Check for updates

Article

Journal of Conflict Resolution
2022, Vol. 66(4-5) 809–835
© The Author(s) 2022
Article reuse guidelines:
sagepub.com/journals-permissions
DOI: 10.1177/00220027211070621
journals.sagepub.com/home/jcr
SAGE

Competition and Civilian Victimization

Michael Gibilisco[1], Brenton Kenkel[2] and Miguel R. Rueda[3]

Abstract

Violence against civilians in civil war is widely thought of as a strategic choice by combatant groups. We argue that a common strategic logic of competition underlies diverse theories of civilian victimization. We develop a theory of strategic complements in victimization, hypothesizing that an armed group's propensity to victimize civilians will increase with its expectation that its competitors will act likewise. We test this argument by structurally estimating a formal model of strategic interdependence between armed groups using data from the Colombian civil war. Our findings indicate that strategic expectations are responsible for a substantial amount of violence against civilians: the two major combatant groups would have systematically victimized civilians in at least 9% fewer municipalities if they had expected no violence by their rival. Examining causal mechanisms, we also find that victimization in the Colombian case was more likely aimed at controlling civilians than at influencing peace negotiations.

Keywords

Colombia, game theory, civilian casualties, civil wars, structural estimation

Why do rebel groups and organizations commit violence against civilians during civil wars? To understand these atrocities, conflict scholars have found evidence that they result from a process of competition among civil war actors. Violence against civilians,

[1]California Institute of Technology, Pasadena, CA, USA
[2]Vanderbilt University, Nashville, TN, USA
[3]Emory University, Atlanta, GA, USA

Corresponding Author:
Michael Gibilisco, Division of Humanities and Social Science, California Institute of Technology, 1200 E. California Blvd, MC 228-77, Pasadena, CA 91125, USA.
Email: michael.gibilisco@caltech.edu

according to these theories, is a strategic choice in a contest for valuable information (Kalyvas 2006), for positioning in postwar bargaining (Wood and Kathman 2014), or for material resources held by civilians (Bagozzi, Koren, and Mukherjee 2017).

Although these theories advance distinct explanations about the benefits that armed groups expect to yield from violence against civilians, they conceptualize victimization as a *strategic tool* of *competition* among armed groups. In this article, we study previously unexplored implications of the logic underlying these diverse theories. If civilian victimization advantages the perpetrator when its own violence surpasses that of its rival, then an armed group's incentive to engage in violence should be greater if it expects its competitors to employ similar tactics. Specifically, we theorize that civil war actors' decisions to victimize civilians exhibit *strategic complements*.[1] In other words, the expectation of violence by one group may cause violence by other groups. We provide a theoretical foundation for this strategic process as a cause of civilian victimization and present evidence that it was a substantial factor in the Colombian civil war.

Our theory of strategic complements in civilian victimization is built on two premises. First, violence against civilians is costly, not valued in itself by the armed groups. Second, the benefits that groups gain from violence against civilians are primarily relative—what matters is to get more than one's competitors. For example, in competition over information, having better information than one's competitors is more important than the raw quantity of information one has. With absolute costs and relative benefits, each group's expectation of violence by others will be mutually reinforcing: the more one group expects its competitors to victimize civilians, the greater its incentive to do the same, so as not to fall behind in the competition among groups.

To evaluate this theory, we quantify the extent of civilian victimization due to strategic complements in the Colombian conflict between 1998 and 2005 using a structural model of strategic interdependence between armed combatant groups. We analyze municipality-level data on violence against civilians by the left-wing guerrilla group Fuerzas Armadas Revolucionarias de Colombia (FARC) and the right-wing paramilitary group Autodefensas Unidas de Colombia (AUC) during this period.

Colombia is a particularly good case for studying competitive victimization. During our period of analysis, multiple armed actors fought for territorial control and held peace or disarmament talks with the government. Relative benefits, such as contention for territorial control or leverage during negotiations, are important scope conditions for our theory. Moreover, research design and data quality considerations also make the Colombian case appealing. Strategic incentives to victimize civilians vary at the local level, making a within-country design ideal for assessing our theory. Due to recent efforts by the Colombian government and non-governmental organizations, there is high quality sub-national data on the timing, location, nature, and perpetrator of victimization events. Although we focus on the Colombian case, our methodology could be used to study strategic interdependence and its underlying mechanisms in other civil wars with civilian victimization, assuming appropriate data are available.

Our key contributions are as follows. First, we develop a formal model of strategic competition in violence against civilians whose parameters can be estimated from real-world data. The structural approach allows us to pin down the parameters of this model—including, critically, the existence and strength of strategic complements—that are most consistent with the data.[2] Second, applying the model to the Colombian case, we find a statistically significant and substantively strong effect of strategic expectations. We estimate that each group (FARC and AUC) would have committed systematic violence against civilians in at least 9% fewer municipalities if it had expected the other group never to victimize civilians. Third, by extending our core model to incorporate different types of violence (selective versus non-selective) or multiple strategic actors, we identify which specific source of competition best fits the evidence from Colombia.

As our argument draws from the common notion of victimization as a strategic choice in a competitive process, our findings buttress the diverse set of theories premised on this idea. This contrasts with accounts of civilian victimization as the result of within-group influences, such as principal-agent problems or other internal control failures in armed groups (e.g., Abrahms and Potter 2015). While these within-group factors may certainly drive violence to some degree, our analysis shows that they provide at most a partial explanation in the Colombian case.

In extensions to our main analysis, we consider which specific mechanism best explains the patterns in the Colombian case. We focus on the distinction between theories of victimization as a tool to control the behavior of civilians themselves (e.g., Kalyvas 2006) and those that see it as a way to signal strength to other actors involved in postwar bargaining (e.g., Wood and Kathman 2014). We draw distinct implications from each theory about variation in the nature and strength of strategic complements across different types of violence and pairs of actors. We test these implications in auxiliary analyses, one distinguishing between choices of selective and non-selective violence and the other introducing a third strategic actor (the left-wing Ejército de Liberación Nacional). Our findings support the idea that the strategic complementarities in violence observed in Colombia were primarily driven by the goal of controlling civilians' behavior.

An alternative explanation for the victimization patterns we observe is that they are driven by revenge dynamics, where a group victimizes civilians as an emotional response to prior victimization by its rivals. At a broad level, both our theory and this alternative explanation would predict relatively high FARC violence in areas where the AUC commits violence, and vice versa. We rule out the possibility of revenge dynamics in two ways. First, in our model and empirical analysis, our groups are forward looking: their victimization decisions in a locality are driven by their expectations about what their rival will do, not by the history of violence in the locality. Furthermore, in a robustness check, we purge the influence of realized violence within a municipality from our measure of expected violence there and find even stronger support for strategic complements. Second, we investigate an empirical implication specific to the revenge explanation. If revenge dynamics were driving the interdependence we

observe in victimization, we would expect this to show up in our data as non-selective violence by one group causing non-selective violence by the other. In fact, we find no such effect.

This article builds on the literature studying the relationship between competition and violence in civil conflict (Balcells 2010; Cunningham, Bakke, and Seymour 2012; Metelits 2009). While existing work has established that competition alone may drive civilian victimization, we demonstrate further how the strategic expectations *among* competitors may shape decisions to engage in violence. Specifically, an increase in expected violence by one group can increase actual violence by its competitors even holding fixed the presence and strength of competition, often proxied empirically by the number (or changes in the number) of groups involved in conflict. Empirically, we innovate on previous work by using a structural model that allows us to *directly* estimate the strength of competitive incentives. Thus, we sidestep the need to proxy for group competition, which is important because the number of groups can be confounded by extraneous factors such as state strength or polarization in local preferences. Furthermore, we can explain variation in victimization decisions within a particular conflict even when the set of major competing actors remains constant, as in the time period we study in the Colombian case.

Our notion of strategic complements in civilian victimization during civil war is closely related to the concept of outbidding in the terrorism literature (Bloom 2004; Kydd and Walter 2006). Both theories draw from a similar logic, in which competition creates an incentive to outdo one's competitors. A key difference between these settings is that terrorist outbidding usually takes place among groups with a common enemy (usually a state government), whereas we predict strategic complements between groups on different sides of a civil conflict.

Theories of Competition and Victimization

In this section, we identify a common component in theories of violence against civilians during civil wars: armed groups victimize civilians for competitive advantage. In these theories, violence against civilians is a strategic choice that carries an *absolute* cost and a *relative* (or positional) benefit. We then argue that the strategic logic of victimization ought to be similar to other political and economic processes where participants take on absolute costs in order to receive relative benefits. This gives rise to our prediction of a positive relationship between expected violence by one group and the choice of violence by its competitors.

We use the term "strategic" in its game-theoretical sense. When we theorize that victimization is a strategic choice, we are saying that a civil war actor chooses whether to use violence in anticipation of how other actors will act and react. In this sense, evidence that anticipated violence by one group increases the likelihood of violence by others is evidence of strategic decision-making in the choice to victimize. Meanwhile, we do *not* claim that violence against civilians is "strategic" in the sense of being the best choice for a group to advance its long-term political goals. Many scholars posit that

violence against civilians is politically counterproductive (e.g., Abrahms 2006; Stephan and Chenoweth 2008), including in civil war contexts specifically (Fortna 2015). We do not model civilian responses to violence, and thus, our analysis cannot speak directly to whether other strategies would be more effective. Nevertheless, drawing on the large body of work arguing that violence against civilians helps groups extract information from the population or signal resolve for postwar bargaining, we posit that civil war actors perceive some benefit from victimizing civilians. Indeed, our empirical strategy allows for a test of this proposition. If civilian victimization were not perceived as beneficial and were only undertaken by mistake or at random, or exclusively as a result of internal armed group dynamics, then we would expect no strategic interdependence—and thus, contrary to our main hypothesis, no strategic complements—in victimization decisions. We do not rule out the idea that some violence is due to these factors, but evidence for our hypothesis would rule out the stronger claim that *all* victimization is non-strategic in nature, at least in the Colombian case.

Victimization as Civilian Control

An influential body of work, originating with Kalyvas (2006), posits that armed groups undertake violence to control civilian behavior, inducing civilians to behave in line with the group's goals. Several researchers have expanded on this theory and examined empirical evidence for its major hypotheses (Balcells 2010; Lyall 2009; Rueda 2017; Wood 2010). A key precept is that violence can coerce civilians into providing information to one's own side while deterring them from cooperating with the other side.

Importantly for our purposes, information has little *intrinsic* value to armed organizations in these theories. Civil war actors do not derive consumption value from the information they extract from civilians nor can they sell it on global markets. Instead, information is valuable insofar as it provides a military advantage over one's armed competitors. Kalyvas (2006, 174) describes information as "the link connecting one side's strength with the other side's weakness." An armed group's success depends not on the raw quantity of information it possesses, but rather on having better information than the other side.

Another critical idea in this literature is that violence against civilians is costly to the groups that employ it. In other words, victimization is valued primarily insofar as it achieves a political purpose, not desirable in itself. Kalyvas (2006, 23–28) further emphasizes the limitations of "expressive" motivations as an explanation for widespread violence against non-combatants. Victimization occurs when the indirect informational benefits outweigh these direct costs.

Related research focuses on victimization as a tool to extract material resources from civilians rather than information (Bagozzi, Koren, and Mukherjee 2017; Wood 2014). These theories maintain the assumption that violence against civilians is costly and employed only instrumentally. They differ from informational theories because the benefits of food extraction are not purely relative or positional. When armed groups

distribute food to win the hearts and minds among potential supporters, the strategic logic is broadly similar to the informational case. When the food extracted is used for the group's own subsistence, however, the incentive to extract food should depend little on how much other groups possess. Consequently, violence to extract material resources from civilians partially fits under the umbrella of our theory of strategic complements, depending on how combatant groups use the resources.

Victimization as Signaling

A separate body of work emphasizes victimization's effects on bargaining power among civil war actors in postwar negotiations. According to these theories, violence against civilians is a costly exercise of the "power to hurt," a coercive bargaining tactic to bring the other side to the negotiating table (Chu and Braithwaite 2018; Hultman 2007; Thomas 2014; Wood and Kathman 2014). Violence against civilians increases the costs of continued fighting, incentivizing the government to negotiate and signaling the perpetrator's willingness to bear these costs.

As above, there are two important features underlying this logic. First, civilian victimization is perceived as a strategic tool in competition between armed groups. Here the competition is not over information held by civilians but rather over bargaining power in the eventual negotiations to end the conflict. Bargaining power is relative—a group's expected value of negotiations depends not only on its own willingness to walk away from negotiations but also that of the other parties involved.[3] Therefore, as in competition over information held by civilians, what matters most in competition for bargaining position is to come out on top.

The second key commonality in the strategic logic of coercive and signaling theories is the assumption that violence is costly to undertake. Victimization is employed instrumentally to gain a competitive edge, not because armed organizations desire or value violence in itself. This is inherent in the costly signaling logic expounded by Kydd and Walter (2006) and Wood and Kathman (2014). Simply put, civilian victimization cannot be a costly signal unless it is costly. The argument that violence against civilians signals a group's willingness to keep fighting relies on the assumption that less-resolved groups would be unwilling to victimize civilians.

Our Theory: Strategic Complements

The two main premises of our theory draw from the strategic logic described above. First, whatever benefit a group derives from victimizing civilians is primarily relative or positional—that is, the value of the resources gained depends on how much of that resource the group's competitors possess. Whether this benefit is tactical information, material resources, or positioning for postwar bargaining, the important thing is that the benefits come primarily from possessing more of the resource than one's competitors do.[4] Second, violence is a costly choice, not valued in itself. In the absence of a

competitive benefit, armed groups would prefer not to expend effort victimizing non-combatants.

Under these two conditions, we expect strategic complements in armed actors' choices to victimize civilians. Civil war actors face a strategic tradeoff in the choice to commit violence against civilians. On one hand, because violence carries direct costs, armed groups would ideally refrain from violence or at least minimize the extent to which they partake in it. On the other hand, by refraining from violence, a group risks falling behind in the competition for whatever resource or input is at stake. Facing this strategic tradeoff, the optimal policy for an armed group is to commit enough violence to keep up with one's competitors, without engaging in more than would be necessary for these competitive purposes. How much is enough depends on the choices of one's competitors, meaning there are strategic complements in civilian victimization.

We are not the first to suggest that civilian victimization is a strategic choice by civil war actors—this is a foundational principle of the previous research that we build on. Our main innovations are (i) drawing out a common logic of competition underlying existing theories and (ii) showing that this logic implies strategic complements in victimization.

The latter theoretical claim is about how a group's choice of violence is influenced by its strategic expectations about other groups' choices. Our empirical method is designed to isolate this effect. We do not claim that these strategic expectations are the sole determinant of civilian victimization. Our formal model explicitly includes external influences on the choice to use violence, and we expect our model to apply to cases in which these forces are operative. For example, recent scholarship has identified how groups disproportionately target localities dominated by their rivals' coethnics (Fjelde and Hultman 2014). Our model allows for a locality's coethnicity with Group A to raise Group B's utility from victimizing civilians, as this scholarship would predict. Our claim is that insofar as Group B's chance of victimizing increases, including due to local ethnic composition, it increases Group A's incentive to respond in kind. This positive indirect effect may be partially or wholly offset by a negative direct effect of local coethnicity on Group A's utility from victimizing. The structural model allows us to decompose these direct and indirect effects; our primary expectation is that the indirect effect exists and is positive.[5]

Our focus on strategic spillovers contrasts with other research that portrays violence as the consequence of principal-agent problems within armed organizations (Abrahms and Potter 2015; Humphreys and Weinstein 2006) or between armed groups and outside funders (Hovil and Werker 2005; Salehyan, Siroky, and Wood 2014). This work disputes the idea that victimization is a strategic choice to gain an advantage over other armed groups. Our structural model accommodates this possibility, however. We expect these factors to have *some* influence but not to be the exclusive causes of violence against civilians. Identifying strategic complements in civilian victimization would allow us to rule out the idea that violence is wholly determined by internal control failures. If each group would ideally prefer to refrain from violence but cannot stop some members from freelancing, then there should be no systematic increase or

decrease in one group's incentives to engage in violence as a result of other armed groups' behavior. However, a finding of strategic complements would not lead us to claim that non-strategic influences have no effect—merely that there is a strategic component alongside other influences.

Formal and Statistical Model

Our theory leads us to expect an interdependence in civil war actors' choices to victimize civilians, where costly competition over positional benefits is the mechanism linking the decisions of distinct combatants. In this section, we model the groups' victimization choices, where the utility from victimizing civilians in a particular locality depends on both local characteristics and other actors' choices. Our central claim amounts to a parameter restriction on this model: violence by one actor increases its competitors' utility from violence (strategic complements). We test the claim via structural estimation, which identifies the form of the model that best comports with the observed data.

Two civil war actors or combatants, indexed $i = 1, 2$, compete in a given locality.[6] Each actor simultaneously chooses whether to victimize civilians, where $v_i = 1$ indicates victimization and $v_i = 0$ indicates no victimization. After these choices, players receive their respective payoffs.

Actor i's payoffs are functions of three components. The first is a set of fixed, local-level characteristics, collected in the vector $\mathbf{x}_i$. These represent systematic, predictable influences on i's incentive to commit violence against the civilian population, such as economic factors or local political sympathies. This means our model is compatible with theories that posit such effects. The second is the competitor's choice of whether to victimize, v_{-i}. Whether violence by one actor encourages or discourages violence by the other plays a key role in our analysis. Finally, we allow for idiosyncratic private influences on the actor's choice, modeled as a stochastic shock $\epsilon_i(v_i)$ to its utility from each possible action. This captures the possibility that combatants might not perfectly understand each other's incentives and cannot perfectly predict their competitors' behavior.[7]

Formally, combatant i's utility function is

$$u_i(v_i, v_{-i}, \varepsilon_i) = v_i[\underbrace{X_i \cdot \beta_i}_{\text{local factors}} + \overbrace{v_{-i} \cdot \alpha_i}^{\text{stratrgic interdependence}}] + \underbrace{\varepsilon_i(v_i)}_{\text{stochastic shock}} . \quad (1)$$

There are two parameters to be estimated in equation (1). The most important parameter for our substantive purposes is α_i, which characterizes how i's net benefit of victimization depends on its rival's choice to victimize.[8] If $\alpha_i > 0$, then i has stronger incentives to commit violence against civilians when it expects its rival to do so; that is, violence against civilians exhibits strategic complements as hypothesized. The model also allows for alternative hypotheses, however. If $\alpha_i < 0$, then i has weaker incentives to

engage in violence when its rival does so (strategic substitutes). If $\alpha_i = 0$, then i's decision is strategically independent from its rival, as we would expect if civil war actors perceived no positional or relational benefit from victimizing civilians. Meanwhile, β_i captures the impact of contextual variables—for example, political, economic, and geographic characteristics—on i's net payoff from victimization. These contextual effects may also differ across combatants.

Our assumption of simultaneous choice is, of course, a simplification. In reality, there may be a dynamic interaction as groups observe and respond to each other's choices. Nonetheless, the simultaneous model is the best approach for the Colombian context. First, as in previous analyses of violence in Colombia (e.g., Acemoglu, Robinson, and Santos 2013), we aggregate data over time in order to discern whether there is a systematic pattern of violence indicating a strategic choice as opposed to idiosyncratic incidents. Due to potential imprecision in the reporting of when incidents occurred, we do not believe the temporal sequences could properly be interpreted as sequential responses. Second, as a practical matter, in order to estimate a sequential model of strategic victimization decisions, we would need to specify a particular actor (e.g., the FARC or AUC) to act as the first mover in each locality. We know of no principled way to do this nor any way to infer from observed violence data which group had the first opportunity to commit violence (which may be different than the first group that actually committed violence).

We have also modeled the groups' decisions of whether or not to victimize civilians rather than their decisions about the amount of victimization to use. This is helpful for theoretical and empirical reasons. Theoretically, by focusing on discrete actions we sidestep the need to characterize and compute first-order conditions, and these discrete-choice models have well-understood equilibrium existence, identification conditions, and estimation procedures. Empirically, the binary decision reduces the number of parameters to estimate, allowing for a more powerful test of our central hypothesis.

We structure our empirical analysis around the game-theoretic model. Our goal is to identify the parameters of the model, namely the strategic effects α_i and the local influences β_i, that best correspond to observed data. Following from our theory of strategic complements in victimization, we expect to find $\alpha_i > 0$, indicating that each group responds in kind to expected victimization by its rival.

In Appendix C, we describe the empirical estimation of the model. Briefly, we assume a game of this form is played across a set of localities. In each locality, we observe the groups' victimization choices, as well as local characteristics that might influence their baseline incentive for victimization. To estimate the model, we follow a two-step procedure proposed by Hotz and Miller (1993). In the first stage, we obtain an initial estimate of each group's probability of victimization in each location. These must be consistent estimates of the true probabilities; we use a nonparametric regression of victimization choices on local characteristics. In the second stage, we plug these estimated choice probabilities into each group's expected utility and then find the α_i and β_i that maximize the corresponding pseudo-likelihood, assuming the groups are playing a Bayesian Nash equilibrium of the game.[9] Notice that we do not presuppose

$\alpha_i > 0$ while estimating the model's parameters. The goal is to find the version of the model that maximizes the likelihood of the observed data to identify whether strategic complementarities exist.

Our estimation approach faces two identification challenges. The first is to disentangle the effects of strategic expectations from those of municipality characteristics. As we describe in greater detail in Appendix C, given that we estimate the choice probabilities as functions of covariates in the first stage, it is possible for a collinearity problem to arise in the second stage, because the covariates also directly affect *i*'s payoffs from using violence via the local factors. To avoid this, a sufficient condition for identification is that each player's net payoff from victimization depends on a variable that has no direct effect on its rival's payoff once other municipality characteristics are held fixed (Bajari et al. 2010). We use the distance between the municipality and the group's area of origin, which given the set of economic, political, and geographical characteristics that we include as controls (listed below), plausibly satisfies the exclusion restriction. The second challenge is the possibility of multiple equilibria. Our two-step estimates are consistent in the presence of multiple equilibria as long as the following condition holds: in any pair of localities with the same observable characteristics, the same equilibrium is played. This is likely the case if the armed groups have interacted with each other over time.

The Colombian Case

The conflict we study began in the 1960s, when the FARC formed as a reaction to anti-communist repression by the Colombian government. While the FARC began as a small band of rebels, by the 1980s it had grown into a powerful national organization financed by kidnapping and extortion. The 1980s also saw the emergence of self-defense groups opposed to the FARC and other guerrillas. Although these groups did not have official ties to the government, national legislation provided a legal basis for military training of private citizens as well as the creation of neighborhood watch groups.[10]

We study an important period of the conflict beginning with the expansion of paramilitary groups in the late 1990s. In 1997, representatives of major self-defense organizations met in Antióquia to form the AUC.[11] The organization rapidly accelerated its territorial expansion by sending groups of fighters from Antióquia and Córdoba across the country to join local private forces in military campaigns against guerrillas and ostensible guerrilla sympathizers. The arrival of the paramilitaries often came with massacres, forced displacement, and kidnappings—which were, by their own accounts, part of their military strategy (Aranguren 2001).

While the paramilitary expansion occurred, the FARC entered into peace negotiations with the government in a demilitarized zone. Although negotiations were ongoing, the confrontation between guerrillas and the government continued, and the FARC used the demilitarized zone to consolidate territorial control in the south and to expand operations to other regions. After high-profile kidnappings and violent actions

by the FARC, the government canceled the negotiations in 2002. That year, Álvaro Uribe was elected president on an anti-guerrilla platform. Uribe strengthened the military, and the government gained the upper hand in the conflict. At the same time, the Uribe administration started negotiations with the AUC, which agreed to demobilize in 2005.

We analyze the strategic interaction between the FARC and the AUC from 1998 to 2005. In these years, both groups sustained *simultaneous* presence in multiple municipalities. This is necessary for the estimation of the model's parameters, as the two groups have to interact in each of the municipalities in our sample. Before 1998, the groups that formed the AUC had a more limited geographic presence. After 2005, the demobilization of the AUC and the military decline of the FARC reduce the number of municipalities where both groups operate. We divide these years into two periods: 1998–2001 and 2002–2005. The first period is marked by the consolidation of the AUC as a national force and the failed peace negotiations with the FARC. In this period, both groups were expanding their operations throughout the country. The second period is one of military retreat for the FARC and the transition to demobilization for the AUC.

The Colombian case is particularly well suited for our analysis. There are multiple armed actors contending for territorial control while at the same time pursuing peace negotiations or disarmament talks with the government. Contention for territorial control is a scope condition for theories of victimization as a tool of civilian control (e.g., Kalyvas 2006) while the pursuit of negotiations is an assumption of theories of victimization as a costly signal in bargaining (e.g., Wood and Kathman 2014). Therefore, our story of strategic complements, which draws on the logic of these theories, ought to apply in the Colombian case.

In contrast with internal conflicts involving state forces that directly victimize civilians, in Colombia the presence of a powerful paramilitary organization allowed rogue members of the military to delegate this task. Even though the paramilitaries were not officially part of the state forces, and there was no centralized policy of cooperation with the paramilitaries, the ties between the military and the paramilitary groups are extensively documented (e.g., Aranguren 2001; Gutiérrez-Sanin 2019; Human Rights Watch 1996, 2000). Recent evidence indicates that paramilitary and state forces shared logistical support, arms, and ammunition (Dube and Naidu 2015). The extent of this cooperation, however, has been difficult to establish given the strong incentives to hide such links. Because the government acted through the paramilitaries in many regions and time periods (some of which remain unknown), state forces themselves victimized civilians far less often than non-state groups in our data.[12] As such, we do not treat the military as a separate strategic actor. This avoids estimation challenges that arise from not knowing the specific years and municipalities where the government was acting as ally or enemy of the AUC. Importantly, we do account for government influence in our analysis by including proximity to army bases as a contextual variable, as discussed below.

We do not claim that civilian victimization in the Colombian case was driven *exclusively* by strategic interdependence. Others have documented how the FARC and

the paramilitaries have employed violence against civilians to achieve objectives for which the level of violence against civilians carried out by the enemy group was either not relevant or could imply strategic substitution of violence. For example, the paramilitaries sometimes victimized civilians to appropriate their land (Ibáñez 2009). Both the FARC and the paramilitaries were also heavily involved in illegal drug trafficking (e.g., Contreras 2018; Gutiérrez-Sanin 2019). It is possible that the group that controls an area where drugs are produced would protect the civilians processing the drugs, while the rival group might target those civilians to disrupt the supply lines of its competitor. If violence in the Colombian case is exclusively due to factors besides the competition we expect, then this should result in a null estimate of the strategic parameter, α_i. Even if we find evidence of strategic complements with a positive estimated α_i, the variation in victimization not explained by these strategic considerations could be associated with other factors of the Colombian case.

Data and Measurement

Our unit of observation is the municipality-period (1998–2001 or 2002–2005). Our information on civilian victimization comes from a database of conflict-related events created by the Grupo de Memória Histórica, which was established in 2011 to gather and disseminate accurate information about Colombia's conflict. The database aggregates reports from 10 sources, including the Interamerican Commission of Human Rights, official confessions from paramilitaries given to prosecutors, the Permanent Committee for the Defense of Human Rights, and the periodical *Noche y Niebla* published by the NGO Centro de Investigación y Educación Popular (CINEP). The information was compared and compiled across the sources to avoid duplication and misreporting. Catholic church reports of violent incidents are an important source of information for the CINEP data. They are particularly valuable given the church's widespread presence across Colombia and its neutral role in the conflict. Given these characteristics, the CINEP reports have been used as a key input in other Colombian conflict datasets (e.g., Restrepo, Spagat, and Vargas 2004).[13]

For each incident with a civilian fatality, the dataset reports the geographic location, dates, groups involved, and the number of victims. Incidents are classified into six categories: massacres, selective killings, incursions into population centers, clashes between armed groups, attacks to infrastructure, and terrorist attacks. Massacres are defined as the intentional killing of four or more civilians under the same set of circumstances. Selective killings are those with three or fewer civilian fatalities. Terrorist attacks involve explosives, excluding attacks targeting infrastructure or military forces. Incursions denote incidents where a group's forces enter a population center and assault local businesses and military government buildings.

We use this classification scheme to construct the dependent variables in our analyses. We construct a binary indicator for whether a group systematically chose to attack civilians in each municipality-period. In particular, we code victimization as occurring if the fraction of civilians killed intentionally by the group (victims of

massacres, selective killings, or terrorist attacks) out of the total of the group's victims is greater than or equal to the sample median. We graph municipality-level variation in our victimization measure in Appendix B. In the Robustness section, we discuss robustness checks showing that our results hold even when changing the threshold used to code the dependent variable.

The Grupo de Memória Histórica data allows us to identify municipalities where civilians were victims of violence, but we also need to identify where armed groups operated but did not victimize civilians. To do this, we follow a growing literature on the Colombian conflict that uses cumulative general violent incidents involving these groups to create indicators of presence (Acemoglu, Robinson, and Santos 2013; Ch et al. 2018; Fergusson et al. 2016). We use information from the Centro de Estudios sobre Desarrollo Económico (CEDE), which draws from the Observatory of Human Rights of the Vice-Presidency and the National Department of Planning.[14] The CEDE dataset reports the timing and location of various violent events.[15] We code an armed group as present in a municipality in the period of interest if (1) the average of the annual number of violent incidents involving this group is above the median across municipalities or (2) the group engages in systematic civilian victimization as defined above. In the Robustness section we discuss robustness tests with alternative sample selection criteria; our results are virtually unchanged.

As noted above, for each actor we need a variable that affects that actor's payoffs from victimization but does not directly affect the payoffs of other groups. We use the distance to each group's respective area of origin. Larger distances from where a group originated could affect the perceived dominance of that group, its military capabilities, and its ability to maintain discipline among the troops—all of which might determine victimization choices. For example, the paramilitaries were known to send their most experienced fighters to different areas of the country to train new troops (Gutiérrez-Sanin 2019). It is more likely that those experienced fighters are located in areas where the paramilitaries first appeared. Once we hold constant other municipality-period characteristics (described below), it is plausible that the distance to the place of origin of one group affects the payoffs of its rival only indirectly through the group's victimization choices.

We identify the areas of origin using sources on the historical development of each group. We compute the distance by road connecting each municipality to the closest municipality of early origin for a each group.[16] The origin area for the FARC includes 25 municipalities in which the FARC operated in the mid-1960s. We also identify 12 municipalities with self-defense group origins during the early 1980s. Figure A2 in Appendix B shows that there is variation across municipalities in the distances from the areas of early influence of one group to the closest area of influence of the group's rival. There is close proximity in the central Magdalena river basin region and northwest (Uraba) and more distance between the ones in the south (Meta and Huila).

We account for the influence of political, economic, and other contextual variables that affect baseline variation in victimization across municipalities. Colombia's democracy has been regularly affected by the influence of armed groups in elections, and

voting patterns have been found to drive violence (Acemoglu, Robinson, and Santos 2013; Balcells and Steele 2016; Steele 2011). Because of this, it is important to control for pre-determined proxies of political preferences. As baseline controls we use data on the 1994 and 1998 national elections from the Registraduría Nacional to build the share of votes for left parties in the Senate following the classification of parties on the left used in Acemoglu, Robinson, and Santos (2013). We also include the standard deviation of the liberal party's election share in the 1974–1994 presidential elections as a proxy for stability of political preferences. Other controls are the fraction of the population with unsatisfied basic needs, the gini coefficient, the share of municipal revenue from royalties associated with the exploitation of oil and other minerals, the area of the municipality where coca is grown, and the distance to the Magdalena River. All these variables come from the CEDE database.[17] Appendix A reports the summary statistics.

As mentioned earlier, we do not model government forces given the extensive links between paramilitary groups and rogue elements in the military. It is important, however, to account for the presence of government forces because they may alter the relative benefits and costs of victimization for the non-state armed groups. To this end, we identify the location of army bases using information published in the Colombian Army web portal.[18] As a baseline control, we use the distance along the geodesic between the closest army base and a given municipality augmented by the variability of altitude connecting these points.[19] This accounts for potential difficulties moving forces due to Colombia's mountainous terrain.[20]

Finally, our baseline analysis uses municipality-periods as the unit of observation. Our substantive conclusions still hold even when using data with municipality-year observations (see Appendix E.6), but we prefer the coarser coding as our baseline analysis for two reasons. First, smaller time frames would introduce greater time dependence. For example, if a group uses violence against civilians in one year, this may alter its costs and benefits of using violence in the same municipality but in the next year. In conflict studies, scholars typically use functions of past actions to account for this serial correlation in reduced-form regressions (Beck, Katz, and Tucker 1998). This approach would treat past endogenous actions as exogenous covariates, and in our structural framework it would imply that the groups do not internalize how their actions in one year affect payoffs in future years.

Second, there are measurement issues. With a smaller time frame, there is a greater possibility to mismeasure the outcome variable of interest. One possibility is under-reporting where we do not detect violence within a time period, not because violence did not occur, but rather because it was not reported. Another possibility is that violence in one municipality is misattributed to another municipality due to the incident being close to a border. Both of these issues also could affect our indicators of group presence. When the outcome variable of a discrete-choice model is subject to classical mis-classification, attenuation bias arises, a problem which does not appear with classical measurement error in linear regression.[21] By aggregating violence over time, we hope to reduce these sources of measurement error and hence guard against attenuation bias.

Table 1. Frequency of Victimization Patterns Across all Municipality-Periods Where the FARC and the AUC Were Both Present.

	AUC		
	No victimization	Victimization	Total
FARC			
No victimization	154 [25.20]	303 [49.59]	457 [74.80]
Victimization	30 [4.91]	124 [20.29]	154 [25.20]
Total	184 [30.11]	427 [69.89]	611 [100]

Note:Percentages are in brackets.

As a first look at the data, Table 1 reports the frequency of victimization by the FARC and the AUC in municipality-periods in which both are present. The proportion of victimization by the FARC is higher in those observations where the AUC victimizes (124/427, or 29%) than where the AUC does not (30/184, or 16%). The pattern is similar for the AUC, which victimizes 81% of the time where the FARC does so (124/154), compared to 66% where the FARC does not (303/457). These patterns provide suggestive evidence of strategic complements.

Structural Estimation Results

We estimate the parameters of the utility functions in equation (1) of the FARC and the AUC, assuming the data are generated by equilibrium play of the victimization game. Our goal is to estimate how the utility of a group changes as a function of its rival's victimization decision, parameterized by α_i. We also estimate the influence of municipality-period covariates, which are collected in the parameters β_i. Table 2 presents our estimates. A coefficient in the table can be interpreted as the change in log odds for choosing to victimize civilians relative to not victimizing them when a given explanatory variable changes by one unit.

In line with our theory, we find strong support for strategic complements in victimization decisions between the FARC and the AUC. In Panel A, each group's payoff from choosing victimization increases with the probability of its rival doing the same. For example, if the FARC expected the AUC to increase its probability of victimization of civilians by 45 percentage points (a standard deviation in the sample), this would increase the odds of the FARC choosing victimization by 2.05 ($\approx e^{1.517 \times 0.45}$). Thus, victimization is responsive not only to local conditions but also to the expected decision of one's rival.

Turning briefly to the local characteristics, we see a few notable patterns. The FARC is more likely to victimize civilians in poorer regions and who live closer to its early areas of influence. The AUC, on the other hand, is more likely to victimize those who live far from its areas of early operations but closer to the Magdalena river, in more populated areas, and in places with less stable political preferences. Notice that the municipality-level influences tend to work differently for each group. With the

Table 2. Estimates of the Payoff Parameters in the Victimization Model.

	FARC	AUC
Panel A. Strategic factors: α_i		
Rival's victimization probability	**1.517 (0.34)**	**1.882 (0.478)**
Panel B. Controls: β_i		
Coca area	0.039 (0.16)	0.098 (0.231)
Distance army base	−0.002 (0.002)	−0.000 2 (0.001)
Distance group's place of origin	**−0.002 (0.001)**	**0.002 (0.001)**
Distance Magdalena river	0.002 (0.002)	**−0.004 (0.002)**
Gini	1.549 (1.828)	1.876 (1.296)
Period 2002–2005	0.264 (0.235)	**−0.456 (0.217)**
ln(Population)	0.08 (0.141)	**0.396 (0.166)**
Poverty	**0.026 (0.007)**	−0.012 (0.006)
Oil royalties	−0.003 (1.256)	−1.22 (0.969)
Liberal party vote share	0.032 (0.018)	0.013 (0.019)
Variation Liberal party vote share	−1.276 (2.885)	**7.117 (2.593)**
Log-likelihood	−657.96	
Observations	611	

Note: The model includes region intercepts. Bootstrapped standard errors are in parentheses. Estimates in bold are statistically significant with $p < 0.05$.

exception of population, each covariate that has a statistically significant impact on one group's utility has the opposite sign in the other group's utility function. This further increases our confidence that correlation between the FARC and the AUC's victimization decisions is due to strategic incentives arising from competition, not because certain types of municipalities are generally more attractive targets for violence.

Table 3 shows the average (across municipality-periods) probability of victimization by the two groups under different assumptions about how victimization by a rival affects the group's own choice. The estimates in the first row give the average probability of victimizing civilians according to our parameter estimates—these are quite close to the raw proportions reported in Table 1. The estimates in the second row represent the counterfactual average probability of victimization if both groups chose violence independently from their rivals' choices.[22]

The exercise demonstrates the strength of strategic complementarities as a driver of civilian victimization: if each group no longer cared about its rival's decision, then overall violence against civilians would be much lower. In particular, the FARC victimizes civilians on average with a probability of 25% in our baseline model. If the FARC were not accounting for the AUC's decision to use violence, its average probability of victimization would drop to 11%. In other words, about half of the violence committed by the FARC can be traced to strategic complementaries. The AUC would experience a similar 9 percentage point decrease in its probability of

Table 3. Counterfactual Estimates of the Average Probability of Victimization by Each Group.

	Average Pr(Victimization)	
	FARC	AUC
Baseline	0.25 [0.22, 0.28]	0.70 [0.66, 0.73]
Without strategic interdependence	0.11 [0.05, 0.17]	0.61 [0.55, 0.66]

Note:95% confidence intervals in brackets.

victimization if its utility did not depend on the FARC's decision, meaning about 13% of AUC violence is driven by strategic complementaries.

Robustness

Several robustness checks ensure that the results are not an artifact of a particular specification or coding choice. The robustness checks are reported fully in Appendix E; we summarize them briefly here. Although temporal aggregation should reduce measurement error, we may have excluded municipalities where groups operated undetected or included those where groups tended not to operate but were detected in an one-off incident. This would be the case, for example, if groups that are geographically dominant do not need to engage in violent actions (Kalyvas 2006). To address this, we explore alternative sample inclusion criteria by expanding the sample to include municipalities where only one group operated, as well as by varying our threshold on the number of incidents in the CEDE data to code a group as present in a municipality. Similarly, we demonstrate that our results are robust to the specific threshold we use to code civilian victimization, our dependent variable. In addition, to ensure that the observed strategic complements are not driven by unobserved municipality characteristics, we alter our control specification by replacing region intercepts with state intercepts and by interacting all time-invariant variables with a period dummy.[23] We also estimate a model that controls for a measure of the intensity of conflict (the sum of all violent incidents in which at least one of the three largest non-state armed groups in the country participated) at the beginning of the period to rule out that the complementarities are driven by overall violence. Across these analyses, we continue to find strategic complements between the FARC and the AUC in the choice to victimize civilians.[24]

Another concern is that the strategic complements we observe might be an artifact of revenge dynamics. We pursue two alternative strategies to rule out this explanation. Here, we describe the first one while leaving a second one, which tests a key implication of this alternative explanation on the way different types of victimization (selective versus non-selective) are employed, for the next section. Our first strategy employs an alternative model for our first-stage estimates of victimization probabilities where the predicted probability for a given municipality is completely independent of observed violence there. Our resulting estimate of strategic expectations of violence in a municipality is thus based only on the observable characteristics of similar municipalities

and cannot be affected by violence within the municipality itself. We provide further details and report the results in Appendix E.3. Our results are unchanged, and the estimated strategic complements may be even stronger.

In a final robustness check, reported in Appendix E.6, we further disaggregate the data to municipality-years. Consistent with our expectation of attenuation bias due to temporal disaggregation, the estimated strategic spillover parameters are smaller than in our main model. Nevertheless, the estimates are positive and statistically significant, confirming our expectation of strategic complements.

Mechanisms of Competition

We have identified strategic complements in the victimization decisions of the FARC and the AUC, in line with our theory of competition with absolute costs and positional benefits. As discussed above, this finding is compatible with both civilian control and postwar signaling theories of the origins of civilian victimization. We can go further and examine *which* type of competition best fits the evidence.

To do this, we identify empirical implications that differ between the civilian control and postwar signaling mechanisms. We focus on hypotheses about strategic complements and substitutes in victimization decisions, as these are what our empirical methods are best suited to test. Overall, the evidence supports the idea that the FARC and AUC were primarily in competition to control civilians' actions, rather than to influence eventual settlements.

Selective Versus Non-Selective Violence

When violence is a tool for civilian control, Kalyvas (2006) argues that only selective violence is effective at engendering civilian assistance. If civilians will be the target of violence regardless of their choices to defect, then they have no reason not to. Consequently, we should see strategic complements primarily in the choice of selective violence. Moreover, non-selective violence by one group should lower the amount of selective violence by its opponents (Wood 2010). By making civilians less sympathetic to a group's cause, non-selective violence reduces the amount of selective violence necessary to prevent defection.

In contrast, if violence is primarily a tool to signal resolve, then we expect a different pattern of strategic spillovers. More frequent and less restrained attacks should convey the power to hurt even more strongly than selected limited killings. By this logic, strategic complements should be present in non-selective violence as well as in selective attacks. Relatedly, Fortna (2015) suggests that non-selective violence can signal military weakness, which undermines the signal of a group's power. If the negative signal of military strength outweighs the positive signal of the power to hurt, then we expect empirical patterns similar to those arising if violence were aimed at controlling civilian behavior.

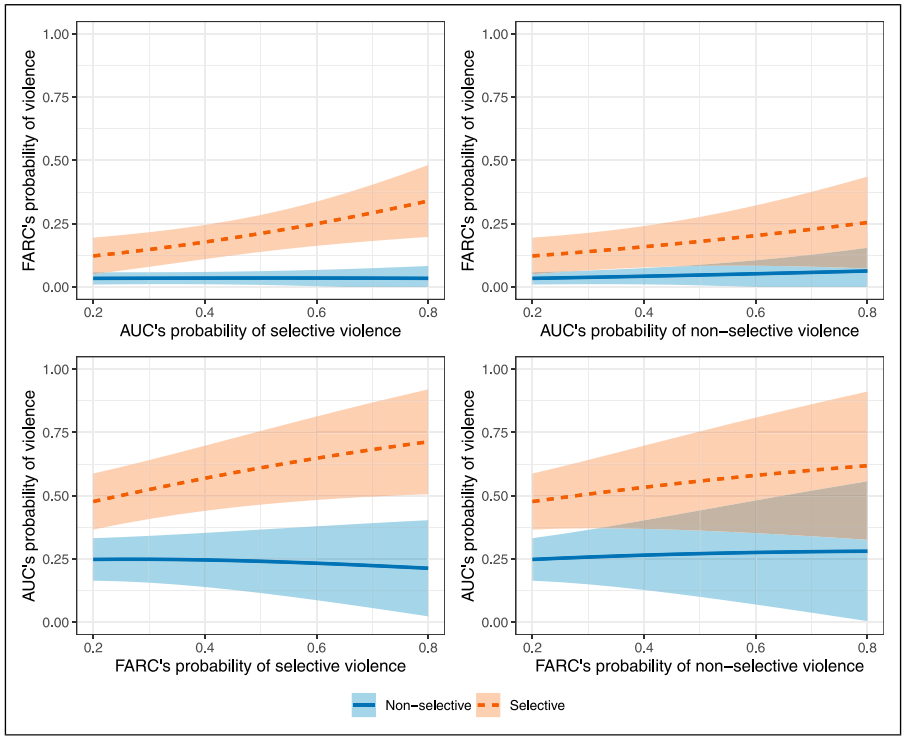


Figure 1. Strategic interdependence distinguishing selective and non-selective victimization.

To better discern whether the object of competition in the Colombian conflict was information from civilians or signaling for postwar bargaining, we extend our model, allowing for three choices of victimization: none, selective, or non-selective (see Appendix F). For the municipality-periods in our sample, we code no victimization as above. For the cases coded as victimization in our baseline analysis, we identify victimization as selective if the ratio of civilians killed in selective attacks to civilians killed in all intentional attacks (selective attacks plus massacres and terrorist attacks with explosives) is above the sample median. All other cases are coded as non-selective victimization. We then estimate the model where each group's net utility to each variety of victimization depends on the type of victimization by its rival.[25]

Figure 1 summarizes the estimated strategic spillovers in the extended analysis.[26] Three observations are apparent. First, both groups have a higher propensity for selective attacks when expecting selective attacks. This is consistent with the logic of victimization as civilian control. Second, each group's propensity for non-selective violence is invariant to its rival's choices. This contradicts the logic of non-selective violence as a particularly effective mechanism to signal the power to hurt, indicating either that it is aimed at controlling civilians or that the signal sent by non-selective

violence is a net negative. Third, both groups have a higher propensity for selective violence when expecting their rivals to engage in non-selective violence. This does not directly contradict the logic of violence as signaling, but it shows a preference for each group to exercise restraint when its rival does not, supporting the idea that violence in Colombia was aimed at securing civilian control.

Finally, these results do not appear consistent with revenge dynamics, where one group's use of civilian violence triggers a non-strategic cycle of violence. If this were the case, then one would expect responses to be stronger when attacks were less restrained. This is not what we observe in Figure 1. Instead, we see that strategic interdependence is strongest when looking at how one group's use of selective (more restrained) violence encourages the other to use selective violence. Although non-selective (less restrained) violence by one group weakly increases the likelihood of selective violence by other, it does not affect non-selective violence in a meaningful way.

Actors with Similar Policy Preferences

Theories of victimization as a signaling mechanism emphasize the importance of positioning in postwar bargaining. We have argued that this produces strategic complements in victimization between groups with competing goals. Because violence against civilians is costly, then the signaling model should predict *free-riding* dynamics between groups with similar goals. If one group commits violence against civilians, moving the expected postwar settlement toward its own ideological goals, then other groups that share those goals have less incentive to incur the costs of violence themselves. Under signaling theories, we should observe strategic substitutes among ideological allies when victimizing civilians.

To evaluate this prediction, we introduce a third actor to our model: the Ejército de Liberación Nacional (ELN), the second-largest left-wing guerrilla group in Colombia. The ELN was organized during the 1960s by radical members of left-wing political organizations and Catholic priests associated with the liberation theology movement. The goal of its founders was to replicate the Cuban revolutionary experience in Colombia. The ELN is relatively small compared to the FARC and the AUC. In 2002, it had 4,000 combatants while the FARC and the AUC had 19,000 and 12,000, respectively (Ugarriza and Ayala 2017). Although the FARC and ELN shared some policy preferences and even operated jointly during the 1987–1991 period, it was not uncommon for the groups to clash when competing over resources like royalties from mineral exploitation and control over drug trafficking routes. In fact, these localized clashes produced hundreds of casualties and occurred in many regions of the country in different periods.[27]

If civilian victimization in Colombia was aimed to influence postwar bargaining, then we should observe strategic substitution between victimization by the ELN and FARC, two ideological allies. On the other hand, if victimization was to control civilian behavior, then we should not see subtitution but rather complementarities or no

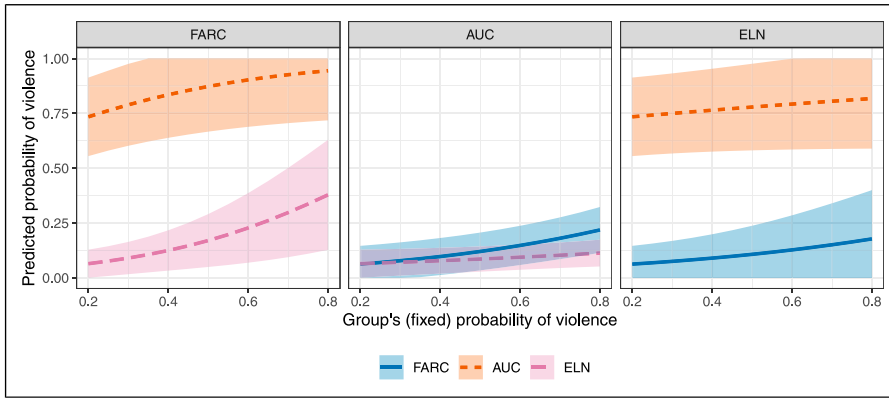


Figure 2. Strategic interdependence adding the ELN as a third group.

spillovers. We therefore extend the model to include the ELN and allow each group to have a separate strategic spillover parameter for each of the other two groups—see Appendix F. The information structure, timing, and available actions remain the same.

Figure 2 illustrates the strategic spillovers in the three-player analysis. Contrary to what we would expect under the signaling mechanism, we find no evidence of strategic substitution between the FARC and the ELN. Indeed, the estimated strategic independence parameters are all positive and significant at conventional levels.[28] Note that during this period, the FARC sustained peace negotiations with the government, a scope condition for these theories. Yet the ELN does not free ride on the victimization by the FARC that would theoretically push the government to offer policies favored by both left-wing guerrilla groups. If anything, the ELN responds with more victimization when it expects victimization by the FARC. Unlike the absence of strategic substitution in victimization between FARC and ELN and the complementarities between the violence of the FARC and AUC, however, this latter observation is not robust to other tests (see Appendix E.6).

Although our structural analysis of the FARC and the ELN contradicts expectations from the signaling model, there is potential evidence of free-riding incentives on the other side of the conflict. Insofar as the government operated through the AUC in certain areas, this may reflect strategic substitution between actors with similar policy preferences. Consequently, while the bulk of the evidence from our auxiliary analyses comports better with the coercion model than the signaling model for the Colombian case, we cannot wholly rule out the latter.

Conclusion

We develop a theory of strategic complements in civil war actors' choice to harm civilians, hypothesizing that the expectation of victimization by a group's rivals

increases the group's propensity to itself victimize civilians. Using a structural model, we evaluate this hypothesis in the context of the Colombian conflict and find strong support: the FARC and AUC each would have systematically victimized civilians in 9% fewer municipalities if not for these strategic spillovers. Strong strategic complementarities in the Colombia case are consistent with several accounts of strategic victimization in a competitive environment. In auxiliary analyses, we find more evidence that victimization in Colombia was intended to control civilians than to signal strength for postwar bargaining.

While the Colombian case is ideal for our analysis, we expect to find strategic complements in civilian victimization decisions in other conflicts meeting our scope conditions—namely, that victimization has absolute costs and positional benefits. An important task for future research is to investigate this expectation in other settings outside of Colombia, especially in a context where the government is an independent strategic actor. Given appropriate data, it is straightforward to adapt the structural approach we take here. In particular, in order to estimate strategic spillovers, we need to observe multiple interactions between the same actors, meaning any reasonable application of our empirical framework would require sub-national data.

Relatedly, our structural approach has applications outside of the civil war literature. Several empirical analyses study how competition among terrorist groups leads to more frequent or more destructive attacks. As in the civilian victimization literature, these studies proxy for competition using the number of groups. These proxies are problematic, however, because they may also be measuring factors that confound the relationship between competition and violence, such as latent animosity toward the groups' target. Given that commonly used datasets in this field identify the timing of and the group responsible for each attack over time (e.g., the Global Terrorism Database), our structural approach can be used to directly estimate the competitive incentives among rival terrorist groups and to quantify the proportion of attacks that stem from outbidding.

An important avenue for theoretical and empirical extension is to incorporate civilians' strategic responses to the victimization decisions of armed groups. In order to study the strategic interactions between armed organizations, we have black-boxed the question of civilian behavior, simply assuming the armed participants see some strategic benefit to be gained. One clear step for future research would be to characterize the assumptions about *civilian* incentives that could give rise to strategic interdependence between armed groups in victimization. For example, these dynamics might be reduced when civilians coordinate on non-cooperation, effectively tamping down competition. A related exercise would be to examine how strategic spillovers in victimization vary as a function of resistance by civilian populations in war-torn areas.

Our findings also have implications for policymakers. Insofar as violence against civilians results from group competition, a direct reduction in victimization by any single group will have beneficial externalities. According to our theory and findings, as the chance of violence by one group decreases, so too does the incentive for others to commit violence, as less is required to keep up in the competitive process. If a third

party with leverage over a civil war actor—for example, a geopolitical ally of a government or an external funder of an insurgency—can persuade its protege to refrain from victimizing civilians, then violence against civilians by the other side would be reduced indirectly. If the effects of strategic interdependence are similar in other civil wars to the Colombian case, then these indirect effects could substantially increase human welfare.

Acknowledgements

We thank Brett Benson, Andrew Coe, Cassy Dorff, Dani Reiter, Peter Schram, Brad Smith, and seminar audiences at Emory University, the London School of Economics, the University of Oxford, Vanderbilt University, the 2018 Peace Science conference, and the 2019 APSA conference for their comments and suggestions. We thank Laura Huber and Pearce Edwards for excellent research assistance. This work was conducted in part using the resources of the Advanced Computing Center for Research and Education at Vanderbilt University.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

ORCID iD

Michael Gibilisco https://orcid.org/0000-0002-6484-1314

Supplemental Material

Supplemental material for this article is available online.

Notes

1. More generally, strategic complements describe settings in which one player taking an action increases the propensity of others to do the same. Its converse is strategic substitutes.
2. A few studies of civil war have used structural estimation techniques (e.g., Gent 2007; Gibilisco and Montero N.d.; Nieman 2015). Ours is the first to use these methods to study the determinants of violence against civilians in civil war.
3. Signaling theories posit an asymmetry between rebel and government incentives under the assumption that victimization is disproportionately costly for the government (Wood and Kathman 2014, 691). If so, then our theory would apply best to those conflicts—like the Colombian case—featuring multiple non-state actors with distinct preferences.
4. This would not hold if, for example, the purpose of victimizing civilians were to obtain commodities for the groups' own consumption or to be sold on the world market.

5. We do not include ethnic variables in the application to Colombia as ethnicity was not a major factor in that conflict, but the model could be used precisely this way when applied to conflicts with an ethnic dimension.
6. In the Colombian case, the two actors are the FARC and AUC. In the Appendix, we extend the model to incorporate additional actors and a finer distinction among levels of victimization. We discuss the role of the government in *The Colombian Case* section.
7. In estimation, we assume these are type-one extreme value.
8. The *ex ante* expected utility (i.e., before the stochastic shock) for no victimization is normalized to zero, which is a standard identification condition in discrete-choice models.
9. Implicitly, this is assuming that the payoff parameters α_i and β_i are constant across municipalities. Such an assumption is plausible in the Colombia context where there is relatively high group cohesion.
10. Examples are Law 48 of 1965 and Law 356 of 1994.
11. "Paramilitares se habrían unido" (Paramilitaries have united), *El Tiempo*, 20 April 1997.
12. We see police victimization in 1% of municipality-periods and military victimization in 8%.
13. In Appendix D, we expand on the description of the original data and highlight differences with other Colombian conflict datasets.
14. The original sources of these data are newspaper and national police reports.
15. These include attacks with explosives, incendiary terrorist acts, assaults to private property, homicides, ambushes, kidnappings, incursion to population centers, overland piracy, illegal checkpoints, attacks to politicians, clashes with the state armed forces, and demobilization of its members.
16. If no road connects the municipalities, we use the distance along the geodesic.
17. We compute the gini coefficient and the unsatisfied basic needs index at the beginning of the period of analysis using a cubic spline interpolation. The original census data is only available for 1993 and 2005.
18. Military bases that were built during the period of analysis or later are not included.
19. We use the the distance along the geodesic multiplied by one plus the variation of altitudes along the same path. We take the variation of altitudes from Acemoglu, García-Jimeno, and Robinson (2015).
20. Appendix B contains a map with base locations.
21. Cook et al. (2017) have a recent discussion of this problem in political science.
22. Specifically, it is the average probability of violence if we were to fix $\alpha_{\text{AUC}} = \alpha_{\text{FARC}} = 0$; or, equivalently, if each group expected zero violence by its rival.
23. A drawback of the state-intercept specification is that by focusing on within-state comparisons, the explanatory power of the distance to an armed group's early place of influence is necessarily reduced. The inclusion of these intercepts, however, do not significantly alter the strategic parameters of interest in terms of magnitude or estimation precision.
24. We compare our model to a non-strategic variant via a non-nested model test, finding that ours fits the data better.
25. Because each player has three actions, we adopt a semiparametric approach to first-stage estimation, using a multinomial model with cubic splines for continuous explanatory variables.

26. Full results are reported in Table A2 in Appendix A.
27. See, "FARC and ELN se enfrentan" (FARC and ELN clash) *Caracol Radio* 10 january 2000; "Enfrentamiento FARC y ELN" (Clashes between the FARC and ELN), *El Tiempo* 7 July 2000; "FARC contra ELN" (FARC against ELN), *Semana*, 3 February 2007; "Masacre in Cauca habría sido por enfrentamientos entre disidencia de las FARC y el ELN" (Massacre in Cauca linked to clashes between the FARC and ELN), *El Espectador*, 30 October 2018.
28. See Table A3 in Appendix A.

References

Abrahms, Max. 2006. "Why Terrorism Does Not Work." *International Security* 31 (2): 42-78.

Abrahms, Max, and Philip B. K. Potter. 2015. "Explaining Terrorism: Leadership Deficits and Militant Group Tactics." *International Organization* 69 (2): 311-342.

Acemoglu, Daron, Camilo García-Jimeno, and James A. Robinson. 2015. "State Capacity and Economic Development: A Network Approach." *American Economic Review* 105 (8): 2364-2409.

Acemoglu, Daron, James A. Robinson, and Rafael J. Santos. 2013. "The Monopoly of Violence: Evidence from Colombia." *Journal of the European Economic Association* 11 (S1): 5-44.

Aranguren, Mauricio. 2001. *Mi Confesión, Carlos Castaño Revela sus Secretos*. Bogotá, Colombia: Editorial Oveja Negra.

Bagozzi, Benjamin, Ore Koren, and Bumba Mukherjee. 2017. "Droughts, Land Appropriation, and Rebel Violence in the Developing World." *Journal of Politics* 79 (3): 1057-1072.

Bajari, Patrick, Han Hong, John Krainer, and Denis Nekipelov. 2010. "Estimating Static Models of Strategic Interactions." *Journal of Business & Economics* 28 (4): 469-482.

Balcells, Laia. 2010. "Rivalry and Revenge: Violence against Civilians in Conventional Civil Wars." *International Studies Quarterly* 54 (2): 291-313.

Balcells, Laia, and Abbey Steele. 2016. "Warfare, Political Identities, and Displacement in Spain and Colombia." *Political Geography* 51: 15-29.

Beck, Nathaniel, Jonathan N. Katz, and Richard Tucker. 1998. "Taking Time Seriously: Time-Series-Cross-Section Analysis with a Binary Dependent Variable." *American Journal of Political Science* 42 (4): 1260-1288.

Bloom, Mia M. 2004. "Palestinian Suicide Bombing: Public Support, Market Share, and Outbidding." *Political Science Quarterly* 119 (1): 61-88.

Ch, Rafael, Jacob Shapiro, Abbey Steele, and Juan F. Vargas. 2018. "Endogenous Taxation in Ongoing Internal Conflict." *American Political Science Review* 112 (4): 996-1015.

Chu, Tiffany S., and Jessica Maves Braithwaite. 2018. "The Effect of Sexual Violence on Negotiated Outcomes in Civil Conflicts." *Conflict Management and Peace Science* 35 (3): 233-247.

Contreras, Jaime. 2018. *FARC-EP. Insurgencia, terrorismo y narcotráfico en Colombia: memoria y discurso*. Madrid, Spain: DYKINSON.

Cook, Scott J., Betsabe Blas, Raymond J. Carroll, and Samiran Sinha. 2017. "Two Wrongs Make a Right: Addressing Underreporting in Binary Data from Multiple Sources." *Political Analysis* 25 (2): 223.

Cunningham, Kathleen Gallagher, Kristin M. Bakke, and Lee Seymour. 2012. "Shirts Today, Skins Tomorrow: Dual Contests and the Effects of Fragmentation in Self-Determination Disputes." *Journal of Conflict Resolution* 56 (1): 67-93.

Dube, Oeindrila, and Suresh Naidu. 2015. "Bases, Bullets, and Ballots: The Effect of US Military Aid on Political Conflict in Colombia." *Journal of Politics* 77 (1):249-267.

Fergusson, Leopoldo, James A. Robinson, Ragnar Torvik, and Juan F. Vargas. 2016. "The Need for Enemies." *Economic Journal* 126: 1018-1054.

Fjelde, Hanne, and Lisa Hultman. 2014. "Weakening the Enemy: A Disaggregated Study of Violence against Civilians in Africa." *Journal of Conflict Resolution* 58 (7): 1230-1257.

Fortna, Virginia Page. 2015. "Do Terrorists Win? Rebels' Use of Terrorism and Civil War Outcomes." *International Organization* 69 (3): 519-556.

Gent, Stephen E. 2007. "Strange Bedfellows: The Strategic Dynamics of Major Power Military Interventions." *Journal of Politics* 69 (4): 1089-1102.

Gibilisco, Michael, and Sergio Montero. N.d. "Do Major-Power Interventions Encourage the Onset of Civil Conflict? A Structural Analysis." *Journal of Politics*. Forthcoming.

GMH. 2013. *Basta Ya! Colombia:memorias de guerra y dignidad*. Bogotá, Colombia: Imprenta Nacional de Colombia.

Gutiérrez-Sanin, Francisco. 2019. *Clientelistic Warfare. Paramilitaries and the State in Colombia (1982-2007)*. Oxford: Peter Lang.

Hotz, V. Joseph, and Robert A. Miller. 1993. "Conditional Choice Probabilities and the Estimation of Dynamic Models." *The Review of Economic Studies* 60 (3): 497-529.

Hovil, Lucy, and Eric Werker. 2005. "Portrait of a Failed Rebellion: An Account of Rational, Sub-Optimal Violence in Western Uganda." *Rationality and Society* 17 (1): 5-34.

Hultman, Lisa. 2007. "Battle Losses and Rebel Violence: Raising the Costs for Fighting." *Terrorism and Political Violence* 19 (2): 205-222.

Human Rights Watch. 1996. *Colombia's Killer Networks: The Military-Paramilitary Partnership and the U.S.* Human Rights Watch. https://www.legal-tools.org/doc/22208e/pdf/

Human Rights Watch. 2000. "The Ties That Bind: Colombia and Military-Paramilitary Links." https://www.hrw.org/legacy/reports/2000/colombia/

Humphreys, Macartan, and Jeremy M. Weinstein. 2006. "Handling and Manhandling Civilians in Civil War." *American Political Science Review* 100 (3): 429-447.

Ibáñez, Ana Maria. 2009. "Forced Displacement in Colombia: Magnitude and Causes." *Economics of Peace and Security Journal* 4 (1): 48-54.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge University Press.

Kydd, Andrew H., and Barbara F. Walter. 2006. "The Strategies of Terrorism." *International Security* 31 (1): 49-80.

Lyall, Jason. 2009. "Does Indiscriminate Violence Incite Insurgent Attacks? Evidence from Chechnya." *Journal of Conflict Resolution* 53 (3):331-362.

Metelits, Claire. 2009. *Inside Insurgency: Violence, Civilians, and Revolutionary Group Behavior*. NYU Press.

Nieman, Mark David. 2015. "Statistical Analysis of Strategic Interaction with Unobserved Player Actions." *Political Analysis* 23 (3): 429-448.

Restrepo, Jorge, Michael Spagat, and Juan F. Vargas. 2004. "The Dynamics of the Colombian Civil Conflict: A New Data Set." *Homo Oeconomicus* 21 (2): 396-428.

Rueda, Miguel R. 2017. "Popular Support, Violence, and Territorial Control in Civil War." *Journal of Conflict Resolution* 61 (8): 1626-1652.

Salehyan, Idean, David Siroky, and Reed M. Wood. 2014. "External Rebel Sponsorship and Civilian Abuse." *International Organization* 68 (3): 633-661.

Steele, Abbey. 2011. "Electing Displacement: Political Cleansing in Apartadó, Colombia." *Journal of Conflict Resolution* 55 (3): 423-445.

Stephan, Maria J., and Erica Chenoweth. 2008. "Why Civil Resistance Works: The Strategic Logic of Nonviolent Conflict." *International Security* 33 (1): 7-44.

Thomas, Jakana. 2014. "Rewarding Bad Behavior: How Governments Respond to Terrorism in Civil War." *American Journal of Political Science* 58 (4): 804-818.

Ugarriza, Juan Esteban, and Nathalie Pabón Ayala. 2017. *Militares y guerrillas. La memoria histórica del conflicto armado en Colombia desde los archivos militares 1958-2016*. Bogotá, Colombia: Editorial Universidad del Rosario.

Wood, Reed M. 2010. "Rebel Capability and Strategic Violence against Civilians." *Journal of Peace Research* 47 (5): 601-614.

Wood, Reed M. 2014. "From Loss to Looting? Battlefield Costs and Rebel Incentives for Violence." *International Organization* 68 (4): 979-999.

Wood, Reed M., and Jacob D. Kathman. 2014. "Too Much of a Bad Thing? Civilian Victimization and Bargaining in Civil War." *British Journal of Political Science* 44 (3): 685-706.

Online Appendix to "Competition and Civilian Victimization"

A Other Tables **i**

B Other Figures **iv**

C Pseudo-likelihood and Identification **vii**

D Victimization Data **viii**

E Robustness Checks **ix**

E.1 Presence Criteria ix

E.2 Threshold of Victimization x

E.3 First Stage Estimates xi

E.4 Control Specification xi

E.5 Non-Nested Model Comparison xiii

E.6 Municipality-Year Results xiii

F Expanded Model **xvii**

Additional References **xviii**

A Other Tables

Table A1. Summary Statistics

Variable	Observations	Mean	Std. Dev.	Minimum	Maximum
Coca area	611	0.16	0.96	0	9.74
Distance army base	611	92.09	107.28	0	884.99
Distance FARC's place of origin	611	199.95	170.89	0	750.76
Distance AUC's place of origin	611	293.52	200.68	0	918.67
Distance Magdalena river	611	107.02	74.88	0.2	409.96
Gini	611	0.43	0.12	0	0.53
Population	611	1.36	0.9	0.15	6.46
Poverty	611	45.17	19.75	8.17	100
Royalties (oil)	611	0.03	0.12	0	0.83
Variation Liberal party vote share	611	0.08	0.05	0.01	0.34
Vote share left	611	4.6	6.93	0	64.82

This table presents summary statistics of controls.

Table A2. Selective and Non-Selective Victimization

	FARC		AUC	
	Selective	Non-Selective	Selective	Non-Selective
Panel A. Strategic factors				
Rival's selective victimization probability	2.197**	0.507	2.867***	1.944*
	(0.856)	(1.244)	(0.928)	(1.075)
Rival's non-selective victimization probability	1.572*	1.392	2.109*	1.881
	(0.9)	(1.239)	(1.12)	(1.253)
Panel B. Controls				
Coca area	-0.126	0.451	0.048	0.119
	(0.173)	(0.684)	(0.269)	(0.275)
Distance army base	-0.002	-0.0003	0.0005	-0.0003
	(0.002)	(0.003)	(0.001)	(0.001)
Distance group's place of origin	-0.002**	-0.004**	0.002**	0.002*
	(0.001)	(0.002)	(0.001)	(0.001)
Distance Magdalena river	0.002	0.001	-0.006***	-0.0001
	(0.002)	(0.004)	(0.002)	(0.002)
Gini	-0.885	16.28**	1.865	0.444
	(1.883)	(7.570)	(1.536)	(1.803)
ln(Population)	0.024	0.264	0.182	0.528***
	(0.171)	(0.300)	(0.185)	(0.188)
Period 2002-2005	0.261	0.312	-0.073	-1.167***
	(0.334)	(0.502)	(0.256)	(0.271)
Poverty	0.03***	0.01	-0.02**	-0.01*
	(0.008)	(0.01)	(0.01)	(0.01)
Royalties (Oil)	0.461	-2.99	-1.09	-1.4
	(1.473)	(18.09)	(1.05)	(1.445)
Variation Liberal party vote share	-0.364	-8.474*	6.627**	8.303**
	(3.41)	(4.644)	(2.933)	(3.268)
Vote share left	0.018	0.058**	0.008	0.005
	(0.024)	(0.028)	(0.025)	(0.023)
Log likelihood				-1001.32
Observations				611

This table presents maximum likelihood estimates of the parameters of the civilian victimization model with three actions (non violence, selective victimization, and non-selective victimization). The model includes region intercepts. Bootstrapped standard errors are in parentheses. *** p<0.01, **p<0.05, *p<0.1.

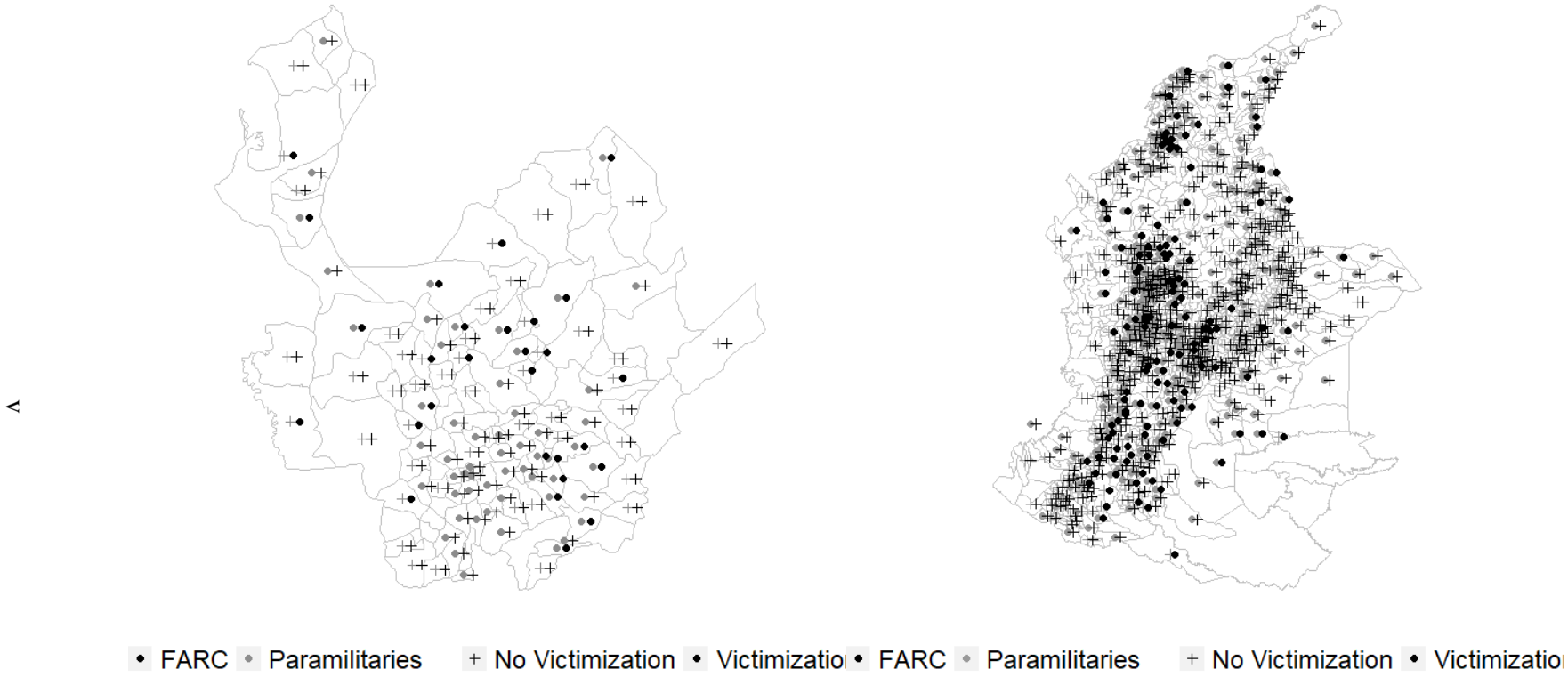
Table A3. Three-Player Game Estimates

	FARC	AUC	ELN
Panel A. Strategic factors			
AUC's Victimization Probability	2.387***		1.022
	(0.69)		(0.701)
ELN's Victimization Probability	1.956**	0.806	
	(0.852)	(0.787)	
FARC's Victimization Probability		3.026***	3.631***
		(1.01)	(1.134)
Panel B. Controls			
Coca area	1.196	-0.313	-0.19
	(0.936)	(0.697)	(1.34)
Distance army base	-0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)
Distance group's place of origin	-0.002	0.001	-0.003**
	(0.002)	(0.001)	(0.002)
Distance Magdalena rive	0.004	-0.006*	-0.006
	(0.004)	(0.004)	(0.004)
Gini	13.958*	13.811**	5.159
	(7.449)	(6.999)	(8.537)
ln(Population)	0.061	0.58**	0.051
	(0.216)	(0.247)	(0.242)
Period 2002-2005	0.2	-0.593*	-0.64*
	(0.364)	(0.318)	(0.383)
Poverty	0.003	-0.019*	0.013
	(0.013)	(0.01)	(0.012)
Royalties (oil)	0.583	-0.271	-1.439
	(1.819)	(1.299)	(2.884)
Variation Liberal party vote share	-0.206	7.87	-3.049
	(4.735)	(5.615)	(4.859)
Vote share left	0.004	-0.017	-0.029
	(0.037)	(0.03)	(0.058)
Log likelihood			-443.45
Observations			320

This table presents maximum likelihood estimates of the parameters of the three-player civilian victimization model. The model includes region intercepts. Bootstrapped standard errors are in parentheses. *** p<0.01, **p<0.05, *p<0.1.

B Other Figures

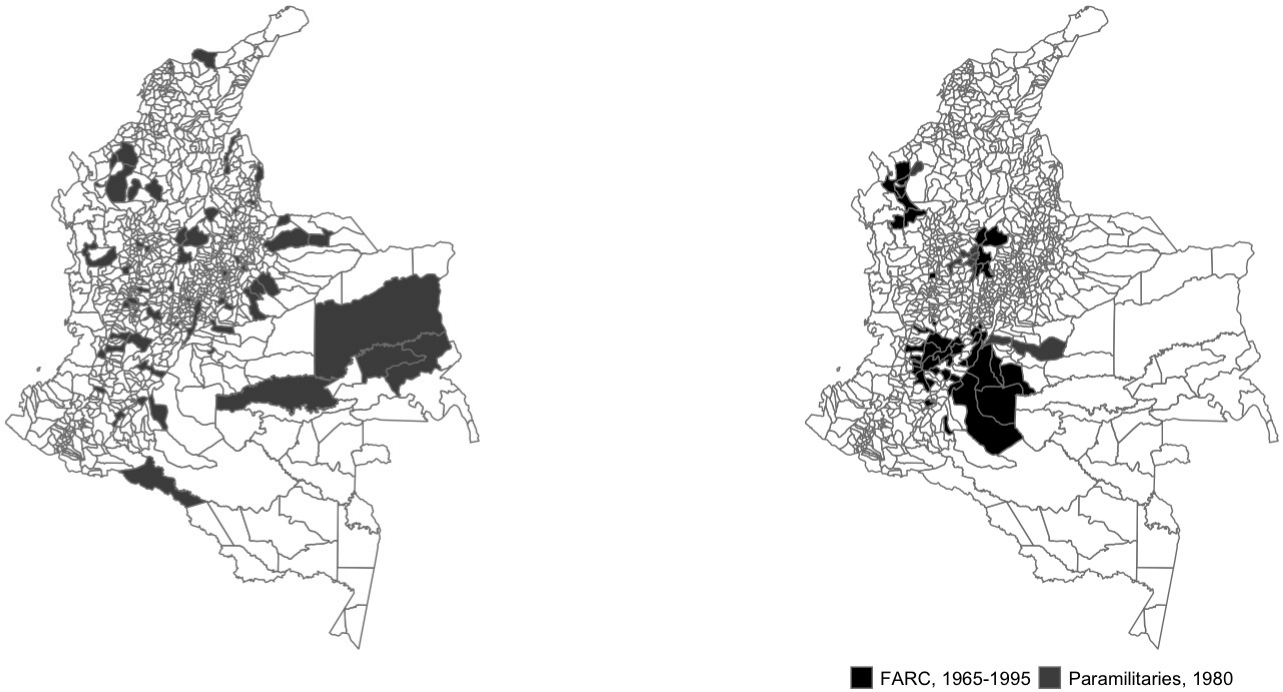
Areas of early influence are identified according to a variety of sources (Ugarriza and Ayala 2017; Alonso 1997; Bejarano 1997; Carlos Medina Gallego 2009; Centro Nacional de Memoria Histórica 2014; González 1991; Molano 2015; *Verdad Abierta* 2019).



(a) Antioquia, 2002–2005.

(b) Colombia, 2002–2005.

Figure A1. Geographical variation in victimization strategies.



(a) Army bases, 2000.

(b) Armed groups' early areas of influence.

Figure A2. Locations used in covariate specifications.

C Pseudo-likelihood and Identification

As this is a game of incomplete information with simultaneous moves, our solution concept is Bayesian Nash equilibrium. Because each player's utility is stochastic, in equilibrium i has a probability $p_i \in (0,1)$ of choosing victimization. In fact, per McKelvey and Palfrey (1995), an equilibrium is characterized by a pair of probabilities satisfying a rational expectations condition:

$$\begin{aligned} p_1 &= \Pr(\mathbf{x}_1 \cdot \beta_1 + p_2 \cdot \alpha_1 > \epsilon_1(0) - \epsilon_1(1)), \\ p_2 &= \Pr(\mathbf{x}_2 \cdot \beta_2 + p_1 \cdot \alpha_2 > \epsilon_2(0) - \epsilon_2(1)). \end{aligned} \tag{A1}$$

Substantively, this means each actor's strategy maximizes its own utility given the probability with which it expects the other group to victimize civilians. The equilibrium condition requires that neither player systematically over- or under-estimates the other's likelihood of engaging in violence given the local environment.

Collect the parameters of the model in $\theta = (\alpha_i, \beta_i)_{i=1,2}$, and let $\Psi_i(p_{-i}; \mathbf{x}_i, \theta)$ denote the corresponding best-response probabilities:

$$\Psi_i(p_{-i}; \mathbf{x}_i, \theta) = \int \mathbb{I}[\mathbf{x}_i \cdot \beta_i + p_{-i} \cdot \alpha_i > \epsilon_i(0) - \epsilon_i(1)]\, dF(\epsilon_i), \tag{A2}$$

where $\mathbb{I}$ is the indicator function and F is the prior distribution of the stochastic shocks. The equilibrium condition of Equation A1 is equivalent to $p_i = \Psi_i(p_{-i}; \mathbf{x}_i, \theta)$ for each $i = 1, 2$.

We obtain estimates of equilibrium beliefs, $\hat{p}_i^{mt}$, using the Nadaraya-Watson kernel estimator and covariates x_i^{mt} for each group $i \in \{\text{FARC}, \text{AUC}\}$, in each municipality m and time period $t \in \{1998-2001, 2002-2005\}$. Collect these estimates in vector $\hat{\mathbf{p}}$. Then we write the (conditional) pseudo-likelihood function from Hotz and Miller (1993) as

$$\mathcal{L}(\theta \mid \hat{\mathbf{p}}, \mathbf{X}) = \prod_t \prod_m \prod_i \Psi_i(\hat{p}_{-i}^{mt}; \mathbf{x}_i^{mt}, \theta). \tag{A3}$$

Given the definition of Ψ_i in Equation A2, Equation A3 has a natural interpretation: it is the likelihood assuming that each actor best responds to the equilibrium beliefs estimated in the first step. If $\hat{p}_i^{mt} = p_i^{mt}$ for all i, m and t, then it is the true likelihood in the data generating process. Furthermore, because ϵ_i^{mt} are drawn from the type one extreme value distribution and are independent across actions, the integral in Equation A2 takes the standard logistic form:

$$\Psi_i(\hat{p}_{-i}^{mt}; \mathbf{x}_i^{mt}, \theta) = [1 + \text{Exp}\{-\mathbf{x}_i^{mt} \cdot \beta_i - \hat{p}_{-i}^{mt} \alpha_i\}]^{-1}. \tag{A4}$$

Equation A4 helps to illustrate an identification problem that can arise when estimating this game. The first-stage estimate $\hat{p}_{-i}^{mt}$ is a function of x_{-i}^{mt} (estimated via the kernel estimator), and i's local municipality payoff of using violence ($x_i^{mt} \cdot \beta_i$) is a function of x_i^{mt}. If these covaraites are the same ($x_i^{mt} = x_{-i}^{mt}$), the analysis might suffer a collinearity problem when trying to separately identify the effects of β_i and α_i on the observed choices. This problem would be particularly accute if we used a linear probability model in the first stage, in which case both the local payoffs and the first-stage choice probabilities would be linear combinations of covariates. As described in Bajari et al. (2010), a sufficient (but not necessary) condition to separate the effects of β_i and α_i is an exclusion restriction. That is, find some variable that affects group i's local payoffs but that

do not enter group $-i$'s local payoff. In our analysis, distance from a group's early area of control serves as our restricted variable, and this is inspired from previous work. Gibilisco and Montero (N.d.), for example, estimate a game of major-power interventions into civil wars and use a major power's distance from a war to specify its costs of intervention. In economics, Ellickson and Misra (2011) describe how distance from regional headquarters or regions of early openings are used to specify costs of retailers in market-entry game. That is, Kmart (founded in Michigan) and Walmart (founded in Arkansas) might be more profitable in the Midwest and South, respectively.

D Victimization Data

Our information on civilian victimization comes from information provided by the Historic Memory Group (*Grupo de Memória Histórica*, GMH). This group was created by the Colombian government (Law 1448 of 2011) as part of the National Center for Historical Memory with the explicit aim of gathering and disseminating accurate information about the recent history of Colombia's internal conflict. We use 6 datasets from GMH on the following forms of violence: terrorist attacks, infrastructure and property attacks, incursions in population centers, clashes between groups, massacres, and selective killings. The datasets contain detailed information about violent incidents including geographic location, dates, groups involved, and the number of victims. Unlike fatalities, in some of these datasets, the total number of injured is included but it is not possible to dissagregate it into civilians and combatants. The GMH also publishes information on victims of mines and kidnappings. Although these forms of violence greatly affect civilians in the Colombian conflict, we focus on fatalities. This is because with mines the perpetrator is unknown, and with kidnappings the dataset is missing information on the confirmed perpetrator for most cases. Along with datasets from Restrepo, Spagat and Vargas (2004) and Palao-Mendizabal et al. (2019), the GMH datasets uses the periodical Noche y Niebla published by the NGO *Centro de Investigacíon y Educacíon Popular* (CINEP) as one of its main sources of information. The CINEP data have been used a source for documenting the conflict by the U.S. State Department, Human Rights Watch, and Amnesty International (Palao-Mendizabal et al. 2019). A key difference with other datasets is that the GMH complemented the CINEP data, with multiple sources including the Interamerican Commission of Human Rights, the Permanent Committee for the Defense of Human Rights, and importantly, official confessions from paramilitaries given to prosecutors as part as their demobilization process, among other publications and NGOs.[29] The information was compared across the sources by the GMH to avoid duplication. A more extensive number of sources and in particular the fact that members of one of the armed groups provided information regarding their crimes could reduce underreporting. A separate difference with the dataset of Restrepo, Spagat and Vargas (2004) is that the GMH datasets allows us to separate victims by the type of attack, a feature that is used in our analysis of selective and non-selective victimization. For example, a bomb that explodes and kills civilians will be classified as a terrorist attack while civilian fatalities in a clash between two groups would be included in the clashes dataset. The original CINEP data as well as the dataset in Restrepo, Spagat and Vargas (2004) are both more comprehensive in other aspects of the conflict like combatants' fatalities, and number of captured and injured combatants. The CINEP data also includes information on other forms of victimization like threats.

[29] For the full list of sources see (GMH 2013).

E Robustness Checks

E.1 Presence Criteria

We first consider the robustness of our results to alternative sample inclusion criteria. As described in the text, we use the CEDE dataset to identify municipalities in which armed groups operate but do not engage in systematic violence against civilians according to the Grupo de Memória Histórica information. Although our temporal aggregation should reduce the noise in the data, it is still possible that we have excluded municipalities where groups operated undetected or included those where groups tended not to operate but were detected in a one-off incident.[30] This can potentially bias our estimates. In this section, we examine these issues with two robustness checks. In the first one, we address potential under-inclusion by adding municipalities with only one group present to the baseline sample. To estimate our model in this case, we impute the beliefs of the group that is present about whether its rival would victimize civilians if it were present as well. In the second exercise, we vary our threshold on the number of incidents in the CEDE data that determine whether a group is considered to be present or not.

Recall that in our main analysis, we code an armed group as present in a municipality-period if the average number of CEDE incidents involving the group is above the pooled sample median or if the group engaged in civilian victimization there. With this coding, we identify 402 municipality-periods where only the FARC entered and 193 municipality-periods where only the AUC did. Our baseline sample excludes these observations, but in the first robustness exercise, we add them by imputing first-stage beliefs. If group i enters a municipality-period but its rival $j \neq i$ does not, we first assume i believes its rival j uses violence with probability $\frac{1}{2}$. That is, the group has maximum uncertainty about what its rival does. We also explore the scenario where i believes j uses violence with probability equal to j's pooled propensity to use violence, as measured in Table 1. These exercises add considerable noise to the estimation of the groups' best responses because, in more than half of the new sample, we are estimating the entering group's beliefs about a rival that does not in fact enter.

With the expanded sample and the two methods of imputed beliefs, we re-estimate the model, and the first four rows of Table A4 report the strategic interdependence parameters. When compared to Table 2, the expanded samples indeed attenuate the strategic complements in violence decisions, but the coefficients are still positive and significant at conventional levels. Consequently, we are confident saying that our main finding—that strategic incentives drive violence that would not otherwise occur—is not an artifact of our sample selection criteria.

In the second robustness check, we explore stricter or looser criteria of presence. Specifically, we examine requiring the average number of CEDE incidents to be above the 75th or 25th percentile in the sample, in contrast with the 50th percentile threshold in our main analysis. In loosening the threshold, we account for the possibility that low levels of reported group activity actually reflect a group's dominance (Kalyvas 2006). As noted by Ch et al. (2018), presence without reports of illegal activities is unlikely to occur in long periods of time because challenges by other groups eventually arise as well as opportunities to exploit that dominance by breaking the

[30] Using yearly data and survey based information in a subsample of municipalities, Arjona and Otálora (2011) find that indicators of presence based on the CEDE information underestimate presence of both FARC and AUC. Ch et al. (2018), however, show significant correlations of CEDE indicators of presence with measures based on areas where the groups demobilized for the periods 2007–2010 for the FARC and 1997–2002 for the AUC.

	Exercise	α_i	SE	t	p-value
Including single-entrant municipality-periods					
FARC	Random beliefs	1.20	0.50	2.40	0.02
AUC	Random beliefs	1.47	0.49	3.02	<0.01
FARC	Mean beliefs	1.16	0.60	1.95	0.05
AUC	Mean beliefs	1.85	0.63	2.93	<0.01
Alternative entry criteria					
FARC	75% percentile	2.39	0.87	2.76	<0.01
AUC	75% percentile	2.45	0.87	2.81	<0.01
FARC	25% percentile	1.44	0.48	3.01	<0.01
AUC	25% percentile	1.96	0.65	3.03	<0.01
Alternative threshold of victimization					
FARC	25% percentile	1.46	0.66	2.21	0.03
AUC	25% percentile	1.94	0.71	2.74	<0.01
Alternative first-stage model					
FARC	Leave-one-out	1.89	0.93	2.04	0.04
AUC	Leave-one-out	2.34	1.01	2.32	0.02

Table A4. Robustness of the strategic spillover parameter estimates across four alternative sample selection rules, first-stage estimates, and alternative definition of victimization. Standard errors are estimated from the outer product of gradients.

law. Nevertheless, our main results persist under these alternative criteria—see the middle four rows of Table A4. Even when municipality-periods where both groups are choosing not to victimize are eliminated by adopting a much more stringent requirement to classify groups as entering the municipality (CEDE incidents above the 75th percentile), we still find significant strategic complements. The strategic complements are also maintained if we include municipalities where one group's dominance might induce the other not to engage in many illegal activities captured by the CEDE indicators. In general, the coefficients reported in Table 2 are on the more conservative side.

E.2 Threshold of Victimization

Recall that in the baseline results we code victimization as occurring if the fraction of civilians killed intentionally by the group (victims of massacres, selective killings, or terrorist attacks) out of the total of the group's victims is greater or equal than the sample median in a given period. Formally, the indicator is coded as 1 if $\frac{\sum_{a\in C} K^i_{m,t,a}}{\sum_a K^i_{m,t,a}}$ is above or at the median of these fractions across municipalities and groups, where $K^i_{m,t,a}$ is the number of civilians killed by group i in the period t in municipality m in a type of attack a and C is the subset of types of attacks in which the intention was to kill civilians (massacre, selective, terrorist). Our conclusions still hold, however, if we define victimization as occurring when the fraction of civilians killed intentionally by the group in a given period is greater or equal than the 25th percentile. The results are reported in Table A4. Given that the median of the sample is already 1 (all killings are intentional) in both periods, the

results will be identical as those reported in the paper if one was to use a higher percentile than the median to define the victimization threshold.

E.3 First Stage Estimates

We also would like to ensure that our finding of strategic complements is not driven by revenge dynamics or other non-strategic behavior within a municipality. Specifically, we want to ensure that our first-stage estimates of victimization probabilities only capture *ex ante* strategic expectations, not *ex post* observed violence within the given municipality. To this end, we recalculate our choice probability estimates using a leave-one-out procedure: to estimate each municipality's victimization probabilities, we take out-of-sample predictions from a model trained using only the data from outside that municipality. This rules out the possibility that our estimated *ex ante* probability of victimization is mistakenly picking up realized victimization within the given municipality. The estimated strategic complements in this robustness check are even stronger than those in the baseline model, as shown in the bottom rows of Table A4. This reinforces our claim that the correlation between FARC and AUC victimization is driven by strategic expectations in a competitive process.

The procedure for generating the first-stage estimates of choice probabilities is as follows. Remember that our goal in the first stage is to consistently estimate p_i^{mt}, the probability of victimization by group i in municipality m during period t. For each municipality m' in our data (441 total), we extract the subset of observations from other municipalities, i.e., in which $m \neq m'$. With this subset of data excluding observations from m', we train random forest models (one for FARC, one for AUC) to predict victimization as a function of the same set of covariates as in the baseline model.[31] Finally, for each time period t at which municipality m' enters our data, we let our first-stage estimate $\hat{p}_i^{m't}$ equal the out-of-sample prediction from our model of group i. Therefore, our first-stage estimate $\hat{p}_i^{m't}$ is not even partially a function of realized violence $v_i^{m't}$, as the model from which it is computed does not "see" data from within municipality m'.

Given the computational intensity of this procedure, resampling-based inference like the bootstrap is infeasible. We thus report nominal standard errors in Table A4. In order to conclude that the strategic spillover parameters were statistically insignificant at the 0.05 level, the true standard errors would have to be 1.41 times the nominal ones in case of the FARC, or 1.30 times in case of the AUC.

E.4 Control Specification

Finally, we explore the robustness of our findings to changes in the controls used in the players' utility functions. Our ability to identify the strength of strategic interdependence resides in isolating characteristics of the municipalities that could make different non-state armed actors treat civilians in the same way. Although our baseline specifications covers key demographic, economic, and political determinants of violence, as well as time-invariant region characteristics, it is possible there are still sources of unobserved heterogeneity driving our findings. To address this, we estimate a model in which we replace the region intercepts with state intercepts, a finer-grained geographical grouping. A drawback of this specification is that by focusing on within-state comparisons, the

[31] Our baseline specification uses kernel regressions rather than random forests. We use a random forest here due to technical problems extracting out-of-sample predictions from the kernel regression model.

explanatory power of the distance to an armed group's early place of influence is necessarily reduced. With this caveat in mind, Table A5 below presents the parameters of interest, showing that there are still strategic complements in the use of violence. We also estimate a model that includes interaction terms of the period 2002–2005 dummy with all time-invariant municipality characteristics, finding substantively similar results. Finally, to rule out the possibility that overall intensity of conflict is generating the observed patterns, we estimate a model that controls for such intensity at the beginning of the period. Because overall conflict intensity is a function of victimization, the specification implicitly assumes that the groups do not take into account how their actions in one period affects their future actions. With this important caveat in mind, we still find very similar estimates. We also see that initial intensity of conflict is not significant for any of the groups, indicating that the baseline specification captures the main determinants of victimization.

Table A5. Strategic victimization (alternative specifications)

	FARC	AUC
Model 1. State effects		
Rival's victimization probability	1.244***	1.223***
	(0.337)	(0.416)
Distance group's place of origin	-0.002	0.002
	(0.002)	(0.002)
Log likelihood		-600.37
Model 2. Time invariant controls interacted with period 2002-2005 dummy		
Rival's victimization probability	1.544***	1.937***
	(0.357)	(0.506)
Distance group's place of origin	-0.004**	0.0002
	(0.002)	(0.001)
Log likelihood		-644.57
Model 3. Initial intensity of violence as control		
Rival's victimization probability	1.447***	2.025***
	(0.353)	(0.488)
Distance group's place of origin	-0.002**	0.002**
	(0.001)	(0.001)
Initial violent incidents	0.015	0.008
	(0.01)	(0.012)
Log likelihood		-654.26

This table presents maximum likelihood estimates of the parameters of the civilian victimization model. Model 1 includes state intercepts and baseline controls. Model 2 includes region intercepts, baseline controls, and interactions of the period 2002-2005 dummy with all the time invariant controls. Model 3 includes the sum of all violent incidents involving the FARC, paramilitaries, and ELN at the beginning of the period. All models use 611 observations. Bootstrapped standard errors are in parentheses. *** p<0.01, **p<0.05, *p<0.1.

Table A6. Strategic and Bivariate Normal Models Comparison

Voung test (p-value)	Clarke Test (p-value)	AIC		BIC	
		Strategic	Binormal	Strategic	Binormal
0.634	0.037	1383.91	1385.40	1534.02	1531.10

The null hypothesis in Voung and the Clarke test is that the models are equivalent, $H_0 : E_0[ln(f/g)] = 0$, where E_0 denotes expectations over the true data generating process, f the likelihood of the strategic model, and g that of the bivariate normal. The alternative hypothesis is $H_f : E_0[ln(f/g)] > 0$. The model with the smallest AIC and BIC is preferred.

E.5 Non-Nested Model Comparison

We execute a comparison of our model with a simple bivariate normal model that effectively assumes victimization choices are not strategic (i.e., do not depend on the expectations of the other group's actions), but that allows the unobserved determinants of victimization of each group to be correlated with each other. For the bivariate normal model we include contextual variables and the distance to the group's areas of influence as determinants of victimization choices. Table A6 reports the results of this comparison. While the Voung test of non-nested models does not discriminate between the two and the BIC favor the bivariate probit, the Clarke test, and the Akaike Information criterion favor the strategic model. The Clarke test has been shown to perform better than the Vuong test when the empirical distribution of individual log-likelihood ratios has a high kurtosis coefficient (Clarke 2007), as is the case here where we find a kurtosis of 25.02.

E.6 Municipality-Year Results

In this appendix we assess the robustness of the results to choosing as unit of observation the municipality-year rather than the municipality-period.[32] Before we present these results, we discuss some limitations of this approach. In addition to the potential attenuation caused by mismeasurement described in the main text, having temporally more dissagregated observations introduces stronger time dependence. For example, if a group uses violence against civilians in one municipality-month, this may alter its costs and benefits of using violence in the same municipality but in the next month. While a common solution to account for this dependence is to include lags of the dependent variables, in our structural framework, however, such an approach would treat past endogenous actions as exogenous covariates and would imply that the groups do not take into account how their actions in one month affect payoffs in future months. This is an unappealing assumption given our view on the strategic nature of the AUC and FARC.

With these caveats in mind, Table A7 replicates the baseline analysis with municipality-year observations. Here, we see that the strategic interdependence parameters are positive and significant at conventional levels. Even though we still find evidence of strategic complementarities, the strategic coefficients are smaller, a finding that is consistent with the having a noisier measure of victimization with the municipality-year data. Figure A3 replicates the analysis of different types of violence against civilians. It shows that the strategic complementarities in the use of civilian

[32]We include the same control variables as in our baseline specification. Population and oil royalties are available every year at the municipality level. For the liberal party vote share, we used the one from the most recent election. For other controls that vary over time but that are not available every year, we use interpolations.

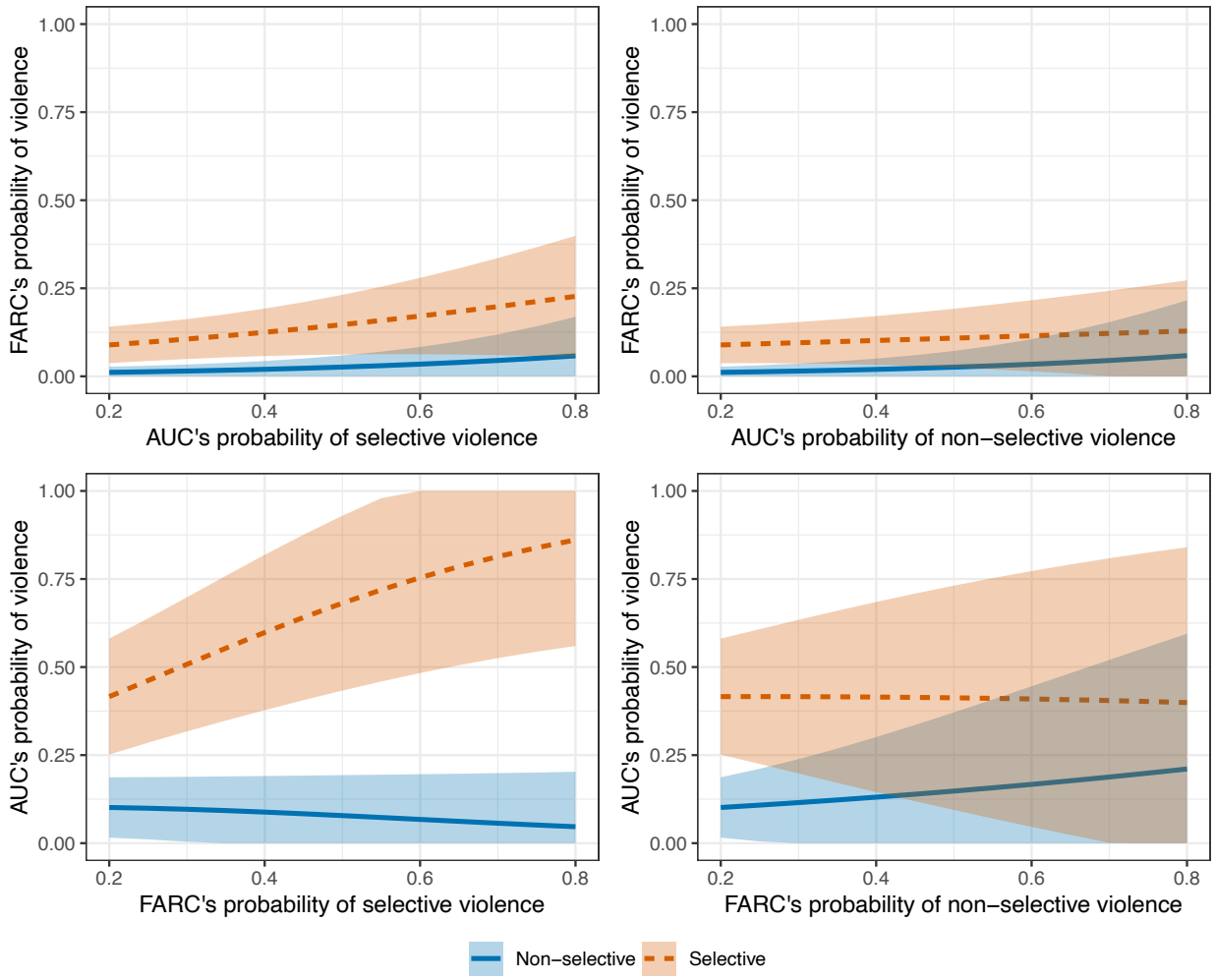
victimization are driven by selective victimization. When including the ELN as a third player, no strategic substitution between the ELN and the FARC violence is evident (Figure A4).

Table A7. Strategic Victimization (municipality-year)

	FARC	AUC
Panel A. Strategic factors: α_i		
Rival's victimization probability	**1.242 (0.213)**	**1.008 (0.181)**
Panel B. Controls		
Coca area	0.01 (0.084)	0.098 (0.081)
Distance army base	-0.001 (0.001)	-0.0004 (0.001)
Distance group's place of origin	**-0.002 (0.001)**	0.001 (0.001)
Distance Magdalena river	0.001 (0.001)	**-0.003 (0.001)**
Gini	1.19 (1.145)	0.951 (0.799)
ln(Population)	**0.186 (0.088)**	**0.44 (0.089)**
Poverty	**0.024 (0.005)**	-0.001 (0.004)
Oil royalties	0.888 (0.795)	**-1.174 (0.571)**
Liberal party vote share	0.011 (0.017)	0.023 (0.015)
Variation Liberal party vote share	0.942 (1.846)	**4.611 (1.517)**
Log-likelihood	-1363.05	
Observations	1318	

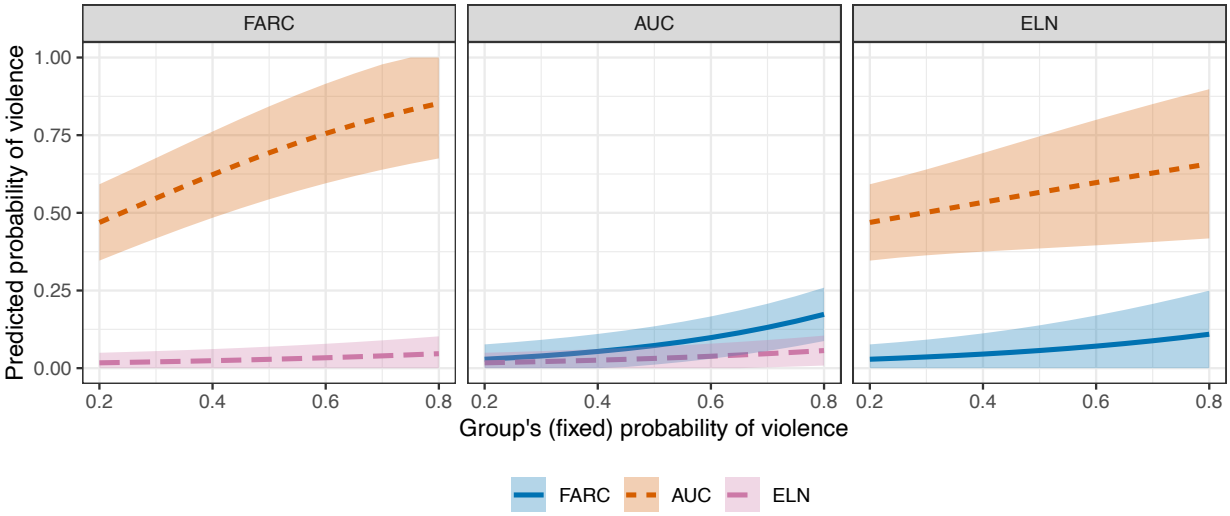
Estimates of the payoff parameters in the victimization model using municipality-year observations. The model includes region intercepts. Bootstrapped standard errors are in parentheses. Estimates in bold are statistically significant with $p < 0.05$. The results should be compared to Table 2 which uses municipality-period observations.

Figure A3. Strategic interdependence distinguishing selective and non-selective victimization (municipality-year).



Notes: Using municipality-year observations, we reestimate the model with two types of violence (selective and non-selective), and then use estimated model to predict how i's propensity of each violence type varies as it expects more or less violence from its rival $-i$. The results should be compared to Figure 1, which uses municipality-period observations.

Figure A4. Strategic interdependence adding the ELN as a third group (municipality-year).



Notes: Using municipality-year observations, we reestimate the model with three groups (FARC, AUC, and ELN), and then use estimated model to predict how i's propensity of each violence type varies as it expects more or less violence from its rival $-i$. The results should be compared to Figure 2, which uses municipality-period observations.

F Expanded Model

In this section, we expand our baseline model to incorporate an arbitrary number of groups and types of victimization. This version of the model covers the additional analyses in Section 6.

There are G groups, indexed by $i = 1, \ldots, G$. Each group simultaneously chooses a type of civilian victimization $v_i \in A_i = \{0, 1, \ldots, K\}$. Here, we interpret $v_i = 0$ as the choice not to victimize and $v_i > 0$ as a choice to employ victimization with type v_i. For example, in Section 6.1, we would set $K = 2$, where $v_i = 1$ represents selective victimization and $v_i = 2$ represents non-selective.

Payoffs are as follows:

$$u_i(v_i, v_{-i}, \epsilon_i) = \mathbf{x}_i \cdot \beta_i^{v_i} + \sum_{j \neq i} \alpha_{i,j}^{v_i, v_j} + \epsilon_i(v_i). \tag{A5}$$

In the above equation, $\beta_i^{v_i}$ captures the impact of contextual variables $\mathbf{x}_i$ on group i's payoff from choosing action $v_i \in A_i$. In addition, $\alpha_{i,j}^{v_i, v_j}$ captures the impact of group j's victimization choice of $v_j \in A_j$ on i's payoff from choosing $v_i \in A_i$.

As in the baseline model, we normalize the *ex ante* expected utility (i.e., before the stochastic shock) for no victimization to zero. That is, $\beta_i^0 = 0$ and $\alpha_{i,j}^{0, v_j} = 0$ for all i, all $j \neq i$, and all $v_j \in A_j$. In addition, we normalize $\alpha_{i,j}^{v_i, 0}$ to zero for all i, all $j \neq i$ and all $v_i \in A_i$, which is also carried over from the baseline model in Equation 1. Essentially, we can only identify the effect of j's victimization choice on i's utility relative to a baseline action v_j, where we use not committing violence $v_j = 0$ as the relative baseline action.

Collect the to-be-estimated payoff parameter in θ. As above, equilibria can be represented as choice probabilities satisfying a rational expectations condition. Let $p_i(v_i)$ denote the probability that Group i chooses $v_i \in A_i$. Let $\Psi_i(v_i, p_{-i}; \mathbf{x}_i, \theta)$ denote the corresponding best-response probabilities:

$$\Psi_i(v_i, p_{-i}; \mathbf{x}_i, \theta) = \int \mathbb{I}\left[v_i = \arg\max_{a_i \in A_i} \left\{ \mathbf{x}_i \cdot \beta_i^{a_i} + \sum_{j \neq i} \sum_{v_j \in A_j} \alpha_{i,j}^{a_i, v_j} p_j(v_j) + \epsilon_i(a_i) \right\} \right] dF(\epsilon_i)$$

An equilibrium is a vector of choice probabilities $(p_1, \ldots, p_G)$ such that for all i and all $v_i \in A_i$ we have

$$\Psi_i(v_i, p_{-i}; \mathbf{x}_i, \theta) = p_i(v_i).$$

When the action-specific payoff shocks $\epsilon_i(v_i)$ are drawn i.i.d. from the type-one extreme value distrubtion, $\Psi_i(v_i, p_{-i}; \mathbf{x}_i, \theta)$ takes the form:

$$\Psi_i(v_i, p_{-i}; \mathbf{x}_i, \theta) = \frac{\exp\left\{ \mathbf{x}_i \cdot \beta_i^{v_i} + \sum_{j \neq i} \sum_{v_j \in A_j} \alpha_{i,j}^{v_i, v_j} p_j(v_j) \right\}}{\sum_{a_i \in A_i} \left[\exp\left\{ \mathbf{x}_i \cdot \beta_i^{a_i} + \sum_{j \neq i} \sum_{v_j \in A_j} \alpha_{i,j}^{a_i, v_j} p_j(v_j) \right\} \right]}.$$

Additional References

Alonso, Manuel A. 1997. *Conflicto armado y configuración regional: El caso del Magdalena Medio*. Medellin, Colombia: Universidad de Antioquia.

Arjona, Ana M. and Laura Otálora. 2011. "Presencia vs. violencia: problemas de medición de la presencia de actores armados en Colombia.". http://focoeconomico.org/2011/12/20/presencia-vs-violencia-problemas-de-medicion-de-la-presencia-de-actores-armados-en-colombia/.

Bejarano, Jesús A. 1997. *Colombia : inseguridad, violencia y desempenño econoómico en las áreas rurales*. Bogotá, Colombia: Universidad Externado de Colombia.

Carlos Medina Gallego. 2009. *FARC EP. Notas para una historia política 1958-2008*. Bogotá, Colombia: Universidad Nacional de Colombia facultad de derecho ciencias políticas y sociales.

Centro Nacional de Memoria Histórica. 2014. *Guerrilla y población civil. Trayectoria de las FARC 1949-2013*. Bogotá, Colombia: Imprenta Nacional de Colombia.

Ch, Rafael, Jacob Shapiro, Abbey Steele and Juan F. Vargas. 2018. "Endogenous Taxation in Ongoing Internal Conflict." *American Political Science Review* 112(4):996–1015.

Clarke, Kevin A. 2007. "A Simple Distribution-Free Test for Nonnested Model Selection." *Political Analysis* 15(3):347–363.

Ellickson, Paul B. and Sanjog Misra. 2011. "Estimating Discrete Games." *Marketing Science* 30(6):997–1010.

Gibilisco, Michael and Sergio Montero. N.d. "Do Major-Power Interventions Encourage the Onset of Civil Conflict? A Structural Analysis." *Journal of Politics*. Forthcoming.

González, José J. 1991. "Espacio, sociedad y conflicto en Colombia. Las repúblicas independientes en Colombia 1955-1965." *Revista UIS Humanidades* 20(1):67–75.

Hotz, V. Joseph and Robert A. Miller. 1993. "Conditional Choice Probabilities and the Estimation of Dynamic Models." *The Review of Economic Studies* 60(3):497–529.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge University Press.

McKelvey, Richard D. and Thomas R. Palfrey. 1995. "Quantal Response Equilibria for Normal Form Games." *Games and Economic Behavior* 10(1):6–38.

Molano, Alfredo. 2015. Fragmentos de la Historia del Conflicto Armado 1920-2010. In *Conflicto social y rebelión armada en Colombia. Ensayos críticos*. Bogotá: Gentes del común.

Ugarriza, Juan Esteban and Nathalie Pabón Ayala. 2017. *Militares y guerrillas. La memoria histórica del conflicto armado en Colombia desde los archivos militares 1958-2016*. Bogotá, Colombia: Editorial Universidad del Rosario.

Verdad Abierta. 2019. https://verdadabierta.com/. Accessed: 2019-02-04.