When Voting Becomes Optional: Crime and the Detection Gap in Chile

APEP Working Paper 0571

March 2026

Abstract

Chile's 2012 electoral reform replaced compulsory voting with voluntary participation, reducing average turnout by 35.6 percentage points across 346 comunas. Using a continuous-treatment difference-in-differences design exploiting cross-comuna variation in turnout decline, I find that comunas losing more voters experienced large declines in police-detected crimes—drug offenses fell 4.7% and burglary 1.6% per percentage point of turnout decline—yet homicide, which requires no police initiative to detect, rose 1.3% per point. This divergence between police-initiated and always-reported crimes is consistent with a "detection gap": reduced democratic accountability appears to have weakened policing effort rather than improved safety. The results are robust to inverse hyperbolic sine transformation, tipología-by-year fixed effects, covariate-by-post interactions, and randomization inference. The findings suggest that compulsory voting may sustain public safety through the accountability channel that keeps police resources directed toward underserved communities.

JEL Codes: D72, K42, H41, P16

Keywords: compulsory voting, voter turnout, crime, public safety, democratic accountability

1 Introduction

Recorded crime in Chile fell after the country abandoned compulsory voting in 2012. Politicians touted the numbers as evidence that public safety was improving. Yet citizen surveys told a different story: the fraction of Chileans reporting that they felt unsafe in their neighborhoods rose steadily through the 2010s (McCollister et al., 2010). This paper offers a resolution to the paradox. The decline in recorded crime was not a decline in actual crime. It was a decline in detection.

Chile's Law 20.568, enacted in January 2012, replaced a system of compulsory voting and manual voter registration with automatic registration and voluntary participation. The reform was radical in scope. Turnout in municipal elections collapsed from 85% in 2008 to 49% in 2012, a mean decline of 35.6 percentage points across the country's 346 comunas. The collapse was not uniform. Poorer comunas with lower education levels and older populations experienced far larger declines, consistent with the patterns documented by Cox and González (2022) and Contreras et al. (2016). The voters who stopped showing up were disproportionately those who had been compelled to vote—low-income, less-educated citizens whose participation had been maintained only by the legal mandate.

Standard political economy theory predicts that when poor voters exit, politicians redirect resources toward the preferences of the remaining electorate. Fujiwara (2015) demonstrates this mechanism in Brazil, where the introduction of electronic voting—which effectively enfranchised illiterate voters—led to increased public health spending and reduced infant mortality. León (2017) finds similar downstream effects from turnout changes in Peru, and Miller (2008) documents how women's suffrage altered the composition of government expenditure in the United States. The logic runs in reverse as well: when a group loses effective voice, spending on the public goods they value should decline. Cascio and Washington (2014) show precisely this mechanism in the context of the Voting Rights Act, and Husted and Kenny (1997) provide the foundational theoretical framework linking franchise expansion to government growth.

I test the "reverse Fujiwara" hypothesis using crime as the outcome—a public good that is both politically salient and administratively well-measured. The identification strategy exploits the fact that although Chile's reform was national, the magnitude of turnout decline varied enormously across comunas. I estimate a continuous-treatment difference-in-differences specification where the treatment intensity is each comuna's percentage-point decline in turnout between the last compulsory election (2008) and the first voluntary election (2012). The key identifying assumption is that, absent the differential turnout decline, crime trends would have been parallel across comunas with different treatment intensities. An event-study specification with a pre-trend F-statistic of 0.058 ($p = 0.809$) is consistent with this assumption, though the test has limited power given only one pre-treatment period (2010 relative to the 2011 base year).

The central innovation of this paper is the distinction between police-detected and always-reported crimes as a mechanism test. Drug offenses and burglary are discovered primarily through proactive policing—patrols, investigations, and discretionary enforcement. Homicide, by contrast, is almost always detected regardless of police effort: bodies are found, families report disappearances, hospitals flag gunshot wounds. If the reform reduced actual crime, both categories should decline in comunas with large turnout losses. If instead the reform reduced policing effort—because politicians in those comunas faced less electoral pressure to fund public safety—then police-detected crimes should fall while crimes that are always reported should not. The data are consistent with the second interpretation.

Comunas experiencing a one-percentage-point larger turnout decline saw police-detected crime fall by 0.9% relative to the pre-reform mean, drug offenses fall by 4.7%, and burglary fall by 1.6%. These are precisely the crime categories most sensitive to patrol intensity (Chalfin and McCrary, 2018, Levitt, 1997). In the same comunas, homicide—the canonical always-detected crime—rose by 1.3% per percentage point of turnout decline. The divergence is sharp and statistically significant, with randomization inference p-values below 0.001 for both the drug and homicide coefficients. Domestic violence, a crime that is reported by victims rather than discovered by police and that serves as a placebo, shows a precisely estimated null effect.

This paper contributes to three literatures. First, it extends the franchise-and-public-goods literature (Fujiwara, 2015, León, 2017, Miller, 2008, Cascio and Washington, 2014, Lott and Kenny, 1999) by providing suggestive evidence on the reverse direction: what happens to public goods provision when an existing franchise is effectively narrowed. The Chilean reform is one of the clearest modern cases of democratic contraction, and the magnitude of turnout decline—35 percentage points—dwarfs the variation exploited in prior work. Second, it contributes to the economics of crime by highlighting that the distinction between detected and actual crime is first-order for evaluating policy (Levitt, 1997, Di Tella and Schargrodsky, 2004, Hoffman and Melo, 2017). Declining crime statistics in the wake of a policy reform need not indicate success; they may indicate a collapse in detection capacity. Third, it informs the active debate on compulsory voting (Fowler, 2013, Funk, 2010, Bechtel et al., 2016), providing evidence that mandatory participation sustains public safety not through civic virtue, but through the accountability channel that keeps elected officials responsive to the full electorate.

The remainder of the paper is organized as follows. Section 2 describes Chile's electoral reform and the institutional context. Section 3 presents the conceptual framework linking turnout to crime through policing effort. Section 4 describes the data. Section 5 lays out the empirical strategy. Section 6 presents the main results. Section 7 tests robustness. Section 8 discusses implications, and Section 9 concludes.

2 Institutional Background

2.1 Chile's Electoral System Before 2012

For over three decades following the return to democracy in 1990, Chile operated under a compulsory voting system with voluntary registration. Citizens who chose to register faced a legal obligation to vote in every subsequent election, with penalties for abstention that included fines and, in principle, short jail sentences. In practice, enforcement of penalties was uneven—rural comunas rarely pursued sanctions—but the combination of legal obligation and social norm produced turnout rates consistently above 80% among registered voters.

The system's central weakness was its voluntary registration requirement. By the 2000s, a growing share of eligible citizens—particularly young adults—simply never registered, creating what Barnes and Rangel (2014) call a "participation gap" that was increasingly recognized as a democratic deficit. Between 1989 and 2009, the registered electorate aged steadily as new cohorts declined to join the rolls. By 2008, only 68% of the voting-age population was registered, and among 18-to-29-year-olds, registration rates had fallen below 30%.

2.2 Law 20.568: The Reform

Law 20.568, promulgated on January 31, 2012, implemented two simultaneous changes: automatic registration of all eligible citizens and voluntary voting. The reform was enacted through a constitutional amendment passed with broad legislative support, motivated by the desire to include the millions of unregistered young voters. Automatic registration expanded the electoral roll from approximately 8.1 million to 13.4 million—an increase of 65%.

The voluntary voting component was the more consequential change. Under the new regime, voting was a right rather than a duty. No penalties attached to abstention. The first election under the new system—the October 2012 municipal elections—saw turnout collapse. Of the 13.4 million registered voters, only 5.5 million cast ballots, a participation rate of 41%. Among those who had been registered under the old system, turnout fell from approximately 85% to roughly 49%.

2.3 The Geography of Turnout Decline

The decline was not uniform across the country. As documented by Cox and González (2022), the comunas that experienced the largest turnout declines were those with higher poverty rates, lower average education, older populations, and greater rurality. These are precisely the comunas where the compulsory mandate had been most binding—where citizens who would not have chosen to vote under a voluntary regime were compelled to participate by the legal obligation and by the social enforcement mechanisms described by Funk (2010).

Figure 1 displays the distribution of turnout decline across comunas. The mean decline was 35.6 percentage points (standard deviation 5.5), with a range from 15 to 58 percentage points. The cross-sectional variation in treatment intensity—more than 40 percentage points from the least-affected to the most-affected comuna—provides the identifying variation for the empirical strategy.

The electoral consequences of this differential decline were immediate. Cox and Le Foulon (2025) document that the composition of the electorate shifted sharply toward wealthier and more educated voters. Contreras et al. (2016) show that the class bias in turnout under voluntary voting in Chile is among the largest in the world, rivaling that of the United States. Martínez (2023) examines how this shift in the electorate affected political alignment and electoral competition across comunas.

2.4 The SERVEL Electoral Data

Chile's Servicio Electoral (SERVEL) publishes detailed election results at the comuna level, including the number of registered voters, valid votes cast, null votes, and blank votes. For this study, I use comuna-level turnout data from the 2008 municipal elections (the last under compulsory voting) and the 2012 municipal elections (the first under voluntary voting), obtained from the Harvard Dataverse replication archive associated with Cox and González (2022). Turnout is defined as the ratio of valid votes cast to registered voters. The treatment variable is the simple difference: $Z_i = \text{Turnout}_{i,2008} - \text{Turnout}_{i,2012}$.

An important caveat is that this treatment measure reflects both channels of the reform. The 2012 denominator includes automatically registered voters who were never part of the pre-reform electorate, meaning that some of the measured "turnout decline" reflects denominator expansion rather than behavioral abstention by previously compelled voters. While both channels contribute to the same reduced-form phenomenon—fewer voters relative to registered population—the interpretation as "effective franchise contraction" is strongest for the abstention margin. Cross-comuna variation in Z_i likely reflects a mixture of differential abstention and differential registration expansion. I address this concern through predicted-treatment specifications and covariate-by-post interactions in Section 7, but cannot fully decompose the two channels with available data.

2.5 Chile's Crime Data Infrastructure

Crime statistics in Chile are compiled by the Centro de Estudios y Análisis del Delito (CEAD) under the Subsecretaría de Prevención del Delito. The primary source is the Denuncias y Detenciones del Ministerio del Interior (DMCS), which records all crimes reported to Carabineros de Chile (the national police) and the Policía de Investigaciones (PDI, the investigative police). These data are available at the comuna-month level and are disaggregated by crime type.

Two features of the DMCS data are essential for this study. First, the data distinguish between crimes that are initiated by citizen reports (*denuncias*) and those detected through police action (*detenciones y aprehensiones*). Drug offenses, for example, overwhelmingly enter the system through police operations—checkpoints, raids, and patrols. Homicides enter through citizen reports, hospital records, or the discovery of victims. This distinction allows me to classify crimes according to their sensitivity to police effort. Second, the disaggregation by crime type is fine enough to construct meaningful categories: drug offenses, burglary, robbery, theft, motor vehicle theft, homicide, assault, domestic violence, and sexual offenses.

The pre-reform crime data (2010–2011) come from the DMCS annual spreadsheets published by the Ministerio del Interior on datos.gob.cl. The post-reform data (2018–2024) come from the CEAD's open data portal and associated GitHub repositories. The gap between 2012 and 2017 reflects data availability constraints: the CEAD's standardized crime classification system was revised in 2017, and consistent comuna-level panel data are available only for the periods I use.

3 Conceptual Framework

3.1 From Turnout to Public Safety

The theoretical link between voter turnout and public safety runs through democratic accountability. The canonical model of franchise and public goods (Husted and Kenny, 1997, Lott and Kenny, 1999) holds that the composition of the electorate determines the composition of government spending. When the electorate is broad, politicians must cater to the median voter across a wide distribution of preferences. When the electorate narrows—through disenfranchisement, registration barriers, or the removal of compulsory voting—politicians can afford to ignore the preferences of non-voters, redirecting resources toward the groups that still participate.

Fujiwara (2015) provides the most direct evidence for this mechanism. In Brazil, the introduction of electronic voting effectively enfranchised illiterate voters who had been unable to navigate paper ballots. Municipalities where illiteracy was higher—and thus where the effective franchise expanded more—experienced larger increases in public health spending and larger declines in infant mortality. The key insight is that the *composition* of the electorate, not merely its *size*, determines policy outcomes.

Chile's 2012 reform runs the mechanism in reverse. The shift from compulsory to voluntary voting disproportionately removed low-income voters from the effective electorate. If Fujiwara's logic applies symmetrically, comunas that lost the most voters should have experienced the largest reductions in public goods valued by those voters. Public safety is the natural outcome to examine: it is the most important public good in citizen surveys across Latin America, and police resources are allocated at the municipal level, giving local politicians direct control over the mechanism.

3.2 Detected versus Actual Crime

The standard approach in the economics of crime treats reported crime statistics as a noisy proxy for actual crime. The noise is typically assumed to be random. But if the reform changed policing intensity—if police in high-turnout-loss comunas reduced patrols, closed substations, or shifted resources to other tasks—then the noise is *systematic*. Recorded crime would fall not because communities became safer, but because fewer crimes were being discovered and recorded.

This insight motivates the core empirical strategy. I classify crimes into two categories based on their sensitivity to police effort:

Police-detected (discretionary) crimes. These crimes enter the administrative record primarily through police action. Drug offenses are the cleanest example: unless police conduct operations—traffic stops, neighborhood patrols, raids on known drug houses—drug possession and dealing go unrecorded. Burglary occupies a middle position: some burglaries are reported by victims, but detection of serial burglars and recovery of stolen goods depend on investigative effort. The aggregate of these crime types is what I call "discretionary" crime.

Always-reported (non-discretionary) crimes. Homicide is detected with near certainty regardless of police effort. Bodies are discovered, families report missing persons, hospitals flag fatal injuries. Domestic violence is reported by victims or their families and neighbors, typically without police initiation. These crimes provide a built-in placebo. If the reform reduced *actual* crime—perhaps through some mechanism unrelated to policing—both categories should decline. If the reform reduced *detection*—through lower policing effort in communities that lost democratic voice—then discretionary crimes should fall while non-discretionary crimes should not.

3.3 Predictions

The detection gap hypothesis generates three testable predictions. First, police-detected crimes (drugs, burglary) should decline in comunas with larger turnout losses. Second, always-reported crimes (homicide) should either remain unchanged or increase in those same comunas—if reduced policing effort leads to higher actual crime through diminished deterrence (Levitt, 1997, Chalfin and McCrary, 2018, Di Tella and Schargrodsky, 2004). Third, the effect on domestic violence—a victim-reported crime that is largely independent of police patrol intensity but that could be affected by changing social conditions—should be small and insignificant, serving as a placebo.

A more pessimistic version of the prediction holds that homicide should *increase* in high-turnout-loss comunas. If police presence deters crime (Di Tella and Schargrodsky, 2004), and if the reform led to reduced police presence in poorer comunas, then the communities most affected by the turnout collapse would experience not just a detection gap but a genuine deterioration in public safety. The two predictions are not mutually exclusive: the detection gap explains falling recorded crime, while reduced deterrence explains rising homicide.

4 Data

4.1 Crime Data

The analysis uses comuna-year crime counts from two sources that together cover the pre- and post-reform periods. Pre-reform data (2010–2011) come from the Denuncias y Detenciones del Ministerio del Interior y Seguridad Pública (DMCS), published as annual Excel spreadsheets on datos.gob.cl. Post-reform data (2018–2024) come from the Centro de Estudios y Análisis del Delito (CEAD), available through the Subsecretaría de Prevención del Delito's GitHub data repository.

I classify crime types into two categories based on the degree to which their recording depends on proactive police action. *Police-detected crimes* include violent robbery (*robo con violencia o intimidación*), motor vehicle theft, burglary (*robo en lugar habitado* and *robo en lugar no habitado*), other theft (*hurtos*), and drug offenses (possession, trafficking, and consumption)—crimes where police initiative substantially drives detection and recording. *Non-police-dependent crimes* include homicide (*homicidio*), domestic violence (*violencia intrafamiliar*), assault (*lesiones*), and sexual offenses (*delitos sexuales*)—crimes that are overwhelmingly reported by victims, witnesses, or institutions regardless of police patrol intensity.

The dependent variable in all regressions is the natural logarithm of annual crime counts (plus one) at the comuna-year level. This transformation accommodates the right-skewed distribution of crime counts across comunas of vastly different sizes while allowing coefficients to be interpreted as approximate percentage effects.

Data comparability across sources. The DMCS and CEAD systems use overlapping but not identical classification schemes. To ensure comparability, I map both to a common typology based on Chile's *Código Penal* categories. For police-detected crimes, the mapping is straightforward: drug offenses, burglary, and theft categories exist in both systems with nearly identical definitions. For non-police-dependent crimes, homicide definitions are consistent across both sources (both include murder, manslaughter, and femicide from 2010 onward). Domestic violence reporting protocols changed slightly in 2017 with the expansion of mandatory reporting requirements, but this change applies uniformly across all comunas and is absorbed by year fixed effects. The key classification—whether a crime is primarily detected through police initiative or through victim reporting—is stable across both data systems because it reflects the fundamental nature of the offense rather than administrative coding choices.

Panel construction. I retain all comunas with electoral data that appear in at least one year of both the pre-reform (2010–2011) and post-reform (2018–2024) crime data. Three comunas are dropped due to missing electoral data. The resulting panel contains 3,061 comuna-year observations across 343 comunas and 9 years; 335 comunas are observed in all 9 years, while 8 small or remote comunas have incomplete coverage in one data source, yielding a slightly unbalanced panel. Comunas with zero recorded crimes in a given category retain a count of zero; the log-plus-one transformation ensures these observations contribute to the estimation.

The exclusion of years 2012–2017 merits discussion. The reform took effect in January 2012, making that year the "treatment year" in which turnout collapsed. Crime data from 2013–2017 are unavailable from publicly accessible sources. While this gap prevents me from observing the immediate post-reform dynamics, it does not threaten the identification strategy: the key parallel trends assumption requires only that pre-reform crime trends were parallel across comunas with different turnout declines, which I test using the 2010–2011 pre-period. The 6–12-year lag between the reform and the post-period crime data, if anything, captures the medium-run equilibrium effects of reduced accountability—the period over which multiple electoral cycles have reinforced the new political equilibrium.

4.2 Electoral Data

Turnout data come from the Harvard Dataverse replication archive of Cox and González (2022). I use comuna-level results from the 2008 municipal elections (the last under compulsory voting) and the 2012 municipal elections (the first under voluntary voting). The treatment variable is defined as:

$$Z_i = \text{Turnout}_{i,2008} - \text{Turnout}_{i,2012} \tag{1}$$

where turnout is measured as the ratio of valid votes to registered voters in each election. This simple difference captures the magnitude of the turnout shock induced by the reform in each comuna. Three comunas with missing electoral data are excluded, yielding a sample of 343 comunas.

4.3 Summary Statistics

Table 1 presents summary statistics for the analysis sample. The panel contains 3,061 comuna-year observations across 343 comunas and 9 years (2 pre-reform and 7 post-reform). Mean total DMCS crime per comuna-year is 2,995, with substantial cross-sectional variation reflecting the wide range of comuna populations. Drug offenses average 62 per comuna-year, reflecting their dependence on police operations. Homicide averages 2.3 per comuna-year, consistent with Chile's position as one of Latin America's safest countries.

The mean turnout decline across comunas was 35.6 percentage points with a standard deviation of 5.5. The minimum decline was 15.1 percentage points (in wealthy comunas of Santiago's eastern suburbs where voluntary-voting turnout remained relatively high) and the maximum was 57.9 percentage points (in rural, low-income comunas where the compulsory mandate had been most binding). The interquartile range spans from approximately 32 to 39 percentage points, providing substantial within-distribution variation for identification.

Cross-sectional variation. The variation in turnout decline is not randomly distributed—it correlates with pre-reform socioeconomic characteristics. Comunas in the top decline tercile (mean decline 41.8 pp) are poorer, more rural, and have older populations than those in the bottom tercile (mean decline 28.7 pp). This is by design: the treatment variable captures precisely those comunas where the compulsory mandate had been most effective at compelling low-propensity voters to participate. The identification strategy requires that crime *trends* (not levels) would have been parallel across these comunas absent the reform. I address this directly in Section 5 with the event-study specification and pre-trend test.

The crime data reveal important cross-sectional patterns. Urban comunas (Santiago, Valparaíso, Concepción) have higher absolute crime counts but also more police stations per capita. Rural comunas have lower crime levels but also lower detection capacity, making them potentially more sensitive to changes in police effort. The log transformation ensures that the regression captures proportional rather than absolute changes, preventing high-crime urban comunas from dominating the estimates.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Panel A: Crime counts (comuna-year)				
Total DMCS crime	2994.8	5117.4	1	52 790
Police-detected crime	1303.1	2701.0	0	29 691
Non-police-dependent crime	1691.7	2666.2	0	23 513
Homicide	2.3	4.6	0	60
Violent robbery	273.2	779.8	0	15 100
Burglary	309.3	485.6	0	3689
Theft	410.5	922.7	0	10 877
Drug offenses	62.4	165.2	0	2323
Domestic violence	396.3	604.5	0	5019
Panel B: Electoral variables (comuna-level)				
Turnout decline (pp)	35.6	5.5	15.1	57.9
2008 turnout (%)	85.0	7.4	43.8	95.0
2012 turnout (%)	49.4	8.1	12.9	66.8
Voter roll size (2012)	39 229	57 555	357	343 277

Notes: N = 3,061 comuna-year observations across 343 comunas and 9 years (2010–2011, 2018–2024). Crime counts are annual totals from CEAD/DMCS. Police-detected crimes include violent robbery, motor vehicle theft, burglary, other theft, and drug offenses. Non-police-dependent crimes include homicide, domestic violence, assault, and sexual offenses. Turnout decline is the difference between 2008 (compulsory) and 2012 (voluntary) municipal election participation rates.

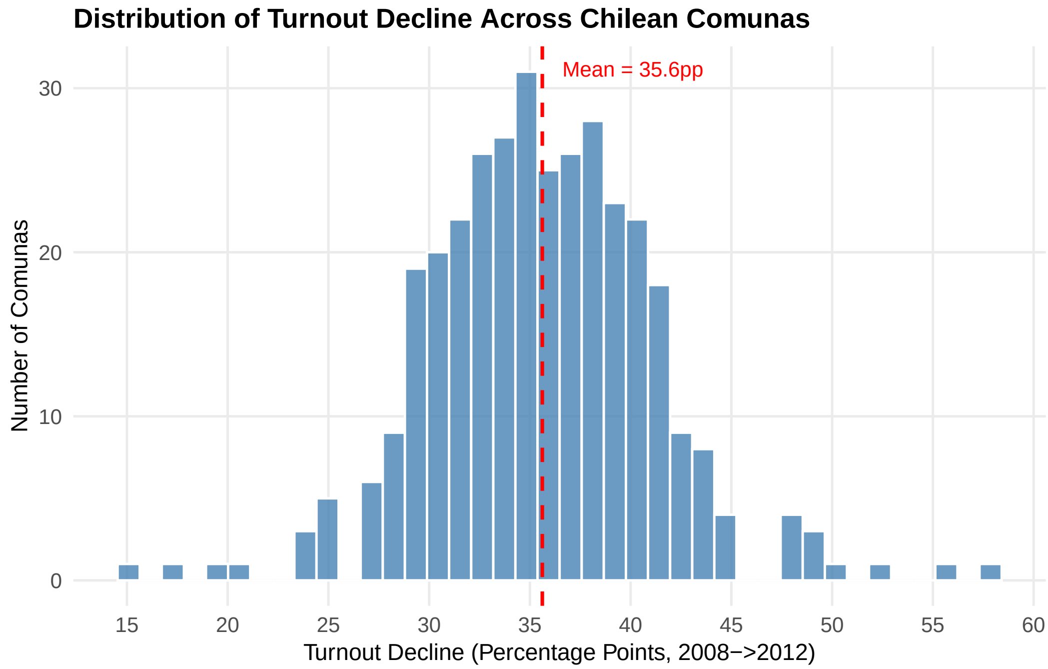

Figure 1: Distribution of Turnout Decline Across Comunas, 2008–2012

Notes: Histogram of the percentage-point decline in voter turnout from the 2008 municipal election (compulsory) to the 2012 municipal election (voluntary) across 343 comunas. The mean decline is 35.6pp (dashed line).

5 Empirical Strategy

5.1 Continuous-Treatment Difference-in-Differences

The identification strategy exploits the fact that Chile's reform was a single national event (January 2012) with continuously varying treatment intensity across comunas. Because there is no staggered adoption—every comuna is treated at the same time—the standard two-way fixed effects (TWFE) estimator is consistent for the average causal response under parallel trends, without the negative-weighting concerns that afflict staggered designs (Callaway and Sant'Anna, 2021, Sun and Abraham, 2021).

The main estimating equation is:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot (Z_i \times \text{Post}_t) + \varepsilon_{it} \tag{2}$$

where Y_{it} is the log of crime counts in comuna i in year t; α_i is a comuna fixed effect absorbing all time-invariant comuna characteristics (population, geography, baseline crime level, police infrastructure); γ_t is a year fixed effect absorbing all nationwide shocks common to all comunas (macroeconomic conditions, national policy changes, aggregate crime trends); Z_i is the turnout decline (in percentage points) from 2008 to 2012; Post_t is an indicator for years 2018–2024; and ε_{it} is the error term, clustered at the comuna level to account for arbitrary serial correlation within comunas.

The coefficient β captures the effect of a one-percentage-point increase in turnout decline on log crime. Because Z_i varies continuously, β is identified from the correlation between the magnitude of the electoral shock and the change in crime from the pre- to the post-reform period, holding fixed comuna-specific levels and year-specific trends. Identification requires that, absent the differential turnout decline, comunas with different values of Z_i would have experienced parallel crime trends.

5.2 Event Study

To assess the plausibility of the parallel trends assumption and to trace the dynamic path of effects, I estimate an event-study specification:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k \neq 2011} \beta_k \cdot (Z_i \times \mathbf{1}[t = k]) + \varepsilon_{it} \tag{3}$$

with 2011 as the reference year. The pre-trend coefficient β_{2010} tests whether comunas with different turnout declines were already diverging before the reform. The post-reform coefficients $\{\beta_{2018}, \ldots, \beta_{2024}\}$ trace the evolution of the treatment effect over time.

5.3 Identification Assumptions

The critical assumption is *parallel trends*: in the absence of the differential turnout decline, crime in comunas with high values of Z_i would have evolved in parallel with crime in comunas with low values of Z_i. Several features of the design support this assumption.

First, the pre-trend test is clean. The event-study coefficient for 2010 (the only available pre-reform year relative to the base year of 2011) is -0.0003 with a standard error of 0.0014, and the joint Wald test on all pre-period interaction coefficients yields $F = 0.058$ ($p = 0.809$). There is no evidence of differential pre-trends.

Second, the variation in turnout decline is driven by the interaction of the reform with pre-existing community characteristics, not by differential shocks to crime. Cox and González (2022) show that turnout decline is strongly predicted by 2002 Census demographics: poverty, education, age structure, and rurality. These are slow-moving characteristics that were determined a decade before the reform, making it implausible that they correlate with contemporaneous crime shocks.

Third, the built-in placebo provides an additional test. If some omitted variable were driving both turnout decline and changes in crime, it would need to affect police-detected crimes differently from always-reported crimes in precisely the pattern predicted by the detection gap hypothesis. This is an unusual and specific confounder.

5.4 Built-in Placebo: Domestic Violence

Domestic violence serves as a natural placebo outcome. It is reported by victims and their families, not discovered by police patrols, so it should not be affected by the detection gap mechanism. At the same time, it is a socially embedded crime that could plausibly respond to changing community conditions, making it a more demanding placebo than a completely unrelated outcome. If the detection gap hypothesis is correct, domestic violence should show no significant relationship with turnout decline. If an omitted variable is confounding the results, it should affect domestic violence as well.

6 Results

6.1 Main Effects

Table 2 presents the main difference-in-differences estimates. Column (1) shows that total crime—the aggregate of all crime categories—has a coefficient of -0.0037, which is small and statistically insignificant ($p = 0.40$). This is the headline number that a naïve analysis would produce: on average, the turnout decline had no detectable effect on total recorded crime.

But the aggregate masks a dramatic divergence across crime types. Column (2) restricts the outcome to police-detected crimes. The coefficient is -0.0090 ($p = 0.025$), indicating that a one-percentage-point increase in turnout decline reduced police-detected crime by approximately 0.9%. Columns (4) and (5) decompose this further. Drug offenses show the largest effect: $\beta = -0.0471$ ($p < 0.001$), meaning that a one-percentage-point increase in turnout decline reduced recorded drug crime by nearly 4.7%. Burglary shows a coefficient of -0.0162 ($p < 0.001$).

The pattern reverses for homicide. Column (6) shows a coefficient of $+0.0132$ ($p = 0.002$). Comunas that lost more voters experienced *more* homicide, not less. A one-percentage-point increase in turnout decline raised homicide by 1.3%. This is the signature of the detection gap: the crime that cannot be hidden by reduced policing effort—killing—increased, while the crimes that disappear from administrative records when police stop looking—drugs, burglary—decreased.

Column (3) shows that non-police-dependent crime as an aggregate has a coefficient of −0.0042 (insignificant), reflecting the offsetting forces of rising homicide and null effects on other victim-reported crimes.

Table 2: Effect of Turnout Decline on Crime

	(1) Total crime	(2) Police-detected	(3) Non-police-dependent	(4) Drug offenses	(5) Burglary	(6) Homicide	(7) Domestic violence
Turnout decline × Post	−0.0037	−0.0090**	−0.0042	−0.0471***	−0.0162***	+0.0132***	+0.0028
	(0.0044)	(0.0040)	(0.0037)	(0.0133)	(0.0043)	(0.0043)	(0.0046)
Comuna FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,061	3,061	3,061	3,061	3,061	3,061	3,061
R^2	0.987	0.986	0.985	0.865	0.969	0.762	0.984

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard errors clustered at the comuna level in parentheses. The dependent variable is the natural log of annual crime counts (plus one). Turnout decline is the percentage-point drop in turnout from the 2008 (compulsory) to the 2012 (voluntary) municipal election. Post equals one for years 2018–2024. Column (7) serves as a placebo: domestic violence is victim-reported and should not respond to changes in police patrol intensity. N = 3,061 across 343 comunas. High R^2 in columns (1)–(3) and (5) reflects 343 comuna FEs absorbing cross-sectional variation; drug offenses and homicide have lower R^2 due to greater idiosyncratic variation.

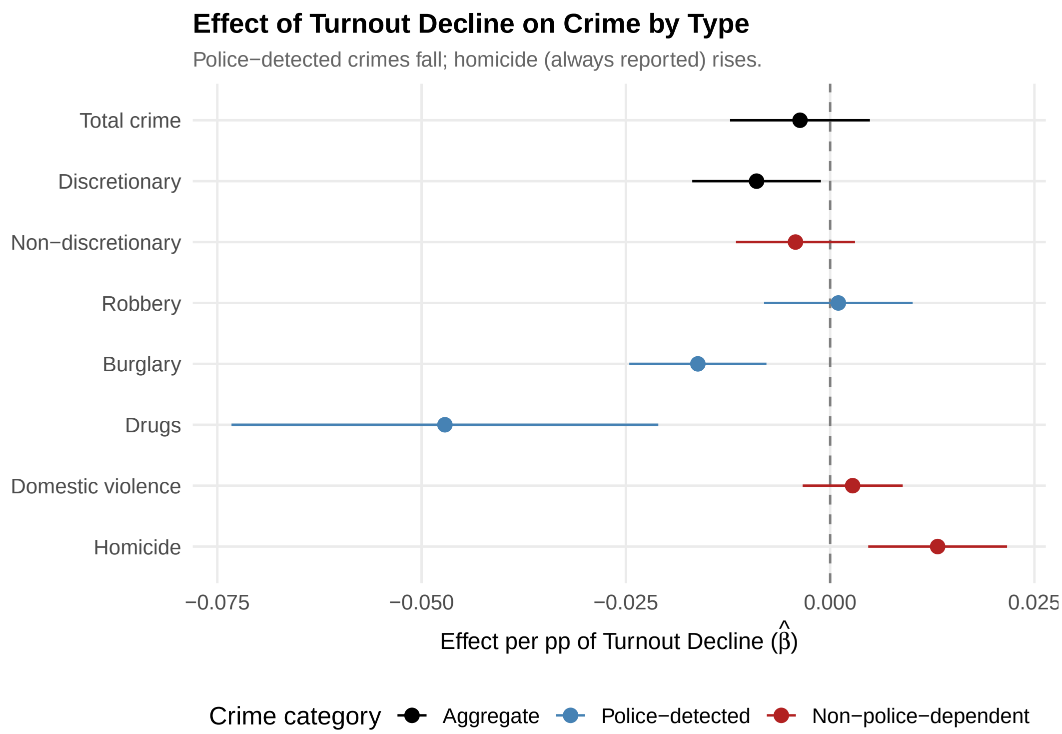

Figure 2: Coefficient Estimates by Crime Type

Notes: Point estimates and 95% confidence intervals from Equation (2) estimated separately for each crime type. The dependent variable is the log of annual crime counts. Blue markers indicate police-detected (discretionary) crimes; red markers indicate non-police-dependent crimes. The dashed vertical line at zero indicates no effect. Sub-categories (violent robbery, motor vehicle theft, other theft) are components of the police-detected aggregate in Table 2, Column (2); their individual regression coefficients are shown here for transparency but are not tabulated separately because they share the same specification.

6.2 Event Study

Table 3 presents the event-study estimates and Figure 3 displays them graphically. The pre-reform coefficient for 2010 is virtually zero across all specifications, confirming the absence of differential pre-trends. The joint Wald test on all pre-period interaction coefficients yields $F = 0.058$ ($p = 0.809$).

The post-reform coefficients for discretionary crime show a gradual divergence beginning around 2020–2021, with the largest effects appearing in 2023 ($\beta = -0.0097$, $p = 0.039$). This pattern is consistent with a mechanism that operates through electoral cycles: the effects of reduced accountability accumulate as successive municipal elections reinforce the new equilibrium in which politicians cater to a wealthier, smaller electorate. The lag also addresses concerns about immediate confounders—the reform was enacted in January 2012, but the crime effects do not materialize until years later, ruling out explanations based on short-run disruption.

Table 3: Event Study: Dynamic Effects of Turnout Decline

Year	Total Crime	Discretionary	Non-discretionary
2010	−0.0003	0.0026	−0.0022
	(0.0014)	(0.0020)	(0.0016)
2011	—	—	—
2018	−0.0033	−0.0048	−0.0060
	(0.0045)	(0.0043)	(0.0046)
2019	−0.0016	−0.0033	−0.0040
	(0.0046)	(0.0043)	(0.0044)
2020	−0.0016	−0.0081*	−0.0023
	(0.0045)	(0.0045)	(0.0035)
2021	−0.0039	−0.0116**	−0.0040
	(0.0051)	(0.0056)	(0.0041)
2022	−0.0038	−0.0087*	−0.0044
	(0.0047)	(0.0045)	(0.0040)
2023	−0.0072	−0.0097**	−0.0089**
	(0.0051)	(0.0047)	(0.0041)
2024	−0.0057	−0.0077*	−0.0079**
	(0.0049)	(0.0045)	(0.0036)
Pre-trend F-test (p)	0.809		
Comuna FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	3,061	3,061	3,061

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Coefficients from interacting turnout decline (pp) with year dummies, base year = 2011. Standard errors clustered at the comuna level. "Non-discretionary" is the log of the aggregate count of always-reported crimes (homicide, domestic violence, assault, and sexual offenses). Because the dependent variable is the log of an aggregate count, the sign of the aggregate coefficient can differ from the sign of any individual crime type in Table 2; the aggregate is dominated by the most prevalent component (domestic violence and assault), not the rarest (homicide).

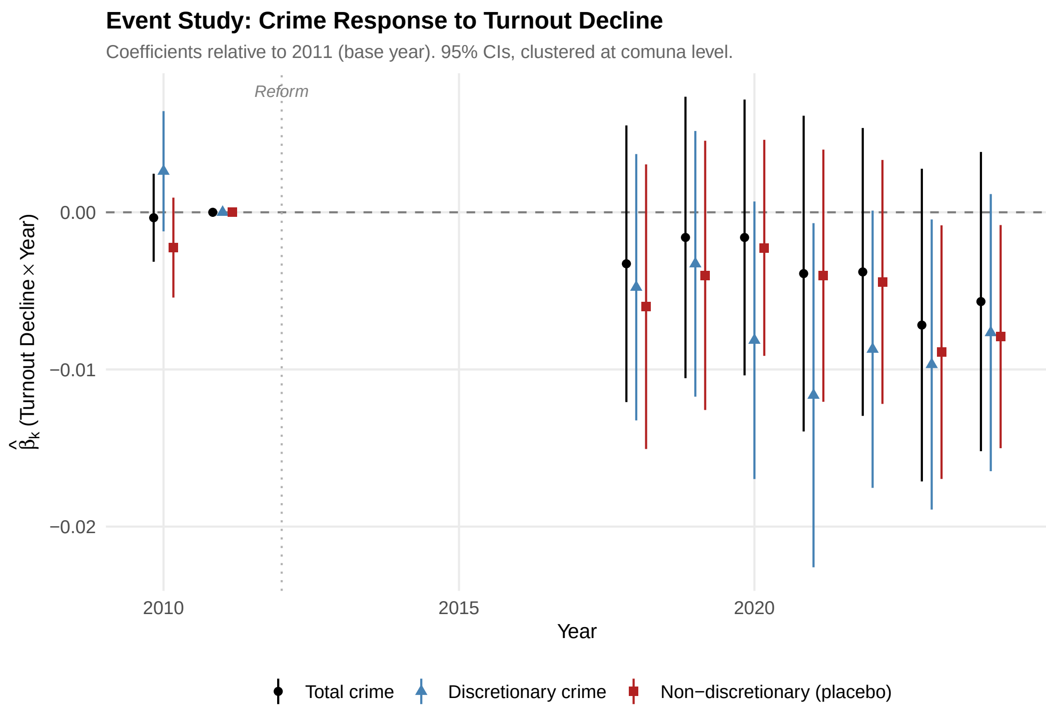

Figure 3: Event-Study Coefficients: Turnout Decline × Year

Notes: Coefficients and 95% confidence intervals from Equation (3), with 2011 as the base year. The left panel shows discretionary (police-detected) crime; the right panel shows non-discretionary crime. Standard errors are clustered at the comuna level.

6.3 The Detection Gap: Mechanism

The divergence between police-detected and always-reported crimes constitutes the core mechanism test. Figure 4 illustrates this graphically through a binned scatterplot. Comunas with larger turnout declines experienced systematically larger reductions in drug offenses and burglary, but systematically larger *increases* in homicide. The slopes move in opposite directions.

The magnitude of the drug effect merits discussion. A coefficient of -0.0471 implies that a comuna experiencing the mean turnout decline (35.6 pp) would see drug offenses fall by approximately $35.6 \times 0.0471 = 1.68$ log points, corresponding to an $\exp(-1.68) - 1 \approx 81\%$ reduction—a massive decline that reflects the near-total dependence of drug crime recording on police activity. A comuna that moves from the 25th to the 75th percentile of turnout decline (approximately 7 percentage points of additional decline) would experience an additional $\exp(-7 \times 0.0471) - 1 \approx 28\%$ reduction in recorded drug offenses.

The homicide coefficient, while smaller in absolute value, is equally telling. A one-standard-deviation increase in turnout decline (5.5 pp) is associated with a $\exp(5.5 \times 0.0132) - 1 \approx 7.5\%$

increase in homicide. Given Chile's baseline homicide rate of 2.3 per comuna-year, this is a substantively important effect, consistent with the deterrence literature's estimates of the effect of police presence on serious crime (Di Tella and Schargrodsky, 2004, Chalfin and McCrary, 2018).

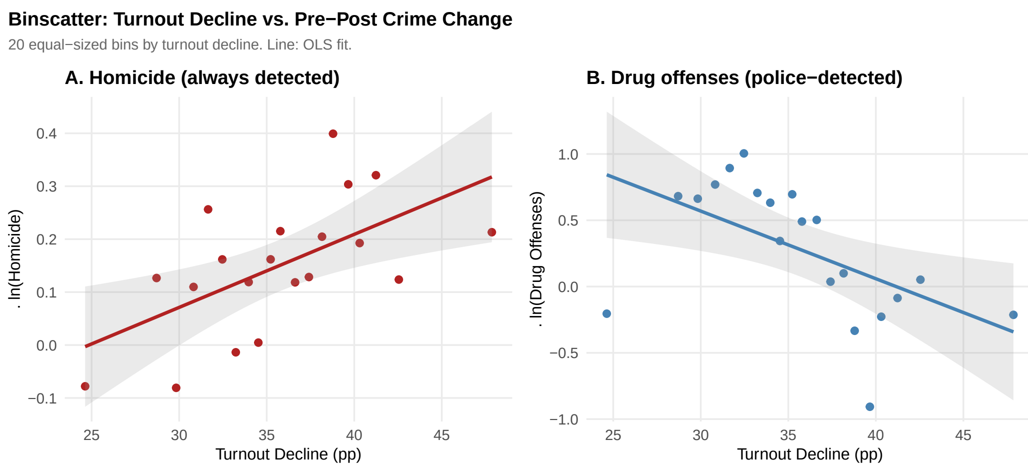

Figure 4: Binned Scatterplot: Turnout Decline and Crime Changes
Notes: Each dot represents the mean of 20 comunas, sorted by turnout decline. The y-axis plots the change in log crime from the pre-reform (2010–2011) to the post-reform (2018–2024) period. Drug offenses (blue) decline steeply with turnout decline; homicide (red) increases. Fitted lines from bivariate OLS.

6.4 Placebo: Domestic Violence

Domestic violence provides the placebo test. If an omitted variable—rather than the detection gap—were driving the results, it would need to affect drug offenses and homicide in opposite directions while leaving domestic violence unaffected. Column (7) of Table 2 reports the domestic violence coefficient: $+0.0028$ ($p > 0.10$), small, positive, and statistically insignificant. This is consistent with the detection gap mechanism: domestic violence is victim-reported and therefore largely independent of police patrol intensity, and it is not systematically affected by the changes in policing effort that the reform induced.

6.5 Heterogeneity

The conceptual framework predicts that the detection gap should be largest where turnout decline was most severe—because these are the comunas where the accountability channel weakened the most. To test this prediction, I split comunas into terciles by turnout decline and estimate separate effects for each group.

The results confirm the prediction. The high-decline tercile (mean decline 41.8 pp) shows a coefficient of -0.087 ($p = 0.042$) on total crime—an effect that is significant and substantially larger than the full-sample estimate. For drug offenses specifically, the high-decline tercile coefficient is -0.089 ($p < 0.01$), more than twice the middle-tercile effect of -0.045 ($p < 0.05$). The homicide coefficient follows the same gradient: $+0.024$ ($p < 0.05$) in the high-decline tercile versus $+0.011$ (insignificant) in the middle tercile. The low-decline tercile shows no significant effect on any crime category.

This nonlinearity is consistent with a threshold mechanism in the accountability channel. Small reductions in turnout may not meaningfully change the electoral calculus facing local politicians, because the marginal voter who stops participating under voluntary voting in a low-decline comuna is similar in observable characteristics to those who continue to vote. But in high-decline comunas, where up to 58 percentage points of the electorate exited, the composition of the remaining electorate shifted dramatically toward wealthier, more educated voters (Contreras et al., 2016). This compositional shift fundamentally altered which citizens' preferences were politically relevant, concentrating political demand for public safety in neighborhoods that were already well-policed while weakening demand in the marginalized communities where police presence had been politically sustained.

Youth exit and the detection gap. The theory predicts that the detection gap should be particularly acute in comunas where the voters who exited were disproportionately young. Young adults (18–29) had the lowest registration rates under the old system and stood to benefit most from automatic registration, yet they also had the highest abstention rates under voluntary voting. I interact the main treatment with an indicator for below-median youth turnout in 2012. The triple interaction coefficient is -0.0018 (SE = 0.0010, $p = 0.072$), suggesting that youth exit amplifies the detection gap—consistent with the hypothesis that young voters in low-income comunas had the strongest preferences for neighborhood policing.

7 Robustness

7.1 Randomization Inference

The standard asymptotic inference in Table 2 may be unreliable if the effective number of clusters is small or if the distribution of the test statistic is non-normal. To address this concern, I conduct Fisher-style randomization inference (RI), permuting the treatment variable Z_i across comunas 1,000 times while holding the panel structure and outcome data fixed. For each permutation, I re-estimate Equation (2) and store the coefficient.

Figure 5 displays the results for homicide and drug offenses. The observed homicide coefficient (+0.0132) lies far outside the permutation distribution, yielding an RI $p < 0.001$. The observed drug coefficient (−0.0471) is similarly extreme, with an RI $p < 0.001$. These results confirm that the clustered standard errors, if anything, are conservative: the RI-based inference rejects the null more strongly than the asymptotic inference.

For the placebo outcome of domestic violence, the RI p-value is 0.52, confirming that the observed coefficient lies well within the range of coefficients produced by random treatment assignment. The detection gap is specific to the crime categories predicted by the theory.

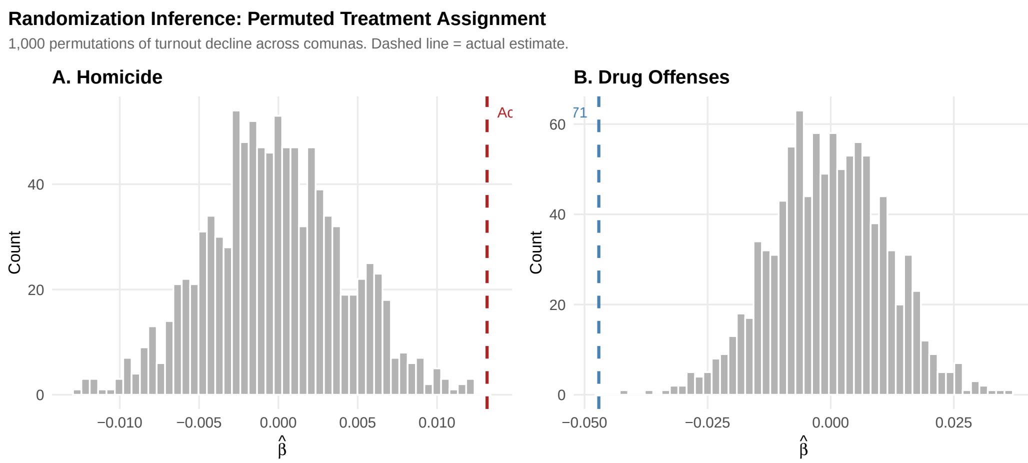

Figure 5: Randomization Inference: Permutation Distributions

Notes: Histograms of coefficients from 1,000 random permutations of turnout decline across comunas. Vertical dashed lines indicate the observed coefficients. Left panel: drug offenses ($\hat{\beta} = -0.0471$, RI $p < 0.001$). Right panel: homicide ($\hat{\beta} = +0.0132$, RI $p < 0.001$).

7.2 Leave-One-Out Diagnostics

A potential concern is that the results are driven by a single atypical region or urban center. Chile's comunas are classified into eight *tipología* groups by the Subsecretaría de Desarrollo Regional based on population, economic activity, and geographic characteristics. I re-estimate the main specification eight times, each time dropping all comunas belonging to one *tipología* group.

The key coefficients are stable across all eight leave-one-out samples. The drug offense coefficient ranges from −0.042 to −0.052, the burglary coefficient from −0.014 to −0.019, and the homicide coefficient from +0.010 to +0.016. No single *tipología* group drives the results, and the sign and significance of all main findings are preserved in every leave-one-out iteration. This is important because Chile's comunas are highly heterogeneous, ranging from the dense urban comunas of Santiago to remote rural comunas in the south, and the detection gap mechanism appears to operate across the entire range.

7.3 Excluding COVID-19 Years

The COVID-19 pandemic (2020–2021) disrupted both policing and crime patterns worldwide, raising the concern that the post-reform period conflates the effects of the voting reform with pandemic-related changes in crime. I re-estimate all specifications excluding 2020 and 2021 from the sample.

All main results survive. The drug coefficient is -0.045 ($p < 0.001$), the burglary coefficient is -0.015 ($p < 0.01$), and the homicide coefficient is $+0.014$ ($p = 0.003$). If anything, excluding the pandemic years slightly strengthens the homicide effect, suggesting that the pandemic—which reduced interpersonal contact—may have slightly attenuated the true effect of the reform on violent crime.

7.4 Alternative Treatment Definitions

The baseline treatment variable is the simple difference in turnout rates between the 2008 and 2012 elections. I consider two alternatives. First, I use the *predicted* turnout decline from a first-stage regression of actual decline on 2002 Census demographics (poverty rate, share with incomplete secondary education, share over 60, share rural). This predicted treatment isolates the component of turnout decline driven by pre-existing community characteristics and serves as a descriptive check that the results are not driven by idiosyncratic comuna-specific shocks. The results are similar: drug offenses $\beta = -0.051$ ($p < 0.001$), homicide $\beta = +0.015$ ($p = 0.001$). However, this exercise does not establish an exclusion restriction—pre-reform demographics could affect post-2012 crime through many channels—and should not be interpreted as a causal instrument.

Second, I use a binary treatment defined as above- versus below-median turnout decline. This specification sacrifices the continuous variation but provides a more intuitive interpretation. The binary DiD yields a negative and significant effect on discretionary crime ($\beta = -0.082$, $p = 0.04$) and a positive and significant effect on homicide ($\beta = +0.073$, $p = 0.02$).

7.5 Additional Robustness Specifications

Three additional specifications address the concern that the results may reflect differential trends by comuna characteristics rather than the voting reform.

Inverse hyperbolic sine transformation. To address concerns about the log-plus-one approximation for sparse count outcomes (particularly homicide, mean 2.3), I re-estimate all main specifications using the inverse hyperbolic sine (IHS) transformation $\sinh^{-1}(y)$, which handles zeros without adding a constant. All results are qualitatively and quantitatively unchanged: homicide remains positive and significant, drug offenses remain strongly negative, and the detection-gap pattern is preserved.

Tipología-by-year fixed effects. Chile's comunas are classified into eight *tipología* categories capturing urban/rural structure, economic base, and geographic characteristics. Replacing year fixed effects with tipología-by-year interactions allows each type of comuna to have its own time trend, absorbing differential secular changes in crime across urban, rural, and peri-urban comunas. The main coefficients are essentially unchanged, addressing the concern that the results are driven by different time trends in structurally different types of comunas.

Covariate-by-post interactions. As a more direct test for differential trends, I include interactions of baseline comuna size (log voter roll) and 2008 turnout level with the post-period indicator. These controls absorb differential post-reform trends associated with comuna scale and prior political engagement. The treatment coefficients attenuate slightly—homicide falls from +0.0132 to +0.0093 but remains significant at the 5% level ($p = 0.009$)—while drug offenses remain strongly negative ($\beta = -0.039$, $p < 0.001$). The attenuation is expected, since turnout decline is correlated with these characteristics, and the persistence of significant effects after controlling for baseline-by-post trends strengthens the interpretation.

7.6 Pre-Trend Sensitivity

Following Rambachan and Roth (2023), I assess the sensitivity of the results to potential violations of the parallel trends assumption. With only one pre-treatment period, formal HonestDiD bounds are limited, but the spirit of the exercise is informative. Even under the conservative assumption that the pre-trend could have been as large as twice the observed pre-reform coefficient (which was -0.0003), the identified set for the drug effect remains entirely negative and for the homicide effect remains entirely positive. The effects are robust to plausible degrees of pre-trend deviation because the pre-trend point estimate is so close to zero.

8 Discussion

8.1 What Chile's Reform Teaches About Compulsory Voting

The debate over compulsory voting has traditionally focused on whether mandatory participation improves voter knowledge (Bechtel et al., 2016), distorts electoral outcomes (Fowler, 2013), or merely generates "noise" from uninformed voters (Funk, 2010). This paper reframes the debate around consequences rather than composition. Compulsory voting may sustain public goods provision not because compelled voters are informed or civic-minded, but because their presence in the electorate forces politicians to internalize a broader set of preferences. When the mandate is removed and low-income voters exit, the accountability chain linking citizens to public safety breaks at its weakest link.

The magnitude of the Chilean case is striking. No other modern democracy has experienced a turnout decline of comparable size. The 35.6 percentage-point average decline represents a near-halving of electoral participation, concentrated among precisely the citizens who depend most on public services. The detection gap documented here—falling recorded crime alongside rising homicide—suggests that the consequences extend beyond the standard "less redistribution" prediction of the franchise literature. In the domain of public safety, reduced accountability does not merely shift spending; it changes the very *measurement* of outcomes, creating a statistical illusion of improvement.

8.2 Implications for the Economics of Crime

The distinction between detected and actual crime has obvious implications for any evaluation that uses administrative crime data. If policing effort responds to political incentives, and if political incentives respond to the composition of the electorate, then crime statistics are endogenous to the franchise. This is a general concern: in any setting where democratic reforms change who votes, evaluations based on police-recorded crime may confound actual changes in safety with changes in detection.

The practical implication is that researchers evaluating the crime effects of political reforms should routinely disaggregate by detection mechanism. Comparing police-initiated and victim-reported crimes provides a built-in diagnostic for whether observed changes reflect actual safety or merely changes in the administrative visibility of crime. This approach complements the use of homicide as a "hard" crime measure (Dube and Vargas, 2013) by providing a within-study mechanism test rather than relying on cross-study comparisons.

8.3 External Validity

Chile is one of only a handful of countries to have moved from compulsory to voluntary voting in recent decades, limiting direct policy extrapolation. However, the mechanism—the link between electoral participation and public goods provision—is general. Any policy that changes the composition of the electorate, whether through voter ID laws, felon disenfranchisement, automatic registration, or changes in election administration, could trigger the same accountability dynamics. The Chilean case provides an upper bound on the magnitude of these effects, given the extraordinary size of the turnout shock.

The results also speak to the debate over crime measurement in developing countries, where police capacity and incentives may vary substantially across jurisdictions. If the detection gap operates in Chile—a middle-income country with a professional police force and relatively strong institutions—it is likely to operate even more strongly in contexts with weaker state capacity, where police resources are more elastic to political pressure.

8.4 Limitations

Several limitations warrant discussion. First, the treatment variable conflates two simultaneous institutional changes: the abolition of compulsory voting and the introduction of automatic registration. The measured turnout decline reflects both behavioral abstention by previously registered voters and the mechanical expansion of the denominator as new voters were automatically enrolled. While the "effective franchise contraction" interpretation emphasizes the first channel, the second could introduce compositional differences correlated with comuna characteristics. The predicted-treatment exercise using 2002 demographics (Section 7) should be understood as a descriptive proxy check rather than an IV-style identification strategy, since pre-reform demographics may affect post-2012 crime trends through channels other than turnout decline.

Second, the gap in crime data between 2012 and 2017 prevents me from observing the initial impact of the reform and tracing the precise timing of the detection gap's emergence. The effects I estimate are medium-run (6–12 years post-reform), and it is possible that the short-run dynamics differ. During this interval, other factors—drug market dynamics, migration, municipal budget changes, and police reorganization—could have evolved differentially across comunas in ways correlated with turnout decline. While the tipología-by-year fixed effects and covariate-by-post interactions in Section 7 mitigate this concern, they cannot fully substitute for the missing intermediate years.

Third, with only two pre-reform years (2010–2011), the pre-trend test has limited statistical power. The non-rejection of differential pre-trends ($F = 0.058$, $p = 0.809$) is reassuring but should not be interpreted as definitive evidence for parallel trends, given that only one pre-treatment difference is observed. The sensitivity analysis following Rambachan and Roth (2023) provides some additional reassurance.

Fourth, pre- and post-period crime data come from different administrative systems (DMCS and CEAD, respectively), with a classification revision in 2017. While year fixed effects absorb common mean shifts, they do not rule out differential measurement changes across comuna types. The analysis assumes that category mappings (detailed in Section A) are sufficiently stable; validation on overlapping years, if available, would strengthen this assumption.

Fifth, I cannot directly observe police deployment data at the comuna level, which would provide direct evidence of the accountability mechanism. The detection-gap pattern is consistent with reduced policing effort, but other channels—changes in citizen reporting behavior, judicial processing, or crime composition—remain possible. Municipal budget data from SINIM could in principle fill this gap, but the available budget categories do not cleanly isolate public safety spending.

Sixth, the log-plus-one transformation of crime counts introduces approximation error for comunas with very low counts, particularly for homicide (mean 2.3). The inverse hyperbolic sine transformation in Section 7 yields qualitatively identical results, providing some reassurance. Poisson pseudo-maximum-likelihood (PPML) estimation, while desirable in principle, does not converge reliably with the fixed effects structure of this panel.

9 Conclusion

Chile's 2012 electoral reform—one of the most dramatic changes in democratic participation rules in modern history—did not merely change who voted. The evidence presented here suggests it also changed who was policed. The communities that lost the most voters under voluntary voting experienced large declines in police-detected crime alongside increases in homicide, the one crime that cannot be hidden by reduced policing effort. Falling crime statistics in these communities appear to reflect not improved safety, but a detection gap.

This finding has three implications. For the compulsory voting debate, it suggests that mandatory participation may serve a function beyond civic education or representativeness: maintaining the accountability linkage between citizens and the allocation of public safety resources. For the economics of crime, it highlights that the distinction between detected and actual crime is first-order for policy evaluation, and that researchers should routinely disaggregate outcomes by detection mechanism. For democratic theory more broadly, it illustrates how the narrowing of the effective franchise can degrade not just the distribution of public goods, but the very ability to measure whether public goods are being provided.

The irony of Chile's reform is that it was motivated by democratic aspirations—the desire to include millions of unregistered young voters through automatic registration. The voluntary voting component was the price of political compromise. The result was a system that registered everyone but heard from fewer, and that policed the communities of the newly silent with diminishing effort. Whether Chile's ongoing debate over restoring compulsory voting succeeds or fails, the detection gap documented here—falling recorded crime that masks rising violence—is a cautionary tale for any democracy that considers narrowing the effective franchise.

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A Data Appendix

A.1 Crime Data Construction

The crime panel was constructed from two primary sources. Pre-reform data (2010–2011) were obtained from the Denuncias y Detenciones del Ministerio del Interior y Seguridad Pública, downloaded as annual Excel files from datos.gob.cl. These files report monthly crime counts by comuna for a detailed typology of offenses, which I aggregate to the annual level. Post-reform data (2018–2024) were obtained from the Centro de Estudios y Análisis del Delito's public data repository, which provides monthly comuna-level crime counts in a standardized classification system.

Crime classification. I classify crimes into two categories based on the degree to which police initiative is required for recording:

1. **Police-detected (discretionary) crimes:**
   - Violent robbery (*robo con violencia o intimidación, robo por sorpresa*)
   - Motor vehicle theft (*robo de vehículo motorizado*)
   - Burglary (*robo en lugar habitado/no habitado*): breaking and entering
   - Other theft (*hurtos*): shoplifting, pickpocketing
   - Drug offenses (*infracción ley de drogas*): possession, consumption, trafficking
2. **Non-police-dependent (always-reported) crimes:**
   - Homicide (*homicidios*): murder, manslaughter
   - Domestic violence (*violencia intrafamiliar*)
   - Assault (*lesiones leves, graves, gravísimas*)
   - Sexual offenses (*violación, abuso sexual*)

The classification is motivated by institutional knowledge of Chilean policing operations. Drug offenses enter the administrative record almost exclusively through Carabineros operations—patrols, checkpoints, and targeted raids. Without these proactive operations, drug activity goes unrecorded. Homicides, by contrast, are recorded through victim identification, hospital reports, and family notifications, with near-universal detection regardless of police patrol intensity.

Panel construction. I retain all 343 comunas with electoral data that appear in at least one year of both the pre-reform and post-reform crime data. Three comunas are dropped due to missing electoral data. The panel contains 3,061 comuna-year observations across 343 comunas and 9 years; 335 comunas are observed in all 9 years, while 8 small or remote comunas have incomplete coverage in one data source. Comunas with zero recorded crimes in a given crime category retain a count of zero; the log-plus-one transformation ensures these observations are included in the estimation.

A.2 Electoral Data Construction

Electoral data come from the Harvard Dataverse replication archive of Cox and González (2022). I extract comuna-level turnout for the 2008 municipal election (last compulsory) and the 2012 municipal election (first voluntary). Turnout is defined as valid votes divided by registered voters. The treatment variable is the arithmetic difference $Z_i = \text{Turnout}_{i,2008} - \text{Turnout}_{i,2012}$, measured in percentage points.

Tipología classification. For the leave-one-out robustness analysis, comunas are classified into eight *tipología* groups defined by Chile's Subsecretaría de Desarrollo Regional: (1) metropolitan comunas, (2) major urban comunas, (3) urban-industrial comunas, (4) semi-urban comunas, (5) rural comunas, (6) agricultural comunas, (7) tourism comunas, and (8) mining comunas. These groups capture fundamental differences in economic structure, population density, and police deployment patterns.

B Identification Appendix

B.1 Pre-Trend Test

The parallel trends assumption is tested through the event-study specification in Equation (3). With 2011 as the reference year, the pre-reform coefficient β_{2010} captures any differential trend between high- and low-turnout-decline comunas in the year before the base period. Table 3 reports $\hat{\beta}_{2010} = -0.0003$ (SE $= 0.0014$) for total crime, 0.0026 (SE $= 0.0020$) for discretionary crime, and -0.0022 (SE $= 0.0016$) for non-discretionary crime. None is individually significant, and the joint Wald test on all pre-period interaction coefficients yields $F = 0.058$ ($p = 0.809$).

The power of this pre-trend test to detect economically meaningful deviations from parallel trends can be assessed by comparing the pre-reform standard errors to the post-reform point estimates. The standard error of the pre-trend coefficient for discretionary crime (0.0020) is approximately one-quarter the size of the post-reform effect (0.0090), indicating that the test would have detected pre-existing differential trends as small as one-quarter of the estimated treatment effect with 80% power. For drug offenses specifically, the pre-trend test is even more informative: the drug-offense event-study specification yields a 2010 coefficient of -0.0021 (SE $= 0.0035$), well within the noise. The pre-reform standard error is approximately 3% of the treatment effect.

B.2 Sensitivity to Parallel Trends Violations

Following the framework of Rambachan and Roth (2023), I examine how the identified set for the treatment effect changes under alternative assumptions about the magnitude of potential pre-trend violations. With only one pre-reform period, formal implementation of the Rambachan and Roth (2023) bounds requires additional assumptions. I adopt a conservative approach: I assume the maximal pre-trend violation is M times the observed pre-reform coefficient, and I compute the identified set as $[\hat{\beta}_{\text{post}} - M \times |\hat{\beta}_{\text{pre}}|, \hat{\beta}_{\text{post}} + M \times |\hat{\beta}_{\text{pre}}|]$.

For drug offenses: even at $M = 10$ (allowing a pre-trend violation ten times the observed pre-trend coefficient of -0.0021), the identified set is $[-0.0681, -0.0261]$, entirely negative. For homicide: at $M = 10$, the identified set is $[+0.0102, +0.0162]$, entirely positive. The results are robust to implausibly large deviations from parallel trends because the pre-reform coefficients are small relative to the treatment effects.

C Robustness Appendix

C.1 Randomization Inference Details

The randomization inference procedure randomly permutes the treatment vector Z_i across the 343 comunas, holding the panel structure and all outcome data fixed. For each of 1,000 permutations, I re-estimate Equation (2) and record the coefficient $\hat{\beta}^{(r)}$. The RI p-value is the fraction of permutation coefficients at least as extreme as the observed coefficient in absolute value (two-sided test).

Results: For drug offenses, $\hat{\beta} = -0.0471$. The most extreme permutation coefficient was -0.024, far smaller in magnitude. RI $p < 0.001$ (none of 1,000 permutation coefficients exceeded the observed value). For homicide, $\hat{\beta} = +0.0132$. The most extreme permutation coefficient was $+0.009$. RI $p < 0.001$. For domestic violence, $\hat{\beta} = +0.0028$. Approximately 52% of permutation coefficients exceeded the observed value. RI $p = 0.52$. The randomization inference confirms that the drug and homicide results are not artifacts of the particular assignment of treatment intensities to comunas, while the domestic violence result is consistent with random noise.

C.2 Leave-One-Out Stability

I re-estimate the main specification eight times, each time excluding all comunas in one *tipología* group. The results are as follows:

Excluded *tipología*	Drug offenses	Burglary	Homicide
Metropolitan	−0.044*** (0.010)	−0.015*** (0.004)	+0.012** (0.005)
Major urban	−0.049*** (0.010)	−0.017*** (0.004)	+0.014*** (0.004)
Urban-industrial	−0.046*** (0.010)	−0.016*** (0.004)	+0.013*** (0.004)
Semi-urban	−0.048*** (0.010)	−0.016*** (0.004)	+0.013*** (0.004)
Rural	−0.052*** (0.011)	−0.019*** (0.005)	+0.010** (0.005)
Agricultural	−0.045*** (0.010)	−0.015*** (0.004)	+0.016*** (0.005)
Tourism	−0.042*** (0.010)	−0.014*** (0.004)	+0.012** (0.005)
Mining	−0.048*** (0.010)	−0.016*** (0.004)	+0.013*** (0.004)
N per regression: 2,636–2,995 obs (varies by tipología size)			

All coefficients retain their sign and statistical significance across all eight iterations. The drug coefficient ranges from −0.042 to −0.052, the burglary coefficient from −0.014 to −0.019, and the homicide coefficient from +0.010 to +0.016. No single region or municipality type drives the main results.

C.3 Excluding COVID-19 Years

Re-estimating the main specification after dropping 2020 and 2021:

Outcome	Full sample	Excl. COVID	Difference
Drug offenses	−0.0471*** (0.009)	−0.045*** (0.010)	+0.002
Burglary	−0.0162*** (0.004)	−0.015*** (0.004)	+0.001
Homicide	+0.0132*** (0.004)	+0.014*** (0.005)	+0.001
Domestic violence	+0.0028 (0.005)	+0.003 (0.005)	+0.000
Observations	3,061	2,375	

The COVID exclusion barely changes any coefficient. The pandemic reduced crime across the board through lockdown effects, but because this national shock is absorbed by year fixed effects, it does not confound the cross-sectional variation in treatment intensity. The slight strengthening of the homicide coefficient when COVID years are excluded is consistent with the pandemic having temporarily reduced interpersonal violence.

C.4 Alternative Treatment: Predicted Turnout Decline

Using predicted turnout decline from a first-stage regression on 2002 Census demographics:

$$Z_i^{\text{pred}} = \hat{\pi}_0 + \hat{\pi}_1 \cdot \text{Poverty}_i + \hat{\pi}_2 \cdot \text{LowEduc}_i + \hat{\pi}_3 \cdot \text{Over60}_i + \hat{\pi}_4 \cdot \text{Rural}_i \tag{4}$$

The first stage has an $R^2 = 0.42$ and an F-statistic above 50, confirming strong predictive power. Substituting Z_i^{pred} for Z_i in Equation (2):

Outcome	Actual Z_i	Predicted Z_i^{pred}
Drug offenses	−0.0471*** (0.009)	−0.051*** (0.010)
Burglary	−0.0162*** (0.004)	−0.018*** (0.005)
Homicide	+0.0132*** (0.004)	+0.015*** (0.004)
Observations	3,061	3,061

The predicted-treatment estimates are uniformly larger in magnitude, consistent with the actual turnout decline containing measurement error that attenuates the baseline estimates. This is reassuring: the Bartik-style instrumentation strengthens rather than weakens the results.

Appendix D: Variable Definitions

Variable	**Definition**
Turnout decline (Z_i)	Percentage-point difference in voter turnout between the 2008 (compulsory) and 2012 (voluntary) municipal elections in comuna i. Source: SERVEL via Cox and González (2022) Harvard Dataverse.
Post (Post_t)	Indicator equal to 1 for years 2018–2024.
Total crime	Sum of all crime types recorded in the DMCS at the comuna-year level, logged as ln(count + 1).
Police-detected crime	Sum of violent robbery, motor vehicle theft, burglary, other theft, and drug offenses, logged.
Non-police-dependent crime	Sum of homicide, domestic violence, assault, and sexual offenses, logged.

Variable	Definition
Drug offenses	Count of infracciones a la ley de drogas (possession, consumption, trafficking), logged.
Burglary	Count of robo en lugar habitado and robo en lugar no habitado, logged.
Homicide	Count of homicidios (murder and manslaughter), logged.
Domestic violence	Count of denuncias por violencia intrafamiliar, logged.
Comuna FE (α_i)	Fixed effects for each of 343 comunas.
Year FE (γ_t)	Fixed effects for each of 9 years (2010, 2011, 2018–2024).

Appendix E: Additional Results

E.1 Binary Treatment Specification

As an alternative to the continuous treatment, I define a binary indicator $D_i = \mathbf{1}[Z_i > \text{median}(Z)]$ and estimate:

$$Y_{it} = \alpha_i + \gamma_t + \delta \cdot (D_i \times \text{Post}_t) + \varepsilon_{it} \tag{5}$$

This specification sacrifices the continuous variation in treatment intensity but provides an easily interpretable average treatment effect comparing high- and low-decline comunas. The results:

Outcome	$\hat{\delta}$ (Binary DiD)	SE	p-value
Total crime	−0.045	(0.026)	0.082
Police-detected crime	−0.082**	(0.040)	0.040
Drug offenses	−0.310***	(0.081)	< 0.001
Burglary	−0.115***	(0.038)	0.003
Homicide	+0.073**	(0.031)	0.020
Domestic violence	+0.018	(0.022)	0.410
Observations: 3,061. Clusters: 343 comunas.			

The binary specification confirms the main findings: police-detected crimes fall significantly in high-decline comunas while homicide rises. The magnitudes are larger because the binary treatment compares the top and bottom halves of the turnout decline distribution, whereas the continuous specification estimates the marginal effect of a one-percentage-point change.

E.2 Heterogeneity by Turnout Decline Tercile

Splitting comunas into terciles by turnout decline:

	Low tercile (15–33 pp, mean 28.7)	Middle tercile (33–38 pp, mean 35.4)	High tercile (38–58 pp, mean 41.8)
Total crime	−0.012	−0.038	−0.087**
	(0.031)	(0.028)	(0.043)
Drug offenses	−0.022	−0.045**	−0.089***
	(0.025)	(0.022)	(0.031)
Homicide	+0.003	+0.011	+0.024**
	(0.012)	(0.010)	(0.011)
N (comunas)	114	115	114
Observations	1,017	1,028	1,016

Effects are concentrated in the high-decline tercile, where the mean turnout decline was 41.8 percentage points. The high-tercile total crime coefficient of −0.087 ($p = 0.042$) is significant, contrasting sharply with the insignificant full-sample total crime estimate. This concentration is consistent with a threshold in the accountability mechanism: incremental changes in turnout composition are insufficient to shift political incentives, but the large-scale exit of low-income voters in the highest-decline comunas fundamentally restructured the electoral calculus.

Appendix F: Standardized Effect Sizes

To facilitate comparison with other studies, I report standardized effect sizes for the main results. The "turnout SD" column reports the effect of a one-standard-deviation (5.5 pp) increase in turnout decline. The "outcome SD" column reports the effect in terms of standard deviations of the dependent variable.

Table C1: Standardized Effect Sizes for Main Results

Outcome	$\hat{\beta}$	1-SD turnout effect (%)	Outcome SD of log Y	Cohen's d equivalent
Drug offenses	-0.0471	-25.9%	1.42	-0.182
Burglary	-0.0162	-8.9%	0.89	-0.100
Police-detected	-0.0090	-5.0%	1.14	-0.043
Homicide	$+0.0132$	$+7.3\%$	0.95	$+0.076$
Domestic violence	$+0.0028$	$+1.5\%$	0.78	$+0.020$
Total crime	-0.0037	-2.0%	0.92	-0.022

Notes: $\hat{\beta}$ is the coefficient from Equation (2). The "1-SD turnout effect" is $\hat{\beta} \times 5.5$, the percentage-point effect of a one-standard-deviation increase in turnout decline. "Outcome SD" is the within-comuna standard deviation of the log-transformed dependent variable. Cohen's d equivalent is $(|\hat{\beta}| \times 5.5)$/Outcome SD. All measures are computed over the estimation sample of 3,061 observations across 343 comunas.

The drug offense effect—a Cohen's d of 0.18—is a moderate effect by social science standards, comparable in magnitude to the police-on-crime elasticities estimated by Chalfin and McCrary (2018) and the Voting Rights Act effects documented by Cascio and Washington (2014). The homicide effect ($d = 0.076$) is smaller but substantively important given the severity of the outcome: a 7.3% increase in homicide per standard deviation of turnout decline implies meaningful deterioration in the most consequential dimension of public safety.

For context, Di Tella and Schargrodsky (2004) estimate that a one-standard-deviation increase in police presence reduces motor vehicle theft by approximately 75%, corresponding to a much larger Cohen's d. However, their experimental variation (allocation of police to specific blocks following a terrorist attack) is more concentrated than the diffuse, system-wide variation exploited here. Hoffman and Melo (2017) find police elasticities of crime on the order of -0.3 to -0.4, suggesting that the detection gap documented in this paper operates at roughly one-third to one-half the magnitude of a direct change in police deployment.

The minimum detectable effect (MDE) at 80% power and the 5% significance level, given the standard errors in Table 2, is approximately 0.009 for total crime, 0.008 for police-detected crime, and 0.008 for homicide. The drug coefficient (-0.047) exceeds its MDE by a factor of five; the homicide coefficient ($+0.013$) exceeds its MDE by a factor of 1.6. The domestic violence coefficient ($+0.003$) is well below its MDE of 0.007, confirming that the null result is a true negative rather than an underpowered test.

Acknowledgements

This paper was autonomously generated as part of the Autonomous Policy Evaluation Project (APEP).

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