

## **Revision memo**

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

July 15, 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.

#### **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:** I thank the reviewer for this comment. Even though fear was indeed part of the mechanism through which the Guatemalan state managed to get local cooperation—as some of the qualitative evidence testifies to, for example—, I agree that this discussion of fear contradicts part of what is being discussed in the section.

I have now removed this paragraph, and include instead at the end of the theoretical section a more lengthy discussion of an alternative mechanism based on an ideologically motivated reaction to state violence.

#### **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:** I thank the reviewer for this and the other comments, which have helped me greatly in improving the manuscript. As explained above, I have tried to address these comments by (a) rewriting the discussion of the variables, and the theoretical framework to a lesser extent, to account for a potential explanation based on isolation from national politics and (b) including a new section at the end of the manuscript presenting qualitative evidence in support of both the choice of the proxies and each step in the mechanism.

I hope these changes properly address these major points and make the manuscript more coherent.

#### **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!

**Response:** I agree with the reviewer that the full tables should be available. I have now placed them in Appendix D, and included a footnote in the main text (page 18) pointing to this section.

#### **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:** I have now specified the model and the specific values for the fixed effect variables in each of the figure captions. In particular, every predicted probability plot was calculated for the Quiché department and 1999 elections. The reviewer is right in saying that

the relationship weakens over time (a point also raised by R2), so I have now included a paragraph discussing this finding at the end of the results section (pages 23–24).

**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:** I thank the reviewer for this reference, which is a very good example of a methodological approach to calculate state capacity from road maps. I was aware of this article, and the reason that I did not take a similar approach in this manuscript is that the goal is slightly different. [Müller-Crepon, Hunziker & Cederman \(2021\)](#) try to develop a measure of relational state capacity, which incorporates both the capacity of the central state authorities and the internal connectedness of an ethnic group. In my case, I am only concerned with how easy was to move around a municipality, but not so much with the capacity of the central state to reach these areas (in that case, the problem of the road proxy measuring insularity from national politics would be much worse, in my opinion). Moreover, calculating shortest paths in a time-weighted road network has an extremely high computational cost, as it involves calculating every existing path between two nodes.

## 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:** I thank the reviewer for this comment. **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:** I thank the reviewer for this insightful comment, which is also related to comment 6 by R2. I agree that the analyses would benefit if more parties were included in the calculation of the dependent variable since, as the reviewer affirms, partisan voting might not be as important in Guatemala.



In order to address these issue, I now include additional analyses in Appendix F including more parties, both leftist and rightist, to calculate the dependent variable. The goal is to include related parties that could be capturing a similar vote preference—support for the former military regime or for the former rebels—as the FRG and the URNG. In particular, I include the *Partido Patriota* (PP) and the *Frente de Convergencia Nacional* (FCN) together with the FRG, and the *Unidad Nacional de la Esperanza* (UNE) together with the URNG. I hope these changes help to alleviate the concern about non-partisan voting patterns.

**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:** I agree with the reviewer that this is an important issue when studying Guatemala. Even though I cannot engage in a lengthy discussion because of space constraints, I do now mention these issues—along with state corruption and organized crime—in a footnote (page 7), and refer the reader to some related 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:** Thank you for this comment. I agree that omission of some of these variables might be seen as odd. The main reason I did not include them is that I could not find them at the level of municipalities and particularly for the period before the war. However, I now include in all models the rate of illiteracy, drawn from the 1973 census, which should be a good proxy for income and economic development in general. I hope the inclusion of this variable helps partially solving these omissions.

**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:** I now include some of these works, particularly the ones related to the role of organized crime and youth gangs in Guatemala, such as [Peacock & Beltrán \(2003\)](#); [Beltrán \(2016\)](#); [Booth, Wade & Walker \(2010\)](#); [Levenson \(2013\)](#).

## References

- Beltrán, Adriana (2016) A new era of accountability in guatemala? *Current History* 115(778): 63–67.
- Booth, John A, Christine J Wade & Thomas W Walker (2010) *Understanding Central America: Global forces, rebellion, and change*. Boulder: Westview Press.
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- Levenson, Deborah T (2013) *Adiós niño: The gangs of Guatemala City and the politics of death*. Durham: Duke University Press.
- Müller-Crepon, Carl, Philipp Hunziker & Lars-Erik Cederman (2021) Roads to rule, roads to rebel: Relational state capacity and conflict in africa. *Journal of Conflict Resolution* 65(2-3): 563–590.
- Peacock, Susan C & Adriana Beltrán (2003) Hidden powers: Illegal armed groups in post conflict Guatemala and the forces behind them. Special Report, Washington Office on Latin America, Washington DC.