

## REVIEW

### **“Violence, co-optation, and postwar voting in Guatemala”**

First, let me acknowledge that the author(s) put in a lot of work into this manuscript. I think it addresses an important research question and makes a valuable contribution to the literature. I commend the author(s).

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.

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?

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.

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).

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.

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.

### **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!
- 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.
- 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).