

Policy Preferences after Crime Victimization: Panel and Survey Evidence from Latin America*

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Abstract

In multiple countries, policy preferences tend to be explained by citizens' partisanship. Can these preferences be modified by particular negative events, such as being a crime victim? It is difficult to assess the political effects of crime, mainly because of the presence of unmeasured confounders. I use panel data from Brazil and strategies for reducing sensitivity to hidden biases to study how crime victims update their policy preferences. Additionally, I use survey data from 18 Latin American countries to improve the external validity of the findings. I show that crime victims are more likely to support strong-handed measures to reduce crime, such as allowing state repression, but without modifying their party identification. This reveals that (i) crime can change what people think the state is allowed to do, which can have important political implications; and that (ii) citizens can have flexible policy preferences that are not necessarily shaped by their partisanship.

Keywords: Policy Preferences, Partisanship, Crime Victimization, Panel Data, Hidden Biases

1 Introduction

In multiple countries, citizens' policy preferences tend to be linked to their party identification or ideological disposition. For example, left-wing voters are more likely to support welfare policies (Shapiro, 2009) and redistribution (Alesina and Giuliano, 2009), while right-wing citizens have a higher probability of focusing on crime issues (Mayer and Tiberj, 2004). Yet there is evidence that short-term events, such as terrorist attacks and unemployment, can affect citizens' policy preferences (Berrebi and Klor, 2008; Margalit, 2013). This paper answers two different questions. First, can crime victimization increase support for strong-handed or iron-fist policies to reduce crime, such as allowing state repression? Second, if the answer to the previous question is affirmative, is crime victimization modifying voters' policy preferences by changing their party identification or through a different causal mechanism?

The application of strong-handed measures to fight crime is associated with military policing and the erosion of procedural guarantees (Holland, 2013). Accordingly, these policies represent a statement about what the state can and cannot do to provide greater security. Studying citizens' preferences regarding crime is particularly important in contexts where delinquency is common, where politicians may exploit populist strategies to improve their electoral performance, and where the police have been involved in human rights abuses.

Latin America is one of the most violent regions in the world (UNODC, 2013). 43 of the world's 50 most dangerous cities are located in Latin America, even though this region represents less than 8% of the world's population (Magaloni, Franco and Melo, 2015). As a consequence, survey respondents tend to highlight crime as one of the most critical issues their countries face (Perez, 2015). The fear of crime has boosted the popularity and support of politicians who base their platforms on tough measures to combat crime (Azpuru, 2003). These iron-fist policies have been implemented in different Latin American countries, and are tied to extralegal detention, arbitrary punishment, and the military-style occupation of entire neighborhoods (Dammert and Malone, 2006).

The problems associated with crime are highly visible in the largest country in the region, Brazil, where the homicide rate in 2006 was 29.2 per 100,000 inhabitants, making it the third most violent country in Latin America after El Salvador and Venezuela (Carreras, 2013). These statistics have not improved in recent years, and "no country in the world has more cities plagued by violent crime than Brazil" (Rapoza, 2016). In Rio de Janeiro alone, the increase of property crimes between 1995 and 2003 was 122 percent (Bergman, 2006). This social context of insecurity and violence has been exploited by populist candidates who promise to bring "authority" back when fighting crime; this pattern was evident in the 2016 local elections (Winter, 2016). The Brazilian military police have been associated with the perpetration of human right abuses and extrajudicial and summary executions (Huguet and Szabó de Carvalho, 2008). More examples of police misconduct in Brazil include unwarranted searches, beatings, and torture (Arias, 2006; Magaloni, Franco and Melo, 2015).

It is important to better understand the political effects of crime in violent regions like Latin America. Previous studies have shown that crime can decrease victims' support for democracy (Merolla, Mezini and Zechmeister, 2013), increase political participation (Bateson, 2012), and undermine incumbents' share of the vote (Marshall, 2015). We do not know, however, much about whether crime can modify victims' policy preferences and what they think the state is allowed and not allowed to do to protect them.¹

It is challenging to address this research question for four main methodological reasons. First, being a crime victim is not a random event. Particular social circumstances can be correlated with crime victimization, generating a serial victimization problem. In other words, previous crime victims might be more likely to be crime victims again. Consequently, when using survey data it is hard to know if victimization is a unique event in a respondent's life or a common negative situation (Bateson, 2012). This problem can introduce biases, since the previous treatment status can affect the outcome (e.g. serial victims might get used to crime). Second, there might be a reverse causality problem. People who want strong-handed policies might be more likely to report

¹Bateson (2012) mainly focuses on the impact of crime on political participation, but she also provides evidence about how crime correlates with support for vigilantism and authoritarianism.

a crime as a way to increase crime statistics and push for the implementation of those policies. Third, in any observational study the presence of hidden biases is a significant issue. Victims and non-victims can differ across multiple unobserved characteristics. This is particularly true when we use a national sample and compare individuals from different cities and, therefore, from diverse socioeconomic contexts. Finally, and related to the previous issue, neighborhood effects can be crucial (Bateson, 2012). Some sectors or areas within a city might be more or less secure, affecting the probability of being a crime victim. This point is particularly salient when analyzing data from multiple countries or from diverse cities or states within a country. Crime has a very local nature, and neighborhood characteristics are hard to adjust for.

In this paper I pay careful attention to study design to address each of these concerns. I use panel data from two cities in Brazil (Baker, Ames and Renno, 2006; Baker et al., 2015) to compare crime victims and unaffected respondents. I focus on individuals who were not crime victims in the previous wave to decrease the problems associated with serial victimization and reverse causation. Additionally, I reduce sample heterogeneity to decrease sensitivity to hidden biases (Rosenbaum, 2005, 2011) by comparing citizens from the same neighborhoods.

I use recent developments in optimal matching and mathematical programming to generate comparable groups of victims and non-victims that are similar on 48 pretreatment covariates. When using matching, there can be concerns about pruning observations to achieve balance. Based on this issue, I construct the largest representative matched sample using the `designmatch` package for R (Zubizarreta and Kilcioglu, 2016). Put simply, the matched groups obtained are not only balanced, but also similar to the unmatched sample. Moreover, I use survey data from 18 Latin American countries to improve the external validity of the findings obtained using panel data.

I show that crime victims are 7 percentage points more likely to support strong-handed policies to reduce crime, such as state repression, than non-victims. A possible causal mechanism explaining these results is the lower support for democracy generated by direct exposure to crime. As a consequence, victims are more willing to tolerate strategies that imply the erosion of basic rights. A second possible mechanism is that voters are strategically supporting parties that are "tough

on crime," and as a result, are updating their policy preferences. The evidence shows that crime victimization undermines citizens' support for democracy but does not affect their partisanship. Victims keep their party identification but have flexible policy preferences.

Voters' willingness to accept non-democratic measures, such as repression, can have critical consequences for the quality of democracy. Support of iron-fist policies can inform politicians about citizens' tolerance for human right abuses by the state. This issue becomes even more relevant because voters' policy preferences can actually shape the adoption of policies (Brooks and Manza, 2008; Lupu and Pontusson, 2011). Consequently, understanding the factors that influence citizens' policy preferences regarding crime is crucial.

This article provides four main contributions to the existing literature. First, it adds to a growing body of research that studies the political effects of crime (Bateson, 2012; Kronick, 2014; Marshall, 2015); and in particular, it focuses on support for iron-fist policies. They delineate the limits of the state and what it is allowed to do to ensure public security. Therefore, it is crucial to understand the factors explaining voters' support for these measures. Second, it provides novel evidence about the causal mechanisms explaining voters' new policy preferences. In particular, it shows how the reduction of democratic values among victims might lead them to support particular measures such as repression. Third, it dialogues with studies of how negative events can affect voters' policy preferences. Negative shocks, such as crime, economic crises, and natural disasters, are common situations in the developing world, and constantly deteriorate citizens' living conditions. For example, voters might also update their policy preferences after being a disaster victim, but probably in a different direction. Finally, it contributes to the discussion about the importance of study design for reducing sensitivity to unmeasured factors and model dependence.

2 Crime Victimization and Political Outcomes

Crime victimization has clear psychological effects on victims, such as increasing their levels of anger, fear, and sadness (Greenberg and Ruback, 2012). However, it can also have important

political and electoral implications.

A significant number of studies have attempted to determine if crime affects incumbents' share of the vote. According to the theory of retrospective voting, crime victims will sanction the government in the consecutive elections. Similar arguments have been used to study how economic conditions affect voters' electoral decisions, and if citizens reward or sanction incumbents based on economic perceptions ([Lewis-Beck and Stegmaier, 2000, 2007](#)). There is mixed evidence regarding the effects of crime on aggregate electoral results. [Cummins \(2009\)](#) analyzes gubernatorial elections in the US from 1986 to 2004. He finds that crime has a large impact on state but not on national elections, and that this effect is greater in states with a more educated population.

In the case of Latin American countries, [Marshall \(2015\)](#) shows that voters punish the government for local homicides in Mexico, depending upon whether they consume information. Conversely, [Perez \(2015\)](#) finds, using survey data from the AmericasBarometer, that crime victimization does not affect voters' electoral decisions; however, perceptions of high levels of insecurity do impact respondents' political choices. [Kronick \(2014\)](#) attempts to reconcile these mixed findings, showing that incumbents can escape electoral punishment under particular circumstances. External factors can decrease political authorities' ability to manage crime. For example, the counternarcotics operations in Colombia had a spillover effect in Venezuela. The author finds that previous to this episode, Venezuelan voters held politicians accountable based on changes in local homicide rates, but during the operations in Colombia, voters stopped punishing incumbents because the origin of the negative events could not be attributed to them.

A natural extension of studying the electoral impact of crime is exploring its effects on political participation. [Bateson \(2012\)](#) argues that crime victims tend to engage more in political and civic activities than non-victims. Using survey evidence from five continents, she shows that the impact of crime victimization on political participation can be compared to five to ten additional years of education. However, [Trelles and Carreras \(2012\)](#) provide a different finding using data from Mexico: they show that criminal violence reduces turnout. Crime victims tend to abandon public participation, such as voting in elections. The authors offer two possible explanations for this

result: either victims may be disenchanted with the political system or they may not willing to risk their personal safety by participating in public places. [Berens and Dallendörfer \(2017\)](#), in contrast, argue that the impact of crime on political participation is conditional to the level of violence.

Crime victimization can also undermine support for and the legitimacy of democracy. This negative link has been supported by multiple studies. [Carreras \(2013\)](#) shows that victimization and high perceptions of violence have a negative impact on support for democracy in Latin America. [Fernandez and Kuenzi \(2010\)](#) find a similar negative correlation between perceptions of public safety and attitudes toward democracy in the region. In a similar vein, [Malone \(2010\)](#) studies how crime affects support for the rule of law in Central America. Finally, [Merolla, Mezini and Zechmeister \(2013\)](#) provide survey and experimental evidence showing that crime reduces support for democracy in Mexico.

The literature has paid less attention to how crime can modify citizens' policy preferences. [Krause \(2014\)](#) studies the link between crime news and support for authoritarian measures in Guatemala. She finds that news about crime reduces trust in government, which increases support for authoritarian strategies of controlling crime. However, this study focuses on the effects of exposure to the news but not on the direct consequences of crime victimization.

In summary, there is systematic evidence about how crime victimization can yield different political outcomes. However, the literature has paid less attention to how this type of negative event can modify victims' policy preferences and what they think the role of the state is in fighting crime. Furthermore, most of the literature based on survey evidence has not adequately addressed relevant endogeneity concerns. For example, because political preferences can influence voters' perceptions of insecurity, the literature might be overstating the political impact of these perceptions. To circumvent this issue, I focus on crime victimization, which should be less endogenous to respondents' electoral choices. Moreover, another problem when using survey data is that the treatment and covariates are measured at the same time, which can lead to potential post-treatment biases. The use of panel data can help address this previous issue.

3 Crime Policy Preferences

Crime-reduction policies can adopt one of two main approaches. The first is based on social policies and emphasizes treatment and rehabilitation, while the second sees crime as a concrete problem that can be solved with effective and strong actions (Estrada, 2004).²

Iron-fist or strong-handed policies can be associated with the latter approach. They represent different direct and tough measures to reduce and fight crime: for example, increasing discretionary rules to detain suspects and militarizing policing. These strategies are a radical form of "penal populism," and in general imply greater repression and the deterioration or dilution of procedural rights (Holland, 2013).

Support for these measures can have crucial political implications, because they refer to the limits of the state's power when fighting crime, and in particular to the boundaries that cannot be transgressed in the attempt to increase security. Moreover, state repression can affect citizens' human rights and erode democratic institutions. The inviolability of citizens' bodily integrity is a basic principle in contemporary democracies that can be undermined by the implementation of iron-fist policies (Fuentes, 2005). In multiple countries in Latin America the state is the main actor involved in human rights violations due to the implementation of military strategies to fight crime (Cruz, 2010).

In contexts of high crime rates, it becomes important to understand whether victimization makes citizens more or less likely to support these different policy approaches. What explains the support for tougher crime-fighting measures? Prior research suggests two main explanations for citizens' attitudes toward these particular policies. The first relies on voters' ideological preferences and/or party identification. The second focuses on how specific circumstances, such as a change in media coverage, can shape voters' policy preferences.³

² Of course, we can also understand these two approaches as a continuum from a total focus on rehabilitation to a total focus on repression.

³ Holland (2013) also mentions a third factor: the role of public opinion in shaping preferences towards strong-handed policies. However, it is possible to merge that third variable with the second one (i.e. how specific circumstances shape policy preferences).

Regarding the first explanation, right-wing voters are more likely to care more about crime than left-wing voters (Mayer and Tiberj, 2004). In a similar vein, Gerber and Jackson (2016) show that right-wing authoritarianism can predict support for punitive measures. Furthermore, the policies that emphasize punitive sanctions tend to be associated with conservative rather than liberal politicians. For example Republican former US president Ronald Reagan summarized his views about how to fight crime by declaring that "here in the richest nation in the world, where more crime is committed than in any other nation, we are told that the answer to this problem is to reduce our poverty. This isn't the answer (...) [The] government's function is to protect society from the criminal, not the other way around" (Beckett, 1999, p.48). Moreover, there is evidence in the US that the proportion of Republican legislators is correlated with imprisonment rates at the state level (Beckett and Western, 2001).

The link between ideology and crime policies is also evident in Latin America. Right-wing candidates in Honduras, Mexico, and Peru have promoted strong-handed policies to combat crime (Cohen and Smith, 2016). In El Salvador, the conservative party ARENA attempted to boost its support in a context of high crime rates by implementing iron-fist policies, such as diluting due process guarantees (Holland, 2013). In the case of Brazil this pattern is also clear, as in the case of the right-leaning former governor of the state of Rio de Janeiro, Marcello Alencar. Alencar decided to provide semi-automatic weapons to the police and to implement a "bravery bonus" to officers who engage in violent confrontations (Magaloni, Franco and Melo, 2015). In summary, right-wing politicians can be linked with these kind of measures to combat crime. Right-wing citizens, similarly, are more likely to support tougher measures to reduce crime and to focus less on social policies.

Nevertheless, it is hard to believe that voters have static policy preferences, particularly when they are exposed to adverse conditions that might affect their priorities and primary concerns. These include negative events that deteriorate victims' living conditions, a common situation in the developing world. For example, Latin American voters are vulnerable to income shocks generated by economic volatility (Murillo and Visconti, 2017), high crime rates (Carreras, 2013), and natural

disasters ([Charvériat, 2000](#)). These adverse conditions might affect the policies citizens would like to see implemented. However, a negative event such as crime victimization can modify victims' policy preferences through different causal mechanisms. In this paper I provide empirical evidence to support the argument that voters have flexible political preferences that respond to their personal circumstances, and to illuminate the causal mechanisms connecting crime and preferences for strong-handed policies.

I argue that crime victimization can have substantive and meaningful effects on victims' policy preferences. This can happen through two different causal mechanisms. First, it can change the value attached to democracy. Second, it can alter voters' partisanship (only if we understand partisanship as a running tally and not as a political identity).

The first mechanism is based on consistent evidence showing that crime can affect victims' democratic values and support for the rule of law ([Merolla, Mezini and Zechmeister, 2013](#); [Carreras, 2013](#); [Krause, 2014](#)). Crime can undermine the legitimacy of the political system ([Cruz, 2010](#)) and increase support for a radical change ([Seligson and Azpuru, 2000](#)). In fact, fear of crime has been connected with support for regimes that reduce civil liberties ([Pérez, 2003](#)). Additionally, there is evidence of a correlation between democratic preferences and support for policies that protect citizens' due process rights ([Seligson, 2003](#)). Consequently, a lower attachment to democratic values might explain why crime victims might be willing to accept the erosion of some basic rights in favor of more punitive measures to combat delinquency in their countries. Civil liberties are directly linked to democratic values and the rule of law, and direct exposure to crime can increase victims' willingness to sacrifice these rights. Simply put, the causal mechanism that connects victimization with the new policy preferences is the lower value attached to the democratic system by victims.

The second mechanism is based on the conceptualization of partisanship as a running tally. Citizens may change their partisanship based on which party will benefit them more ([Achen, 1992](#)). In other words, party identification can be understood as the result of a rational calculation by voters ([Fiorina, 1981](#)). In particular, citizens exposed to crime might think that a specific party can

better address their main concerns, and will update their partisanship accordingly.

Historically, party identification has not been relevant for explaining electoral behavior in Brazil (Ames, 2001), where the party system was candidate instead of party-centered (Mainwaring, 1999). However, that political context has changed in recent decades. For example, Samuels and Zucco (2014, p.2) use survey experiments to study partisanship in Brazil, and find that "exposure to party cues strongly shapes voter opinion." Additionally, Lupu (2015, p.244) uses panel data from Brazil in 2002 to show a "consistent causal effect of partisanship on vote intentions." Lupu (2015, p.228) also uses survey data from AmericasBarometer to provide "evidence that patterns of partisanship in Latin America closely resemble those in advanced democracies." Therefore, Latin America is not a region lacking of partisanship.

Since some parties in Brazil have clear platforms regarding crime, we would expect certain voters to connect party labels with policy outcomes. For example, in 2006 the PSDB presidential candidate, Geraldo Alckmin, focused his platform on topics related to public security and promoted strong-handed policies to combat crime (Ayllón and García, 2006). As a consequence, parties like the PSDB might become more attractive to crime victims.

Party identification can also be understood as a form of social group identification, like religion and social class (Green, Palmquist and Schickler, 2002). In this case, voter attachment to parties should persist over time (Campbell et al., 1960). According to this understanding, a particular crime event should not affect voters in such a deep way as to modify their identities or how they define themselves politically. Results showing that voters are changing their policy preferences but not their party identification can be read as evidence of partisanship being a political identity in Latin America.

Consequently, I will test the impact of crime on the main outcome (i.e. policy preferences) and on two possible causal mechanisms (i.e. support for democracy and partisanship).⁴ I expect to find a substantive and significant effect of crime victimization on policy preferences. I hypothesize that this change is explained either by a lesser degree of support for democratic values or by a new

⁴ This strategy is called a *single-experiment approach* because both the outcome and the mechanisms are captured within the same study (Imai et al., 2011).

party identification.

The study of negative events has been dominated by a retrospective voting approach, whose most common prediction is that victims will punish incumbent candidates. In this paper, however, I focus on the prospective dimension of voters' decisions by paying attention to the policies they most care about after crime victimization: in particular, support of state repression.

4 Research Design

Random assignment is the best strategy for establishing the causal effect of a particular intervention, because treatment assignment is independent of potential outcomes (Morgan and Winship, 2014), and in expectation, observed and unobserved covariates should have similar distributions between treatment and control groups (Bowers, 2011). However, randomization is not always feasible for ethical or practical reasons. The alternative strategy for studying a phenomenon that cannot be randomized, such as crime victimization, is a well-designed observational study structured to resemble a simple randomized experiment (Rosenbaum, 2010), and to use elements from the design-based approach to improve the study design (Keele, 2015). These include focusing on endogeneity (Imbens, 2010), not including final outcome data (Rubin, 2008), and not relying on statistical modeling (Keele, 2015).

What makes an observational study good? Following some of the recommendations provided by Rosenbaum (2010, 2011): first, the treatment should be well-defined. This means that we know when it starts and therefore what the pretreatment and post-treatment covariates are. Second, even though there is no random assignment, the intervention should seem haphazard or not obviously related to potential outcomes. Third, treated and control groups should be comparable: in other words, the distributions of observed covariates should be similar across both groups. Fourth, the design should make use of strategies for reducing sensitivity to unobserved biases, such as decreasing unit heterogeneity. I apply these four previous criteria in the design of this observational study.

Regarding the first recommendation, the main problem when working with survey data is the lack of pretreatment covariates, since adjusting for post-treatment characteristics can introduce biases (Rosenbaum, 1984). Therefore, I use panel data from Brazil collected between 2002 and 2006 (Baker, Ames and Renno, 2006; Baker et al., 2015) to adjust only on covariates captured in waves before respondents were victimized by crime. The survey questionnaire asked a standard battery of questions about political preferences, demographics, media exposure, crime victimization, feeling thermometers, and social networks.⁵ The panel structure allows me to include pretreatment measures of the outcomes, the oldest and most basic tool for reducing the ambiguity of the effect of a treatment in an observational study (Rosenbaum, 2015).

Second, though crime victimization is not randomly assigned, it is possible to exploit certain aspects of the study design to make this situation more haphazard. In particular, I only select respondents that in the wave t were not affected by crime. Then, if in wave $t + 1$ they were crime victims, they are incorporated into the treated group, and if they keep being non-victims they go into the control. Consequently I exclude by design citizens who are serial victims of crime.

The third recommendation emphasizes the need to compare similar groups of exposed and unexposed individuals. I construct these groups by using an optimal matching algorithm that finds the largest representative pair-matched sample that is balanced by design (Zubizarreta and Kilcioglu, 2016). I explain the details of this technique later.

The fourth strategy focuses on decreasing sensitivity to hidden biases by reducing the heterogeneity of the sample. As Rosenbaum (2005) shows, reducing unit heterogeneity implies that larger unobserved biases will be needed to explain away a particular effect. A good example of this strategy are the studies based on identical twins (see Ashenfelter and Rouse (1998)). Consequently, in an observational study it is preferable to focus on more homogeneous and comparable subsets (Keele, 2015) or on natural blocks (e.g. neighborhoods), since unmeasured covariates should be more similar between treated and control groups (Pimentel et al., 2015). The use of national surveys does not help achieve this goal, because they increase the heterogeneity of the

⁵ See the supplementary appendix and Baker et al. (2015) for more details about this panel survey.

sample. Consequently, I exploit the design of the panel data since it focuses only on two mid-sized cities in Brazil: Juiz de Fora in the state of Minas Gerias and Caxias do Sul in Rio Grande do Sul. Both cities have similar characteristics, such as the size of the electorate, their educational and income levels, and racial composition (Baker, Ames and Renno, 2006).⁶ According to the unmatched sample, they also have similar crime rates in wave $t + 1$: 15% of respondents were crime victims in Juiz da Fora, and 14% in Caixas do Sul. Additionally, the data provides neighborhood indicators, which allows me to achieve balance in terms of respondents' geographic location.

How does one go about building a group of affected and unaffected citizens that are balanced in their observed characteristics? One alternative is matching, which attempts to generate a treated and control group with similar covariate distributions (Ho et al., 2007; Stuart, 2010). However, traditional matching techniques, such as propensity score and Mahalanobis distance, do not guarantee covariate balance and in some occasions can even make balance worse across observed covariates (Sekhon, 2009). These methods often involve a process of manually iterating the model until covariate balance is obtained (Hainmueller, 2011). Moreover, a possible concern when using any type of matching technique is that it requires some level of pruning to obtain balance. This means that the matched sample might be different than the unmatched sample.

In the attempt to address these limitations, I use the `designmatch` package developed by Zubizarreta and Kilcioglu (2016), which allows me to find the largest representative sample that achieves covariate balance. This algorithm maximizes the size of the sample that: (i) meets the balance requirements defined beforehand and (ii) is similar to a target sample also defined beforehand (in this case the unmatched sample). Point (i) addresses the limitations of traditional matching techniques because the algorithm directly balances the original covariates without needing to estimate a propensity score. Point (ii), furthermore, means that the samples before and after matching are similar, making pruning less of a concern.

I use mean balance constraints for 47 covariates. The algorithm matches individuals such that the treated and control matched groups cannot differ in their means by more than 0.1 standard de-

⁶ They are different in terms of strength of political parties and salience of ideological cleavages (Baker, Ames and Renno, 2006).

viation from the unmatched sample. As a consequence, the standardized differences between the matched treated and control group cannot be larger than $0.1 * 2$ standard deviation. In other words, the standardized differences between the matched groups cannot be larger than twice the standardized differences between the matched sample (i.e. both matched groups) and the unmatched sample (see [Zubizarreta and Kilcioglu \(2016\)](#) for more details).

All of the mean balanced covariates are ordinal or binary; thus, adjusting their means is a meaningful decision.⁷ I also use fine balance for neighborhood, which implies that both groups will have the same frequency for this covariate but without restricting who is paired with whom ([Rosenbaum, Ross and Silber, 2007](#); [Zubizarreta, 2012](#)). Therefore, I am adjusting for a total of 48 different observed covariates.⁸

In the matching procedure I include covariates that can affect both the treatment assignment and the outcome ([Stuart, 2010](#)). The full list is provided in [Figure 1](#) and in the supplementary appendix, but some of the most relevant respondent characteristics are age, education, gender, ideology, job in the formal sector, media consumption, partisanship, policy preferences, political knowledge, race, and religion. All of these are pretreatment covariates.

The treatment is a binary indicator for being a witness or victim of crime⁹ in wave $t + 1$ (only among a group of respondents who were not witnesses or victims of crime in wave t). The question used to construct the treated and control groups is the following: "Have you been a witness or a victim of crime in the past 12 months? This includes crimes such as assault, robbery, or aggression." Unfortunately, the question does not differentiate between different types of crimes.

The main outcome is a binary indicator of support for the use of strong-handed measures and repression to reduce crime (wave $t + 1$).¹⁰ I also focus on the following outcomes to understand

⁷ In the case of nominal covariates, it is advisable to use other forms of covariate balance (see [Zubizarreta \(2012\)](#); [Resa and Zubizarreta \(2016\)](#))

⁸ See the supplementary appendix for details about the structure of the panel data, and the construction of covariates and outcomes.

⁹ The treatment involves being a crime victim but also being a witness. Even though they are different events, both would be expected to affect voters in similar ways. In the worst case scenario, any effect can be seen as a conservative estimate.

¹⁰ Support for the following statement: "The best way to reduce crime is with repression and an iron fist."

the causal mechanisms: (i) a binary indicator of support for democracy,¹¹ (ii) a binary indicator of identification with the PT;¹² (iii) with the PMDB;¹³ (iv) with the PSDB;¹⁴, (v) and with the PFL.¹⁵

To estimate the effect of crime victimization I use a linear regression with cluster standard errors at the neighborhood level:

$$Y_{it+1} = \alpha + \beta_1 T_{it+1} + \beta_2 P_{it} + \beta_3 X_{it} + \sigma_n + \varepsilon_i \quad (1)$$

Y is a binary indicator that represents the outcome of interest in wave $t + 1$. T depicts the treatment (crime victimization in wave $t + 1$), P describes a pretreatment measure of the outcome from wave t , and X corresponds to a set of pretreatment covariates that might predict the outcome (education and age). σ_n represents neighborhood fixed effects. I also provide the unadjusted estimates to increase transparency (Lin, 2013); this means no controls or fixed effects. Moreover, in the supplementary appendix I use a one-sided Wilcoxon signed rank test statistic as another method of inference since it is less dependent on distributional assumptions, and allows us to conduct the amplification of a sensitivity analysis for hidden biases (Rosenbaum and Silber, 2009).

5 Results Panel Data

The unmatched sample has 1916 subjects in the control group (not crime victims in wave t and $t + 1$) and 320 in the treated group (not crime victims in wave t but crime victims in wave $t + 1$). The matching algorithm will find the largest representative matched sample that fulfills the following criteria: (i) mean balance for 47 covariates between the matched and unmatched sample, (ii) mean balance for 47 covariates between the matched treated and control group, and (iii) fine balance for neighborhood between the matched treated and control group. After optimizing these criteria, the matched sample has 271 subjects in each group, which makes a total of 542 individuals that are similar to the 2236 subjects in the unmatched sample.

¹¹ Support for the following statement: "Democracy is always better than other forms of government."

¹² PT: Partido dos Trabalhadores (Workers' Party).

¹³ PMDB: Partido do Movimento Democrático Brasileiro (Brazilian Democratic Movement Party).

¹⁴ PSDB: Partido da Social Democracia Brasileira (Brazilian Social Democracy Party).

¹⁵ PFL: Partido da Frente Liberal (Liberal Front Party).

Figure 1 shows the standardized differences between the matched and unmatched samples (black dots), and between the matched treated and control groups (gray asterisks). By design, the first standardized differences cannot be larger than 0.1, and the second cannot be larger than 0.2 pooled standard deviations. The dotted lines represent the different tolerances for each comparison. To confirm covariate balance, the gray asterisks cannot be above the gray line, and the black dots cannot be above the black line. The figure shows how these balance requirements are met by default when using the `designmatch` package.

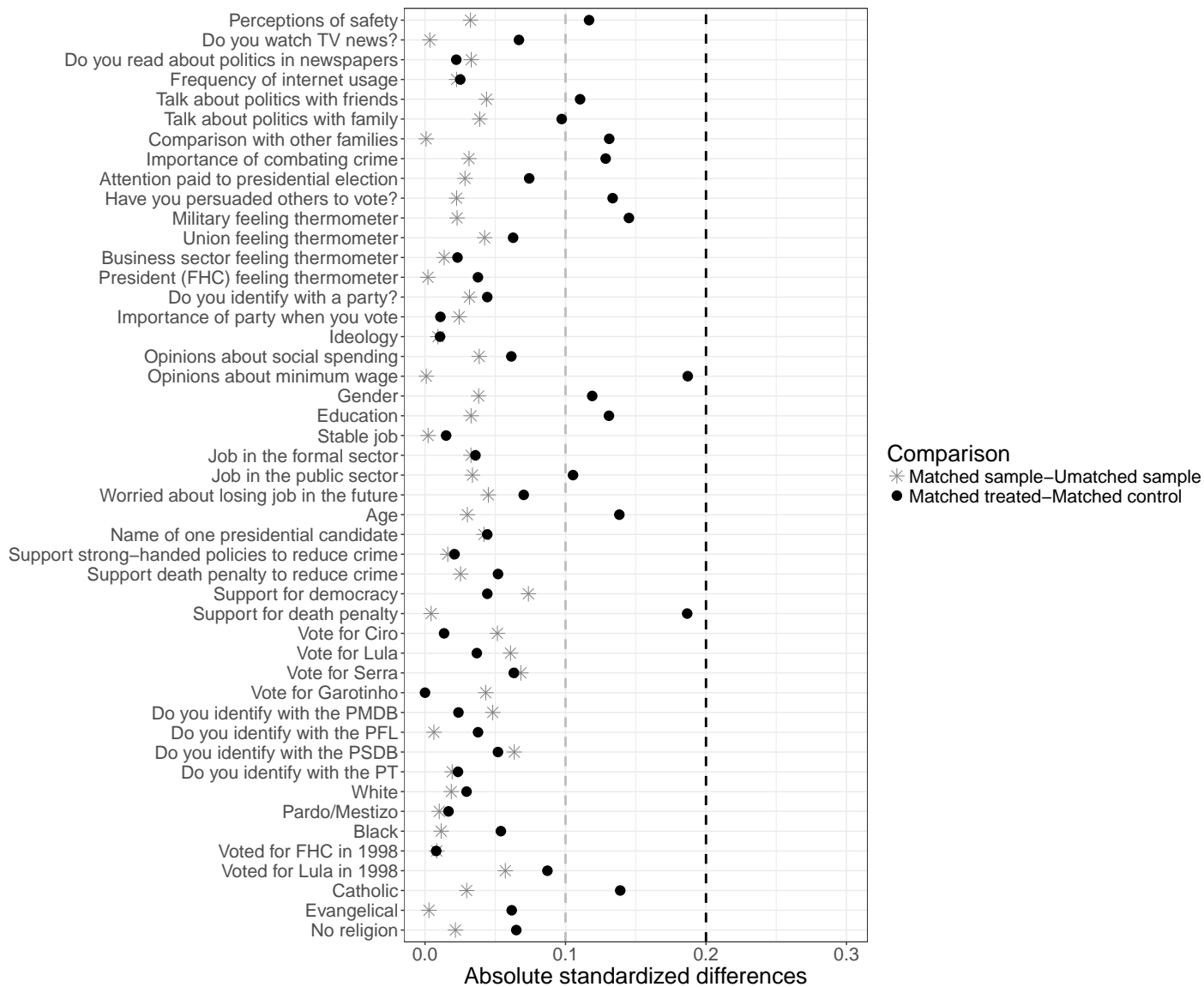


Figure 1: Mean balance

Additionally, I constrain the marginal distribution of neighborhoods using fine balance. This means that the treated and control groups will have the same number of subjects in each neighborhood. However, this balance constraint does not focus on pairing.

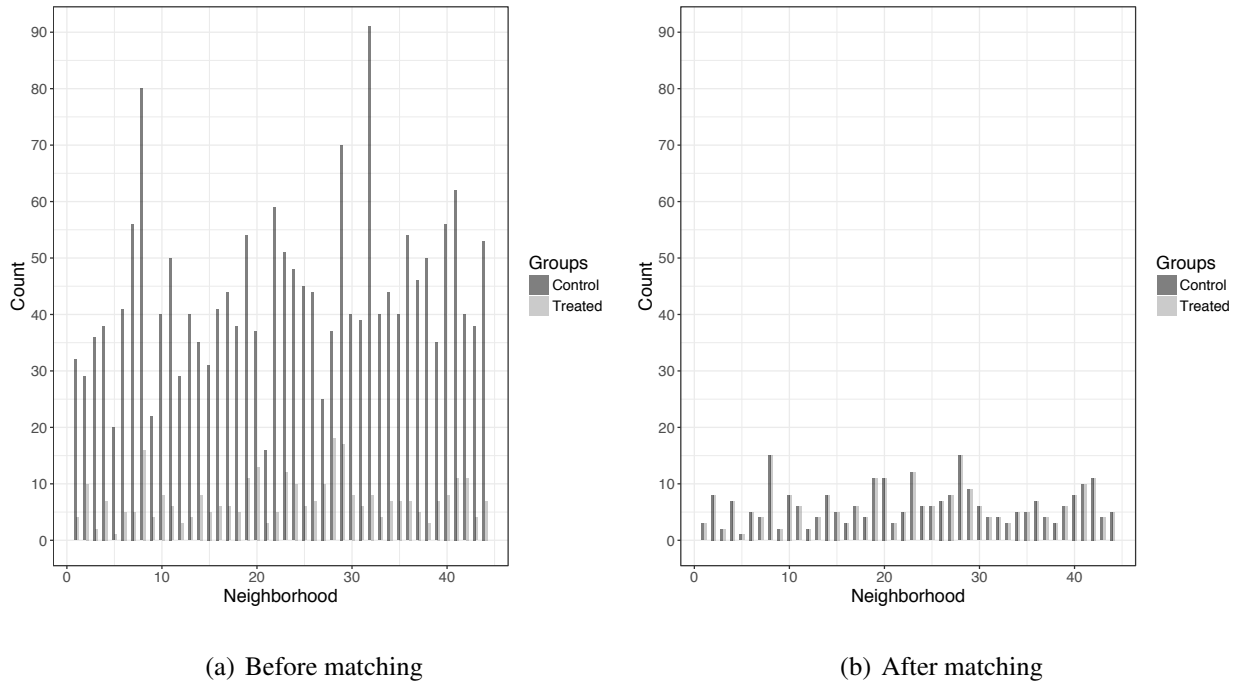


Figure 2: Fine balance for neighborhood

The main outcome of interest is a binary indicator of support for the following statement: "The best way to reduce crime is with repression and an iron fist." The treatment is to be a crime victim in wave $t + 1$ conditional on not being a victim in wave t .

It is also important to confirm that party identification correlates with policy preferences in Brazil, because the causal mechanism that voters will sympathize with the party more likely to benefit them can only be credible if parties are associated with policy outcomes. The evidence shows that parties are not irrelevant in Brazil. 47% of respondents in the matched sample identified themselves with one of the four main parties, and only 24% of participants in the matched sample said that candidates' party affiliation is not relevant for them when making electoral decisions. These numbers are congruent with evidence showing that partisanship has become more salient in this country in recent decades.

Table 1 reports the impact of crime victimization on policy preferences. Columns 2, 3, and 4 provide unadjusted estimates. Column 5 includes a pretreatment measure of partisanship¹⁶ to check if it correlates with preferences about iron-fist policies.

Table 1: Regression results

	Strong-handed policies and repression to reduce crime (wave t+1)				
	(1)	(2)	(3)	(4)	(5)
Crime Victimization	0.070** (0.031)	0.063** (0.031)	0.069** (0.029)	0.063** (0.030)	0.073** (0.030)
PT (wave t)					0.012 (0.031)
PSDB (wave t)					0.237* (0.131)
PMDB (wave t)					0.061 (0.052)
PFL (wave t)					-0.159*** (0.049)
Controls	Yes	No	Yes	No	Yes
Neighborhood fixed effects	Yes	Yes	No	No	Yes
Observations	542	542	542	542	542

Note:

*p<0.1; **p<0.05; ***p<0.01

The results show that the treatment increases the chances of supporting strong-handed policies and repression after being a crime victim by 7 percentage points (column 1). 18% of victims support strong-arm policies, while 12% of non-victims have that policy preference. Here is crucial to remember that both groups are balanced on the pretreatment measure of this outcome (besides

¹⁶A binary indicator of sympathy for a particular party.

being balanced in 47 other covariates). These are important results because they represent a substantive effect on the understanding of what the state is allowed to do to protect citizens. These crime policy measures involve more than the implementation of a particular program or a budget increase; on the contrary, they directly imply the use of repression as a valid method for combating crime.

Column 5 reveals a significant and substantive correlation between sympathy toward the PSDB and PFL and support/nonsupport for iron-fist policies, and no evidence of correlation for the PT and PMDB. The PSDB can be associated with more conservative measures, while the PFL advocates for the protection of democratic rights in its ideological platform (Alcántara and Freidenberg, 2001). These findings tell to us that some parties might be clearly associated with platforms regarding how to deal with crime, and if partisanship is fluid, citizens might change their party identification in correspondence to being a crime victim.

What mechanism explains the impact of crime victimization on policy preferences? I hold that there are two main possibilities. First, crime might be reducing support for democracy, and making citizens more willing to tolerate repression and non-democratic practices. Second, voters might be changing their partisanship according to a running tally model. They will feel closer to the party most likely to benefit them, and consequently update their policy preferences.

Analysis of the causal mechanisms requires the untestable assumption that conditional on observed pretreatment covariates, the treatment assignment is independent of potential outcomes and potential mediators; and that conditional on the observed treatment and pretreatment covariates, the observed mediator is independent of potential outcomes (Imai, Keele and Tingley, 2010; Imai et al., 2011). In the attempt to make this assumption plausible I only adjust the matching procedure for covariates captured before the treatment and for pretreatment measures of the outcome and mediators. Thus, both groups are balanced on the pretreatment measures of the possible causal mechanisms. Table 2 reports the effect of the negative shock on a binary indicator of support for democracy¹⁷ and expressing sympathy for one of the main political parties in Brazil: the PT,

¹⁷ Support for the following statement: "Democracy is always better than other forms of government."

PMDB, PSDB, or PFL.

Table 2: Regression results

	Causal Mechanisms (wave t+1)				
	Democracy	PT	PMDB	PSDB	PFL
	(1)	(2)	(3)	(4)	(5)
Crime Victimization	-0.066*	-0.003	0.022	-0.005	0.003
	(0.039)	(0.034)	(0.028)	(0.012)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes
Neighborhood fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	542	542	542	542	542

Note:

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Though crime victimization does affect respondents' support for democracy, there is no evidence to confirm the idea that victims are changing their partisanship. This shows us that policy preferences are not necessarily always shaped by voters' party attachments, but rather can be modified by particular negative experiences. Citizens can have dynamic preferences, even though their partisanship is less flexible. In other words, victims' new policy preferences are mainly explained by their new attitudes towards democracy and not by a new party identification.

6 External Validity: Results Survey Data

Are these results a consequence of a particularity of the sample composition? Or of the year the survey was conducted? Is this pattern only present in Brazil? In an attempt to answer these questions, I use data from the Latin American Public Opinion Project (LAPOP) to study the cor-

relation between crime victimization and policy preferences in 18 Latin American countries in the year 2012.¹⁸ Since there is an evident trade-off between internal and external validity, this second study is less robust than the first because it is harder to reduce sensitivity to hidden biases without panel data. Nevertheless, it does help us check if similar results are obtained when we study all Latin American countries.

The main dependent variable is support for strong-arm policies.¹⁹ I also test the effect of crime on the two mechanisms of interest: support for democracy,²⁰ and sympathy for the first, second, third, and fourth-most preferred party in each country.²¹ To estimate the effect of crime victimization, I use a linear regression with cluster standard errors at the municipality level, and only include "placebo" covariates as controls. Covariates should not be affected by crime victimization, because that can introduce post-treatment biases. Therefore, I use the following four controls: age, education, gender, and ethnicity. I also include country fixed effects in the estimation. I do not use matching in this section to avoid any concerns about pruning observations since the main goal of this analysis is to improve external validity (see supplementary appendix for more detail).

$$Y_i = \alpha + \beta_1 T_i + \beta_2 P_i + \sigma_c + \varepsilon_i \quad (2)$$

Y is a binary indicator that represents the outcome of interest. T depicts the treatment (crime victimization), P describes the set of "placebo" covariates (age, gender, education, and ethnicity). σ_c represents country fixed effects. Table 3 displays the main results.

¹⁸ Support for strong-arm crime-reduction policies was not asked about in most of the countries in the most recent LAPOP survey conducted in 2014.

¹⁹ Support for the following statement: "In order to catch criminals, do you believe that authorities can occasionally cross the line?"

²⁰ Support for the following statement: "Democracy is preferable to any other form of government."

²¹ Binary indicator of sympathy for a party. Party 1 represents the party with most sympathizers in a given country, Party 2 the second, Party 3 the third, and Party 4 the fourth.

Table 3: Regression results

	<i>Dependent variable:</i>					
	Democracy	Party 1	Party 2	Party 3	Party 4	Strong-handed policies
	(1)	(2)	(3)	(4)	(5)	(6)
Crime Victimization	-0.013*	-0.009	0.005	0.002	0.000	0.057***
	(0.007)	(0.005)	(0.004)	(0.002)	(0.001)	(0.008)
Placebo covariates	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Countries	18	18	18	18	18	18
Observations	28,803	28,803	28,803	28,803	28,803	28,803

Note:

*p<0.1; **p<0.05; ***p<0.01

The findings are similar to the results obtained using panel data. Crime victimization reduces support for democracy, and increases support for strong-handed policies for reducing crime. There is no evidence that crime affects sympathy for the first, second, third or fourth-most preferred parties. This analysis allows us to increase the external validity of the results obtained in the two cities in Brazil.

7 Conclusions

The study of the political consequences of crime victimization is particularly necessary in countries where crime is a common phenomenon, and where candidates exploit the ideas associated with "radical penal populism" as a political strategy to gain votes. Crime victimization is a common negative event in the developing world, particularly in Latin America. This can lead to support of repression, which implies a new understanding of what the state is allowed to do to guar-

antee the security of its citizens. In particular, the adoption of tough policies against delinquency can foster the systematic violations of citizens' rights (Fuentes, 2005). Strong-handed measures to reduce crime tend to be present in the rhetoric of political campaigns, and many candidates emphasize their capacity to deal with crime and implement iron-fist policies to decrease victimization.

This article's findings can have important political implications. When affected citizens are more likely to support a repressive state, a rise in crime during electoral years can be exploited by populist candidates who propose iron-fist policies for controlling crime. The effect of crime victimization can have long-term consequences when it leads to the actual adoption of those policies. There is evidence of voters in the region supporting ex-authoritarian candidates accused of human right abuses because they promise to combat crime at any cost (Seligson, 2002). In this context, victims' new policy preferences can have meaningful consequences in terms of the quality of candidates elected and the policies implemented.

Previous literature has mainly focused on how voters evaluate politicians, following a classic retrospective voting approach. However, crime victimization can modify the policies voters would like to see implemented, in addition to punishing the incumbent. There is evidence of issue voting in Latin America, which means that voters might choose the candidate whose platform best matches their policy preference (Baker and Greene, 2011, 2015). As a consequence, citizens' new policy preferences can be an important factor in understanding their electoral decisions.

Having new policy preferences, however, does not necessarily mean adopting a new party identification. Crime victimization modifies voters' policy preferences mainly by changing their democratic values, and therefore makes them more willing to support strategies that erode basic rights in the attempt to combat crime.

Studying the effects of crime is complicated, and studies that do not incorporate longitudinal data tend to have several shortcomings, such as a lack of pretreatment covariates, and an endogeneity and serial victimization problem. The statistical theory of design sensitivity shows how elements of the design can reduce sensitivity to hidden biases (Rosenbaum, 2004). I heed these recommendations to construct a more robust observational study. In particular, I focus on reducing

heterogeneity, which can meaningfully decrease the impact of unmeasured confounders. Additionally, the use of panel data provides pretreatment covariates and pretreatment measures of the outcomes, which helps generate better comparisons.

Crime victimization is one of the many negative events that can modify voters' policy preferences. Economic shocks, natural disasters, and terrorist attacks may have similar implications, but they may alter voters' preferences in different directions and by different magnitudes. Unemployment and natural disasters can be associated with welfare and social policies, while terrorist attacks can be linked to the adoption of strict security measures. Therefore, we might expect voters to not just rely on a purely retrospective evaluation of the incumbent's performance when making electoral decisions, but rather also select candidates based on new policy preferences. In particular, affected citizens may prefer political authorities who want to implement the policies they most care about.

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