British Journal of Political Science (2019), page 1 of 15
doi:10.1017/S0007123418000297

British Journal of Political Science

ARTICLE

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

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(Received 17 May 2017; revised 20 December 2017; accepted 29 May 2018)

Abstract
Can crime victimization increase support for iron-fist crime-reduction policies? It is difficult to assess the political effects of crime, mainly because of the presence of unmeasured confounders. This study uses panel data from Brazil and strategies for reducing sensitivity to hidden biases to study how crime victims update their policy preferences. It also examines survey data from eighteen Latin American countries to improve the external validity of the findings. The results show that crime victims are more likely to support iron-fist or strong-arm measures to reduce crime, such as allowing state repression. Affected citizens are also found to value democracy less, which might explain their willingness to accept the erosion of basic rights in favor of radical measures to combat delinquency. These findings reveal that exposure to crime can change what people think the state should be allowed to do, which can have important political implications.

Keywords crime victimization; policy preferences; democratic values; panel data; hidden biases

Crime has become a critical concern in Latin America, one of the most violent regions of the world (UNODC 2013). Forty-three of the world's 50 most dangerous cities are located in South or Central American countries, even though they represent less than 8 per cent of the world's population (Magaloni, Franco and Melo 2015). In this context, it is important to learn whether crime can modify what victims believe the state should be allowed and not allowed to do to address this problem.

Individuals' policy preferences tend to be explained by long-term or slow-moving variables such as party (Campbell et al. 1960) or ideological identification (Jost 2006). For example, left-wing voters are more likely to support welfare policies (Shapiro 2009) and the redistribution of wealth (Alesina and Giuliano 2009), while right-wing citizens are more likely to support tax cuts (Jost 2006). Conversely, in this paper I study whether a short-term event such as crime victimization can modify individuals' preferences about strong-arm or iron-fist policies to reduce crime.

Strong-arm policies include a variety of direct and tough measures to reduce and fight crime that imply a deterioration or dilution of procedural rights (Holland 2013). They have been implemented in multiple Latin American countries, and can take the form of extralegal detention, arbitrary punishment and military-style occupation of entire neighborhoods (Dammert and Malone 2006). These strategies are a radical form of penal populism and constitute a statement about what the state can and cannot do to provide greater security. It is important to distinguish between iron-fist and more punitive crime-reduction policies, such as the use of state repression and an increase in prison sentences, respectively; the latter should not affect citizens' civil and due process rights.[1]

Studying citizen preferences regarding crime-reduction policies is particularly important in contexts such as Latin America, where delinquency is common, politicians exploit populist strategies to improve their electoral performance, and the police have been involved in human rights abuses. 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). However, we know little about whether crime can modify victims' policy preferences about how to reduce crime.[2]

Nevertheless, addressing this research question is challenging due to methodological issues such serial victimization, reverse causality, neighborhood effects, and hidden and post-treatment biases. I expand on these problems in the next section. In this article I pay careful attention to study design to address 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; Rosenbaum 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 forty-eight pre-treatment covariates. When using matching, there can be concerns about pruning observations to achieve balance. I therefore 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 on observed covariates. Moreover, I use survey data from eighteen 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-arm 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 may be more willing to tolerate strategies that erode citizens' rights. The evidence shows that crime victimization deteriorates the legitimacy of the political system, which might make such voters more accepting of iron-fist strategies.

This article provides two main contributions to the existing literature. First, it adds to a growing body of research that studies the political effects of crime. In particular, it focuses on support for strong-arm policies, which delineate the limits of the state and what it is allowed to do to ensure public security. It is crucial, then, to understand the factors explaining voter support for these measures. Iron-fist policies are not just another way to reduce crime. On the contrary, they directly imply the use of strategies that can violate citizens' civil rights and deteriorate the rule of law.

Secondly, it provides evidence for this argument by using a research design that decreases the impact of hidden biases. It thus contributes to the discussion of the importance of the design of observational studies for drawing more credible inferences. It is not easy to study the political effects of crime victimization since there are multiple methodological problems that can introduce biases. However, the consequences of these issues can be mitigated by using panel data and by applying elements of the statistical theory of design sensitivity (Rosenbaum 2004).

[1]Alternative crime-reduction strategies can focus on treatment and rehabilitation (Estrada 2004), multilevel government coordination (Ríos 2015) and local government capacity (Moncada 2016), among others.

[2]Bateson (2012) mainly focuses on the impact of crime on political participation, and provides evidence about how crime correlates with support for vigilantism and authoritarianism.

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). It can also have important political and electoral consequences.

In the case of Latin American countries, there is evidence showing that voters sanction incumbents for local homicides depending upon whether they consume information (Marshall 2015). Conversely, analyses using survey data indicate that crime victimization does not affect voters' electoral decisions; however, perceptions of high levels of insecurity do impact respondents' political choices (Perez 2015). The discrepancies between these studies might be explained by the fact that incumbents can escape electoral punishment under particular circumstances (Kronick 2014).[3] There is also mixed evidence in the studies that explore the impact of crime on political participation (Bateson 2012; Trelles and Carreras 2012; Berens and Dallendorfer 2017; Ley 2017). There is much more agreement about how crime victimization and perceptions about violence undermine support for (and the legitimacy of) democracy. Multiple studies have found evidence of this negative correlation (Carreras 2013; Fernandez and Kuenzi 2010; Malone 2010; Merolla, Mezini and Zechmeister 2013; Liebertz 2015).

The literature has paid less attention to how crime can modify victims' policy preferences. Voters' willingness to accept non-democratic measures, such as repression, can have critical consequences for the quality of democracy. Support for (or opposition to) iron-fist policies can inform politicians about citizens' level of tolerance of human rights abuses by the state. Indeed, voters' policy preferences can shape the adoption of policies (Lupu and Pontusson 2011) and impact parties' ideological positions (Adams et al. 2004). Additionally, a rise in crime might increase the electoral chances of parties associated with iron-fist measures to reduce crime such as right-wing or populist parties. Consequently, understanding the factors that influence citizens' policy preferences regarding crime is particularly important.[4]

It is challenging to address this research question for five main methodological reasons. First, being a crime victim is not a random event. Particular social circumstances can be associated with crime victimization, which can generate a serial victimization problem. This means that 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 or recurrent event in a respondent's life (Bateson 2012). This problem can introduce biases, since the previous treatment status can affect the outcome (for example, serial victims might get used to crime).

Secondly, there might be a reverse causality problem. People who want strong-arm policies might be more likely to report a crime as a way to increase crime statistics and push for the implementation of those policies. Also, most of the literature based on respondents' fear of crime 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.

Thirdly, 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. Fourthly, and related to the previous issue, neighborhood effects are a crucial concern (Bateson 2012). Some sectors or areas within a city might be more or less secure, which affects the probability of being a crime victim. This point is particularly salient

[3]For example, Kronick (2014) shows that the intensification of counternarcotics operations in Colombia had a spillover effect in Venezuela. She 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 for homicide outcomes.

[4]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, in turn, increases support for authoritarian strategies of crime control. Her study, however, focuses on the effects of exposure to the news but not on the direct consequences of crime victimization.

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. Finally, when using survey data, the treatment (crime victimization) and covariates (respondents' characteristics) are measured at the same time, which can lead to potential post-treatment biases. For instance, when trying to explain individuals' policy preferences, the use of specifications that adjust for things such as income or presidential approval can be very problematic, since these covariates might be affected by exposure to crime.

Crime Policy Preferences

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). Populist candidates have exploited this social context of insecurity and violence by promising to bring 'authority' back when fighting crime; this pattern was evident in the 2016 local elections (Winter 2016). However, implementing iron-fist strategies comes at a cost. The Brazilian military police have perpetrated human right abuses such as 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).

Support for these specific measures have important political consequences, because they define the boundaries that cannot be transgressed in an 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[5] to fight crime (Cruz 2010).

Where crime rates are high, it is important to understand whether victimization makes citizens more or less likely to support these policy approaches. What explains 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 and/or party identification (long-term factors). The second focuses on how specific circumstances, for example a change in media coverage, can shape voters' policy preferences (short-term factors).[6]

Regarding the first explanation (long-term factors), right-wing voters are more likely to care more about crime than left-wing voters (Mayer and Tiberj 2004). In a similar vein, right-wing authoritarianism can predict support for punitive measures (Gerber and Jackson 2016). Furthermore, the policies that emphasize punitive sanctions tend to be associated with conservative rather than liberal politicians. For example, former Republican 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, 48). Moreover, there is evidence in the United States that the proportion of Republican legislators is correlated with imprisonment rates at the state level (Beckett and Western 2001).

[5]Support for military intervention in the fight against crime is not the same as support for repression. For example, a relevant number of Latin American citizens perceive the armed forces to be more respectful of human rights than the police (Carreras and Pion-Berlin 2017).

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

The link between ideology and crime policies is also evident in Latin America. Right-wing candidates in Honduras, Mexico and Peru have promoted strong-arm 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 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 to these kinds 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.

Regarding the second explanation (short-term factors), citizens' preferences can also be affected by particular circumstances, such that support for strong-arm policies might not be a static policy preference. For example, the literature has focused on how the media can influence voter preferences. There is extensive research showing that the way the media frame an issue can change how individuals think about that topic (Kinder 1998; McCombs and Shaw 1972). In particular, certain news coverage of crime can impact individuals' attitudes toward crime control policies (Howitt 1998; Krause 2014). Less attention, however, has been paid to the consequences of direct exposure to crime. In this article, I show that crime victimization can have substantive and meaningful effects on victims' policy preferences. Specifically, crime exposure can increase support for iron-fist policies such as state repression.[7]

What causal mechanism explains the increase in support for more repressive measures? Consistent evidence shows that crime can affect victims' democratic values and support for the rule of law (Merolla, Mezini and Zechmeister 2013; Carreras 2013). Crime can also undermine the legitimacy of the political system (Cruz 2010) and increase support for radical change (Seligson and Azpuru 2000). In fact, fear of crime has been connected with support for regimes that reduce civil liberties (Perez 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 may explain why crime victims might accept the erosion of some basic rights in favor of radical measures to combat delinquency in their countries.

Civil liberties are directly linked to democratic values and the rule of law, and less support for democracy due to crime victimization might increase victims' willingness to sacrifice these rights. State repression is not just another strategy to reduce crime. It implies a disposition to tolerate the dilution of procedural rights. Therefore, I expect voters to first need to have lower democratic values (because of the undermining effects of crime on the legitimacy of the political system) in order to then accept repression as a valid strategy.[8]

[7]Some might argue that poor victims living in poor neighborhoods might be less likely to support iron-fist policies because they might be affected by the repression in the place they live. However, that hypothesis requires teasing out the impact of poverty at the individual and neighborhood levels, which is very hard to do in Brazil. As Bahamonde (2017) shows, poor citizens are homogeneously distributed across poor and non-poor municipalities. Therefore, we would need to compare poor victims living in a poor neighborhood, poor victims living in a non-poor neighborhood, non-poor victims living in a poor-neighborhood, and non-poor victims living in a non-poor neighborhood to empirically measure the role of individual and neighborhood characteristics when explaining the heterogeneous effects of crime victimization. Unfortunately, this study does not have enough observations to conduct that analysis; nevertheless, it is an interesting issue to be explored further.

[8]An alternative hypothesis is that crime victims are more likely to support iron-fist policies such as repression and, as a result, are less likely to support democracy. Therefore, lower democratic values might not be a proper causal mechanism to explain preferences for strong-arm policies (i.e., the outcome might be happening before the mechanism). In this article, I test the impact of crime on the main outcome (i.e., policy preferences) and the possible causal mechanism (i.e., support for democracy) at the same time, which is similar to a single-experiment design in which both the outcome and the mechanism are measured within the same experimental treatment (Imai et al. 2011). Thus it is not empirically possible to fully rule out this alternative hypothesis. However, accepting repression comes with a willingness to tolerate the erosion of basic rights. This is the reason why I hold that voters would first need to attach less value to democracy and the rule of law.

In summary, I hypothesize that crime victimization has a substantive and significant effect on victims' policy preferences: in particular, that crime exposure should increase support for iron-fist policies such as state repression. I expect that this change is explained by a lesser degree of support for democratic values, which makes victims more tolerant of certain strategies.

The study of crime victimization has been dominated by a sanctioning argument, the most common prediction of which is that victims will punish incumbent candidates. In this article, 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.

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), which uses 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), I apply four criteria. First, the treatment should be well defined. This means that we know when it starts, and therefore what the pre-treatment and post-treatment covariates are. Secondly, even though there is no random assignment, the intervention should seem haphazard or not obviously related to potential outcomes. Thirdly, treated and control groups should be comparable: in other words, the distributions of observed covariates should be similar across both groups. Fourthly, the design should use strategies to reduce sensitivity to unobserved biases, such as decreasing unit heterogeneity. I apply these four criteria in the design of this observational study.

Regarding the first recommendation, the main problem when working with survey data is the lack of pre-treatment 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.[9] The panel structure allows me to include pre-treatment 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).

Secondly, 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 who in 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. This strategy also contributes mitigating the reverse causality problem since in the first wave I only focus on respondents that did not report a 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; Visconti and Zubizarreta 2018). I explain the details of this technique later.

[9]See the Appendix and Baker et al. (2015) for more details about this panel survey.

The fourth strategy focuses on decreasing sensitivity to hidden biases by reducing the heterogeneity of the sample. As Rosenbaum shows, reducing unit heterogeneity implies that larger unobserved biases will be needed to explain away a particular effect (Rosenbaum 2005). Good examples of this strategy can be found in studies based on identical twins.[10] Consequently, in an observational study it is preferable to focus on more homogeneous and comparable subsets (Keele 2015) or on natural blocks (for example, 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 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 (Baker, Ames and Renno 2006). Both cities have similar characteristics, such as the size of the electorate, their educational and income levels, and racial composition.[11] According to the unmatched sample, they also have similar crime rates in wave $t + 1$: 15 per cent of respondents were crime victims in Juiz da Fora, and 14 per cent in Caixas do Sul. Additionally, the data provides neighborhood indicators, which allows me to balance the sample by respondents' geographic location.

How does one go about building a group of affected and unaffected citizens who are balanced in their observed characteristics? One alternative is matching, which attempts to generate treated and control groups 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 on 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, which may cause the matched sample to be different from the unmatched sample.

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: (1) meets the balance requirements defined beforehand and (2) is similar to a target sample also defined beforehand (in this case the unmatched sample). Point 1 addresses the limitations of traditional matching techniques because the algorithm directly balances the original covariates without needing to estimate a propensity score. Point 2 ensures that the samples before and after matching are similar on observed covariates, making pruning less of a concern.

I use mean balance constraints for forty-seven covariates. The algorithm matches individuals such that the treated and control matched groups cannot differ in their means by more than 0.1 standard deviation from the unmatched sample. As a consequence, the standardized differences between the matched treated and control groups cannot be larger than 0.1 x 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 (that is, both matched groups) and the unmatched sample (see Zubizarreta and Kilcioglu (2016) and Visconti and Zubizarreta (2018) for more details).

All of the mean balanced covariates are ordinal or binary; thus, adjusting their means is a meaningful decision.[12] 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 forty-eight different observed covariates.[13]

[10]See, e.g., Ashenfelter and Rouse (1998).

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

[12]In the case of nominal covariates, it is advisable to use other forms of covariate balance (Resa and Zubizarreta 2016; Visconti and Zubizarreta 2018; Zubizarreta 2012).

[13]See the Appendix for details about the structure of the panel data and the construction of covariates and outcomes.

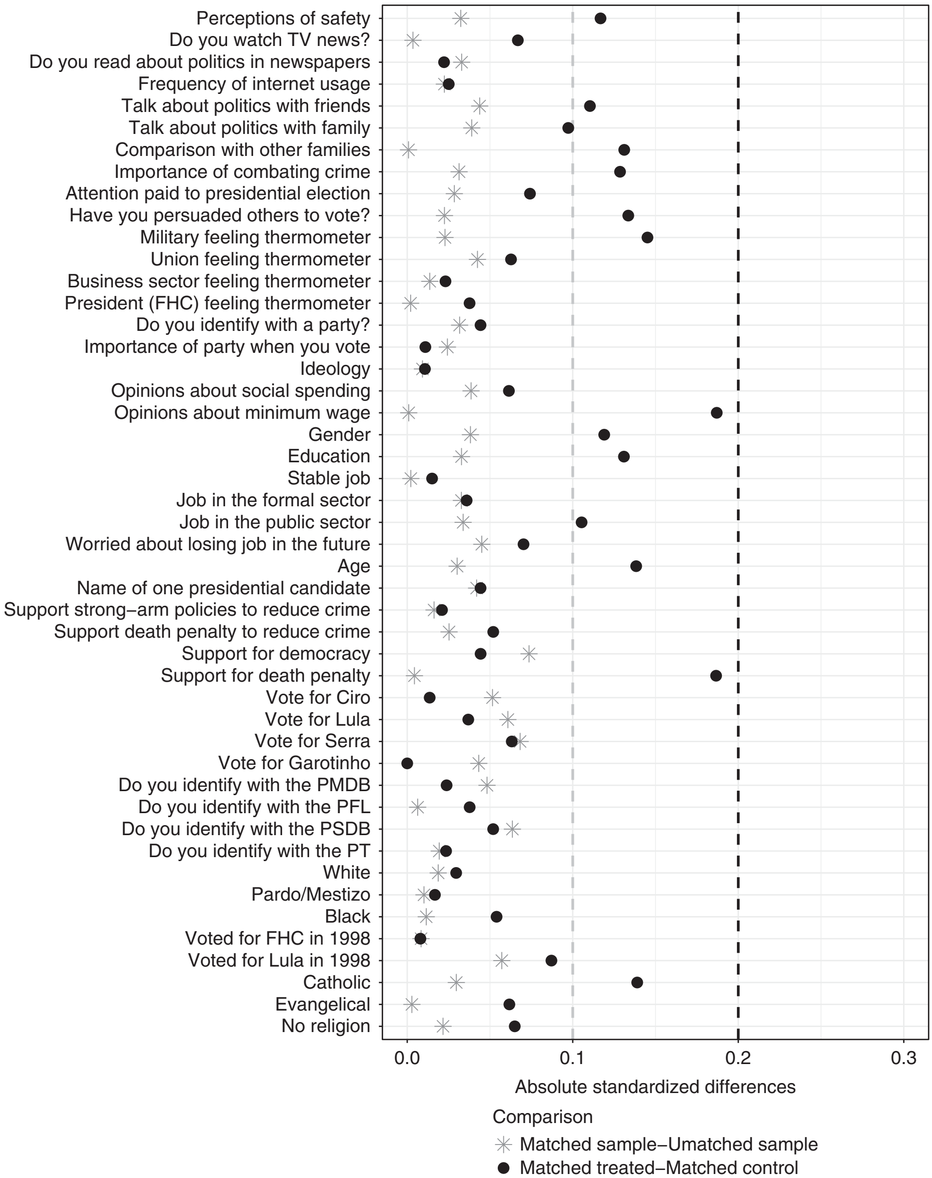

Figure 1. Balancing the means of the observed covariates (i.e., mean balance) and building a representative matched sample. Dots represent the standardized differences between the matched treated and control group (balance requirement), and asterisks represent the standardized differences between the matched and unmatched sample (representative requirement)

Note: PMDB: Brazilian Democratic Movement Party, PFL: Liberal Front Party, PSDB: Brazilian Social Democratic Party, and PT: Workers' Party

In the matching procedure I include pre-treatment covariates that can affect both the treatment assignment and the outcome (Stuart 2010), such as age, education, gender, ideology, job in the formal sector, media consumption, partisanship, policy preferences, political knowledge, race and religion (full list is provided in Figure 1 and in the Appendix).

The treatment is a binary indicator for being a witness or victim of crime[14] 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 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.'[15] I use a binary indicator of support for democracy[16] to explore the causal mechanism. To estimate the effect of crime victimization I use a linear regression with cluster standard errors at the neighborhood level:

$$Y_{it+1} = \alpha + \beta_1 T_{it+1} + \beta_2 P_{it} + \beta_3 X_{it} + \sigma_n + \epsilon_i \tag{1}$$

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 pre-treatment measure of the outcome from wave t, and X corresponds to a set of pre-treatment covariates that might explain policy preferences (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 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).

Results Panel Data

The unmatched sample has 1,916 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 identifies the largest representative matched sample that fulfills the following criteria: (1) mean balance for forty-seven covariates between the matched and unmatched sample, (2) mean balance for forty-seven covariates between the matched treated and control groups and (3) fine balance for neighborhood between the matched treated and control groups. After optimizing these criteria, the matched sample has 271 subjects in each group, which makes a total of 542 individuals who are similar to the 2,236 subjects in the unmatched sample.

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.

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 depicts the distribution of this variable before and after matching.

[14]The treatment involves being a crime victim or a witness. Therefore, this is a compound treatment that incorporates both a direct and an indirect dimension of crime victimization. This would be problematic if the former event generates an impact on policy preferences that goes in the opposite direction from the latter event. However, I expect both to change victims' support for iron-fist policies in the same direction, but perhaps by different magnitudes.

[15]Support for the following statement: 'The best way to reduce crime is with repression and an iron fist.'

[16]Support for the following statement: 'Democracy is always better than other forms of government.'

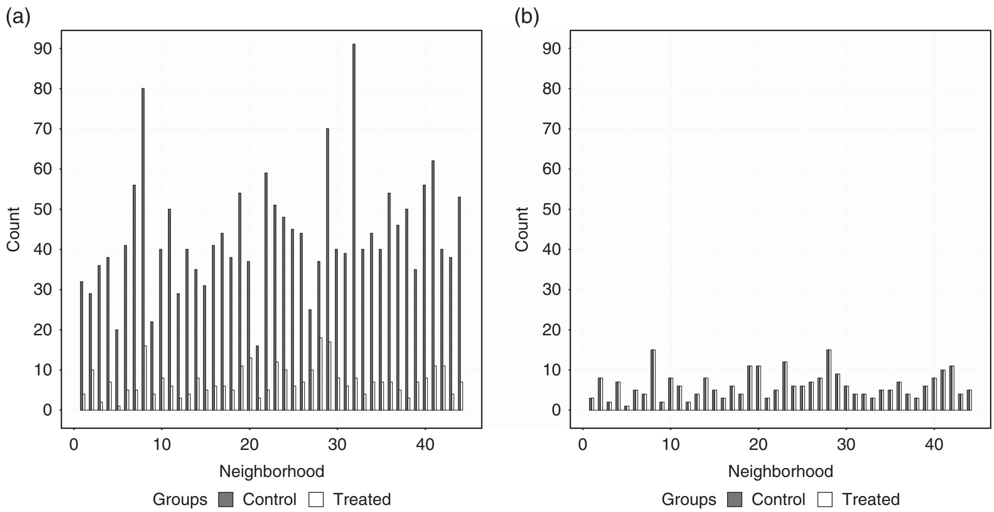

Figure 2. Balancing the marginal distributions of neighborhood (i.e., fine balance), which implies that both groups will have the same frequency for this covariate but without restricting who is paired with whom

Table 1. Policy preferences

	Strong-arm policies and repression to reduce crime			
	(1)	(2)	(3)	(4)
Crime victimization	0.070**	0.063**	0.069**	0.063**
	(0.031)	(0.031)	(0.029)	(0.030)
Controls	Yes	No	Yes	No
Neighborhood fixed effects	Yes	Yes	No	No
Observations	542	542	542	542

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

As a reminder, the outcome is a binary indicator of support for the use of strong-arm measures and repression to reduce crime (wave $t + 1$). The treatment is to be a crime victim in wave $t + 1$ conditional on not being a victim in wave t. Table 1 reports the impact of crime victimization on policy preferences. Columns 2, 3 and 4 provide unadjusted estimates.

The results show that the treatment increases the chances of supporting strong-arm policies and repression after being a crime victim by 7 percentage points (Column 1); 18 per cent of victims support strong-arm policies, compared to 12 per cent of non-victims.[17] Here it is crucial to remember that both groups are balanced on the pre-treatment measure of this outcome (in addition to being balanced in forty-seven 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 implementing a particular program or a budget increase: they directly imply the use of repression as a valid method for combating crime.

What mechanism explains the impact of crime victimization on policy preferences? I hold that crime might be reducing support for democracy, and making citizens more willing to tolerate

[17]Iron-fist policies are radical measures that imply the dilution or deterioration of citizens' rights. Therefore, it is not surprising that they do not enjoy broad support among the population to begin with. For example, in the case of Brazil, strong-arm strategies usually take the form of beatings and torture (Magaloni, Franco and Melo 2015).

Table 2. Causal mechanism

	Support for democracy			
	(1)	(2)	(3)	(4)
Crime victimization	−0.066*	−0.066	−0.067*	−0.066
	(0.039)	(0.045)	(0.038)	(0.043)
Controls	Yes	No	Yes	No
Neighborhood fixed effects	Yes	Yes	No	No
Observations	542	542	542	542

*p < 0.1; **p < 0. 0.05; ***p < 0.01

repression and non-democratic practices.[18] Table 2 reports the effect of the negative shock on a binary indicator of support for democracy.[19] These results should be interpreted with caution, since the study of causal mechanisms comes with strong assumptions. As in the main analysis, Columns 2, 3 and 4 provide unadjusted estimates.

Exposure to crime reduces the likelihood of supporting the statement that democracy is the best form of government by almost 7 percentage points. The sizes of the effects are very similar when comparing Tables 1 and 2. The results are not significant in the models that exclude controls, but coefficients are very stable across all specifications, which shows that including these pre-treatment covariates increases the precision of the estimation (lower standard errors).

External Validity: Results Survey Data

Are these results a consequence of a particularity of the sample composition? Or of the year in which 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 correlation between crime victimization and policy preferences in eighteen Latin American countries in 2012.[20] 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.[21] I also test the effect of crime on the mechanism of interest: support for democracy.[22] To estimate the effect of crime victimization, I use a linear regression with standard errors clustered at the neighborhood 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 concerns about pruning observations, since the main goal of this analysis is to improve external validity (see the Appendix for more detail).

$$Y_i = \alpha + \beta_1 T_i + \beta_2 P_i + \sigma_c + \epsilon_i \tag{2}$$

Y is a binary indicator that represents the outcome of interest. T depicts the treatment (crime

[18]The analysis of the causal mechanisms requires the untestable assumption that, conditional on observed pre-treatment covariates, the treatment assignment is independent of potential outcomes and potential mediators, and that, conditional on the observed treatment and pre-treatment covariates, the mediator is ignorable with respect to the outcome (Imai, Keele and Tingley 2010; Imai et al. 2011).

[19]Support for the following statement: 'Democracy is always better than other forms of government.'

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

[21]Support for the following statement: 'In order to catch criminals, do you believe that authorities can occasionally cross the line?'

[22]Support for the following statement: 'Democracy is preferable to any other form of government.'

Table 3. External validity

	Dependent variable	
	Strong-arm policies	Support for democracy
	(1)	(2)
Crime victimization	0.057***	−0.013*
	(0.008)	(0.007)
Placebo covariates	Yes	Yes
Country fixed effects	Yes	Yes
Countries	18	18
Observations	28,803	28,803

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

victimization), P describes the set of four 'placebo' covariates. σ_c represents country fixed effects. Table 3 displays the main results.

The findings are similar to the results obtained using panel data. Crime victimization increases support for strong-arm policies for reducing crime, which might be explained by a lower support for democracy. This analysis allows us to increase the external validity of the results obtained in the two cities in Brazil.

Conclusions

Studying 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 can lead to support of repression, which implies a new understanding of what the state is allowed to do to guarantee 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-arm measures to reduce crime are often mentioned in political campaign rhetoric, and many candidates emphasize their ability to deal with crime and implement iron-fist policies to decrease victimization.

In this article I show that crime victimization modifies voters' policy preferences by changing their democratic values, and therefore making them more willing to support strategies that erode basic rights in an 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 pre-treatment covariates, as well as endogeneity and serial victimization problems. 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 pre-treatment covariates and pre-treatment measures of the outcomes, which helps generate better comparisons.

Previous studies have mainly focused on how voters evaluate politicians, following a classic sanctioning argument. However, crime victimization can modify the policies voters would like to see implemented, in addition to punishing the incumbent. I believe this is a prospective dimension of victims' electoral decisions: they first sanction the incumbent, and then need to select a challenger. The policies that those candidates propose might be crucial to understanding the affected citizens' electoral choices.

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.[23] The effect of crime

[23]Baker and Greene (2011) show that issue voting is important for understanding voters' electoral choices in Latin America.

victimization can also have long-term consequences for the adoption of those policies. There is evidence of voters in the region supporting ex-authoritarian candidates accused of human rights abuses because they promise to combat crime at any cost (Seligson 2002). In this context, victims' new policy preferences can have meaningful consequences for the quality of candidates elected and the policies they implement.

Acknowledgments. I thank Sarah Berens, Kathleen Griesbach, Shigeo Hirano, Erin Huebert, Eric Magar, Isabela Mares, John Marshall, M. Victoria Murillo, Zoila Ponce de Leon, Oscar Pocasangre, Elizabeth Zechmeister, José Zubizarreta, three anonymous reviewers, and seminar participants at LASA and APSA 2017 annual meetings for their valuable comments and suggestions. I am grateful to the authors of the Two Cities Panel Study and to the Latin American Public Opinion Project (LAPOP) for making their data available. All errors are my own.

Supplementary Material. Data replication sets can be found in Harvard Dataverse at: https://doi.org/10.7910/DVN/IUG9LC and on-line appendices at: https://doi.org/10.1017/S0007123418000297

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Cite this article: Visconti G (2019). Policy Preferences after Crime Victimization: Panel and Survey Evidence from Latin America. *British Journal of Political Science* 0: 1–15, https://doi.org/10.1017/S0007123418000297