Revision memo

Manuscript 'Violence, co-optation, and postwar voting in Guatemala', submitted to Conflict Management and Peace Science (CMPS-21-0042)

June 30, 2021

Response to Editor

> I believe the literature connecting civil war violence to post-war democratic variables is some of the most exciting research done in political science at present - and some of the most important, in terms of the settings and lives it touches. The reviewers in this case seem to concur, in the sense of seeing significant promise in your work. However, the also see a number of issues, best summed up by R2's assessment:"this article had some intriguing empirical findings that could have real value to the literature on the legacy of conflict and violence. As currently constructed, it is however marred by two issues: (1) some substantial slippage between its theory and empirics, and (2) a related lack of qualitative (or quantitative) evidence to nail down the mechanisms at work here." R1 puts this as lack of accounting for alternative explanations - and R3 requests a number of clarifications. The good news is that all three reviewers point to concrete steps you can take to significantly mitigate these concerns. I am, therefore, granting you an opportunity to revise your work and hope you do so in line with the reviewers' suggestions and recommendations.

I would like to thank you for the opportunity to revise the manuscript. As you will see, I have made thorough revisions, seriously considering each individual point raised by the reviewers.

blah blah blah

I hope you will find the manuscript to have significantly improved through this process. Below I address each of the reviewers' comments in turn.

Reviewer 1

CHANGE We would like to thank the reviewer for all the comments. They were all very thoughtful and we found them very helpful in improving the manuscript. We respond to each of them below.

Comment 1:

> My biggest hesitation is the author's use of a proxy in measuring exposure to prewar political mobilization. I completely understand the nature of data limitations and the decision to pursue this route. I'm sure that the author(s) understands that given this decision, much scrutiny will be placed upon the choice of a proxy.

Response: pending

broader framing, better accounting for alt exps, new section delving into the mechanism

Comment 2:

> Currently, I see two alternative, competing theories for your empirical relationship. The first is that the government, given better road-access, is better able to carry out violence in these areas. Thus, we would anticipate greater changes in political preferences following these state-killings. I believe you successfully account for this alternative explanation in Appendix Tables A2 and A3. Although, I do worry about the potential inflation of standard errors due to multicollinearity — not only between your two proxy variables, but also with the other variables, like forest cover, elevation, and distance to capital. I would recommend simpler analyses that isolate these proxy variables. I also question the dependent variables' form, as its not specified explicitly. Is it still log- transformed? Is it still normalized to population?

Response: pending

simpler analyses without control variables specify dependent variable in these tables (a2/a3)

Comment 3:

> The second alternative explanation that I feel is not accounted for is that what is potentially being measured by these proxies is actually insularity from national-level politics, which explains why state killings in these less accessible, more remote areas have a muted effect on political preferences. I would assume these are rural communities with lesser access to information — such as radios, TVs, and

internet-connected devices. I also acknowledge that the author(s) present qualitative evidence that the government was very much involved in propaganda efforts (on Pages 10-11). But I wonder if the author(s) can account for this alternative explanation in their quantitate analysis. I'm not sure how the author(s) could remedy this concern with their existing data. Perhaps if the authors could account for voter-turnout, or if possible, income levels or technology penetration, I would be fully convinced of their theory.

Response: pending

discuss in theory (or somewhere) the alternative explanation of insularity from national politics (thatr it is really not at odds)

models with turnout, include income as a control variable

tv/radio survey? mechanisms section and discussion of propaganda

Comment 4:

Besides accounting for alternative explanations, I believe the author(s) needs to connect their theory more with their choice of a proxy. For example, the author(s) states on Page 18:

> "In particular, I assume that accessibility in terms of road infrastructure determined how much exposure local communities had to these external political actors, who expanded throughout the country from the capital and main cities to bring new political ideas and organize the local population."

It is critical that the author(s) substantiates this assumption. As of now, in the "Historical Context" section on Page 8 and "The role of prewar mobilization" subsection on Pages 11-12, the spatial origins of these opposition groups are not exactly clear; did they originate in lesser developed, more remote regions or did they spread outwards from the more developed cities and communities connected to the Pan-American Highway? From my perspective, the latter argument is necessary in explaining your theory. However, it appears that the Catholic Action movement originated in the cities and expanded outwards, while the peasant organizations among the indigenous populations were already established beforehand (note: I don't have any more background on the Guatemalan conflict than what is presented here).

Response: pending

mechanisms section discussing the assumptions of roads

Comment 5:

Finally, given that your theory suggests that the government co-opted civilian populations through propaganda, it's strange — and somewhat contradictory — that you follow these discussions with this statement on Page 11:

> "Matanock Garcia-Sanchez (2018) show that civilians falsify their reported support for the military when asked about the counterinsurgency in Colombia, particularly in areas previously held by the insurgents. Preference falsification helps to explain the apparent success of counterinsurgent campaigns, as fear is a major factor explaining the negative effect of repression on opposition activities (Young, 2019)."

This suggests co-optation is through fear of repression, not altering public opinion to generate genuine support for the government. It also makes it more unclear how this fear translates to votes, which I presume would not be motivating factor in one's vote during the democratic elections (at least, without greater context). I recommend omission of this part.

Response: pending

thank you, clarify text that it was a bit convoluted, removed this paragraph

Comment 6:

In my view, the largest obstacle to recommending publication is accounting for the alternative explanation of the proxy measures capturing insularity and not the opposition's political mobilization. I also feel it's important for the author(s) to contribute more to substantiating their assumptions regarding their proxy variables (i.e., accessibility = prewar political mobilization). My other recommendations I feel are minor and not fatal to the manuscripts advancement. Otherwise, I feel this manuscript makes a worthy contribution to our knowledge of post-war political attitudes. For these reasons, I recommend a Major Revision.

Response: pending

thank you blah blah, points to a major flaw of the previous version which i've tried to remedy

major rewriting of the theory section accounting for that alt exp (which might not be so much at odds) mechanisms section using qual evidence to highlight all the steps in the mechanism (incl assumptino of roads)

Comment 7:

Minor Points:

- I would prefer the control variables to be shown in the main tables. They are not even provided in the appendix — which is suspicious!

4

Response: pending

show control variables in tables in appendix

Comment 8:

- For the presentation of predicted probabilities, all figures will require the model used to be specified. Further, given the presence of fixed-effects for departments and elections, I don't believe its possible to state "All other variables are kept at their mean," as written on Page 22. Which election year and department were chosen for deriving predicted probabilities? From your appendix results, its quite clear that the hypothesized relationship weakens over time.

Response: pending

specify model choice and categorical variables in pred prob plots

Comment 9:

- A recent article in JCR discusses how road-access is predictive of conflict. The author(s) may find this insightful to their choice of proxy:

  "Roads to Rule, Roads to Rebel: Relational State Capacity and Conflict in Africa." Carl Muller-Crepon, Philipp Hunziker, and Lars-Erik Cederman. Journal of Conflict Resolution (2021).

Response: pending

mention somewhere? perhaps discuss differences (and maybe link it to discussion on state building etc)

Reviewer 2

CHANGE We would like to thank the reviewer for all the comments. They were all very thoughtful and we found them very helpful in improving the manuscript. We respond to each of them below.

Comment 1:

> Overall, I thought this article had some intriguing empirical findings that could have real value to the literature on the legacy of conflict and violence. As currently constructed, it is however marred by two issues: (1) some substantial slippage between its theory and empirics, and (2) a related lack of qualitative (or quantitative) evidence to nail down the mechanisms at work here. While these issues are not trivial, I think they can perhaps be addressed or at least significantly mitigated and so I think an RR opportunity at CMPS would be possible.

Response: pending

thank you blah blah, summary of changes: rewriting of the theoretical framework, new section discussing qual evidence for the mechanisms

Comment 2:

> 1) Theory vs. empirics: in a nutshell, the authors argue that violent events can be interpreted in different ways by different groups of people based on their ability to resist combatant propaganda. In particular, they argue that combatants (here, primarily the state) try to manipulate perceptions of the harm they inflict by denying it or blaming it on their opponents, and are often successful in doing so. However, these efforts can be resisted by those with sufficient "ideological capital" that makes them skeptical of the manipulation.

> While this is an interesting theory, it isn't really well tested by the analysis. The results indicate that the impact of violence on voting depends on prewar leftist mobilization (proxied by roads). There is no measurement of state propaganda about the conflict and people's belief in it, so it is hard to know whether that is really what's driving the observed effects. In contrast, it could just be an ideological story in which those with a more leftist worldview judge the state's intentions as more hostile and punish it more for harm inflicted (a la Lyall, Blair, and Imai 2013). In other words, it's hard to know whether this is due to propaganda and its spread at all.

> So where does this leave us? Well, if the authors can test the state propaganda mechanism more directly by looking at, say, survey evidence from the conflict setting, that would be one thing – but, if not, a broader framing that does not

lean as much on this one specific mechanism would help. The authors should reframe the argument so that it is just suggesting that ideology shapes the effect of violence on postwar political preferences, building on studies like Lyall, Blair, and Imai 2013 and Silverman 2019 but extending them into the arena of longer-run dynamics and postwar voting behavior. Ideologically-driven resistance to combatant propaganda could be one potential mechanism behind the results, but not the only one – and it shouldn't be such a central or essential part of the story since it can't really be directly demonstrated.

Response: pending

  - mentioned mechanism section (what's included re propaganda, not much, right?) AND
- broder framin of the theoretical argument, acknowledge that maybe more than one mechanism was in play ¿¿ Lyall et all, Silverman, etc

Comment 3:

2) Mechanisms: relatedly, it's hard to pin down the mechanism here more broadly, because there isn't much direct evidence about many of the links in the rich causal story that is told – prewar social mobilization (proxied by roads, as noted in the piece), leftist ideological penetration (besides the voting behavior DV), and the state propaganda dimension (as discussed above). There is really a desperate need for rich qualitative evidence here. I think an "identifying the mechanisms" section after the main results would help. Can it be qualitatively shown or at least strongly suggested that liberal priests and activists spread via the road network? Can it be shown that the areas they reached then became sites of leftist agitation? Is there evidence of political activism after the war in these areas to commemorate the violence, define it ideologically, etc.? If there is any available survey evidence from one of the Latin American regional survey projects that could speak to the causal links in these chains (conflict attitudes would be ideal of course, but even leftist ideology, distrust of state media, etc. would be helpful).

Response: pending

  - explain mechanism section

Comment 4:

Other issues:

  - economic development: can you control for economic development across different municipalities? It is possible that with your measure of unpaved roads you're picking up something like this. It could be that poorer people (and/or poorer areas) are more vulnerable to state coercion in this case, as has been shown elsewhere for example in the electoral violence literature.

Response: pending

acknowledge blah blah thank you

now include illiteracy, dont have data on income, but should be good proxy

Comment 5:

- placebo test: can you get data on the leftist vote across areas from the prior democratic period of 1944-54? Since you're trying to measure the effect of the liberalization through the road network that occurred in the 1960s-70s, controlling for this shouldn't impact your results and would show that they weren't due to the areas near the roads having already been more leftist in outlook.

Response: pending

good points but unfortunately don't have data

Comment 6:

- other parties: what about voting for other parties in the 1999-2015 elections? I noticed that the 2 parties in question weren't very popular, especially after 1999. So you're really looking at a very small slice of the vote and trying to predict it. Can the other parties not be sorted ideologically in a way that would allow them to be included in the analysis? And relatedly, if leftist areas were so effective at creating collective memories of victimization which led them to support URNG, how come this support dissipated so rapidly after the first election? This should be at least addressed somewhere in the piece.

Response: pending

new analyses in the appendix including this

Comment 7:

- time decay: related to the last point, we can also see in the Appendix that the effects are pretty robust but in many cases (e.g. Figure A3) seem to fade significantly with time. Is this evidence that supports the argument, since it shows that they are strongest where we'd expect – right after the war? And later on voting happens more for other reasons? Or does it get at the weakness and limited duration of the results? Again, this should at least be engaged with somewhere, possibly in the conclusion.

Response: pending

discuss time decay in the conclusion

Reviewer 3

CHANGE We would like to thank the reviewer for all the comments. They were all very thoughtful and we found them very helpful in improving the manuscript. We respond to each of them below.

Comment 1:

> Below are a few things for the author to consider: What about corruption? This is not mentioned once in the article. Recent events, including the expulsion of CICIG by the Jimmy Morales administration, demonstrate this troubling trend. I would argue that it is important to understand the notion of state fragility that has plagued Guatemala over decades, including the period analyzed in this article. Adriana Beltrán and other scholars have written about corruption and impunity. Some surveys (e.g., LAPOP) ask about perceptions of corruption and bribes from different government actors.

Response: pending
mention corruption and new references

Comment 2:

> While this is not the focus of this article, it is worth mentioning, even if in a footnote, that Guatemala has an intricate relationship between the state, gangs (MS-13 and the 18th Street), and organized crime (see the work of José Miguel Cruz). Scholars like Steven Dudley have referred to Guatemala as a mafia state. There are zones in Guatemala ("red zones") that are controlled by street gangs.

Response: pending
mention organized crime and new references

Comment 3:

> One may question whether people vote for parties or leaders. In Central America, politicians have formed their own political parties (e.g., Bukele and the New Ideas party). There have been dozens of parties in Guatemala during the political election cycle. Today, for instance, there are 28 parties registered in Guatemala.

Response: pending
similar as R2, new analyses in appendix including more parties within groups

Comment 4:

This article does not mention the issue of the police once. Does police corruption and ungoverned spaces impact your analysis? Perhaps this is worth mentioning, even if in a footnote.

Response: pending

mention police corruption and new references

Comment 5:

The author uses pooled OLS regressions. A reader might want to know if the author tested for heteroskedasticity or multicollinearity using the Variance Inflation Factor (VIF).

Response: pending

related to comment by R1, new tests including only the relevant variables etc (VIF?)

Comment 6:

In your pooled OLS model, a reader may wonder why the author did not include several variables (e.g., income, education, corruption measures, and trust in institutions like the military or police). This could be something worth addressing, even if in a footnote.

Response: pending

including now illiteracy could not locate other variables at the local level that go so far back in time

Comment 7:

I would also recommend citing/ reviewing the work of Deborah T. Levenson, Anthony W. Fontes, Adriana Beltrán, Christine Wade, and José Miguel Cruz.

Response: pending

new references added