

# Misgovernance or Malgovernance? The Case of Criminal Justice Reform

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## Abstract

Is bad policy the product of ignorance or of interests? In this note, I consider the case of criminal justice reform in Venezuela. After a new code of criminal procedure increased protections for suspects and defendants, legal scholars and journalists claimed that police reacted by killing those they could no longer arrest and imprison. Using microdata on violent deaths, I find support for this hypothesis, suggesting that, contrary to reformers' expectations, the old code in fact served powerful interests.

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# 1 Introduction

Some bad policies are born of ignorance—misgovernance—and enlightened reform can make a difference. Other bad policies serve powerful interests—malgovernance—and reform efforts face opposition or backlash.<sup>1</sup> Distinguishing the two should be a case-specific empirical exercise (Dilulio, 1992, p. 722).

In this note, I find that theories of malgovernance better describe the case of criminal justice reform in Venezuela, where the government implemented a criminal procedure code similar to those put in place across Latin America since 1990 (Langer, 2007). Reformers saw the old codes as ill-conceived anachronisms; critics warned instead that the old codes served powerful contemporary interests—namely, the police.

Venezuela’s new code of criminal procedure restricted arrest powers, requiring officers either to obtain a warrant or to observe a crime *in flagrante* before making an arrest. This worked: the arrest rate plummeted. But legal scholars, sociologists, and journalists claimed another, unintended consequence: that police who “felt deprived of their enormous power” killed those they could no longer arrest and imprison (Alguíndigue and Pérez Perdomo, 2008, p. 109–110).

I provide a quantitative evaluation of this hypothesis, using daily mortality data to observe changes in lethal violence. I find that violent deaths jumped 25% immediately after the new code came into effect (Section 3.2). I find no comparable jump in other causes of death around the same date, or for violent deaths around the same day (July 1) in other years. While there is no direct, comprehensive measure of police use of lethal force in Venezuela, incomplete data collected by an NGO reveal an increase in the year following the new criminal procedure code (Section 3.2). Journalistic accounts suggest how the new rules might have threatened the economic interests of the police (Section 3.1).

I assess competing explanations for the jump in the violent death rate. As intended, the arrest rate fell when the new code came into effect (Section 2); perhaps, then, the observed increase in lethal violence was driven not by police reaction but by rational response to the reduced probability of arrest, which lowered the cost of committing murder (Becker, 1968). Using data on other crimes, however, I find no jump in robbery rates when the new code came into effect. This finding is inconsistent with

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<sup>1</sup>See, e.g., Banerjee (1997); Acemoglu and Robinson (2000); Hirschman (1991); in the context of criminal justice systems, see especially Brinks (2008), and Weaver (2007), Mummolo (2017), Gottschalk (2015, 2006).

the notion that the jump in lethal violence was driven by a generalized response to lower arrest rates. Another possible explanation is that civilian vigilantism increased in response to the perceived inefficiency of the new criminal procedure code; I view police violence as more plausible principally because of the immediacy of the jump in lethal violence, but I can not quantify private lynchings. Section 3.3 discusses these and other alternative explanations.

The analysis allows me to bound the magnitude of police reaction to the implementation of the new code. The number of additional lethal violence victims in the wake of the new code constitutes approximately 1.8% of the missing arrests. In other words, if we were to attribute the *entire* jump in lethal violence to extra-judicial police killings, we would conclude that “the police counter response was to kill suspected criminals” 1.8% of the time: for each 1,000 missing arrests, there were 18 deaths (quote from Alguíndigue and Pérez Perdomo, 2008).

Were the true magnitude even a small fraction of that number, this finding would constitute evidence that the reform “provoked political economy responses from those who [saw] their economic or political rents threatened” (Acemoglu 2010).

## 2 Intended consequences of the new code of criminal procedure

In 1998, the Venezuelan legislature approved a new code of criminal procedure that entered into force the following year, on July 1, 1999.<sup>2</sup> The code was drafted by lawyers, judges, and legal scholars who sought to change a criminal justice system that “remained a blemish on [Venezuela’s] image of liberal democracy” and “crushed poor and uneducated suspects in its Kafkaesque gears” (Alguíndigue and Pérez Perdomo, 2008, 2013).

Venezuela’s new code of criminal procedure marked a paradigm shift (Birkbeck, 2003, p. 2), replacing the written, inquisitorial criminal procedure typical of civil law systems with the oral, accusatorial procedure typical of common law systems (Langer, 2007; Birkbeck, 2003; Alguíndigue and Pérez Perdomo, 2013, p. 124). Where previously judges participated in investigation and prosecution (Langer, 2007, p. 629), in

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<sup>2</sup>Three articles entered into force immediately, in 1998: mechanisms for alternative dispute resolution (Articles 34–36), procedures for plea bargaining (Articles 376, 504, and 505), and access to exhibits for the defendant and her counsel (Article 313).

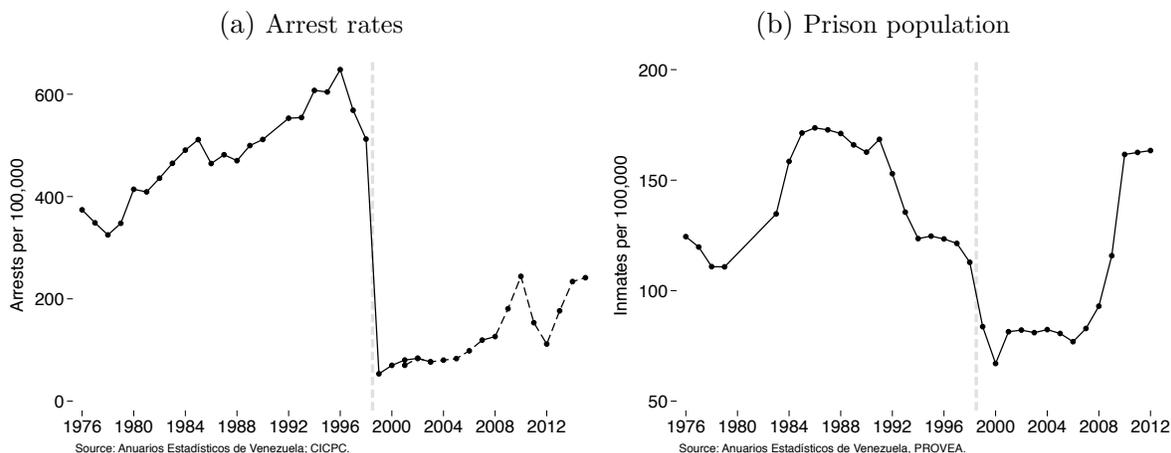
the new system the judge’s role was limited to adjudication (IADB 2002, p. 6).

Two changes are especially relevant for this study. First, the new code allowed the police to make arrests only after (a) obtaining a warrant or (b) observing a crime being committed; the previous code allowed police to make arrests as part of pre-trial investigations (IADB 2002, p. 6–7; CEC, 1962, Article 75-H; COPP, 1998, Articles 259–264; Vásquez González, 2017).

Second, the new code aimed to make pre-trial detention the exception rather than the rule: suspects could be detained pre-trial only under judicial order, and only under new, more restrictive guidelines (Birkbeck, 2003, p. 9–10). Moreover, those detained without a warrant (i.e., those caught *in flagrante*) could be held only for 48 hours, after which the public prosecutor had to either indict or release them (Birkbeck, 2003, p. 10).

These changes had immediate effects both on the arrest rate and on the incarceration rate. The arrest rate fell 90% between 1998 and 1999, from 512 per 100,000 to 53 per 100,000 (Figure 1a); by way of comparison, the Colombian police made 343 arrests per 100,000 in 2008 (PN, 2008). The prison population fell 25% between 1998 and 1999 (Figure 1b).<sup>3</sup> For comparison, the incarceration rate in Colombia was 156 per 100,000 in 2008 (CEJ 2012).

Figure 1: Incarceration and arrests before and after the new criminal procedure code



<sup>3</sup>The prison population also fell quickly in 1992–1994, likely due to a new pre-trial detention law (the *Ley de Libertad Provisional Bajo Fianza*).

## 3 Possible unintended consequences of the new code of criminal procedure

### 3.1 The hypothesis about extra-judicial killings

Police officers publicly lamented their loss of power under the new code of criminal procedure. “They’ve taken away the authority we had,” a Caracas police inspector told the Associated Press in August, 1999, one month after the new code came into effect (Jones, 1999). Police also criticized public prosecutors’ inability to gather evidence or to conduct investigations, both of which were previously responsibilities of the police, and bemoaned the release of suspects arrested without a warrant (i.e., those who the police claimed had been caught *in flagrante*).<sup>4</sup> One human rights group wrote that the the police “waged war on the new system” by trying to convince the public that the new code had increased crime (COFAVIC, 2005, p. 20).

Legal scholars, sociologists, and prosecutors suggested that police officers may have responded to the new code, or to failures in its implementation, by substituting the use of lethal force for unchecked arrest powers. For sociologist Andrés Antillano, the new code and prior reforms

implied the reduction of the legal powers of the police (such as arrests without warrants...). This resulted in a decline in excessive use of legal power, such as arbitrary detentions and the use of torture to obtain confessions. But at the same time, there seems to have been an increase in activity related to the use of informal (and extra-legal) power by the police, such as killings (Antillano, 2010, p. 340–341).

Monsalve Briceño (2006), based on interviews with police officers in Caracas, concluded that the new code restricted arrest powers “that the police had used as effective substitutes for judicial punishment” and that officers might therefore consider it “within their competence” to punish via physical force (p. 14).<sup>5</sup>

Similarly, Alguíndigue and Pérez Perdomo (2008, p. 109–110) wrote:

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<sup>4</sup>For police complaints about prosecutors gathering evidence, see Rivera (2003); Jones (1999); Monsalve Briceño (2006, p. 21). For complaints about releasing detainees, see, e.g., Monsalve Briceño (2006), p. 20, “Pedro” from Polioriente says: “I feel impotent when I see a criminal back on the street two hours after bringing him to the prosecutor.”

<sup>5</sup>Asked whether the police are *legally* responsible for applying punishment, one officer said “currently no, but before the [new criminal procedure code] yes” (Monsalve Briceño, 2005, p. 5).

The police felt that the legal reforms deprived them of the enormous power they previously enjoyed. The police could no longer imprison known criminals . . . But even in cases where people were caught *in flagrante delicto*, the judges could set the criminals free on technicalities . . . The police responded by killing suspected criminals rather than taking the risk that judges would free them later on procedural grounds.

Prosecutors also accused the police of a violent response to the new code. For example, one prosecutor wrote that “some police officials, instead of respecting the civilizing limits imposed by the new code of criminal procedure (which apparently threatens their protagonism), prefer to take justice into their own hands” (Moreno Pineda, 1999).

### 3.2 Evaluating the hypothesis about extra-judicial killings

Ideally, I would evaluate this hypothesis using a direct and comprehensive measure of deaths at the hands of police; unfortunately for the purposes of this study, this measure does not exist (González Mejías and Kronick, 2017). Instead, I consider lethal violence overall—for which I do have a high-quality, high-frequency measure—and then use additional data to adjudicate among competing explanations for the overall violence trend.

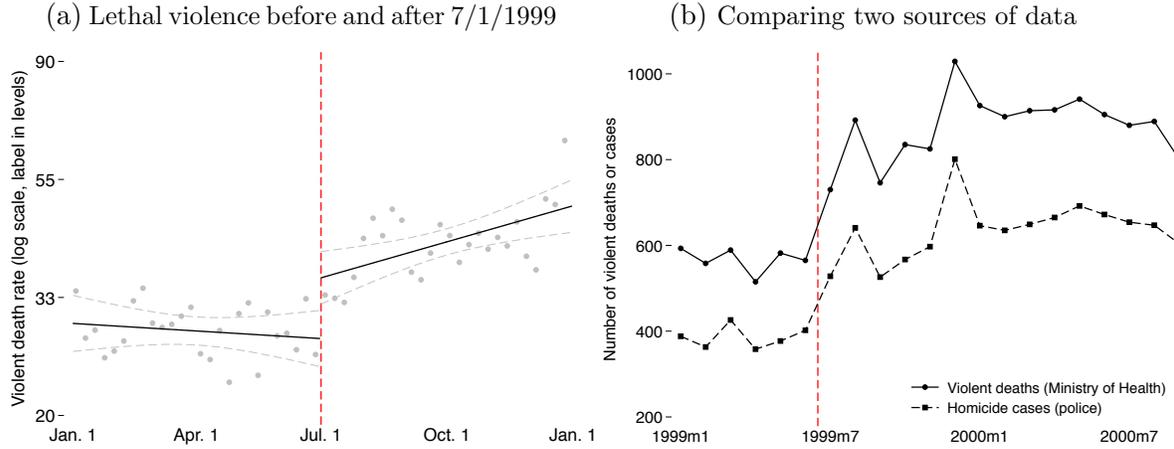
The lines in Figure 2a show predictions, from linear regressions, of log daily violent death rates; the points mark weekly means.<sup>6</sup> The data derive from vital statistics, maintained by the Ministry of Health (see González Mejías and Kronick, 2017, for details). The vertical line marks July 1, 1999, when the new criminal procedure code entered into force. The graph reveals that lethal violence jumped in the weeks following July 1, relative to the weeks prior. Observing the daily data (as opposed to annual data) is critical both because 1999 saw many other changes—among them, Hugo Chávez took office as president in January—and because the violent death rate began increasing in the late 1980s (Kronick, 2017).

Figure 2b compares the monthly violent death count observed in the vital statistics data (that is, the data used for Figure 2a) with homicide cases observed in the police data (not available by day or by week). The police records confirm the trend observed in vital statistics: a sharp increase following the new criminal procedure code. This

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<sup>6</sup>Daily violent death rates were scaled by  $\times 365$ , so that the y-axis is on the more familiar scale of annual violent death rates.

Figure 2: Violent death rates before and after the penal code reform



does not appear to be a seasonal pattern (i.e., a July effect), nor does death from other causes jump around July 1, 1999 (Appendix Figures A.2a and A.2b).

To estimate  $\theta = E(Y|\text{July 1, 1999}) - E(Y|\text{June 30, 1999})$ , where  $Y$  denotes the violent death rate, I first estimate

$$Y_t = \alpha_1 + \alpha_2 \text{Post}_t + \beta_1 t + \beta_2 (\text{Post}_t \times t) + \epsilon_t \quad (1)$$

where  $t$  is a day counter taking a value of one on July 1, 1999 (and 2 on July 2, and 0 on June 30, etc.) and  $\text{Post}_t$  is an indicator for all dates on or after July 1. I then estimate  $\hat{\theta} = \hat{\alpha}_2 + (\hat{\beta}_1 + \hat{\beta}_2) \times 1$ .

This approach yields  $\hat{\theta} = 8.31$  (s.e. 3.52), leading me to reject the null that  $H_0 : \theta = 0$ . For estimates using local linear regression, see Appendix Table A.1 and accompanying discussion. The violent death rate prior to the new code was approximately 30 per 100,000—meaning that the jump of 8.31 per 100,000 was about 27% of the pre-reform level.

Dividing this estimate of the jump in lethal violence (8.31 per 100,000) by the fall in the arrest rate (459 per 100,000), I find that the additional violent deaths were approximately 1.8% of the “missing” arrests. Whether this ratio strengthens or diminishes the plausibility of the police violence hypothesis is a subjective question; in my view, 1.8% is more plausible than, say, 25% or 50%, which would suggest that something else was going on. This ratio would also serve as a key input to any welfare calculation—especially if incarceration implies a “civic penalty” (Lerman and

Weaver, 2014; Weaver and Lerman, 2010, c.f. Gerber et al., 2017).

The analysis thus far says only that lethal violence increased just after July 1, 1999; I do not observe how many of these additional victims were killed by the police. The only direct measure is incomplete: since 1989, the Venezuelan human rights organization PROVEA has compiled press accounts of “violations of the right to life” (that is, extrajudicial killings by state security forces). As PROVEA acknowledges, the data are far from comprehensive; however, under the (strong) assumptions of stationarity in the proportion of violations appearing in the press, and in the proportion of press reports picked up by PROVEA, these data would provide a meaningful measure of the trend in extra-judicial killings by police.

Appendix Figure [A.3a](#) shows that PROVEA’s count of victims of extra-judicial killings was largely flat in the two years prior to July 1999 and then began rising after that date, though there is no jump immediately after the new code came into effect. The same is true of the police count of cases of “resistance to authority,” a crime imputed to many victims of extra-judicial police killings (González Mejías and Kronick, 2017), as shown in Appendix Figure [A.3b](#). I view these trends as consistent with, though not confirmation of, the notion that the new criminal procedure code increased police use of lethal force. In their own annual report for 2001, PROVEA mentioned police reaction to implementation of the new code, which police officials had publicly blamed for “putting thousands of ‘antisocials’ on the streets” (p. 37).

Since new code came into effect across the whole country simultaneously, and since variation in implementation was idiosyncratic, I can not leverage geographic variation in the law or in its implementation (Rosell, 2017). However, I do consider whether geographic variation the pre-reform record of extra-judicial killings correlates with the size of the post-reform jump in lethal violence. Appendix Figure [A.4](#) indicates that, indeed, the post-reform jump in lethal violence was larger in states with higher (pre-reform) rates of extra-judicial killings (as measured in the NGO data). However, the imprecision of state-level estimates of the jump in the violent death rate, together with the limited number of observations ( $N = 24$ ), makes it difficult to evaluate how this correlation changes with the inclusion of other covariates.

If indeed extra-judicial killings by police account for the jump in lethal violence, we might expect victim characteristics to change: in particular, we might expect a higher proportion of adolescent victims and/or of male victims. Prior to the reform, approximately 40% of lethal violence victims were between the ages of 15 and 25; if *all* of the additional victims post-reform had been between the ages of 15 and 25, that proportion would have increased to 53%. In fact, as Figure [A.5b](#) suggests, the

proportion of victims aged 15–25 increased to 44%—about one-third of the maximum possible increase.<sup>7</sup>

Taken together, I interpret these findings as evidence in favor of the hypothesis that police reacted to the new code of criminal procedure by using lethal violence. In the state of Portuguesa, one police-linked death squad proclaimed as much, publishing a pamphlet that “blamed [the new code] for the rise in crime that led the group to act and claim[ing] that they would not stop until [the new code] was repealed” (U.S. Department of State, 2002). The available quantitative data, in my view, are consistent with the notion that their behavior was not unique.

### 3.3 Competing explanations

One obvious alternative explanation for the jump in lethal violence is that the sharp drop in arrest rates—and consequent drop in the cost of committing crimes—led non-police civilians to commit more murders (Becker, 1968). If this were the case, however, I would expect the same incentives to drive people to commit other crimes; as I show in Appendix Figures A.1a and A.1b, there is no jump in reported robbery cases after the new criminal procedure code came into effect.<sup>8</sup> Similarly, if the release of prisoners—which occurred gradually, not all at once on July 1—drove the jump in lethal violence, we might expect an increase in other crimes.

A second alternative explanation, more difficult to rule out, is that the new code increased the perception of impunity, which in turn increased (non-police) lynchings of suspected criminals. Indeed, the press reported several such lynchings; in one case, the police told a robbery victim “that her report could not be processed because the new criminal procedure code had come into effect and there was no public prosecutor in the office to open the investigation” (El Universal, 1999).<sup>9</sup> In my view, however, the immediacy of the jump in lethal violence is more consistent with an organized response than with atomistic civilian reactions.

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<sup>7</sup>If *all* of the additional victims had been male, the proportion of male victims would have increased from 91% (pre-reform) to 93%; the data are not precise enough to detect such small changes (Figure A.5a).

<sup>8</sup>This raises the question of why the drop in arrest rates would *not* increase robberies; to my mind, the most plausible explanation is that, as many critics charged, arrests before the new code were extremely arbitrary.

<sup>9</sup>One state governor was criticized for appearing to encourage lynchings; he said that he “would not use the police to protect thieves, rapists, or murders” (El Mundo, 1999).

## 4 Conclusion

In this note, I find evidence that police in Venezuela reacted to a new code of criminal procedure by using violence in place of unrestricted powers of arrest. This echoes other cases; Chevigny (1995), for example, says that impatience with criminal justice systems across Latin America created “an explosive brew of state power and vigilantism” in which police “bypass the rest of the system and punish by violence” (p. 143).<sup>10</sup> He quotes a jurist in Buenos Aires: “Faced with a criminal, the police think, ‘I’ll punish you just in case the judges don’t’” (p. 181).

Reading press accounts and policy reports about experiences of criminal procedure reform elsewhere in Latin America, some cases seem consistent with the idea that the old codes of criminal procedure were products of malgovernance (as in Venezuela), while others seem more consistent with misgovernance.<sup>11</sup> One possible explanation for this heterogeneity is that reform sparked backlash only where it threatened the economic interests of police officers; evaluating this and other hypotheses strikes me as a useful objective for future work.

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<sup>10</sup>Skolnick and Fyfe (1993) tell a similar story for the United States, where, in their view, police use excessive force in order to control those “underpunished by established law” and to compensate for a judicial process deemed “too ponderous, too indolent, too unaware, or too constrained to deal with ‘the problem’” (p. 24).

<sup>11</sup>Brinks (2008); Zazueta (2017); Angel (2017); Rodríguez Ferreira and Shirk (2013); Blanco (2012).

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# A Online Appendix

## A.1 Estimates using local linear regression

The main text estimates the magnitude of the jump in violent deaths at July 1, 1999 simply by fitting two lines: one for January–June 1999, a second for July–December 1999.

While the motivating graph (Figure 2a) perhaps looks like a typical regression discontinuity figure, the data used here differ from those commonly used in regression discontinuity analysis in two ways. First, the running variable in this case (days) is discrete, precluding the possibility of observations “arbitrarily close” to the treatment date. Second, the data are a single time series—that is, there is only one observation at each value of the running variable.

Table A.1: Estimating the jump using local linear regression

Estimates of  $\theta = E(Y|\text{July 1, 1999}) - E(Y|\text{June 30, 1999})$  using local linear regression, for five groups of causes of death; first row uses optimal bandwidth proposed in Imbens and Kalyanaraman (2012), second row uses half that bandwidth, third row twice the bandwidth.

	(1)	(2)	(3)	(4)	(5)
	All violent death	Violent death <i>unknown intent</i>	Intentional homicide	Circulatory disease	Infectious disease
Pre-reform rate	29.27	14.40	13.75	142.94	30.35
$h = 9.8$	19.19 (10.74), [0.07]	13.96 (6.12), [0.02]	2.80 (4.81), [0.56]	6.67 (7.34), [0.36]	-5.64 (4.36), [0.20]
$h = 4.9$	21.04 (10.14), [0.04]	14.51 (4.83), [0.00]	3.22 (3.62), [0.37]	9.99 (7.84), [0.20]	-4.66 (7.14), [0.51]
$h = 19.6$	9.93 (8.17), [0.22]	9.57 (5.26), [0.07]	1.55 (3.87), [0.69]	-3.64 (6.94), [0.60]	-0.43 (5.28), [0.94]

Standard errors in parentheses; p-values (for test of  $\theta = 0$ ) in brackets. <sup>†</sup>Dependent variable is the death rate (deaths per 100,000 population), except in specification (5), in which it is the number of deaths of children under one year per 1,000 population.

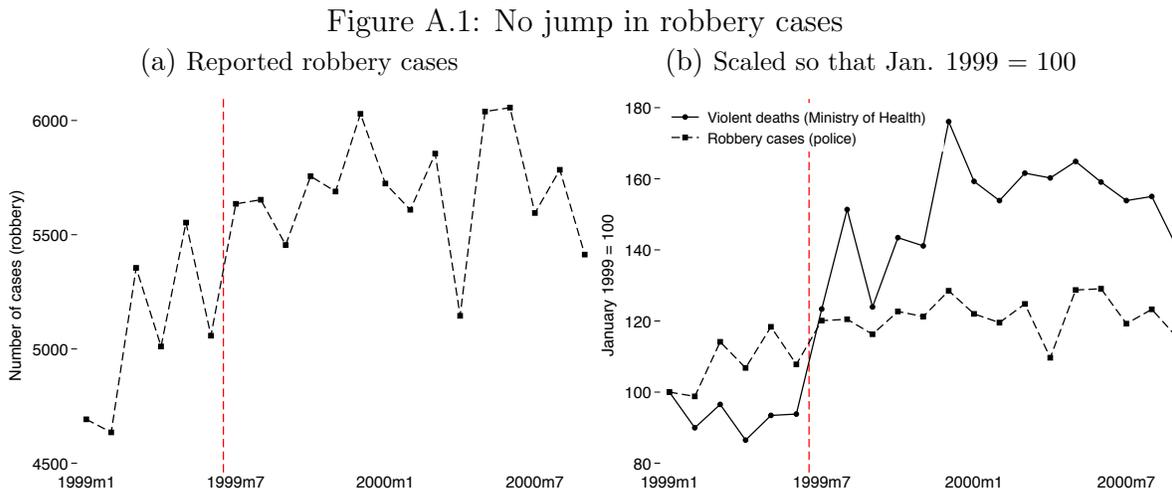
This means that, as I show in Table A.1, local linear fits are highly sensitive to bandwidth choice. This table presents estimates of the jump using local linear regression with the optimal bandwidth proposed in Imbens and Kalyanaraman (2012). The first row uses the optimal bandwidth; the second and third rows use half and twice the optimal bandwidth, respectively, as recommended by the authors. Clearly, the estimate of the jump is highly sensitive to bandwidth choice, though the point

estimate is always positive, which is not the case for two “placebo” causes of death (circulatory disease and infectious disease). As we would expect, the largest bandwidth produces an estimate that is closest in magnitude to the estimate presented in the main text.

Finally, I note that, at least in these estimates, the jump in violent deaths (Column 1, and main text) is driven not by a jump in *intentional homicides* (Column 3) but rather by a jump in violent deaths *of unknown intent* (Column 2); victims of police violence might be more likely to be classified as the latter. See González Mejías and Kronick (2017) for details on the vital statistics data.

## A.2 Other crimes

As noted in Section 3.3, one way to assess the most obvious competing hypothesis—that the sudden drop in arrest rates drove non-police civilians to commit more crimes—is to consider trends in crimes other than homicide. The only other crime for which I have been able to obtain monthly data from 1999 is robbery; the monthly robbery trends are presented in Figure A.1.



A more nuanced version of this competing hypothesis is that organized crime groups anticipated and responded to the legal change, while others did not. In this case, we might expect an increase only in certain crimes—such as drug trafficking, kidnapping, or extortion. While I have not been able to locate monthly data for any of these

crimes, the longer annual series show no increase in 1999 or 2000, though both kidnapping and extortion rates begin to climb in 2002. Indeed, of all the crimes for which I have annual data, the only one to register something of a jump in 1999 is vehicle robbery (driven in large part by an uptick in motorcycle robbery). There is no similar jump in reported vehicle thefts.

### A.3 Additional figures

These figures are referenced in the main text.

Figure A.2: Other explanations: (a) Seasonality or (b) general increase in mortality?

The lines show predictions, from local linear regression, of (log) daily death rates; the points mark the weekly average of these daily death rates. The local linear regressions use an Epanechnikov kernel with the rule-of-thumb bandwidth proposed in Fan and Gijbels (1996), p. 110–113.

(a) Lethal violence before and after 7/1/2000

(b) Infectious disease deaths in 1999

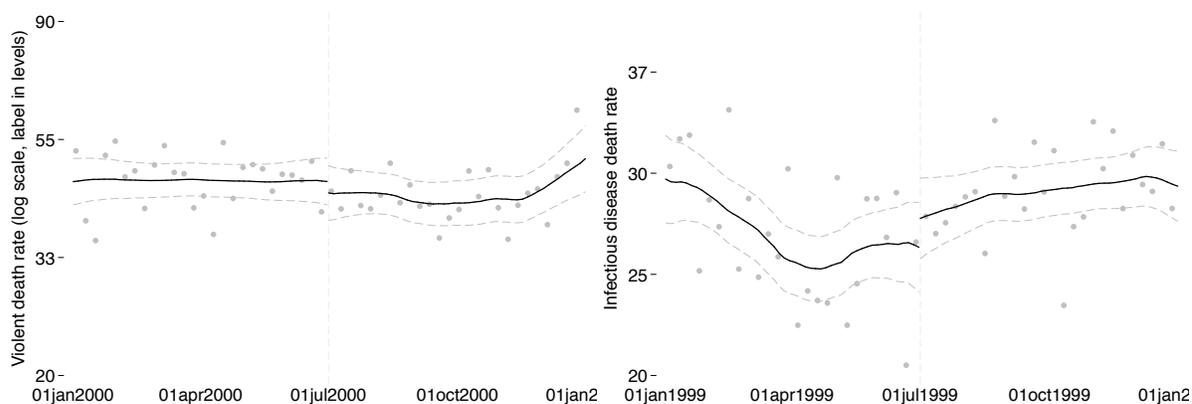


Figure A.3: Direct, but incomplete, measures of police use of force

In Figure (a), the lines show predictions, from local linear regression, of the number of extra-judicial killings by police per day (as registered by the NGO PROVEA; see main text for discussion); the points mark monthly averages of those counts. The local linear regressions use an Epanechnikov kernel with the rule-of-thumb bandwidth proposed in Fan and Gijbels (1996), p. 110–113. Figure (b) plots the number of cases of “resistance to authority” recorded by the police, by month.

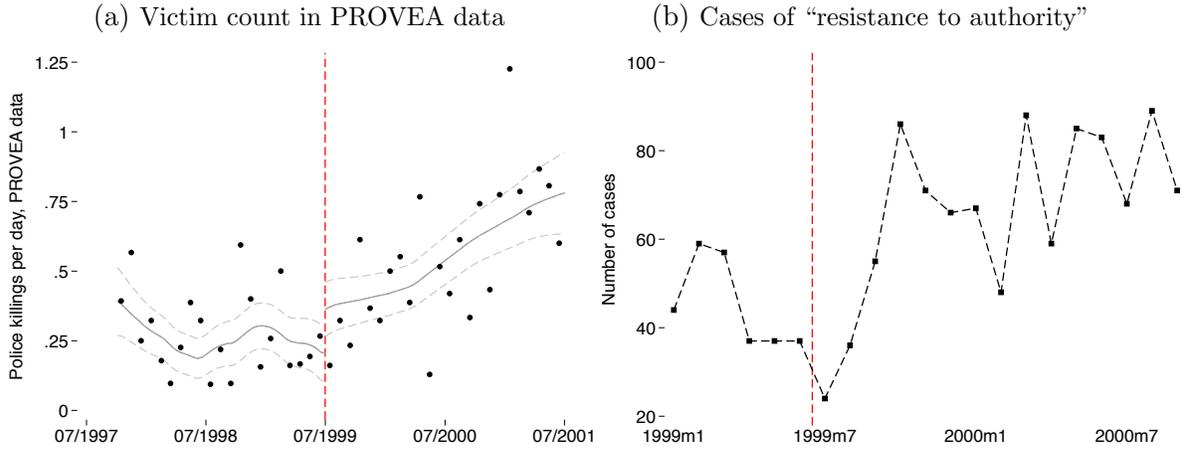
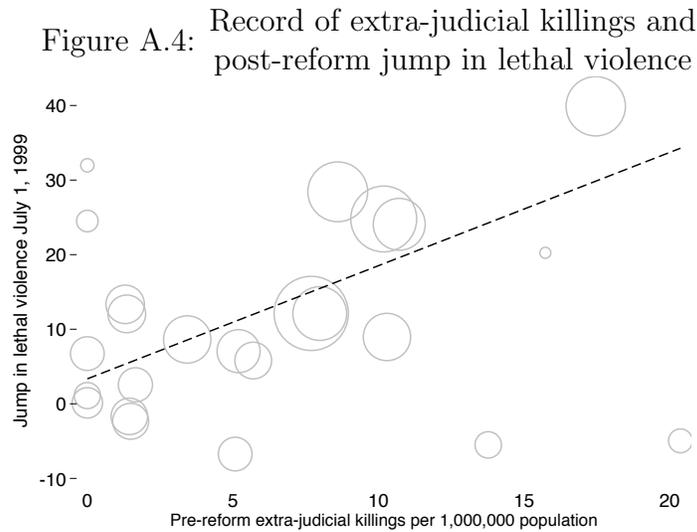


Figure A.4 plots the relationship between (a) the pre-reform record of extra-judicial killings and (b) an estimate of  $\theta_s = E(Y_s | \text{July 1, 1999}) - E(Y_s | \text{June 30, 1999})$ , across Venezuela’s 24 states.<sup>12</sup> The points and the linear fit are weighted by state population. The estimates  $\hat{\theta}$  were obtained by estimating Equation 1 separately for each state, using data from the years 1999 and 2000. As noted in the text, these estimates are imprecise.



<sup>12</sup>Technically, 23 states and the federal district.

Figure A.5: Changes in sex and age composition of lethal violence victims

The lines show predictions, from local linear regression, of the proportion of violent death victims who were male (a) or between the ages of 15 and 25 (b); the points mark weekly averages. The local linear regressions use an Epanechnikov kernel with the rule-of-thumb bandwidth proposed in Fan and Gijbels (1996), p. 110–113.

