

Revision memo

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

July 14, 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, they 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

First, I would like to thank the reviewer for all the comments. They were very thoughtful and useful in improving the manuscript. I respond to each one 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: Thank you for this comment, which does indeed point at a crucial limitation of the manuscript raised by the other reviewers as well. I discuss the changes to each specific comment more in detail below, but in general terms the changes I have implemented with regards to this comment can be found in the theory and in a new section discussing the mechanism. First, I have changed the theory section, accounting for an alternative interpretation of the results that does not rely on the importance of state propaganda and discussing the alternative explanation based on insularity from national politics. Second, I have added a new section after the results ('Identifying the mechanism') where I present qualitative evidence supporting the road accessibility assumption and each of the steps of the mechanism that are not directly tested in the analyses.

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 it's not specified explicitly. Is it still log-transformed? Is it still normalized to population?

Response: I agree with the reviewer that these issues might be of concern when trying to rule out this alternative explanation, which is indeed crucial for the validity of the results. Following the suggestion above, I now include in the corresponding section (Appendix C) models without all the control variables, so the effects of the two proxies can be compared

across the different specifications. Because of the increase of models, I include four tables for the results of state violence for the whole sample and the reduced sample of most affected departments (tables [A3](#) and [A4](#)), and the same for rebel violence (tables [A5](#) and [A6](#)).

Results remain the same. Although the simplest model—without controls and without department fixed effect, in the whole sample—does show a positive relationship between the share of non-paved roads and state violence, this effect disappears when department fixed effects are included (even without including any control variable). It also disappears when the model is run in a sample of only the most affected departments.

The first result is not surprising given that wartime activity was concentrated in the western highlands, where road network and terrain ruggedness are worse. Yet, once we compare only within departments, the relationship disappears. Given that all the main analyses include department fixed effects and that I also test whether the results are also present in the reduced sample of the most affected departments, it should not be a concern for the interpretation of the results.

Finally, as per the reviewer's suggestion, I have specified the dependent variables' form in the same [Appendix C](#): as in the main analyses, state and rebel violence are also log-transformed and normalized to population.

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 quantitative 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: I thank the reviewer for this insightful comment, which I think hints at a very important point. I do agree that insularity from national-level politics as an alternative explanation was not accounted for in the previous version of the manuscript, particularly since not much evidence was provided in support of the role of propaganda. I have made a few changes to better account for this.

First, I include in the section presenting the proxy variables a discussion of how these two variables could also be interpreted as insularity from national politics, instead of a stronger

exposure to state propaganda (in page 17). What I try to argue is that even if I emphasize the role of state propagando, I do not think that this explanation is so much at odds with the current theory, as it was precisely this insularity from national politics which made these communities more vulnerable to state propaganda.

Second, in the new section discussing the qualitative evidence for the mechanism ('Identifying the mechanism'), I present evidence from secondary sources on the role of state propaganda in modifying collective memories of the conflict and how it was precisely more isolated communities the ones that were more vulnerable to these efforts and the ones that did not engage in postwar commemoration activities (pages 26–27).

Finally, following this and other reviewer's suggestions, I have made some changes to the empirical analyses. On the one hand, I now include turnout in the cross-sectional analyses using election-specific samples (Appendix G). I do not include this control variable in the main analyses because of the high number of missing observations for some election years (particularly in 2007 and 2003). However, tables A14 to A19 show that results do not change when including this control variable. On the other hand, I do include now the rate of illiteracy in the main models in the main text which, although it is not directly related to political participation, might be the best proxy available for political isolation.

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: I thank the reviewer for this comment, which is related to other reviewers' comments. I agree that providing more evidence for this assumption is absolutely necessary.

I have included qualitative evidence supporting this relationship in the mechanism section, in pages 24–25. Among other things, I refer to a study by [Esparza \(2018\)](#) of one area in Chupol, in the department of Chichicastenango, where she says that it was precisely in communities close to the Pan-American Highway where the Liberation Theology priests went to more often. I also present evidence that in more isolated communities this process did not take place, at least with the same intensity. Regarding the peasant organizations, the main activists also originated from the main cities, and local organizations usually emerged after external actors arrived and bring the initiative, including Catholic Action and foreign priests.

References

Esparza, Marcia (2018) *Silenced communities: Legacies of militarization and militarism in a rural Guatemalan town*. New York: Berghahn Books.