Thank you for submitting your paper entitled "Exploring the Contagion of Gang Violence Using Relational Events Modeling." (Manuscript ID 4) to the special issue of Network Science on relational event models. I offer my personal apologies for the long delay in the reviewing process caused by difficulties posed by a particularly challenging period for members of academic institutions, and for academic journals.   
  
We sent your paper to two expert reviewers with a demonstrated interest in criminology.  They have now completed their reviews. In addition, as one of the special issue editors, I have been asked to read your paper focusing the attention on the modeling aspects of your work. In consequence, you will find that this document is a mixture of a review and an editorial report. All the manuscripts submitted for publication to the special issue will follow a similar editorial process.   
  
As you can see from the comments received, both reviewers reacted positively to some aspects of your work. I agree with the reviewers that there is a lot to like in your paper. However, reviewers also identified important opportunities for improving the manuscript. One the basis of the reviewers’ comments and my own reading, I am pleased to offer you the opportunity to revise and resubmit your work for further consideration at Network Science.   
  
Below, I highlight what I consider the most critical concerns expressed by reviewers – to which I add my own. Please note that the reviewers made a number of very useful comments. While I do not reference all those comments here, an effective revision entails careful response to every comment received.  Your revised submission will be evaluated in terms of how well it addresses the reviewers’ comments and my own concerns.  
  
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).  
  
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.  
  
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.  
  
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.  
  
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   
  
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.  
  
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.     
  
My own concerns cluster around two broad areas. The first involves the definition of various forms of retaliation – an issue also raised by reviewer 1.   
  
Retaliation deserves special attention not only because is a central aspect of your study – but also because it illustrates well the advantages or REMS over other available statistical models for networks. 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).  
  
“Retaliation” – as the term is used in the study – defines some of the basic forms of dependence assumed to generatethe 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).   
  
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. 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.   
  
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).  
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.  
  
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.  
  
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.        
  
In closing, I note that the figures are less clear that they probably could be, and do not do much to exemplify and support the concepts and ideas described in the text. In summary: Figure 1 is not clear (see also reviewer 1 for a similar opinion). I like Figure 2, but it needs additional explanation to be useful. 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.   
  
I hope that the comments contained in this editorial report will help the further development of your work and facilitate the revision of your paper for re-submission to Network Science. Thank you for the opportunity to read and review your work and for considering Network Science the as a potential publication outlet.   
  
To revise your manuscript, log into [https://urldefense.com/v3/\_\_https://mc.manuscriptcentral.com/nws\_\_](https://urldefense.com/v3/__https:/mc.manuscriptcentral.com/nws__);!!CzAuKJ42GuquVTTmVmPViYEvSg!NGX2sa6olxuiNMAmXMbmMXf\_QvK6lV1QhppRX3p147vsyVSX08XjYqqzAy24NkMGOsSW8QF-irG3DS0dYbMrwctWKw$  and enter your Author Center, where you will find your manuscript under "Manuscripts with Decisions."  Under "Actions," click on "Create a Revision."  Your manuscript number will be appended to denote a revision. You may also click this link to start your revision:  
  
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When submitting your revised manuscript, you will be able to respond to the comments made by the reviewer(s) in the space provided.  Please use this space to document any changes you make to the original manuscript.  In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s). Please also add the name of the Action Editor (“Action Editor: [name]”) to the cover page of your unblinded manuscript, under the author information.  
  
Because we are trying to facilitate timely publication of manuscripts submitted to the Network Science, your revised manuscript should be uploaded as soon as possible. We expect to receive your revision by 07-Oct-2022.  
  
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Once again, thank you for submitting your manuscript to the Network Science special issue on Relational Event Models. I look forward to receiving your revision.  
  
Sincerely,  
  
Prof. Alessandro Lomi  
Guest Editor, Network Science  
[alessandro.lomi@usi.ch](mailto:alessandro.lomi@usi.ch)  
  
  
----------------------------------------------------------------------------------------------------------  
                                           Reviewer(s)' Comments to Author:  
-----------------------------------------------------------------------------------------------------------  
  
  
------------------------------------------- Reviewer: 1 -----------------------------------------------------  
  
Comments to the Author  
This paper presents an interesting analysis of gang violence in a specific area of Los Angeles. It relies on a dataset that it has been, I guess, already utilized in previous studies on gangs and gang activities in the same area. The added value of the work is the use of a longitudinal relational event model, which is both welcome and potentially fruitful. While I am sympathetic with both the approach taken and the aims of the paper, I think the work is not quite yet ready for publication for the reasons that I elaborate on below.   
  
(1) 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.   
  
(2) 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.    
  
(3) As I see it, one of the key focal points of the paper is retaliation. On page 8, this is defined using Jacobs (2004) very broad definition of retaliation. Following this definition, we have ‘reflexive retaliation’, ‘deferred retaliation’, ‘calculated retaliation’ and ‘imperfect retaliation’ – with only the latter operating at the supra-dyadic level (p. 9). There are a few substantive issues here. Firstly, 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”? Secondly, “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.   
  
(4) REM models should be described in more detail, including some formal description of the model used (formulas). On page 12, the sentence “The theoretical foundation of REM….” is repeated.   
  
(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.   
  
(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.   
  
(7) 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 >.  
  
  
------------------------------------------- Reviewer: 2 -----------------------------------------------------  
  
Comments to the Author  
The paper explores the network dynamics of inter-gang conflicts using relational event modeling. Specifically, the paper extends prior cross-sectional research on this topic by 1) modeling inter-gang conflicts using longitudinal data, and 2) demonstrating other network processes important to explaining gang conflict – such as Bonacich power – while confirming the well-established finding that reciprocity also drives this structure. The study answers a critically important research question that is of broad interest and moves theoretical and empirical work in this area forward.  
  
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. Below, I outline some potential issues to consider.   
  
1-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.  
  
2-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.   
  
3-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.  
  
4-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. For instance, much of the gang literature focuses on fatal or non-fatal homicide, whereas here the focus is on a much wider range of gang violence (e.g. simple and aggravated assault, assault with a deadly weapon, homicide and attempted homicide, robbery, kidnapping, and firing a weapon into an inhabited dwelling or vehicle). 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.  
  
5-The data used in this study are approximately from two decades ago. Although the author(s) discuss the limits of the data and its generalizability to other US cities, 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.    
Minor comments  
  
6-It would be useful to also present Bonacich power as one of the model parameters in Table 1.  
  
7-It would also be helpful to present the covariate for territorial distance prior to public housing, as the public housing measure discusses overlapping territories.  
  
8-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).   
  
References  
  
Stuart, F. (2020). Ballad of the bullet: Gangs, drill music, and the power of online infamy. Princeton, NJ: Princeton University Press.  
  
Patton, D. U., Pyrooz, D., Decker, S., Frey, W. R., & Leonard, P. (2019). When Twitter fingers turn to trigger fingers: A qualitative study of social media-related gang violence. International Journal of Bullying Prevention, 1, 205-217.