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CONSEQUENCES
OF VIOLENT
CRIME:
EVIDENCE
FROM
VENEZUELA

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To what extent do voters hold political incumbents accountable for policy outcomes? This paper considers retrospective voting in the context of violent crime. Using a novel panel data set on county -and neighborhood- level homicide incidence and vote shares in Venezuela, I find that, relative to other policy outcomes such as cash transfers, voters are generally unresponsive to changes in homicide incidence. However, responsiveness varies with the type of election and with the nature of local crime control institutions (which change within municipality over time). Noting the role of external shocks (for example, from drug enforcement activities in neighboring Colombia) in producing violence in Venezuela, I interpret these results as evidence that voters (correctly) view homicide outcomes as weak signals of incumbent political quality. The results are therefore consistent with rational retrospective models of voting behavior.

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CONSECUENCIAS ELECTORALES DEL CRIMEN VIOLENTO: EVIDENCIA DE VENEZUELA

Kronick, Dorothy

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RESUMEN

¿Hasta qué punto el electorado exige a los funcionarios públicos rendición de cuentas a través de resultados en la implementación de políticas? Este trabajo considera el voto en retrospectiva en el contexto de crímenes violentos. Usando panel data de la incidencia de homicidios a nivel de municipios -y vecindarios- y los shares de votación en Venezuela, se encuentra que en comparación a otras variables resultado de política como las transferencias de dinero, los votantes, por lo general no responden a cambios en la incidencia del homicidio. Sin embargo, la capacidad de respuesta varía con el tipo de elección y con la naturaleza de las instituciones de control del crimen a nivel local (con cambios en la municipalidad en el tiempo). Tomando en cuenta el papel que shocks externos (por ejemplo, de actividades para combatir el narcotráfico en países vecinos como Colombia) podrían tener en la violencia en Venezuela, se interpretan estos resultados como evidencia que los votantes (correctamente) ven los resultados de homicidios como señales de políticos de mala calidad o poco eficientes. Estos resultados son consistentes con los modelos retrospectivos racionales de comportamiento del voto.

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Electoral Consequences of Violent Crime: Evidence from Venezuela

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Abstract

To what extent do voters hold political incumbents accountable for policy outcomes? This paper considers retrospective voting in the context of violent crime. Using a novel panel data set on county- and neighborhood-level homicide incidence and vote shares in Venezuela, I find that, relative to other policy outcomes such as cash transfers, voters are generally unresponsive to changes in homicide incidence. However, responsiveness varies with the type of election and with the nature of local crime control institutions (which change within municipality over time). Noting the role of external shocks (for example, from drug enforcement activities in neighboring Colombia) in producing violence in Venezuela, I interpret these results as evidence that voters (correctly) view homicide outcomes as weak signals of incumbent political quality. The results are therefore consistent with rational retrospective models of voting behavior.

1 Introduction

That voters respond to outcomes (economic and otherwise) is an undisputed empirical regularity; whether they do so rationally or blindly—a distinction critical for democratic governance—remains the subject of much debate. On one side of this debate are studies in which voters appear sophisticated, carefully holding incumbents to account for policy but not blaming them for the rest of life (e.g., Duch and Stevenson (2008), Healy and Malhotra (2010a)). On the other side are results suggesting that voters can't even tell local incompetence from the biggest global recession in generations (Achen and Bartels (2004b), Bartels (2011)). This paper thus addresses an open question: do voters respond more to outcomes that say a lot about political incumbents than to outcomes that say little? Do voters get it right?

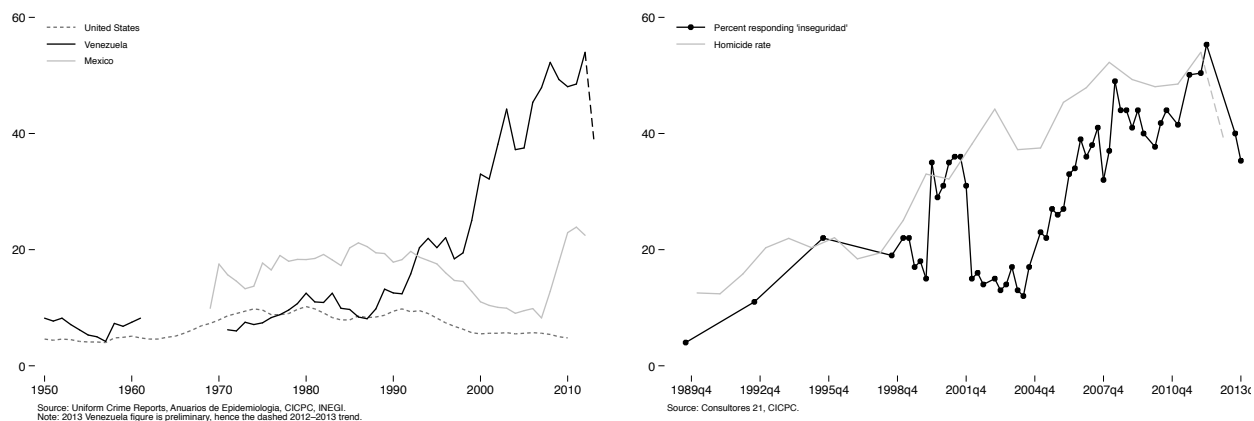
One empirical challenge in this literature is to define “get it right,” or, more precisely, to articulate an objective measure of the informational content of a given outcome. If we as researchers cannot estimate the extent to which, say, economic growth constitutes a valuable signal about a political incumbent, we will be hard-pressed to judge the rationality of voter response to growth. Some recent work confronts this challenge by focusing on outcomes that the authors assume contain (close to) zero signal about politicians, such as shark attacks (Achen and Bartels (2004a)), sports games (Healy and Malhotra (2010b)), or global oil prices (Wolfers (2002)). These papers then compare voter response to the rational baseline of no reaction. However, as others have noted (Healy and Malhotra (2010b)), seemingly irrelevant outcomes may in fact contain valuable information about political incumbents. For example, while resource revenues themselves may not say much about state governors, governors' allocation of those revenues may speak volumes.

In this paper, I compare voter response to homicide rates against explicit estimates of the extent to which homicide rates communicate information about political incumbents. Using a novel panel data set on local murder incidence and local vote shares in Venezuela, I estimate how voters use homicide rates to update their opinions of incumbent mayors, governors, legislators, and presidents. I also measure how this updating changes when local crime-control institutions change: specifically, how voter response to homicide shifts with the introduction of a municipal police force. These estimates provide a window onto voters' view of what murder rates tell them (or don't tell them) about political incumbents. By comparing voters' view against an objective, data-driven assessment of what murder rates say (or do not say) about officeholders—developed fully in (Kronick (2014)) and summarized here—I directly adjudicate between blind-retrospective and rational-retrospective theories of voter behavior.

The results suggest that voters mostly get it right (c.f. Healy and Malhotra (2013)). Over the past three decades Venezuela experienced a homicide wave that dwarfs both that of the United States in the 1970s–1980s and that of the current Mexican drug war (see Figure 2a); while local policy choices certainly exacerbated the violence, I find that external factors

(such as US counter narcotics operations in the Caribbean) played a major role and that, in general, local homicide trends are largely uninformative about Venezuelan officeholders (a point I elaborate in Section 3; this is not the case, for example, in neighboring Colombia). Venezuelan voters accordingly do not (in general) punish incumbents for increases in murder incidence (or reward them for decreases), despite high awareness of and intense concern about citizen security (see Figure 2b). These findings are consistent with survey evidence in which Venezuelan voters do not blame the government for violent crime (Hanson and Smilde (2013)).

Figure 1: The Venezuelan Context: High Violence, High Awareness of Violence
 (a) Homicides per 100,000 population (b) What is the nation's biggest problem?



The data also indicate that the introduction of municipal police forces in Venezuelan municipalities did not much change the extent to which homicide incidence provides an informative signal about a mayor's effect on the homicide rate. Perhaps unsurprisingly, then, voters in cities that have long had municipal police are no more responsive to homicide than voters in cities that do not have municipal police. However, in the first mayoral election after the founding of the local police force, there is a large and precisely estimated negative relationship between changes in the homicide rate and changes in incumbent vote share. This indicates that, in the presence of a new municipal crime-control institution, voters initially consider crime shifts informative about the mayoral incumbent; then, before the next election (that is, before the second election following the establishment of the municipal police), voters realize that they can not learn much from murder incidence. This result recalls Monteiro and Ferraz (2010), in which Brazilian voters initially reward mayoral incumbents for oil-price shocks but then quickly discover that, in and of themselves, windfall revenues don't say much about local officeholders.

In a well-known paper on economic policy and economic voting in the United States, Alberto Alesina, John Londregan, and Howard Rosenthal wrote that, for the naive voter, "growth is growth"—which is to say, voters fail to distinguish between economic outcomes that tell them something about incumbent competence and outcomes that tell them nothing (Alesina et al. (1993)). The results presented here suggest that the opposite is true of violent death.

Voters punish or reward incumbents for homicide in proportion to its informational content.

2 Retrospective voting with multiple outcomes

This section presents a simple model that clarifies and makes precise several concepts introduced above. The model defines voter utility and specifies a production process for the outcomes that affect voter utility. Politician type (or quality) affects these outcomes; it is this type, or quality, about which voters seek to learn when they observe outcomes.

Consider voters whose direct utility depends on two outcomes, X^A and X^B . An election takes place in period t , and voter utility in the post-election period is given by

$$U(X_{t+1}^A, X_{t+1}^B) = X_{t+1}^A + \phi X_{t+1}^B$$

where ϕ reflects the voters' weight on outcome X^B relative to outcome X^A . Initially I assume that all voters have the same relative weight ϕ ; later I relax this assumption.

The outcomes X^A and X^B follow two separate time-series processes μ_t^A and μ_t^B ; I assume here that these are AR(1). In addition, each politician p is endowed with outcome-specific quality parameters θ_p^A and θ_p^B that capture the extent to which he can move each outcome from its natural level. In this model, politician quality encompasses both policy choice (which is not explicitly modeled) and policy implementation capacity (which in much of the literature is called competence (Alesina et al. (1993))). This simple setup therefore abstracts away from politician incentives and treats voter choice as a decision-theoretic problem. Then, for each outcome $j \in A, B$:

$$X_{t+1}^j = \alpha X_t^j + \theta_p^j + \epsilon_{t+1}^j$$

where $\alpha = 1$ and the ϵ_t^j are distributed iid $\mathcal{N}(0, \sigma_{\epsilon^j})$. Thus, next year's crime rate is distributed around this year's crime rate.

Voters observe outcomes X_{t-1}^j in the period before the incumbent I took office and outcomes X_t^j under the incumbent; they then consider

$$s_t^j = X_t^j - X_{t-1}^j = \theta_I^j + \epsilon_t^j$$

as a noisy signal of incumbent quality θ_I^j . They also know that, in the population, $\theta_p^j \sim N(0, \sigma_{\theta^j}^2)$, and that the two dimensions of politician quality $j \in A, B$ are drawn independently. Thus, before observing X_t^j , voters' prior is that politician quality on each dimension is $\mu_{\theta^j} = 0$. Then voters' posterior about incumbent quality at the time of (re)election depends on the change in outcomes ΔX_t^j , and voter i will choose the incumbent if:

$$\frac{\sigma_{\theta^A}^2}{\sigma_{\epsilon^A}^2} \cdot \Delta X_t^A + \phi \frac{\sigma_{\theta^B}^2}{\sigma_{\epsilon^B}^2} \cdot \Delta X_t^B > u_{it} \quad (1)$$

where u_{it} is the difference of voter-candidate-specific utility shocks realized at the time of the election (i.e., for challenger C and incumbent I , $u_{it} = \nu_{itC} - \nu_{itI}$). See Appendix [] for the simple derivation.

Thus, the parameters that condition voter responsiveness are the $(\sigma_{\theta_j}^2/\sigma_{\epsilon_j}^2)$ s: that is, the ratio of the variance in politician quality to the variance in the non-policy component of a given outcome. This ratio captures the extent to which outcomes ΔX_t^j constitute informative signals of politician quality. This result has considerable intuitive appeal. Vote share should respond more to outcomes over which the influence of politicians is strong relative to the influence of other factors. Here, critically, what matters for vote choice is not the magnitude of politician impact on a given outcome but rather the variance (across the politician pool) of that impact relative to the variance of non-policy shocks. In other words, an outcome (say, the crime rate) can be uninformative about politician quality even if, in principle, the right policy could make a big difference (because pursuing that policy might lie outside the support of the politician quality distribution).¹

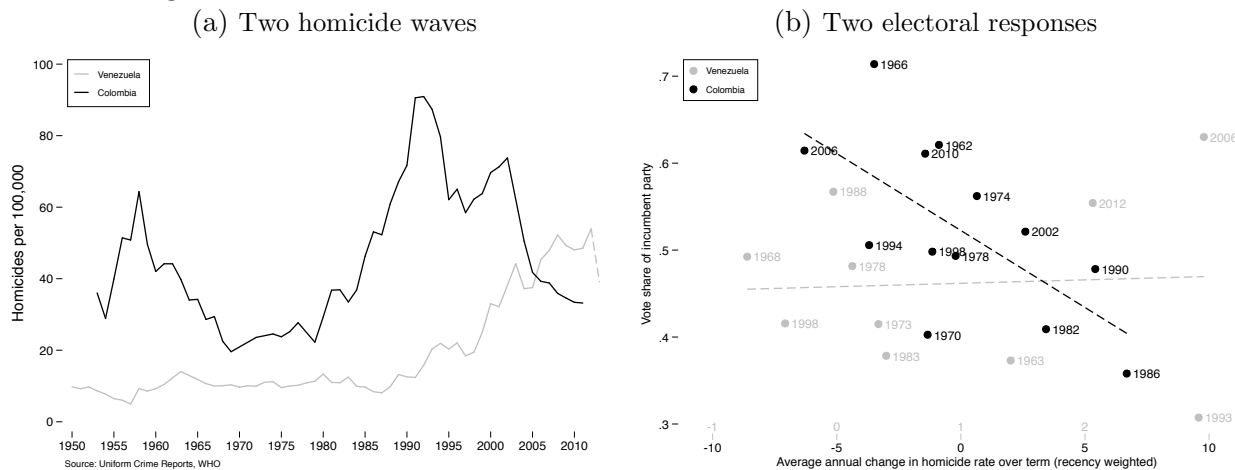
For notational convenience, I refer to the $(\sigma_{\theta_j}^2/\sigma_{\epsilon_j}^2)$ ratio as the signal-to-noise ratio for a given outcome j in a given electoral context m , or SNR_{jm} . In this setup, then, adjudicating between rational and naive retrospection amounts to testing whether the voters' perceived SNR_{jm} —call it \widehat{SNR}_{jm} —corresponds with the econometrician's estimated SNR_{jm} , which I denote \widetilde{SNR}_{jm} . For example, if we could estimate the extent to which economic growth constitutes a signal of politician quality in countries with varying levels of openness to the international economy—that is, estimate $\widetilde{SNR}_{Growth,m}$ for various countries m —we could compare those estimates to voter responsiveness to growth—that is, $\widehat{SNR}_{Growth,m}$. (In fact, this the test that Duch and Stevenson (2008) implicitly conduct).

Consider an informal illustration comparing Venezuela with neighboring Colombia, which has experienced violence on a similar scale (Figure 1a). As I elaborate below, the best available data indicate that Venezuela's murder rate provides little information about presidential incumbents. In other words, the variance of the distribution of presidents' (or potential presidents') effect on the murder rate is small relative to the variance of external shocks. In Colombia, in contrast, much of the violence stems directly from conflict between the military and armed groups; policy response to this conflict varies widely across presidents (and presidential candidates, and, implicitly, the underlying distribution from which these men are drawn) (Meja and Restrepo (2011)). In this framework, then, we would say that $\widehat{SNR}_{Homicide,Col} > \widehat{SNR}_{Homicide,Vza}$. We might then ask whether voters respond more to changes in homicide rates in Colombia than in Venezuela—that is, whether $\widehat{SNR}_{Homicide,Col} > \widehat{SNR}_{Homicide,Vza}$. Figure 1b (and associated regression results in Appendix B) indicate that this is

¹Note on a key model assumption: The model assumes that voters observe changes in outcomes. Figure 2a suggests that this assumption might hold for the case of homicide in Venezuela; for the case of crime in Latin America more broadly, Maris and Ortega (2013) use survey data together with actual (not self-reported) incidence data to demonstrate fairly accurate perceptions of crime levels and trends across the region. Other studies indicate awareness of actual local crime incidence in Minneapolis (McPherson (1978)) and Seattle (Rountree and Land (1996)), among other places.

indeed the case (though of course the much greater variance of homicide rates in Colombia makes the comparison inexact; note the two x-axis scales in Figure 1b).

Figure 2: Violence and Presidential Elections in Colombia and Venezuela



3 Homicide rates and politician quality in Venezuela

To assess whether rational retrospective Venezuelan voters would respond to changes in the homicide rate, I first develop an objective sense of the extent to which homicide rates constitute a meaningful signal of politician quality in the Venezuelan context.

Sociologists, journalists, and the Venezuelan public have debated the causes of Venezuela’s violent crime wave (see, e.g., Briceño-León (2012)); among the most prominent (of many) candidate explanations for the trend in Figure 2a are: (a) the deterioration of policing and other institutions (Briceño-León (2007)), (b) unintended consequences of a major 1999 penal code reform (Duce and Perdomo (2003)), (c) demographic factors (c.f. De Mello and Schneider (2010)), (d) environmental lead exposure during the childhood of today’s criminal cohort (c.f. Reyes (2007)), (e) the establishment of pro-government militias (*colectivos*, Hausmann (2014)) and (e) increased cocaine trafficking (Kirschke (2008)).

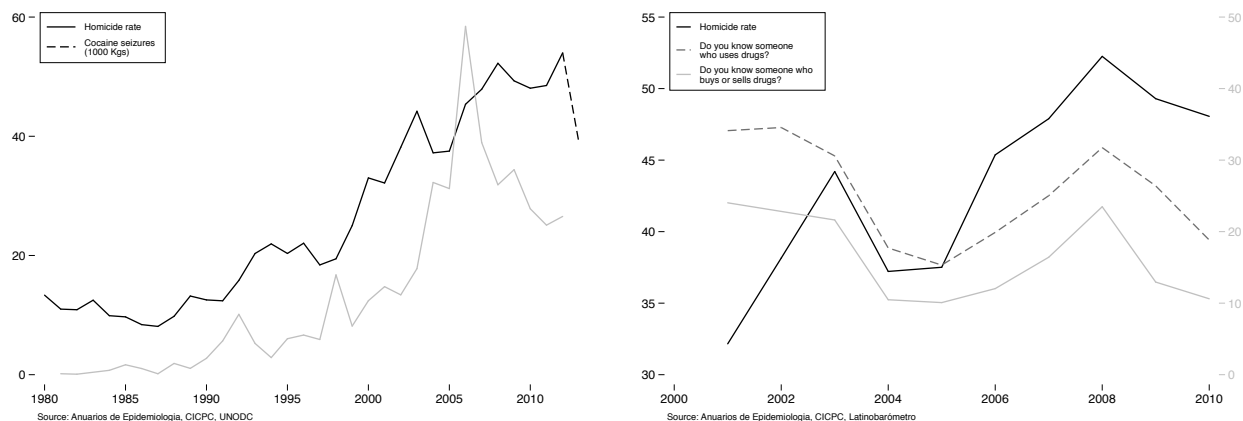
In a separate paper, I find that only the last of these—that an increase in cocaine trafficking produced much of the violence—is supported by available data (Kronick (2014)). Within-municipality over-time variation in policing, demographic trends, lead exposure, and other factors is not predictive of local homicide rates; in contrast, the volume of cocaine trafficked through Venezuela strongly predicts violence in local Venezuelan drug markets (but not elsewhere in the country).

Critically, trafficking-related violence in Venezuela is somewhat different than trafficking-related violence in Mexico (which is perhaps the most familiar case). In Mexico, violent conflict occurs largely in areas crucial for transshipment (*plazas*) (see, e.g., Dell (2011),

Castillo et al. (2013)); in Venezuela, much of transshipment is in the hands of the military (Mayorca (2012), InSight Crime (2012)), and violent deaths arise in part from gang conflict over the local retail drug market. Figure 3a plots cocaine seizures in Venezuela against the national homicide rate; Figure 3b plots domestic drug use (as reported in surveys) against the national homicide rate.

The dramatic increase in cocaine trafficked from Colombia to Venezuelan ports can be attributed in part to the closing of other routes and to the diminished capacity of Colombian cartels (e.g., Kirschke (2008)). The Venezuelan army’s participation in trafficking also plays an important role (ICG (2011)); to some extent, this participation reflects the decisions and preferences of the president. However, as the model articulated above emphasizes, what determines the informational content of a given outcome (in this case, violence) is not whether, in theory, a different officeholder could make a difference, but rather whether, in practice, those effective officeholders populate the underlying distribution of candidates. Since of course that underlying distribution is unobserved by definition, we can only speculate; I note that the administrations immediately preceding Chávez chose similar policies in this area (Malaver (1999)) and that presidential challengers have not promised departures. The specter of military takeover—raised by coup attempts in the early 1990s and revived by a short-lived coup in the early 2000s—may have created incentives for presidents to keep officers happy (by allowing them to profit from illegal businesses).

Figure 3: Drug Trafficking, Drug Use, and Violence in Venezuela
 (a) Trafficking and violence (b) Trafficking and domestic use

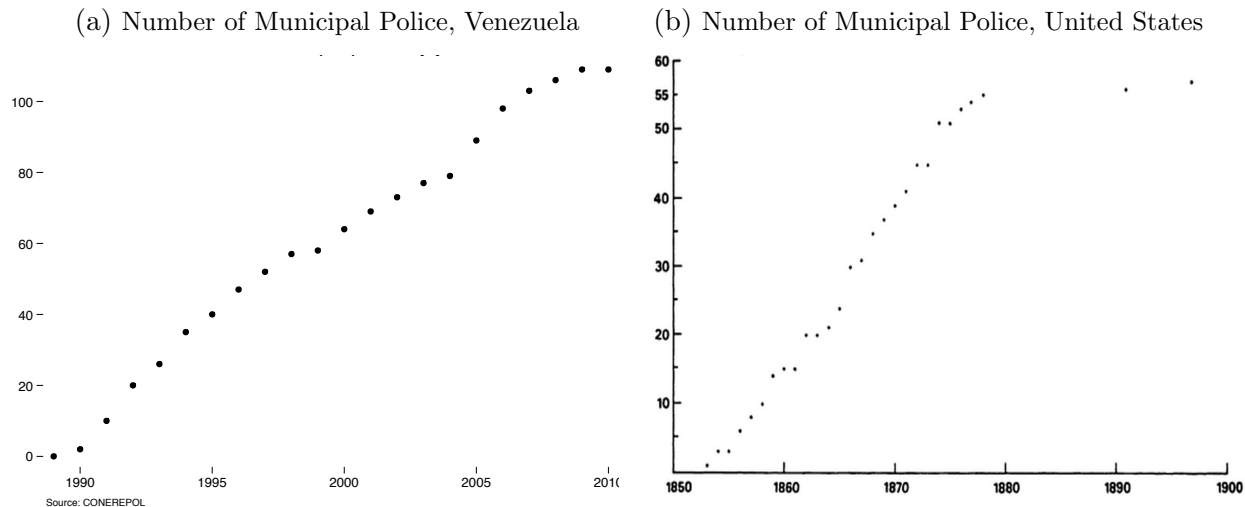


The data also suggest that changes in local homicide incidence are relatively uninformative about Venezuelan mayors. In 1989, a decentralization granted many new powers to the country’s 335 municipalities; among these was the ability to establish local police forces, with police chiefs appointed by the mayors. (Prior to the decentralization, large state police forces and a small national investigative force had been responsible for most of law enforcement). Since 1989, 110 municipalities ($\approx 1/3$) have established local police (see Figure 4a).

One might think that crime trends would determine when mayors establish police, but in fact pretends in crime do not predict municipal police arrival (Kronick (2014)). This pattern

echoes the U.S. case. Historian Eric Monkkonen compiled data on the date of introduction of uniformed police forces in U.S. cities and found that, contrary to assertions made in prior qualitative work, “Their introduction and dispersion was not a function of elite demands for class control, changing urban riots, or rising crime ... conformed to a rank-order dispersion model ... swept down the size hierarchy of U.S. cities, from large to small, in a forty-year period” (Monkkonen (2004)). This simple bureaucratic diffusion story also characterizes the spread of local police in Venezuela (Kronick (2014)).

Figure 4: Phase-in of Municipal Police



Moreover, using a difference-in-differences strategy, I cannot reject the null hypothesis that the establishment of a local police force does not affect either the level or the direction of change in the homicide rate. [Preliminary results also suggest that local police do not affect the relationship between switching mayors and the homicide rate (i.e., whether murder incidence changes with a new mayor)]. In other contexts, a change in the party of the mayor clearly does affect local violence (Dell (2011)). Thus, in the Venezuelan context, relative to outcomes such as the distribution of oil revenues, violence trends generally constitute weak signals about presidential, gubernatorial, and mayoral incumbents.

4 Estimating voter response to homicide outcomes: data and methods

How does voters’ perception of the informational value of violence outcomes compare to the data-driven assessment presented above? This section describes the data and methods used to estimate voter response to murder rates in Venezuela. The parameter of interest is \widehat{SNR} , or rather, voters’ idea of $(\sigma_{\theta_j}^2 / \sigma_{\epsilon_j}^2)$.

Under a set of fairly restrictive assumptions—namely, that the simplified model above may be

considered a behavioral model, such that changes in outcomes are unrelated to the error terms specified in the below equations—the estimators presented below will be consistent for \widehat{SNR} . However, the model abstracts away from strategic behavior on the part of politicians; if, for example, politicians anticipate the magnitude of candidate-place-specific shocks and allocate resources accordingly (c.f. Gordon and Hartmann (2011)), this strategic behavior could create dependence between changes in outcomes and the error term. Failure to correctly specify the vector of outcomes that enter into voter utility (i.e., omitted variables) could also create such dependence. In this case, even if the true conditional mean of voter utility is indeed linear in outcomes (or log of outcomes), the estimates below are best considered descriptive or predictive (rather than structural). Descriptive estimates of the best linear predictor of vote share given outcomes of interest are comparable to those in much of the retrospective voting literature.

In any case, as Equation 1 makes clear, \widehat{SNR} and ϕ —that is, the weight of a given outcome in voters’ utility function—are not separately identified. In other words, estimating voter response to an outcome tells us something about attribution (\widehat{SNR}) and welfare weights (ϕ) together. One way to address this problem is simply to hold ϕ fixed at a set of reasonable, assumed levels in order to separately estimate \widehat{SNR} ; otherwise the results should be interpreted as consistent for the product of these parameters. In the context of violence in Venezuela, we might conclude from survey data (see, e.g., Figure 2b) that public safety enters strongly into voter utility; thus, if we estimate the product of ϕ and \widehat{SNR} as being quite small, we might infer low levels of attribution.

4.1 Restricting the analysis to homicide (rather than other crimes)

While voter response to crimes other than homicide is of substantive interest, the pervasive and systematic underreporting of theft, assault, and other events precludes meaningful analysis of these figures. At a very high level of aggregation (say, the national level) victimization surveys and/or other measures such as internet search queries (Stephens-Davidowitz (2013)) might provide estimates of the incidence of crimes other than homicide; however, these are not available at the local level. Homicide data, in contrast, derive from vital statistics (Health Ministry) data; I discuss data sources in more detail below, but I note here that, while the vital statistics systems of Latin America may not be competent to accurately record subtle or obscure causes of death, they are generally able to correctly code gunshot and knife wounds (for an assessment of these systems, see (Danel and Bortman (2008))). In the Venezuelan case, the vital statistics data on homicide closely track the trend recorded by the police.²

²Homicide detectives and other investigative officers in Venezuela work for the CICPC, which is part of the Interior Ministry, rather than for local police units (as they do in the United States); as such, the CICPC maintains the government’s official data on violent crime. Prior to 2004, the CICPC published these data in an annual volume called *Estadística Delictiva*; in 2004, the government announced that it would cancel this publication and cease any other distribution of CICPC statistics. Sporadic CICPC figures are leaked to the

4.2 Estimating equations

Generalizing Equation 1 from two outcomes $j \in \{A, B\}$ to K outcomes $j \in \{1 \dots K\}$ and allowing m to index a voter's municipality gives the probability that voter i votes for the incumbent I in election t :

$$P(V_{imt} = I) = P\left(\sum_j \phi_j \frac{\sigma_{\theta j}^2}{\sigma_{\epsilon j}^2} \cdot \Delta X_{mt}^j > u_{imt}\right) \quad (2)$$

Different assumptions about the distribution of the u_{imt} produce different functional forms for the relationship between aggregate vote shares and changes in the homicide rate. Letting $\tilde{u}_{imt} = u_{imt} - \bar{u}_{mt}$ (i.e., \tilde{u}_{imt} denote individual deviations from the average relative shock in municipality m in election t), if $\tilde{u}_{imt} \sim U(\underline{a}, \bar{a})$, the incumbent vote share V_{Imt} takes a linear form (Hibbs (2006)). Allowing the mean of incumbent vote share to vary by municipality and by election then yields:

$$V_{Imt} = \alpha_m + \beta_t + \sum_j \psi_j \cdot \Delta X_{mt}^j + \xi_{Imt}, \text{ where} \quad (3)$$

$$\psi_j = \frac{\phi_j}{c} \cdot \frac{\sigma_{\theta j}^2}{\sigma_{\epsilon j}^2}$$

and $c = \bar{a} - \underline{a}$ and, as before m indexes subnational regions (states, counties, or precincts). ξ_{Imt} is a candidate-municipality-election-specific shock term. In this case, the ψ_j can be estimated by OLS.

Alternatively, maintaining the assumption that the u_{itp} have standard type-I extreme value distributions, and allowing for additional unobservable (to the econometrician) candidate-municipality-election-specific shocks γ_{Imt} , we can derive the aggregate logit formulation (see Berry (1994) for derivation):

$$\ln(V_{Imt}) - \ln(V_{0mt}) = \alpha_m + \beta_t + \sum_j \psi_j \cdot \Delta X_{mt}^j + \gamma_{Imt}, \text{ where} \quad (4)$$

$$\psi_j = \phi_j \cdot \frac{\sigma_{\theta j}^2}{\sigma_{\epsilon j}^2}$$

where V_{0t} is the proportion of voters who abstained. Then, as before, the ψ_j s can be estimated by OLS, and the $\sigma_{\theta j}^2/\sigma_{\epsilon j}^2$ s can be estimated by holding the ϕ_j s fixed at various levels (motivated by survey responses).

I estimate both Equation 3 and Equation 4. Some specifications allow ψ_j to vary with the nature of local crime-control institutions. Specifically, some specifications interact the change in homicide rate with indicators for (a) whether a municipality has just established its own

press and to a number of NGOs, but complete disaggregated CICPC data are no longer available.

police force (within this electoral cycle) and (b) whether a municipality has had local police for at least one electoral cycle. These specifications can thus be written:

$$\begin{aligned}
V_{I_{mt}} = & \alpha_m + \beta_t + \psi_1 \Delta Homicide_{mt} + \beta_1 NewPolice_{mt} + \beta_2 OldPolice_{mt} \\
& + \psi_2 (\Delta Homicide_{mt} \cdot NewPolice_{mt}) + \psi_3 (\Delta Homicide_{mt} \cdot OldPolice_{mt}) \\
& + \sum_j \psi_j \cdot \Delta X_{mt}^j + \xi_{I_{mt}}
\end{aligned} \tag{5}$$

where $NewPolice_{mt}$ is an indicator taking the value of one when municipality m established its own local police force between election $t - 1$ and election t (that is, when the incumbent mayor established the municipal police) and $OldPolice_{mt}$ is an indicator taking the value of one when municipality m established its own police force prior to election $t - 1$. Thus, ψ_1 captures the relationship between crime and incumbent vote share in municipalities without local police, $\psi_1 + \psi_2$ captures the relationship between crime and incumbent vote share in municipalities that just established local police, and $\psi_1 + \psi_3$ captures the relationship between crime and incumbent vote share in municipalities that have had their own police for at least one electoral cycle. Some specifications include additional interactions with indicators for whether the incumbent mayor herself is participating in the election (if not, incumbent vote share denotes the share of the incumbent mayor’s party).

Estimating equations for the precinct-level analysis are somewhat different. As explained in the data section below, geocoded homicide data have been collected only since 2010; therefore, it is not (yet) possible to calculate change in neighborhood murder incidence over the entire term in office of incumbent presidents, governors, legislators, or mayors. Therefore, I simply estimate descriptive regressions that predict incumbent vote share on the basis of (a) past vote share and (b) the level of violence in the neighborhood in the weeks leading up to the election. For precinct p in election t , then, I estimate:

$$V_{I_{pt}} = \alpha + \beta_1 V_{I_{p(t-1)}} + \beta_2 Homicide_{pt} + \beta_3 X_{pt} + \nu_{pt} \tag{6}$$

Where $Homicide_{pt}$ is one of several measures of violence in the neighborhood around the electoral precinct in the weeks leading up to the election. In consideration of the fact that local violence might be related to economic or other factors unobserved (to the econometrician), I also estimate versions of Equation 6 that predict incumbent vote share on the basis of violence in the weeks after the election (i.e., $Homicide_{p(t+1)}$), as a kind of placebo.

These regressions, while purely descriptive, explore the possibility that voters respond differently to very local homicide incidence than to city- or county-level trends. Within the time period and geographic region in which the geocoded homicide data are available, all municipalities have had local police forces for several electoral cycles; I therefore cannot estimate how β_2 from Equation 6 varies with crime-control institutions. However, I compare β_2 across different types of elections: presidential, legislative, and mayoral.

4.3 Data

Estimating Equations 3 and 4 requires data on vote shares, crime outcomes, economic outcomes, crime-control institutions, and other correlates.

- **Election returns.** Precinct-level election returns in Venezuela are available on the website of the National Electoral Council (*Consejo Nacional Electoral*, or CNE) and (in a more convenient format) on the website of the university-based group ESDATA. I use these data to construct precinct- and county-level vote shares and abstention rates for mayoral, gubernatorial, and presidential elections in the 1999–2011 period. [Note: A future draft will extend the analysis to previous decades.] This period includes four presidential elections (rather, three presidential elections and one presidential recall referendum), three municipal elections in each of 335 counties, and three gubernatorial elections in each of 23 states. ESDATA also provides geographic coordinates (latitude and longitude) for each polling place (precinct). The Venezuelan electoral system counts votes correctly on election day; for a discussion of fraud accusations, see Appendix [].
- **Homicide incidence.** County-level homicide data derive from the Ministry of Health civil death registry and were obtained privately for the purposes of this project. These data comprise the number of deaths by cause (International Classification of Diseases), year, and municipality, from 1999 through 2011.³ [Note: I am in the process of digitizing analogous paper records for the 1938–1995 period.]

I construct precinct-level homicide incidence using data provided for this project by the police force of the Venezuelan state of Miranda, which has a population of approximately three million people and which includes part of Caracas. For each homicide since the beginning of 2010, these data include the homicide location (latitude and longitude, recorded by the police with a GPS device), time and date, victim name, and victim national ID number (*cédula*).

I use these data to construct two measures of homicide incidence at the level of electoral precinct. The first measure is spatial: using homicide coordinates together with the polling place locations mentioned above, I compute homicide incidence (and homicide trends) in the physical neighborhood around each precinct. The second measure is social: using national ID numbers (*cédulas*), I merge the homicide victim list with the voter registry (*Registro Nacional Electoral*), which lists the precinct of each voter, thereby measuring the incidence of homicide among voters registered in each precinct. Appendix [] presents descriptive information on homicide trends.

³The following ICD-10 codes were included in the homicide count: Codes X85-Y09, which comprise assaults; codes Y20, Y22, Y23, and Y24 (strangling and firearm discharge of undetermined intent), which account for about one-third of all deaths included in the homicide count; codes Y28 (contact with sharp object, undetermined intent) and Y29 (contact with blunt object, undetermined intent); and codes W32 (handgun discharge) and W34 (discharge from other and unspecified firearms).

- **Crime-control institutions.** As described in Section 3, Venezuelan municipalities founded police forces at different times. the dates are compiled and published by the general police council (Atlas Policial de Venezuela (2012)).
- **Other outcomes.** Identification of the parameters in Equations 3 and 4 requires observing and including in the estimation other (non-crime) outcomes that affect vote choice. In the county-level estimates, I include changes in (a) the incidence of cash transfers to households (which have been shown to generate sizable electoral returns in the Venezuelan context (Ortega and Penfold-Becerra (2008))) and (b) various measures of local fiscal revenue and expenditure. In addition, I allow vote-share trends to vary with a rich set of county socioeconomic characteristics included in the 2001 census. In the precinct-level estimates, available data do not permit inclusion of time-varying controls; therefore, I allow vote-share trends to vary with a rich set of neighborhood characteristics, derived from tract-level 2001 census data (linked to electoral precincts with a spatial merge). For details on covariate sources and definitions, see Appendix [].

4.4 Heterogeneity in welfare weights (ϕ)

Section 4.2 assumed that all voters have identical utility functions, that is, that $\phi_i = \phi \forall i$. Relaxing this assumption, Equation 4 takes a mixed logit (random coefficients) formulation; the parameters of the distribution of ϕ_i can then be estimated using the methods articulated in (Berry et al. (1995)) and developed in (Dubé et al. (2012)). A future draft will incorporate these estimates.

5 Estimating voter response to homicide outcomes: Results

5.1 Surveys about government control of crime

In the Venezuelan context, surveys that ask respondents directly about government control over or responsibility for crime do not ask questions specific to homicide. [Note: an in-progress survey will correct this.] While questions about crime in general may be informative, work in the Mexican context has shown that the relationship between other crimes and survey-reported presidential approval is quite different than the relationship between homicide and presidential approval. Specifically, Romero and Magaloni (2013) find that, while auto theft incidence is negatively correlated with presidential approval, homicide incidence is not.

Nevertheless, Hanson and Smilde (2013) interpret the survey evidence from Venezuela as strongly indicative of a weak connection between crime and evaluations of incumbents. “Most people do not consider Venezuelas crime problem to be the fault of the President,” they write, “Neither do they see it primarily as the result of ineffective policing.” Table 1 presents results from various surveys in which respondents are asked about the connection between crime and politicians; a majority of respondents agree with the statement “Crime is such a difficult problem that no president could solve it,” and only 10–21% of people mention government or (lack of) policing in response to open-ended questions about the causes of crime.

Table 1: Survey data (Venezuela only)

Question	Survey	Response
What do you consider the principal causes of crime? (Percent responding government or police on first mention)	Datanálisis 09/2013, N=1295	20.4% (1.1)
Of the following, which do you consider principal causes of crime? (Percent including “scarcity of police” in list of responses)	Latinobarómetro 2004, N=1200	11.8% (0.9)
Crime is such a difficult problem that no president could solve it. (Percent in agreement)	Datanálisis 09/2013, N=1300	56.8% (1.4)
A different president could solve the problem of crime. (Percent in agreement)	Datanálisis 09/2013, N=1300	27.8% (1.2)
	LAPOP 2012, N=1400	

Standard errors in parentheses. Sources: Hanson and Smilde (2013), Datanálisis, Latinobarómetro, Latin American Public Opinion Project.

5.2 Voter response to murder rates in presidential, gubernatorial, and mayoral elections

This section presents estimates of Equations 3 and 4, which predict presidential incumbent vote shares on the basis of changes in local homicide rates. Table 2 presents results for the 2000, 2004, and 2006 presidential elections. [A future draft will extend the series to previous elections and to the 2012 presidential election.] In both the OLS and the aggregate logit specifications, the partial correlation between changes in homicide rate and changes in the vote share of presidential incumbent Hugo Chávez is precisely estimated at zero: the smallest point estimate reflects a -0.5-percentage-point decline in incumbent vote share for a one-standard-deviation increase in homicide rate (statistically indistinguishable from zero); the largest point estimate reflects a +0.2-percentage-point increase in incumbent vote share for a one-standard-deviation increase in homicide rate (also statistically indistinguishable from zero).

Appendix [] reports specifications with additional control variables (for example, controls

for municipality fiscal income and/or fiscal expenditure); the precisely estimated zero is quite stable across these specifications. Appendix [] also reports these results for alternate definitions of change in homicide rate. Table 2 defines change in homicide rate as the weighted average annual change in normalized homicide rate (normalized by the standard deviation of the homicide rate in that municipality over time). Normalizing in this way simply allows voters to respond differently to an average annual increase of 10/100,000 in a municipality where homicide rate fluctuates wildly than in a municipality where homicide rates are largely stable over time. The average over years of the politician’s term places higher weight on more recent years, specifying exponential decay weights as in (Hibbs (2006)). However, as Appendix [] indicates, the results are substantively invariant to defining change in homicide rate as an unweighted average across years, as the previous-year change only, as percent rather than absolute change, and to not normalizing by standard deviation.

These zero estimates stand in stark contrast to the relationship between cash transfers and Chávez’s vote share. As others have noted (Ortega and Penfold-Becerra (2008)), distribution of cash transfers and other benefits through Chávez’s flagship *Misiones* programs brought large electoral returns. The estimates in Table 2 indicate that providing stipends associated with the *Misión Ribas* education program to an additional 1% of the population in a given municipality was associated with an additional two percentage points for Chávez (the maximum proportion of the population covered in that year was 7%). The magnitude of this relationship is in line with that estimated by others and perhaps reflects the fact that stipends are often shared with other household members.

Table 3 presents analogous results for gubernatorial elections. Since [for this draft] gubernatorial vote shares at the municipal level are only available for the 2004 and 2008 presidential elections, I define the dependent variable as the change in gubernatorial incumbent vote share between these two elections; all specifications include state fixed effects, to account for unobserved heterogeneity in gubernatorial incumbent performance. Thus the estimates effectively ask whether a municipality with especially high increases (or especially large decreases) in homicide rates *relative to other municipalities in the same state* support the incumbent less (more) than other municipalities in the same state. The results provide a clear negative response: as in the case of presidential elections, the point estimates are precisely estimated close to zero.

5.3 Voter response to homicide in the context of new local police

As noted above, Venezuelan municipalities established local police forces at different times. In the model of Section 4, voters’ perceptions of whether a different mayor would make a difference—or, in other words, perceptions of the cross-politician variance in effect on

Table 2: Homicide Rates and Presidential Vote Shares

	OLS	OLS	Logit	Logit
Δ Homicide rate (normalized) (Mean=.2, SD=.8)	0.003 (0.003)	0.001 (0.003)	-0.006 (0.009)	0.003 (0.007)
% population w/ Ribas stipend, 2006 (Mean=.01, SD=.01)		1.9** (0.9)		5.8** (2.4)
Mean of DV	0.6	0.6	0.7	0.7
SD of DV	0.1	0.1	0.3	0.3
Observations	995	995	995	995
Municipality & election fixed effects	✓	✓	✓	✓
Additional controls		✓		✓

Standard errors in parentheses. *0.10, **0.05, *** 0.01. Dependent variable is Chávez’s vote share (presidential incumbent) in the 2000, 2004, and 2006 presidential elections (2012 will be included in a future draft); Equations 3 and 4. Additional controls include allowing time trends to vary by state, municipality (log) population, and median income. Normalized change in homicide rate defined as the absolute change in homicide rate divided by the standard deviation of homicide rates within that municipality over time; $\Delta Homicide$ takes a weighted average of these normalized changes over the incumbent’s term in office (weighted by recency).

the homicide rate—determine the extent of electoral response to changes in homicide rates. The results presented in this section test whether the establishment of a local police force (temporarily) changes voter perceptions of this cross-politician variance. These tests are motivated by the notion that voters might initially assume that control of a local police force increases the extent to which a different mayor might change the homicide rate.

Figure 5 plots the change in the vote share of the incumbent mayor (or of the incumbent mayor’s party, if the incumbent candidate is not running) against the weighted average annual change in normalized homicide rate (defined as above) over that incumbent’s term in office. Each dot represents one municipality-election; each municipality therefore appears twice. The left panel, Figure 5a, plots this relationship for two groups of municipalities: in light gray, municipalities that have not established local police; in dark gray, municipalities that have had their own local police for at least one electoral cycle. In both cases, the best linear fit line is flat. The right panel, Figure 5b plots the same relationship for the subset of municipality-elections in which the incumbent mayor (party) established a local police force within the most recent electoral cycle. In these cases, the relationship is strongly negative: mayors with the largest increases in homicide win on average twenty fewer percentage points than mayors with no change in the homicide rate.

These simple descriptives pool elections across municipalities and across years. Table 4 presents estimates of Equation 5, which uses within-municipality, within-year variation to estimate the relationship between changes in the local homicide rate and changes in incumbent (or incumbent party) vote share. In municipalities without local police or in municipalities that have had their own municipal police for at least one electoral cycle, the relationship between change in homicide rate and change in incumbent vote share is precisely estimated

Table 3: Homicide Rates and Gubernatorial Vote Shares

	OLS	OLS	Logit	Logit
Δ Homicide rate (normalized) (Mean=.1, SD=.4)	0.002 (0.01)	0.007 (0.01)	0.05 (0.03)	0.03 (0.03)
Mean of DV	-0.1	-0.1	0.08	0.08
SD of DV	0.2	0.2	0.4	0.4
Observations	324	324	324	324
State fixed effects	✓	✓	✓	✓
Additional controls		✓		✓

Standard errors in parentheses. *0.10, **0.05, *** 0.01. Dependent variable is the change in the incumbent governor’s vote share from the 2004 gubernatorial election to the 2008 gubernatorial election (2004-2000 will be included in a future draft); Equations 3 and 4. Additional controls include allowing time trends to vary by municipality (log) population, median income, and proportion of the population receiving a Misión Ribas stipend. Normalized change in homicide rate defined as the absolute change in homicide rate divided by the standard deviation of homicide rates within that municipality over time; $\Delta Homicide$ takes a weighted average of these normalized changes over the incumbent’s term in office (weighted by recency).

at zero.

In contrast, in municipalities with newly established local police, there is a large negative relationship between change in homicide rate and change in mayoral incumbent vote share. Despite the small number of municipality-elections in which (a) the incumbent established a police force and (b) the incumbent is participating (N=24), this relationship is precisely estimated. In these municipalities, a one-standard-deviation increase in the change in homicide rate is associated with a ten-percentage-point drop in support for the incumbent—approximately the difference between elections in which the incumbent mayor participates and elections in which he does not participate.

Figure 5: Local Police, Homicide, and Incumbent Mayors

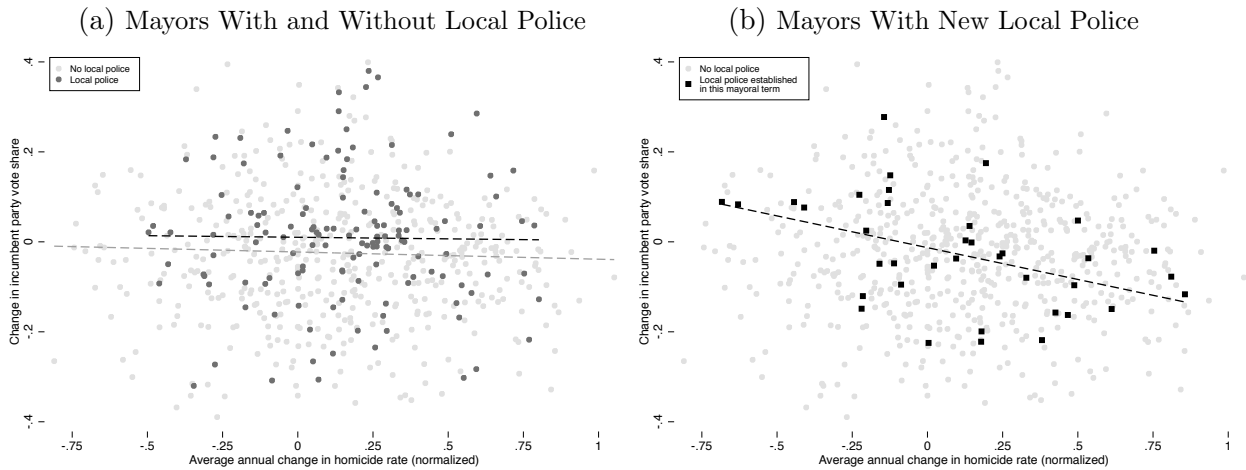


Table 4: Homicide Rates and Mayoral Vote Shares

	(1)	(2)
Incumbent participating (0/1) (Mean=.63)	0.09*** (0.03)	0.1*** (0.03)
ψ_1 : Δ Homicide, Municipalities without local police (Mean=.13, SD=.37)	-0.04 (0.04)	-0.06 (0.04)
$\psi_1 + \psi_3$: Δ Homicide, Municipalities with (old) local police (Mean=.16, SD=.29)	0.009 (0.10)	0.04 (0.10)
$\psi_1 + \psi_2$: Δ Homicide, Municipalities with new local police (Mean=.07, SD=.41)	-0.2* (0.09)	-0.2** (0.09)
Mean of DV	-0.02	-0.02
SD of DV	0.1	0.1
Observations	637	637
Municipality & election fixed effects	✓	✓
Additional controls		✓

Standard errors in parentheses. * 0.10, ** 0.05, *** 0.01. Dependent variable is the change in the incumbent mayor's vote (or incumbent party's vote share) in the 2004 and 2008 mayoral elections (2004 is change relative to 2000); estimates of Equation 5. Change in homicide rate also interacted with dummy for incumbent participation (i.e., coefficients shown are for municipalities in which the incumbent is participating). Additional controls include allowing time trends to vary by state, by municipality (log) population, median income, and proportion of the population receiving a Misión Ribas stipend. Normalized change in homicide rate defined as the absolute change in homicide rate divided by the standard deviation of homicide rates within that municipality over time; $\Delta Homicide$ takes a weighted average of these normalized changes over the incumbent's term in office (weighted by recency).

5.4 Voter response to neighborhood murder rates

The theory outlined above leaves unspecified the level of geographic aggregation at which voters observe citizen security outcomes. Perhaps, as the analysis of Sections 5.2 and 5.3 implicitly assumes, voters observe violence at the level of municipality; in mayoral elections, the municipality is also the electoral jurisdiction, which makes it a natural reference point for voters.

However, homicide incidence varies dramatically across neighborhoods within municipalities, some of which are quite large (the biggest 5% of municipalities contain more than 250,000 people). If voters are more sensitive to violence outcomes within their own neighborhoods than in the municipality as a whole, and if homicide incidence increases in some neighborhoods while decreasing in others, the municipality-level results might mask electoral response to changes in violence. This section thus describes the relationship between incumbent vote shares and homicide incidence at the neighborhood level. I compare this relationship in three elections: the legislative elections of 2010, in which voters elected representatives for all seats in the National Assembly; the presidential election of 2012, in which Hugo Chávez competed against challenger Henrique Capriles, and the mayoral elections of 2013. Section 4 above and Appendix [] describe in more detail the process of constructing both homicide

rates and socioeconomic measures at the level of electoral precinct (using geocoded data on homicide, geocoded data on electoral precincts, and census-tract-level census data).

Table 5 presents estimates of Equation 6 for each of the three elections. The independent variable is an indicator taking the value of one when that precinct experienced unusually high homicide levels in the specified time window. For these specifications, I define unusually high homicide levels by: (a) constructing a weekly (or biweekly, or triweekly, or monthly, etc.) homicide count for each precinct; (b) calculating the mean of that count over the (respective) pre-election time period, (c) calculating the standard deviation of that count over the observed time period (2010–present); (d) normalizing (a) by (b) and (c); and finally (e) defining an indicator that takes a value of one for those precincts where the normalized homicide rate is in the top 5% (across precincts) in the given period. In practice, this means that the indicator takes a value of one for precincts where the homicide count is approximately two standard deviations above its own historic average; defining the indicator alternately as those precincts who are exactly two standard deviations above their past mean, or three standard deviations above, does not substantively change the results (see Appendix []).

The results strongly indicate that perceptions of the *SNR* contained in violence outcomes condition voting behavior. While the data suggest that mayors cannot much affect overall violence levels in their municipalities, they may be able to affect which neighborhoods are most affected within the municipality; perhaps in response to this distributional role, voters do respond to very high neighborhood violence levels in the weeks leading up to the election. On average, incumbents obtain one to two fewer percentage points in precincts with high neighborhood violence just before the election than in precincts without high violence. Because economic outcomes and other covariates are not measurable at the neighborhood-week level, I estimate the same equation using post-election violence in the neighborhood; these estimates are small and statistically indistinguishable from zero.⁴

In contrast, legislative and presidential incumbents perform no worse in precincts with high violence in the weeks leading up to the election. Coefficients on the pre-election violence indicators in these polls are small and statistically indistinguishable from zero.

⁴While voter intimidation is a growing concern in Venezuela, the media actively covers this issue and, to my knowledge, there are no reported cases of election-related homicide in the immediate pre-election periods. Thus it is unlikely that anti-incumbent violent activity drives both the electoral response and the violence. Moreover, covariates such as past vote share do not predict the location of pre-election violence; the data cannot reject the null that these locations are determined by the error term. See Appendix [] for details.

Table 5: Neighborhood Homicide and Incumbent Vote Shares

Week	-6	-5	-4	-3	-2	-1	1	2	3	4	5	6
Mayoral	-2.0** (0.8)	-2.3** (0.9)	-2.3** (1.0)	-1.6 (1.0)	-1.4* (0.8)	-1.3 (0.9)	0.2 (0.6)	0.4 (0.4)	-0.8 (0.7)	-0.8 (0.9)	-0.9 (0.8)	-0.7 (0.8)
Legislative	0.006 (0.4)	0.6 (0.4)	-0.4 (0.4)	-0.003 (0.5)	0.2 (0.5)	-0.04 (0.3)	-0.1 (0.3)	0.1 (0.3)	-0.03 (0.4)	-0.2 (0.4)	0.04 (0.4)	-0.03 (0.3)
Presidential	0.2 (0.7)	0.9 (0.7)	0.5 (0.6)	0.5 (0.6)	-0.6 (0.8)	-0.006 (0.2)	0.1 (0.7)	-0.4 (0.9)	0.3 (0.8)	-0.09 (0.9)	0.1 (0.7)	-0.4 (0.7)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Additional controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses. * 0.10, ** 0.05, *** 0.01. Estimates of Equation 5; dependent variable is incumbent vote share (scaled 0–100); independent variable is an indicator for whether that precinct had unusually high homicide incidence in the specified time window. The mayoral election is that of December 8, 2013; the legislative election is September 26, 2010; the presidential election is October 7, 2012. Included covariates are: population density, median income, proportion of voters with a college degree, maximum income, and proportion of households with an unfinished floor.

6 Discussion

Theories of blind retrospective voting predict that voters punish political incumbents for bad outcomes and reward them for good ones, whether or not the elected official played much role in determining the outcome. In the case of Venezuela, where voters have suffered one of the largest violent crime waves of the modern world, these theories would predict that, conditional on economic and other outcomes, political incumbents would have lost vote share where violence increased the most.

Instead, the results presented in this paper indicate that, by and large, Venezuelan voters did not vote against incumbent presidents, mayors, governors, or legislators in response to rising homicide incidence. In light of objective assessments of the role of these officials in determining violence relative to the role of external shocks such as enforcement events in neighboring Colombia (e.g., Kronick (2014)), these results are more consistent with rational retrospective models of voter behavior, in which the electorate responds to outcomes that provide meaningful signals about politicians. On the eve of the 2006 presidential election, in which incumbent Hugo Chávez bested challenger Manuel Rosales by thirty percentage points, a front-page *New York Times* headline read, “As Crime Soars, Chávez Coasts.” In providing quantitative documentation of that qualitative impression, this paper also explains the political logic behind weak voter response to violence.

The theory and results also serve to highlight two general points about adjudicating between models of voter behavior. First, that assessing the rationality of voters’ perceptions of the signal-to-noise ratio contained in a given outcome depends on a credible, objective estimate of that signal-to-noise ratio. Second, that the need for an objective estimate of that ratio provides additional impetus for studying outcomes other than economic growth (since decomposing the sources of growth is a macroeconomist’s errand). This paper takes a first step toward applying these insights to the study of retrospective voting on violent crime. In doing so, I improve our understanding of voting in the context of a key aspect of human welfare.

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