Thank you for the opportunity to revise our manuscript for consideration in *American Journal of Epidemiology*. We found the comments to be incredibly helpful, and we have substantially revised our article accordingly. This revision has involved changes to the manuscript text as well as addition of new sensitivity analyses. Below, we list the Reviewers’ advice followed by a detailed description of what we did to address each comment (in italics).

Reviewer: 1

Comments to the Author

The authors use a difference-in-differences approach to assess the impact of the Great Recession on explicit and proxy violence diagnoses for child maltreatment, intimate partner violence, elder abuse, and their combination in county-level Minnesota hospital data. Unexposed counties were defined as those less affected by the recession based on county-level foreclosure rates. The authors found no impact of the Great Recession on explicitly-identified violence, but did find an impact on proxy-identified violence for all outcomes.

The use of hospital data is a strength of this study, providing a more objective measure of violence than in previous analyses that used official crime records or survey data. Using both proxy and explicit operationalizations of violence helps elucidate potential biases due to misclassification of injuries as unrelated to violence and is a helpful contribution to the literature. The sensitivity analyses are useful for addressing some of the concerns raised by the main analysis, in particular concerns about changes in county demographic distribution due to mobility. However, the lack of mortality data is a substantial weakness.

There are a few points where additional detail or clarification would aid in interpretability of the results, and some of the methodological choices need further explanation and justification.

1. Most importantly, it appears that injuries resulting in death are not included in these data. This creates a difficulty in interpretation, since more serious injuries (likely associated with worse violence) are more likely to be missing from the data. If possible, violence-related death data should be included. If such data are not available, an estimate of the magnitude of violence-related deaths relative to non-fatal violence-related injuries should be given in order to quantify the potential magnitude of the bias, and the authors should discuss the implications of the missing death data.

*Thank you for this insight. We note that the hospital data does include violence-related deaths if the death occurred in the hospital, which make up a large proportion death due to violence.1 It is estimated that approximately 1% of assaults result in a death,2 meaning our study is missing less than one percent of violence cases stemming from exclusion of non-hospital violence deaths. The small percentage of excluded cases makes it unlikely that the addition of these data would change the results.*

*We added a limitation in the discussion section to address this point:*

*“Sixth, this study does not include death-certificate data. Therefore, those that died due to violence outside the hospital were not included in the study. However, the proportion of those that die due to violence is approximately one percent, and, thus the addition of these data would likely not change the results of the study.”*

1. It’s unclear why the authors chose the median as the threshold for classifying counties as having high or low foreclosure rates. It would be helpful to show the results of an analysis excluding counties near the median value, since counties near the middle of the distribution may be similar enough that small random fluctuations in violence rates could have led to their classification in either group. Are the same patterns seen when comparing the top and bottom quartiles, or top and bottom 10% of counties?

*Thank you for this comment. In response to this concern, we ran two additional sensitivity analyses. The first sensitively analysis repeated the main analysis but excluded those counties that were between the 40th percentile to the 60th percentile of foreclosure rate change. The second sensitivity analysis reclassified the treatment and control counties to compare the foreclosure rate changes of the top tertile (treated) vs. the lower two tertiles (control). The results are added to the appendix of the paper. In addition, they are reported below for the reviewer, along with those from the original analysis for ease of comparison. Overall, these sensitivity analyses produced results very similar to those from the main analysis. Specifically, the results for explicit-identified violence in both sensitivity analyses were very similar to the original analysis. The results for proxy-identified violence were roughly similar across sensitivity and main analyses, with the most notable difference being an attenuation of* *the proxy-identified combined violence outcomes, although directionality is the same across all models. We investigated possible sources of the attenuation in the combined effect estimates, and noted a strong impact of the interaction term between treatment and time for elder abuse (indicating substantial differences in elder abuse time trends in treated versus control counties). We therefore ran further two sensitivity analyses removing this interaction term for the same two models above.*  *Overall, these addition sensitivity analyses produced results very similar to those from the main analysis. Specifically, the results for explicit-identified violence in both sensitivity analyses were very similar to the original analysis.* *The results for proxy-identified violence were roughly similar across each sensitivity and main analyses, with the most notable differences being a directionality change from proxy- identified elder abuse and attenuation of the proxy-identified combined violence outcomes likely caused by the directionality change in proxy-identified elder abuse. All of the sensitivity model estimates had wider confidence intervals than the main analyses, that crossed the null likely due to the reduction in sample size in one or both comparison groups.*



1. Foreclosure rate was calculated as the total number of foreclosures divided by the total number of residential parcels. This measure will be impacted by the proportion of the parcels that have mortgages, and a change in proportion of properties with mortgages may be associated with violence rate changes. It would be more informative to use a denominator of total number of mortgages if it is available.

*Thank you for this comment. The number of active mortgages is not readily available to be used as the denominator for foreclosure rate. Therefore, we added language to the limitation section about the use of the number of residential parcels as the denominator in our foreclosure rate calculation. We note that extant literature that has studied the association between foreclosure rates and different types of violence has used a similar denominator as our study: housing units.2-6 Thus, although we acknowledge that this denominator is not ideal, given lack of availability of active mortgage data, it seems to be in line with the body of literature.*

*“Seventh, the denominator for foreclosure rate, residential parcels, includes those who may not have a mortgage on their house. In spite of this limitation, there is existing literature supporting this type of population denominator to study the association between foreclosure rates and violence.14,86,87”*

1. It would be informative for interpreting the violence subtype results to include distributions of children and seniors who would be potential victims of child abuse or elder abuse in table 1. For example, are the proportions of children similar between treated and control counties? Likewise, it is not clear whether the violence subtype analyses used total county population as denominators, or if the county child or senior population was used as the denominator. The latter is the appropriate measure when the age distribution differs across counties. Similarly, the denominator of the rate of intimate partner violence should not include children.

*We appreciate this point and have added the percent of children and seniors in Table 1.*

*The violence subtypes analyses used county age-appropriate population denominators for each violence subgroup. We have clarified this in the methods section:*

*“The following denominators were used for each violence subtype: age 0 to 17 for child abuse, 65 plus for elder abuse, and 16 plus for intimate partner violence.”*

1. It is unclear why the authors used average violence rates every 2 years rather than using every year. The data include few pre- and post- data points, and the analysis method accounts for random error in the regression step. Please justify why using data every 2 years, and thus sacrificing data points in order to reduce noise, would lead to a better estimate than including each year of data in the model. The authors should either justify this choice with reference to the methodological literature, or provide a sensitivity analysis based on annual data.

*Thank you for your comment. The 2-year averaging is useful for reducing noise in the figures, and, in principle, the statistical analysis controls for noise even if we used annual data instead of 2-year averages. To address this concern, we have estimated a sensitivity analysis that re-scales the year variable for comparability across models. The results are similar across both models (biannual and annual) with one exception: some attenuation in the magnitude for the proxy-identified elder abuse annual trend model compared to the biannual trend model. Elder abuse results appear to be the most sensitive to model specifications, as noted in sensitivity analyses reported above.*



1. Factors that influence violence on a county level may be quite different from those on the individual level (e.g. emotional regulation, as mentioned by the authors). Since the unit of analysis is the county level, the authors’ discussion the association of economic hardship and rates of violence should focus on county level factors (e.g. policing policy, tax revenue) and county demographic distributions (e.g. age distribution, income inequality, unemployment rate), rather than person-level factors.

*We appreciate this point, and in order to focus on language on the county level factors we removed language about the individual factors in the introduction.*

1. There is very little detail describing the falsification test and its interpretation. As currently described it seems as if there is no post-period: the great recession was defined as starting in 2006 and data from 2007 onward were excluded. Please clarify.

*Thank you for this comment. We have added additional detail on the falsification test in the methods and results sections.*

*Methods:*

*“Fourth, a falsification test was run using data from 2004-2006. Specifically, the main analysis was repeated but with shifting the beginning of the simulated Great Recession to occur in 2006 and excluding 2007 and beyond from this analysis to remove the actual Great Recession timeframe from this test.”*

*Results:*

*“Finally, the falsification sensitivity analysis found that the association between the impact of the simulated Great Recession and explicit- and proxy- identified violence were close to the null, as expected.* *One exception was of proxy-identified elder abuse, which indicated a positive effect of a 2006 event on rates, although, the magnitude of association was muted in comparison to the main analysis.*

1. The final conclusions are misstated: the analysis supports the claim that the recession, as measured by foreclosure rates (or an event occurring contemporaneously with the recession, and differentially affecting county foreclosure rates), possibly led to differential county violence rates for some outcomes under the assumptions discussed. However, foreclosures were used as a measure of the recession, not the exposure themselves; the analysis was not designed to address whether foreclosures led to changes in violence rates.

*We have revised the conclusion to incorporate this point.*

*“In conclusion, this study suggests that the Great Recession, measured by foreclosure rates, led to an increase in certain types of violence.”*

1. “IVP” is used as the acronym for interpersonal violence, please change to "IPV" to be consistent with other literature.

*Thank you for catching this typo. It has been corrected.*

Reviewer: 2

Comments to the Author

1. Thank you for the opportunity to review the manuscript titled "". I found the paper to be well-done, particularly wit respect to the analytic design. My key feedback for the authors pertains to inclusion of more theory of frustration-aggression-displacement hypothesis in the Introduction and how that may relate to escalated violence during economic uncertainty. If the authors could include a more detailed description of this theoretical pathway, I believe it would make the paper stronger.

*Thank you for this suggestion. We have added language to the introduction to expand on the theoretical pathway.*

*“On the macro level, there are multiple pathways by which the Recession measured by foreclosures could impact health. For example economic spill-overs of the Recession such as a drop in local property value, physical environmental impacts such as neglected properties, or social environmental impacts such as residential turnover can impact stress and create frustration-aggression.7,8 More specifically, the economic spill-overs may create structurally disadvantaged neighborhoods that inhibit relationships via high population turnover and thus lead to low levels of informal social control and high levels of crime. Further the role of future economic uncertainty leads to strain and conflict such as increased aggression. Understanding the social links that drive violence is useful for informing violence prevention programs and policy.”*

1. On a related but slightly different note, the authors allude to the role of uncertainty being the main underlying driver for violent and stressful reactions during recessions. If this point could also be brought up in the Introduction, with inhibition effects (i.e. people being on their best behavior during uncertain times), a reader may better understand the context of how populations may or may not react violently during economic uncertainty.

*Thank you for this suggestion. We have added language in the introduction to expand on how populations may respond to economic pressure, as indicated in the response above.*

Other than these two theoretical inclusions as minor revisions, I do not have other comments for the paper.

References

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