be that smaller gangs are more likely to perpetrate incidents, but those are less likely on the riskier side to build their violent reputation, whereas large gangs conduct fewer but more violent incidents? However, it is also worthwhile to consider that many of these crimes (e.g. robbery, firing a weapon) are less likely to be reflected in administrative records, as compared to gang homicides.” (R2)

RESPONSE: *We thank Reviewer 2 for pointing this out as we had not considered the potential interaction between size and type of crime. Generally, the range of criminal activity was somewhat misrepresented in the original manuscript. Most events result from serious violent crimes with nearly 94% of incidents involving shootings and/or homicides. The remainder events are assaults with other weapons and threats; there were in fact no robberies in the subset of incidents used in the model. We provided descriptive statistics on the types of crimes on page 15.*

*It is difficult to read much into the distinction in terms of motives between say, a shooting (assault with a weapon/shots fired) and a homicide. These are legalistic categories are assigned by officers not only given the characteristics of an incident, but importantly, on the basis of the strength of evidence available for conviction. So an assault with a firearm or attempted homicide might turn into a “shots fired” charge if the victim is unwilling to testify—an extremely common situation when gangs are involved.*

*We did however investigate some interactions between homicides and statistics indicative of retaliation, but did not find any evidence that homicides were more or less likely to lead to retaliation, perhaps because there are “only” 38 homicides in this dataset. We also examined whether there was an interaction between the seriousness of offenses and the size of the gangs and did not find any significant interactions.*

**RELEVANCE OF OLD DATA TO CURRENT SITUATION**

Editor: Reviewer 2 also finds that additional discussion is needed to place the results (obtained on data that are now old) in the current context of the City of Los Angeles. Much has probably happened since these data were collected and reviewer 2 offers references that you may use to update the interpretation of your results and make it more attuned to the current situation. Alternatively, you may decide to provide a more explicit context-dependent interpretation and then use the discussion section to speculate on how the results of the study may (or may not) help us to understand organized crime in the city of Los Angeles today. In either case, you need to find ways to link the results of the analysis to your understanding of the current (and perhaps future) situation in the city of Los Angeles.

R2: “it would also be useful to acknowledge changes in the gang landscape over time, and whether these findings would still hold today. For instance, recent work by Stuart (2019) and others (e.g. Patton et al. 2019) discuss the important role of social network platforms, including Twitter and Instagram for broadcasting public displays or threats of gang violence, and the role of these virtual platforms to invoke status, and in some instances, promote or mitigate real-world violence. “

RESPONSE: *We have included a discussion of these issues as part of the limitation section (p.29). We agree that the role of social media likely changes the visibility of violence and provides a novel performative platform that did not exist in the early 2000s. However, given the paucity of research on inter-group conflicts in gang research, we feel that our study is an important contribution regardless of the fact that the data is quite old. Our study perhaps establishes a “baseline” of what the dynamics of conflict used to like in the early 2000s, and may be limited to that era. Given that our results are consistent with findings from classic gang studies from the 1950s, 1960s, and 1990s, we think it is safe to assume that what drives inter-group conflicts is relatively independent from the time period. That being said, whether social media has really changed the fundamental nature of inter-gang conflicts is a fascinating empirical question. We provide future research with a model to replicate with more recent data, and a baseline against which to compare their findings.*

**TESTING THE PIECEWISE CONSTANT HAZARD ASSUMPTION**

Editor: Finally, reviewer 2 wants you to test the core assumption of piece wise constant hazard (pwch)that is crucial to your models. I suggest that you discuss alternative formulations and the impact that the assumption of pwch might have on the results you present. Also, additional discussion is needed on how this assumption is linked to contextual empirical features of your data.

R2: “The author(s)’ state that one assumption of REM is that “each potential event (i.e., gang violence) is conditionally independent and the potential event’s hazard, the propensity for a particular social action to transpire, remains constant while accounting for the prior history of events”. Is there a way to test this assumption, similar to event history models, such as interacting covariates with time? For instance, the author(s) could include a variable that counts time from the study start date, and then interact the count variable with covariates to capture whether trends are linearly increasing/decreasing.”

RESPONSE: *Indeed a key property of the relational event model is the assumption of piecewise constant hazards for all dyads that are at risk. This property follows directly from the fact that the waiting times between events follow an exponential distribution. In fact the waiting times are assumed to follow an exponential distribution where the rate parameter is equal to the sum of the rates for all dyads which differs across events. Thus, the waiting times can be normalized using the fitted model by multiplying every waiting time with the estimated rate parameter from the fitted model, which should approximately follow an exponential distribution with a rate parameter of 1. If this is the case, the assumed exponential distribution for the waiting times is reasonable and the piece-wise constant hazard assumption (a key central property of the exponential distribution) is not violated for these data. The histogram of these normalized waiting times looks as follows:*

**

*Even though we see that the slightly more very short normalized waiting times and slightly more longer normalized waiting times than expected under an exponential(1) distribution, overall the deviation seems fairly small. Furthermore because of the good predictive performance of the model based on the other results, we do not find enough evidence to expand the model further (also to keep the model as parsimonious as possible).*

*We added this figure as Figure 3 and added the description above on page 21. Ultimately, we opted not to include interactions of covariates with time (as suggested by reviewer 2) because (i) we don’t have a limited number of observations in our data and (ii) the predictive performance (see Figure 2) did not show a misfit at specific observational periods, which suggests that the model has a reasonable fit over the entire observational period.*

**REFERENCE TO GIBSON**

Editor: Please refer to the (apparently distant) paper by David Gibson on conversational sequences (“Taking turns and talking ties…” AJS, 2005) for an approach to the coding of conversational turns (Table 1 page 1566) of that is strikingly similar to your representation of “turn taking” in gang violence (table 1 of your paper).

RESPONSE: *We have added a discussion of p-shifts with a reference to Gibson on page 14. This was a glaring omission, thanks for pointing it out.*

**INTERNAL TIME STRUCTURE OF RETALIATION**

Editor: “Retaliation” – as the term is used in the study – defines some of the basic forms of dependence assumed to generate the observations. Different “times to retaliation” involve different forms of retaliation. Because retaliation – as defined in the paper – involves forms of reciprocity your work links directly to the argument developed by Bianchi, Stivala and Lomi in a paper published in Methodological Innovations (Multiple clocks in network evolution) arguing that “effects” in relational event models have an internal time structure that makes interpretation of statistical parameters associated with them difficult. In your case – retaliation is not just a configuration of events. Is a process that happens over time, and hence has an internal structure. Note the difference with (say) “public housing” an exogenous covariate (not a process) with no internal time structure. Clearly, interpretation of the associated effects differs. 

In a fundamental sense, the problem of distinguishing between “immediate” vs. “delayed” retaliation has to do with the effect that an event has on future events – and how fast memory of past events decays. A discussion of this point may be found in Amati, Lomi and Mascia (Social Networks, 2019). It may be useful to let readers know that assumptions are needed about “memory decay” to study the time-dependent impact of current events on future events. For an empirical example of how these assumptions may be developed please see the study on MOOCs by Vu, Pattison and Robins (Social Networks, 2015 – refer, more specifically, equation 15 on page 129).

RESPONSE: *We thank the editor for the suggestion. The work by Bianchi et al. (2022), but more specifically the paper by Kitts et al. (2017) they refer to, was instrumental in restructuring our literature review, moving away from the sometimes-confusing retaliation typology we used, and interpreting our findings.*

*Our model includes both an effect for immediate retaliation (via the p-shift AB-BA) and a delayed retaliation (via ‘recent reciprocity’) where the latter is operationalized using the reciprocal of the rank of the last attack of A towards B to determine the attack rate of B towards A. Thus, a specific form of memory decay is assumed via the reciprocal of the rank. Note that by including immediate retaliation in addition to delayed retaliation, we allow the possibility for the most recent attack of A to B to affect the attack rate of B to A even more than only via the rank of the last attack. Table 2 showed a significant effect of immediate retaliation in addition to delayed retaliation. In the paper we rewrote this as follows:*

*Our model also shows that gangs react to the violence that comes to them above and beyond the effect of institutionalized conflicts and their tendency to continue attacking the same groups. The effect of recent reciprocity shows that gangs are more likely to attack other gangs that recently attacked them (β = 0.715, p <0.01). Note that recent reciprocity can be seen as a form of delayed retaliation where the rank of the last attack of A to B affects the attack rate of B to A via the reciprocal of the rank (Table 1). Recent reciprocity shows a general tendency for gangs to reciprocate attacks at some point in the future, but our model also shows a tendency for gangs to retaliate immediately after a previous attack. The effect of immediate retaliation (β = 0.611, p <0.05) is operationalized using the p-shift AB-BA indicating that an attack from gang A on gang B is followed by gang B’s retaliation before any other attack occurs in the entire system. Absent more qualitative data on inter-gang conflicts, such a rapid response to an initial attack is the clearest evidence of retaliation motivated by a specific attack. […]*

*To answer RQ1 it is important to note that the model captures retaliation as a combination of recent reciprocity (0.715) and immediate retaliation (0.611). A consequence is that if gang A attacks gang B, the event rate of B attacking A next is multiplied with exp(0.715 + 0.611) = 3.76.*

*Ultimately, we opted not to explicitly model the influence of time or test different memory decay parameters, mostly because there is little theoretical guidance in gang scholarship about what we should expect. An option would be to assume a specific functional form of recent retaliation would be to estimate the decay function from the data (e.g., using Arena et al.’s (2022) method in “A Bayesian Semi-Parametric Approach for Modeling Memory Decay in Dynamic Social Networks”, Sociological Methods & Research). In order to accurately estimate such decay functions typically more events would be needed based on our experience unfortunately. We agree that an exploratory analysis of the memory decay in retaliation would be an interesting paper idea, which we acknowledge in the discussion where we cite Amati et al. (2019) and others to guide future work (p.32). Regarding the timing of retaliation, we do include some descriptive statistics about the typical “time to retaliation” on page 19.*

**INTERPRETATION OF RESULTS AND EMBEDDEDNESS OF EFFECTS**

Editor: Note that “imperfect retaliation” (which reviewer 1 thinks should not be considered “retaliation” at all) involves an open 2-path (as stated clearly in table 1). As such “imperfect retaliation” is a component (more specifically, an antecedent) of cyclical violence. These effects should be interpreted together.

RESPONSE: *Generally, we have revised our interpretation of coefficients to consider the embeddedness of effects.*

Editor: My second source of concern is the way in which some the results are presented.  I am now putting myself in the seat of readers who like your work so much that they want to replicate it – perhaps on a comparable data they have collected or will collect. To do so, readers would need to understand the link between the results reported and their contextual interpretation.

You offer an interpretation of the parameters that is “regression-like” in the sense that you refer indirectly to marginal effects. Yet, the way covariates are constructed in relational event models (and in statistical models for networks more generally) makes this interpretation problematic. “Ceteris paribus“ assumptions (which you invoke) are particularly implausible because “effects” are not just correlated; they are “embedded” in each other by construction. Each single event may be (and typically is) a component of multiple effects. Furthermore, many event sequences of interest are related by a “parent-descendant“ relation. For example, two-paths are antecedents of three-cycles and other kind of triadic structures. This makes the marginal interpretation of the effects difficult if not impossible. I am asking you to help readers to appreciate this source of complexity in interpreting relational event models. Conclusions such as “Holding all else constant, Gang A is 82.9% more likely to immediately follow an attack on Gang B with a second attack on a different Gang C” might be misleading. Please invite your readers to exercise the necessary caution in drawing conclusions form numerical estimates of parameters relational event models.

RESPONSE: *Indeed endogenous effects which are embedded and which can be highly collinear should not be interpreted in isolation. We have revised the entire results section to take into account the embeddedness of effects.*

*In addition, we also provided a correlation matrix between the predictor variables to facilitate the interpretation. Similar as with ordinary regression models, the interpretation of individual effects is easier when the corresponding predictor variable do not correlate with the other predictors. The predictors that are included in the model on average show little correlation which facilitates the interpretation of the impact of the effects on the event rate.*

*Generally, we have revised our regression-like interpretation of the parameters throughout the results. In addition, we have added the following text before the interpretation of the results:*

*“A researcher needs to interpret the network coefficients of a relational event model (and other statistical network models) with great care. Effects in a REM are often embedded in one or more other effects, making the “ceteris paribus” interpretation of traditional regression models problematic in some REMs. Dyads are embedded in triads, triads are embedded in four-cycles, et cetera, which makes it often infeasible to interpret one network effect independent of others. Hence, researchers need to exercise caution in drawing conclusions from numerical estimates of parameters in relational event models.*

*Fortunately, in our model, this “effect-embeddedness” is low. This can be seen by considering that the endogenous effects are only weakly correlated to one another. When inspecting the correlation matrix between the effects (Appendix 1), we can see that the only high correlation occurs between Inertia and Target recency: the preference for attacking recent targets increases with the tendency to keep targeting the same gangs. All other correlations are low enough that ceteris paribus interpretations of the effects appear safe” (pp.19-20)*

*And the following footnote:*

*“It is important to note that this is a particular feature of this model and these data and cannot be taken as a general result. Hence, we highly recommend inspecting the correlation matrix before interpreting the coefficients of a relational event model.” (p.20)*

**DISCUSSION OF GENERALIZED EXCHANGES AND INCLUSION OF TRANSITIVE CLOSURE**

Editor: Note, further, the clear connection between “imperfect retaliation” and “generalized exchange” or “indirect reciprocity” as discussed, for example, in Bearman (“Generalized exchange,” AJS, 1997), Molm, Collett, & Schaefer, (“Building solidarity…,” AJS, 2007) and, more recently, Lomi and Bianchi in the context of relational event models (“A time to give and a time to receive . .,” Social Networks, 2021). These studies link the parameter that in your study is associated to “imperfect retaliation” to role structures – a sociological notion that is missing from your study but that I suspect would be important to understand - and perhaps predict gang violence in future studies.

RESPONSE: *Again, we thank the editor for pointing out the relevance of generalized exchanges. We have reframed the discussion around “imperfect retaliation” using the more general concept of generalized exchanges (e.g., pp.10-11).*

EDITOR: Finally, the two-path event sequence that provides the structural antecedent to  “imperfect retaliation,” also provide the antecedent of “transitive retaliation” which – surprisingly - is not included in the empirical model specification. I would be curious to know why such an obvious hierarchical component in patterns of gang violence was excluded from the empirical model specification.   
(You may find it useful to refer to the apparently distant work by Ivan Chase and coauthors for additional discussion of the network structures linking acts of aggression to the emergence of dominance hierarchies (e.g.,: Chase, (1980) Social process and hierarchy formation in small groups: a comparative perspective. Am.Soc.Rev; Chase, ID, Bartolomeo C, Dugatkin LA (1994) Aggressive interactions and inter-contest interval: how long do winners keep winning? Anim. Behav 48(2): 393–400). For a more recent piece of work that you may find relevant please see: Silk, M. J., Cant, M. A., Cafazzo, S., Natoli, E., & McDonald, R. A. (2019). Elevated aggression is associated with uncertainty in a network of dog dominance interactions. Proceedings of the Royal Society B, 286(1906), 20190536).

RESPONSE: *The editor was correct in suggesting that we should include transitive closure as part of the model. The effect of cyclic closure actually goes away when introducing transitive closure. We have made it more explicit in our paper and with our research question that we can used cyclic and transitive closure to test for the presence of a status hierarchy in gang violence. This question is now a more prominent part of the paper.*

**CLARITY/USEFULNESS OF FIGURES**

Editor: Figure 1 is not clear (see also reviewer 1 for a similar opinion)

R1: “(5) It would help to have a longer discussion of the structural properties/topology of the network. Incidentally, the network picture presented in Figure 1 is very difficult to read.”

RESPONSE: *We have modified figure 1 and include descriptive statistics about key properties of the network on pages 18 and 19.*

Editor: Figure 2, but it needs additional explanation to be useful.

RESPONSE: *We have added additional sentences describing Figure 2 (now figure 4) on page 22.*

Editor: I find figure 4 counterintuitive. The text (page 24-25) reads “ . . . We computed the relative rank of each observed dyadic attack by ranking all dyads from most plausible (relative rank of 1) to least plausible (relative rank of 1/1056=0.001, as there are 1,056 dyads at risk) according to the fitted model)”.

What I understand from this text is that the inverse rank of the first event is 1/1 (=1) the second is 1/2 (=0.5) the third 1/3 and so on all the way to 1/1056. So how could it be that “The figure shows that almost all relative ranks are very close to 1.” Obviously, I am missing something very fundamental in your argument and I would be grateful if you could help me to identify the source of my misunderstanding. 

RESPONSE: *We included the following text to clarify figure 4 (now Figure 2):*

*To assess the goodness of fit of our model we checked how well it was able to predict the observed gang attacks. We calculated the estimated rate for each dyad, we ordered those from low to high and determined the percentile (from 0 - 1) for each dyad. The least likely dyad has percentile 0 meaning that a conflict between these gangs would be surprising at this point in time. The most likely dyad has percentile 1 meaning that a conflict between these gangs is expected at that particular point in time. For each event, Figure 4 shows the percentiles of realized dyads. The figure shows that almost all realized dyads are relatively well predicted by our model (e.g., scores very close to 1) with the median score being 0.980. Realized dyads tend to be among the top 2% of the potential dyads (for that event) as predicted by our model. Considering that there are 1056 potential dyads (33 x 32) that can occur at each event, this illustrates a very good model fit.*

*Further, the figure also shows that the model’s predictive accuracy is constant over time, without any periods where the model underperforms. This implies that the violence dynamics between gangs is constant over time and that the model can be applied to the entire time range as is. (pp.20-21)*

R1: “(6) The discussion on Clover, Highland Park and Avenues on pages 20-21 is not very clear, I am afraid. It would need to be reworded. Similarly, the discussion on page 23 could be made clearer.”

RESPONSE: *This discussion has been removed, as well as Figure 3, in the current version of the manuscript.*

**HETEROGENEITY IN GANG VIOLENCE**

R2: “Given heterogeneity in gang violence, it would be useful to calculate sender-clustered standard errors to adjust variance estimates for differences in gangs’ activity rates. Figure 3 suggests that some gangs are much more likely to send conflict ties than others.” (R2)

RESPONSE*: Indeed there is heterogeneity in the number of attacks that are initiated by the gangs. The proposed model however does not directly estimate the tendency of each actors to become a sender (i.e., to initiate an attack) but instead aims to estimate (dyadic) network effects and exogenous effects on a global level over the entire network. These estimates are able to capture the heterogeneity of the gangs’ activity, for example, using predictors such as gang size and the known rivalry between gangs.*

*To see this in more detail, the fitted model can be used to determine which gangs are likely to initiate an attack using the estimated coefficients which sheds light in the capability of the model to capture the heterogeneity of the gangs’ activity. To illustrate this, we replicated 10,000 datasets using the fitted model and we calculated how often each gang initiated an attack in the replicated data. This resulted in the following boxplots (where the 33 actors/gangs in the data are indexed on the x-axis):*

*A picture containing scatter chart

Description automatically generated*

*The figure shows the median frequency of each actor to become a sender (i.e., of each gang to initiate an attack) and the corresponding uncertainty (depicted using boxplots) using the fitted model, as well as the frequencies of actors to become a sender in the observed data (red crosses). Here we see that the heterogeneity of the activity of actors is well captured by the model.*

*We ultimately opted to exclude the figure from the paper, but we added the following sentence on page 21: “The model’s predictive accuracy also extends to the rate of violence of individual gangs. Despite some heterogeneity in the number of attacks gangs are involved in, simulations using the fitted model generally replicated the observed rate of violence for gangs regardless of their level of activity.”*

**LEFT TRUNCATION ISSUES**

R2: I also invite the author(s) to consider methods to overcome issues of left truncation in the data. Gang conflicts prior to 2000 are unobserved and are likely relevant for the observed network structure for the 2000 to 2002 time period. For instance, the authors could look to include dyad-level fixed effects in their models to help eliminate unobserved heterogeneity among gangs. (R2)

RESPONSE: *Left-truncation can certainly have an effect on the estimates of a relational event model. We believe that this effect is negligible in our model. First, there are very few effects in the model that build on a substantially large history. Most effects are either exogenous (e.g., rivalry, distance, public housing, etc.) or only require the recent history of interactions. The only effects that may indeed be affected are the recency effects and incoming two-paths (and inertia). However, the consistency with which the model tends to predict the correct dyads (Figure 4) suggests that the model does not suffer from this. Further, important effects such as rivalries capture a stable and long-lasting social structure that predate any events that were not observed before the data started. This, then, is also the case for the Bonacich Power centralities that are calculated on the rivalry network.*

**OTHER ISSUES**  
Editor: It is not clear whether the variables were standardized. Occasionally the interpretation you provide seems to imply standardization, but I could not find any explicit reference to it in the text (apologies if I have missed it). The numerical results also might suggest that standardization has been performed. Please be explicit on this point because it changes the interpretation of the estimates.

RESPONSE: *We understand this confusion. To address this, we explicitly mention which statistics are standardized in the model (Table 2). As can be seen, in this iteration of the model, only cyclic and transitive closure variables and the distance variable have been standardized.*

R1: I think the paper can be shortened considerably. Some of the points made in the ‘discussion section’ are also present elsewhere in the text. Overall, the discussion section can be shortened and made more to the point. There are mistakes and typos here and there, e.g., < Table 2 > on page 15 should read < Table 1 >.” (R1)

R2: The paper could use a final proofread there are some typos in the manuscript (e.g., “discrete even performed” (p. 11); “gangs that whose territory” (p. 12); “number of connections of node has weighted by the degree centralities their alters” (p. 13).  (R2)

RESPONSE: *We have checked the manuscript for typos and errors. Regarding the length, we have made a concerted effort to streamline both the literature review and the discussion section, but given the substantial suggestions and requested additions, the paper is in fact slightly longer than the original manuscript. We have completely re-written the discussion to avoid some repetition.*

R2: It would be useful to also present Bonacich power as one of the model parameters in Table 1. (R2)

RESPONSE: *We opted not to follow Reviewer 2’s suggestions because Bonacich is a node-level covariate. We did not want readers to interpret our Bonacich measure as being captured through evolving conflicts like sequential structural signatures.*

R2: It would also be helpful to present the covariate for territorial distance prior to public housing, as the public housing measure discusses overlapping territories. (R2)

*RESPONSE: We have modified our description of these covariates (p.15) to group these measures together.*