Crime on ICE: Local Police Behavior and Immigration Enforcement in the United States

John J. Donohue* Tai Markman† ‡ Eric A. Baldwin§

October 20, 2026

Abstract

A growing number of local police departments now hold authority to enforce federal immigration law, a role traditionally reserved for federal officers. Despite claims that this authority enhances public safety, empirical evidence remains limited. Using FBI Uniform Crime Report data from 2000–2024, we exploit staggered adoption across U.S. jurisdictions to estimate causal effects on policing. We find that this enforcement authority increases arrests for low-level, discretionary offenses by approximately 7%, with no corresponding decreases in offense rates or serious crime. These results suggest that such agreements realign police priorities toward discretionary enforcement without measurable public safety benefits.

Keywords: Immigration enforcement, 287(g) agreements, Policing, Crime, Arrests, Difference-in-differences

JEL codes: K42, J15, H77, K37

*John J. Donohue is the C. Wendell and Edith M. Carlsmith Professor at Stanford, Senior Fellow at the Stanford Institute for Economic Policy Research, Elected Fellow of the American Academy of Arts and Sciences, and Research Associate at the National Bureau of Economic Research.

†Tai Markman is Predoctoral Research Fellow, Stanford University.

‡Corresponding author: Tai Markman, 559 Nathan Abbott Way, Crown Quadrangle, Stanford, CA, 94305. Email: tbm45@georgetown.edu

§Eric A. Baldwin is Postdoctoral Research Fellow, Stanford University.

I. Introduction

Immigration enforcement is a perennial issue in American public policy. In recent years, much of the debate has centered around whether the enforcement of federal immigration law impacts crime and public safety. 2,650 local law enforcement agencies across 38 states currently hold federal immigration enforcement powers through 287(g) agreements with Immigration and Customs Enforcement (ICE) (U.S. Immigration and Customs Enforcement 2025), deputizing local police to perform functions traditionally reserved for federal immigration officers. These agreements have existed for more than two decades, but recent expansion has pushed them to the forefront of the public policy debate. Despite this growth, we lack credible causal evidence of their effects on local policing. Does deputizing local police to enforce federal immigration law actually make communities safer?

This paper provides the first nationwide causal evaluation of how 287(g) agreements affect local policing. Exploiting staggered adoption of agreements across agencies from 2000–2024, we find that these agreements do not appear to reduce serious crime. Instead, they appear to realign police priorities toward low-level, discretionary enforcement. Following adoption, arrests for violent and property crimes, which involve comparatively little officer discretion, show no significant response. Instead, arrests for discretionary offenses rise by approximately 7% overall, driven primarily by drug arrests, as well as increases in alcohol-related public order offenses such as DUI and liquor laws. We show these patterns reflect changes in police behavior rather than underlying crime trends, as neighboring agencies in the same county show no corresponding increases in arrests, and reported offense rates remain stable even as arrests rise.

Understanding the effect of the 287(g) program has become increasingly urgent, as the initiative has recently expanded to an unprecedented scale. Yet prior research offers limited guidance, as existing studies focus on single states, rely on qualitative methods, or use data ending before major policy shifts in 2012. Critically, prior research does not distinguish between arrests, which reflect police behavior, and reported offenses, which reflect crime known to police independent of enforcement choices. This distinction is essential for separating a program that shifts police priorities toward discretionary, low-level offenses from one that genuinely reduces crime.

We develop a simple conceptual framework with two competing channels through which 287(g) agreements might affect policing and public safety. Under a deterrence channel, enforcement removes repeat offenders or increases perceived risk for offenders, reducing crime (Cook 1980; Nagin et al. 2015). This predicts declines in all types of crime. Under a discretion-reallocation channel, immigration enforcement authority creates incentives for

officers to pursue discretionary, easily observable offenses, shifting police resources away from serious crime investigation (Baicker and Jacobson 2007; Makowsky and Stratmann 2009). This channel predicts no change, or possibly increases, in arrests for serious crime, but increases for low-level offenses where officer discretion is high, even absent any change in underlying offense rates.

Importantly, these channels generate different predictions for arrests versus reported offenses. The deterrence channel predicts both should decline together. The discretion-reallocation channel predicts arrests may rise for discretionary offenses, even if underlying offense rates remain stable. This would be consistent with changing police practices, rather than any actual reduction in crime.

Our findings contribute to two lines of research. First, we speak to the understanding of police behavior and discretionary enforcement (Harcourt and Ludwig 2006; Smith et al. 2005), showing how a federal enforcement mandate can reshape local police priorities even absent any change in underlying crime. Second, we add to the research on the link between immigration enforcement and crime (Miles and Cox 2014), providing the first national causal evidence on this type of local-federal collaboration program.

Our results have clear policy relevance. Federal policymakers are rapidly expanding 287(g) agreements despite no evidence these programs reduce crime. Instead, our findings suggest that these agreements redirect police resources toward low-level discretionary enforcement while leaving serious crimes unaffected.

The paper proceeds as follows: Section II outlines the background of 287(g) agreements and situates our argument within the literature on police discretion and immigration enforcement. Section III presents the data; Section IV presents the empirical strategy; Section V presents results and robustness checks. Section VI discusses the implications of our findings for law enforcement and concludes.

II. Local Immigration Enforcement and Police Activity

II.A. The 287(g) Program and Prior Research

Section 287(g) of the 1996 Illegal Immigration Reform and Immigration Responsibility Act authorizes ICE to enter into Memoranda of Agreement (MOAs) with state and local law enforcement agencies, delegating federal immigration authority to local officers.[1] Deputized

1. There are three types of 287(g) agreements: the Task Force Model (TFM), the Jail Enforcement Model (JEM), and the Warrant Service Officer Model (WSO). Their main functions and differences are discussed in detail in Appendix A.

officers may identify, interview, process, and detain individuals suspected of immigration violations. Selected officers undergo training by ICE agents, ranging from eight hours to four weeks, with ICE covering training costs, while participating agencies bear ongoing expenses. Agreements are voluntary (except in cases where state governments mandate agreements, such as Florida) and non-permanent, so agreements can be terminated by either party.

Florida became the first state to enter a 287(g) agreement in 2002, followed by Alabama in 2003. Adoption remained limited through the mid-2000s, with fewer than 20 agencies participating by 2005. Federal promotion of the program as part of a broader national security effort (Office of the President 2007) helped expand participation to more than 50 agencies by 2008. Growth continued, but all Task Force Model (TFM) agreements were suspended in 2012, amid civil rights and efficiency concerns (Department of Homeland Security 2012), and participation declined through 2016 under Jail Enforcement Model (JEM)-only enforcement. Enrollment later grew again under the JEM model and, beginning in 2019, the Warrant Service Officer Model (WSO), reaching approximately 150 agencies by 2020. Existing agreements were largely maintained but no new agreements were approved for several years thereafter, before a subsequent, unprecedented expansion reinstated the TFM and launched aggressive recruitment of new agencies in 2025.[2] Figure 1 shows the number of agencies with agreements from 2000–2024, and Figure 2 displays the geographic reach of 287(g) agreements over this same period.

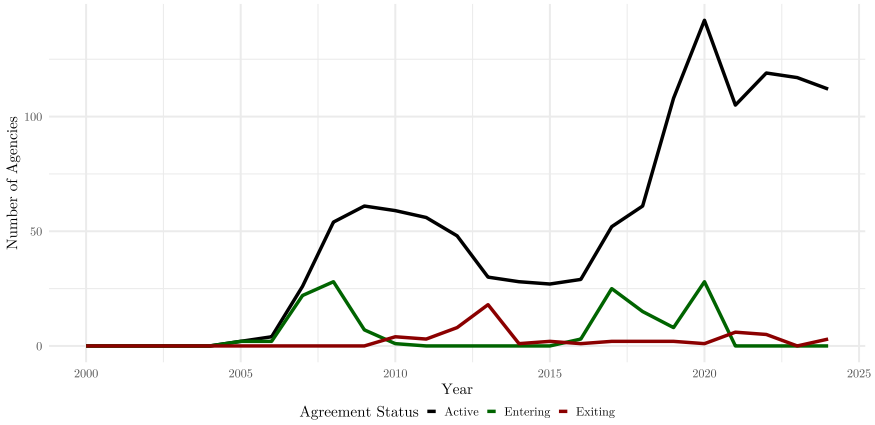


Figure 1: Number of Active, Entering, and Exiting Agencies by Year, 2000–2024

2. While we document the increase in agreements in 2025, since we do not have crime data for 2025, our analysis ends in 2024.

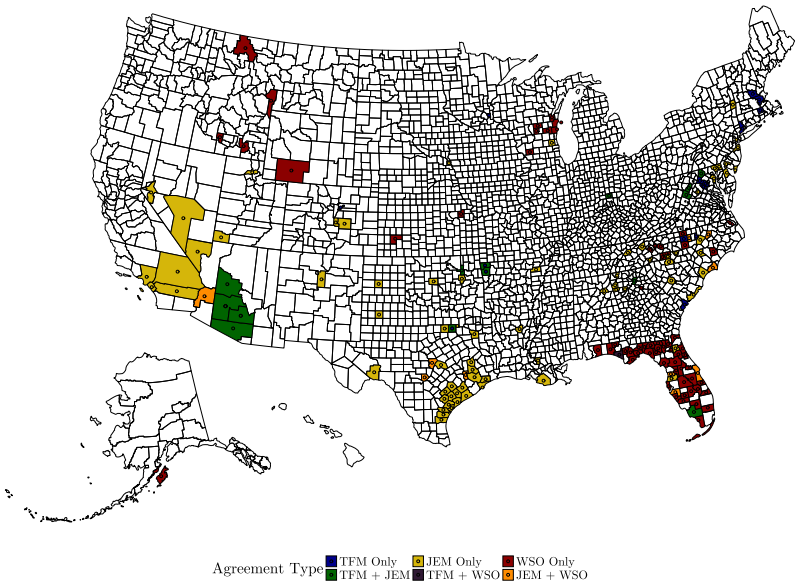


Figure 2: Counties with 287(g) Agreements, 2000–2024

ICE and immigration advocates hold sharply divergent views on program effectiveness. ICE claims 287(g) agreements keep communities "safe from potentially dangerous criminal aliens" (U.S. Immigration and Customs Enforcement 2025), while civil liberties organizations counter that agreements undermine both public safety and constitutional protections (American Civil Liberties Union 2026). Both sides lack rigorous causal evidence, relying on descriptive statistics and qualitative research. As these agreements now operate on an unprecedented scale, empirical evaluation is essential.

Prior research on 287(g) agreements finds little evidence of crime reduction, though studies face significant limitations in scope, temporal coverage, and methodology. The most comprehensive evaluation to date analyzes nationwide data from 2005–2010 using cross-lagged panel models, finding no evidence that 287(g) agreements reduce crime (Capellan and Sorg 2022). However, this analysis predates critical subsequent policy changes, including the 2012 TFM suspension.

Single-jurisdiction studies provide suggestive evidence of behavioral changes without establishing crime effects. Qualitative analysis of police narratives from Nashville documents that references to a suspect's 'foreignness' became more common in reports following 287(g) adoption, but this approach cannot assess crime outcomes (Donato and Rodríguez 2014). A study of North Carolina agencies finds no effects on crime or clearance rates, but documents increases in assaults against police officers following adoption (Forrester and Nowrasteh

2018). Broader qualitative research across multiple jurisdictions similarly finds that 287(g) enforcement is not consistently targeted toward serious offenders (Capps 2011).

II.B. Police Discretion and the Production of Arrests

An arrest is not a direct measure of crime. It reflects two separate things: whether an offense actually occurred, and whether an officer observed it and chose to act on it. This second component of officer discretion is pervasive in policing. Officers cannot and do not act on every violation they encounter. They instead decide which infractions merit a stop, a citation, or an arrest (Smith et al. 2005). This discretion is especially consequential for low-level, discretionary offenses, where officers have latitude over whether to intervene at all, in contrast to serious violent or property crimes, which are more often driven by victim reports rather than officer-initiated activity. As a result, changes in arrest rates can reflect changes in either underlying offending or in how officers exercise this discretion. Evaluations of public policy often overlook this distinction.

Our work relates to a broader literature on police discretion and its consequences for enforcement and public safety. Officers respond to incentives that have little to do with underlying criminal activity: they issue more frequent and larger traffic fines when the fiscal benefit of doing so is greater (Makowsky and Stratmann 2009), and they increase drug enforcement when permitted to retain a greater share of seized assets (Baicker and Jacobson 2007). This discretion carries real consequences for public safety: officers' allocation of harsher sanctions is often misaligned with which offenders are most responsive to them, reducing the deterrent value of enforcement (Goncalves and Mello 2026). At the same time, arrests and public safety can move independently of one another. Reductions in arrests, concentrated in lower-level offenses, are not associated with corresponding increases in crime (Cho et al. 2024). Together, this evidence suggests that changes in arrest activity often reflect enforcement incentives rather than changes in underlying offending.

This distinction is central to our approach. Because 287(g) authority is granted to officers rather than to the public, any behavioral response it induces should operate through this same channel of officer discretion, rather than through a change in the underlying rate of offending. We formalize this logic in a framework that derives testable predictions for how arrests and offenses should respond under two competing accounts of how this authority affects policing.

Existing research establishes that 287(g) agreements likely do not reduce crime and may change policing behavior, but prior studies face significant limitations. Most focus on single states or agencies or use data that ends before major developments in the program in the mid

and late 2010s. Methodological approaches are not robust to staggered treatment adoption and heterogeneous treatment effects. We address these limitations by providing the first nationwide causal evaluation of 287(g) agreements using comprehensive data from 2000–2024 and methods robust to staggered and non-absorbing treatment adoption.

II.C. Conceptual Framework

Economic models of crime largely rely on deterrence theory, modernized by Gary Becker (Becker 1968). In this model, criminal behavior results from rational, conscious decision-making, weighing the expected costs of crime against the opportunity costs of legal alternatives. These costs are typically broken into certainty (the likelihood of being caught) and severity (the punishment upon apprehension). As the expected cost of crime rises, offending should decline.

Beyond deterrence, 287(g) agreements may change how police allocate their attention and resources across offense types. Under a discretion-reallocation channel, immigration enforcement authority creates incentives for officers to focus on discretionary, easily observable offenses. Traffic stops, many drug crimes, and a variety of public order crimes involve substantial officer discretion and require minimal investigative work, compared with more serious violent or property crimes. This incentive operates through the mechanics of routine police-citizen contact: an officer cannot screen someone for immigration status without first establishing a lawful basis for contact, and traffic stops, drug stops, and public-order stops are the lowest-cost, most readily available means of doing so (Rushin and Edwards 2021). Under 287(g) authority, this gives officers an added incentive to initiate exactly this kind of high-discretion, low-investigative-cost stop, independent of any change in the underlying behavior being observed. If officers seek to maximize opportunities to screen for immigration status, we should observe increases in arrests for discretionary offenses even if underlying crime rates remain stable. This pattern is consistent with shifting enforcement priorities, rather than any change in reported offenses.

Alternatively, deterrence theory applied to 287(g) agreements predicts crime reductions through both incapacitation and general deterrence. Under incapacitation, the removal of undocumented immigrants with criminal histories through 287(g) mechanically reduces crime if those undocumented immigrants would have committed additional offenses, an effect that should be concentrated in violent and property crimes. Under general deterrence, if potential offenders perceive that 287(g) increases apprehension risk, either because more officers are screening for immigration status or because immigration-related consequences raise the costs of crime, offending should decline. This heightened enforcement risk may deter all offenders

in treated communities, not only those targeted by the program, if residents broadly perceive increased police scrutiny.

However, several factors may limit deterrence effectiveness. If most residents are unaware of the program's implementation, behavioral responses may be minimal. Moreover, many crimes are not carefully planned or economically rational, limiting responsiveness to perceived risk. Finally, incapacitation effects depend on how much crime removed individuals would otherwise have committed. There is evidence that immigrants, including undocumented immigrants, offend at lower rates than the native-born population (Abramitzky et al. 2024; Texas Department of Public Safety 2024), suggesting that incapacitation effects from removing undocumented individuals through 287(g) may be modest.

These accounts generate distinct and testable predictions. Under the discretion-reallocation channel, we expect increases in arrests for discretionary offenses without corresponding increases in reported offense rates, while arrests for serious violent and property crime remain stable. Under the deterrence channel, we expect declines in both arrests and offenses, with effects concentrated in violent and property crime to the extent incapacitation is the dominant mechanism. We test these predictions by examining both arrests and reported offenses, analyzing heterogeneity across crime types, and conducting spillover analysis to determine whether observed changes reflect changing crime rates or local police behavior.

III. Data

III.A. Treatment Variables

We construct a comprehensive agency-year panel of 287(g) agreements from 2000–2024 using two primary sources. Agreement data from 2000–2020 comes from the Urban Institute's State Immigration Policy Resource (Bernstein et al. 2017), which provides agency-level information on active agreements, agreement type, and start and end dates. We extend coverage through 2024 by scraping ICE's archived web pages using the Wayback Machine (U.S. Immigration and Customs Enforcement 2025), capturing snapshots at intervals throughout the year. We define an agency as treated if it held an active 287(g) agreement at any point during a given year. Importantly, treatment is non-absorbing, which motivates our use of estimation methods robust to treatment reversals.

Certain agencies are excluded from our analysis. State-wide agencies generally lack corresponding jurisdiction identifiers in the Uniform Crime Reporting (UCR) crime data and cannot be matched (e.g., the Arizona Department of Public Safety). Exceptions are agreements administered by a state's primary patrol or police agency (Colorado's and

Minnesota's Departments of Public Safety, and Delaware's and New Mexico's Departments of Corrections), which we associate with that agency's Originating Agency Identifier (ORI). However, county sheriff's offices are included as distinct agencies separate from municipal police departments within the same county, consistent with the reporting structure of the UCR.

III.B. Outcome Variables

Our primary outcomes are arrest rates and offense rates, constructed from the FBI's UCR program. We use Jacob Kaplan's concatenated versions of two UCR datasets: the Arrests by Age, Sex, and Race (ASR) dataset and the Offenses Known and Clearances by Arrest (Return A) dataset (Kaplan 2025b, 2025c).

Arrests measure police enforcement activity, specifically the number of individuals taken into custody. Offenses measure reported crime: incidents known to police either through victim reports or officer observation. This distinction lets us isolate changes in enforcement from changes in underlying crime.

Following established practices for UCR data (Donohue et al. 2024), we address well-documented reporting inconsistencies. We restrict our sample to agencies reporting at least nine months of data per year and at least 15 years of data in the 2000–2024 period. We calculate arrest and offense rates per 100,000 population using agency population estimates provided in the UCR data.

We match crime data to treatment data using agency names. The UCR data provides unique ORIs for each agency, but the agreement data uses agency names without standardized ORIs. Because agency names vary across datasets (e.g., 'City of Austin Police Department' vs. 'Austin Police' or 'Austin PD'), we manually correct common discrepancies to ensure consistent matching. All matches for treated agencies were manually verified for accuracy. This name-based matching allows us to assign ORIs from the UCR data to each agency in the agreement data, which we then use to map to county-level controls. Unmatched agencies in the agreement data are either present in the raw UCR data but excluded due to insufficient reporting or state-level agencies without UCR jurisdiction codes. Any agencies in the UCR data that were not matched are marked as untreated.

The UCR categorizes crimes into two groups based on severity and data availability. Part I crimes include serious offenses: murder, manslaughter, aggravated assault, rape, robbery, burglary, motor vehicle theft, larceny-theft, and arson.[3] For these crimes, the UCR collects

3. Larceny-theft (commonly called theft) is one of the most common crimes. While technically Part I, we exclude it from our primary analysis in Table 2, Figure 3, and Table 3. However, we include it as an individually reported category in Table 7 and Figure 7 for completeness.

both arrest and offense data. Part II crimes include all other offenses.[4] The UCR collects offense data for only a small subset of Part II categories (e.g., simple assault), too incomplete to construct a meaningful Part II offense aggregate, so our offense-based analysis is restricted to Part I crimes.

This data structure shapes our empirical approach. For Part I crimes, we can test whether 287(g) affects both arrests and offenses, distinguishing policing behavior from actual reported crime. For Part II crimes, we only observe arrests, but these are precisely the crimes for which we expect discretionary enforcement to matter most, making arrests a useful measure of police behavior even without offense data.

We organize our analysis around four crime categories that capture distinct enforcement contexts: violent crimes (murder, manslaughter, aggravated assault, simple assault, rape, robbery), property crimes (burglary, motor vehicle theft, arson, theft, vandalism, stolen property), drug crimes (marijuana possession, marijuana sales, opioid possession, opioid sales), and public order crimes (disorderly conduct, drunkenness, DUI, liquor laws, prostitution, vagrancy). These categories allow us to distinguish victim-initiated crimes (often the case for violent and property) from police-initiated crimes (such as drug or public order), and serious Part I offenses from more discretionary Part II offenses. Both categorizations allow us to test our theoretical predictions. The deterrence channel predicts 287(g) should reduce both arrests and offenses across all crime types, with effects largest for serious Part I crimes. The discretion-reallocation channel predicts arrests may increase for discretionary Part II crimes, such as drug and public-order offenses, even if offenses remain stable, while serious crime shows no effect or increases.

III.C. Control Variables

Agency-level demographic and economic data are unavailable, so we construct controls at the county level. We map agency ORIs to county FIPS codes using crosswalks from the Bureau of Justice Statistics, supplemented by Kaplan's UCR documentation (Bureau of Justice Statistics 2018; Kaplan 2025a).

4. These include simple assault, forgery, fraud, embezzlement, stolen property, vandalism, weapons crimes, prostitution, sex offenses other than rape, drug offenses, gambling, offenses against family and children, DUI, liquor laws, disorderly conduct, vagrancy, curfew and loitering, runaways, drunkenness, suspicion, and all other offenses (excluding traffic violations). We exclude from our main analysis offenses against family and children (which are often handled by specialized units), curfew, loitering, and runaway offenses (which apply only to juveniles), suspicion arrests (which represent arrests without formal charges), and all other offenses excluding traffic (an unspecified, heterogeneous residual category).

County-level controls include demographic characteristics (population shares aged 10–19, 20–29, and 30–39, proportion Black, proportion Hispanic, foreign-born population share) (IPUMS NHGIS 1990; U.S. Census Bureau 1999, 2012, 2020a, 2025a), economic conditions (real per capita personal income, poverty rate, unemployment rate, population density) (Bureau of Economic Analysis 2025; Bureau of Labor Statistics 2025a, 2025b; U.S. Census Bureau 2000, 2010, 2011, 2019, 2020b, 2024a, 2024b, 2025b), political environment (Republican presidential vote share) (MIT Election Data and Science Lab 2018), and whether the county has the Secure Communities program (Nunez et al. 2026). Unlike 287(g), Secure Communities operated nationwide, did not require voluntary agreements by local agencies, and granted no privileged authority to individual officers. Studies of the program generally find null or small crime effects (Miles and Cox 2014). We do not treat Secure Communities as a primary object of analysis, but use it as an alternative treatment in Section V.D.

III.D. Summary Statistics

Our final analysis includes 14,948 agencies observed over 25 years, yielding 338,800 agency-year observations.[5] Of these, 183 (1.2%) ever held 287(g) agreements during our sample period across 34 states, representing approximately 0.4% of all agency-year observations. The 132 treated agencies with available population data cover approximately 17.1 million people.

Table 1 presents summary statistics comparing treated and never-treated agencies, with treated agencies measured in the year prior to adoption and never-treated agencies measured as an average across the full panel.[6] Treated agencies are located in counties with significantly different demographic and political characteristics compared to never-treated agencies. Treated agencies serve counties with older populations (lower proportion aged 10–19: 12.81% vs. 13.43%), higher proportions of Hispanic residents (17.23% vs. 10.26%), substantially larger foreign-born populations (9,696 vs. 7,160 per 100,000), but lower growth rates in these demographics (3.10% vs. 4.75% and −0.71% vs. 2.96%, respectively). Treated agencies are also in counties with lower unemployment rates (4.46% vs. 5.83%) and higher Republican vote share (61% vs. 54%), with moderate evidence that treated agencies were more likely to adopt Secure Communities (52% vs. 44%). Notably, population proportions aged 20–29 and 30–39, proportion Black, population density, poverty rate, and per capita income do not

5. Our final sample includes 338,800 agency-year observations, but individual regression tables use fewer: observations must have non-missing population data to construct rates and non-missing county-level controls to enter a given specification, which together account for the reduction to approximately 211,000 observations in our arrest-rate tables (e.g., Table 2). The Part II share-of-arrests outcome drops a further 28 agency-years that report zero arrests of any kind that year.

6. For agencies in an unbalanced panel, we use the closest available year prior to treatment adoption. For one agency where no pre-treatment data exists, we use the first available year.

differ significantly between groups. These systematic differences underscore the non-random adoption of 287(g) agreements and motivate our inclusion of county-level controls and testing of parallel pre-trends in Section IV.

Table 1: Balance Table Between Treated and Untreated Agencies

	Never Treated		Treated		
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means
Population Proportion 10-19	13.43	1.44	12.81	1.96	-0.61**
Population Proportion 20-29	12.80	2.98	12.97	2.85	0.18
Population Proportion 30-39	12.52	1.58	12.68	2.08	0.16
Population Proportion Black	10.35	12.82	10.98	9.28	0.62
Hispanic Share	10.26	13.54	17.23	14.78	6.97**
% Chg. Hispanic Share	4.75	4.51	3.10	2.55	-1.66**
Foreign-Born Rate	7159.93	7884.58	9695.64	7263.45	2535.72**
% Chg. Foreign-Born Rate	2.96	8.12	-0.71	3.20	-3.67**
Population Density	232.49	662.10	210.68	463.20	-21.81
Poverty Rate	13.99	5.21	13.56	4.82	-0.43
Real Per Capita Personal Income	18054.06	5127.88	18347.58	5103.62	293.53
Unemployment Rate	5.83	1.55	4.46	1.30	-1.37**
Republican Vote Share	0.54	0.14	0.61	0.12	0.07**
Secure Communities	0.44	0.08	0.52	0.50	0.07+

Std. Dev. reported in the Std. Dev. column. Stars indicate significance of the difference in means between treated and never-treated agencies: ** $p<0.01$, * $p<0.05$, + $p<0.1$. Never-treated agencies: mean across all years. Treated agencies: year prior to adoption. Treated agencies: N=183. Never-treated agencies: N=14,765.

IV. Empirical Strategy

This study employs a difference-in-differences (DiD) framework to estimate the causal effect of 287(g) agreements on local policing outcomes. Our setting features staggered and non-absorbing treatment adoption across agencies and time, as agencies can enter and terminate these agreements at any time.

Our identification strategy relies on the key parallel trends assumption that, conditional on observed covariates, agencies that obtain 287(g) agreements would have followed similar trends in arrest and offense rates as non-adopting agencies in the absence of treatment.

This assumption is plausible given that timing of 287(g) adoption is driven by federal-local negotiations, state and local political pressures, and administrative capacity, rather than clear trends in crime or policing.

The systematic differences between treated and never-treated agencies documented in Section III highlight the non-random nature of 287(g) adoption. Agencies serving counties with larger Hispanic and foreign-born populations, stronger Republican political leanings, and lower unemployment are significantly more likely to adopt these agreements. While these level differences do not themselves violate the parallel trends assumption, they suggest possible confounders to both program adoption and police activity.

We address these threats to identification by directly controlling for the key observable characteristics that predict 287(g) adoptions: Hispanic population share, foreign-born population, Republican vote share, and unemployment rate. We also directly test the validity of parallel trends through our event study specification.

We first implement a Two-Way Fixed Effects (TWFE) model for our baseline analysis:

$$Y_{it} = \alpha_i + \delta_t + \beta \times \text{Agreement}_{it} + \boldsymbol{X}_{it}\gamma + \epsilon_{it} \tag{1}$$

where Y_{it} is the outcome rate (arrests or offenses per 100,000 population) for agency i in year t; α_i and δ_t are agency and year fixed effects, respectively. Agreement_{it} is an indicator for whether there was an active 287(g) agreement for agency i in year t, and $\boldsymbol{X}_{it}$ is a vector of our county-level controls. The coefficient β captures the average effect of having an active 287(g) agreement on the outcome. We weight regressions by agency population, and cluster standard errors at the state level. We implement this regression for both individual types of crime, and aggregated Part I and Part II crimes.

Recent econometric research has demonstrated that TWFE estimators can produce misleading results under treatment effect heterogeneity and staggered adoption (Goodman-Bacon 2021).[7] Additionally, our setting features non-absorbing treatment, as 32% of agencies that adopt 287(g) agreements subsequently terminate them during our sample period. The de Chaisemartin and D'Haultfœuille (2020) weight diagnostic in Table 2 demonstrates negligible negative weighting in our TWFE estimates, but this does not rule out bias from dynamic treatment-effect paths that our event studies reveal (Figure 3).

To address these concerns, we implement the estimator proposed by de Chaisemartin and D'Haultfœuille (2026, hereafter DCDH) as our primary specification, which yields our

7. The Goodman-Bacon (2021) decomposition assumes an absorbing treatment and is not defined once units can revert to untreated status, as ours do; we therefore do not report it.

event-study estimates.[8] Following the convention for this method, we normalize event time so that $t = 0$ denotes the last pre-treatment period and $t = 1$ denotes the year of adoption. Effects at $t = k$ for $k \geq 1$ therefore correspond to $k - 1$ years after adoption.

V. Results

V.A. Aggregated Effects

We begin by examining aggregate treatment effects for Part I and Part II arrests.[9] The TWFE results in Table 2 suggest no significant change in Part II arrests, alongside a decrease in Part I arrests that drives Part II's share of total arrests up. Our event-study estimates tell a different story.[10] Figure 3 (Table 3) presents event-study estimates from the DCDH estimator. For Part II arrests, pre-treatment placebo estimates are statistically insignificant and close to zero, supporting parallel trends; post-adoption effects rise progressively, from 211 per 100,000 (9%) at $t = 2$ to 236 per 100,000 (10%) at $t = 3$ relative to an untreated baseline mean of 2,279 per 100,000 (the full per-period estimates are reported alongside our robustness checks in Table 12), with an average cumulative effect across all post-treatment periods of 166.5 per 100,000 (7%).

Part I arrests show a different pattern: a violation of parallel trends in the pre-period estimates precludes causal claims about 287(g)'s effect on serious crime. We therefore find no credible evidence that 287(g) agreements reduced serious crime arrests, in contrast to the parallel-trends-consistent increase in Part II arrests.

One plausible source of this violation is reverse causation: agencies experiencing relatively more violent or property crime in the years before adoption may be more likely to seek a 287(g) agreement, for instance if local officials view federal immigration enforcement authority as one available response to a perceived crime problem. Consistent with this channel, our placebo estimates are largest, and only significant, furthest from adoption: 41.8 per 100,000 three periods prior (significant), 31.1 per 100,000 two periods prior (marginally significant), and 17.8 per 100,000 immediately prior (not significant). This channel is independent of the

8. Throughout, we characterize an estimate as statistically significant based on the individual dynamic effect (or average cumulative effect) point estimates and their associated confidence intervals, rather than the joint F-test of nullity across all effects. The joint test can reject even when no individual period is itself significant, particularly when effects are noisy or exhibit offsetting signs across periods; we view the per-period estimates as the more interpretable basis for assessing whether a specific finding reflects a genuine effect.

9. See Appendix B for more detailed TWFE estimates, and Appendix C for additional DCDH event study estimations.

10. The share and difference DCDH specifications are in Appendix C.

demographic and political confounders discussed in Section III, and we cannot rule it out directly, which is precisely why we do not draw causal conclusions from the Part I results. Notably, this concern does not extend to our Part II findings, where the corresponding pre-treatment estimates are statistically insignificant and close to zero.

Table 2: The Impact of 287(g) Agreements on Arrests, TWFE

	Part II Crimes Arrest Rate	Part I Crimes Arrest Rate	Part II Crimes Arrests as Share of Total Arrests	Difference in Part II and Part I Arrest Rates
287(g)	72.882	−19.974+	0.016**	92.856+
	(52.738)	(10.409)	(0.003)	(52.206)
N	210 999	210 999	210 971	210 999
Adj. R^2	0.72	0.76	0.67	0.70
Negative Weights	0.02%	0.02%	0.02%	0.02%
Min. SD (Sign Reversal)	203.8	55.9	0.0	259.7

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

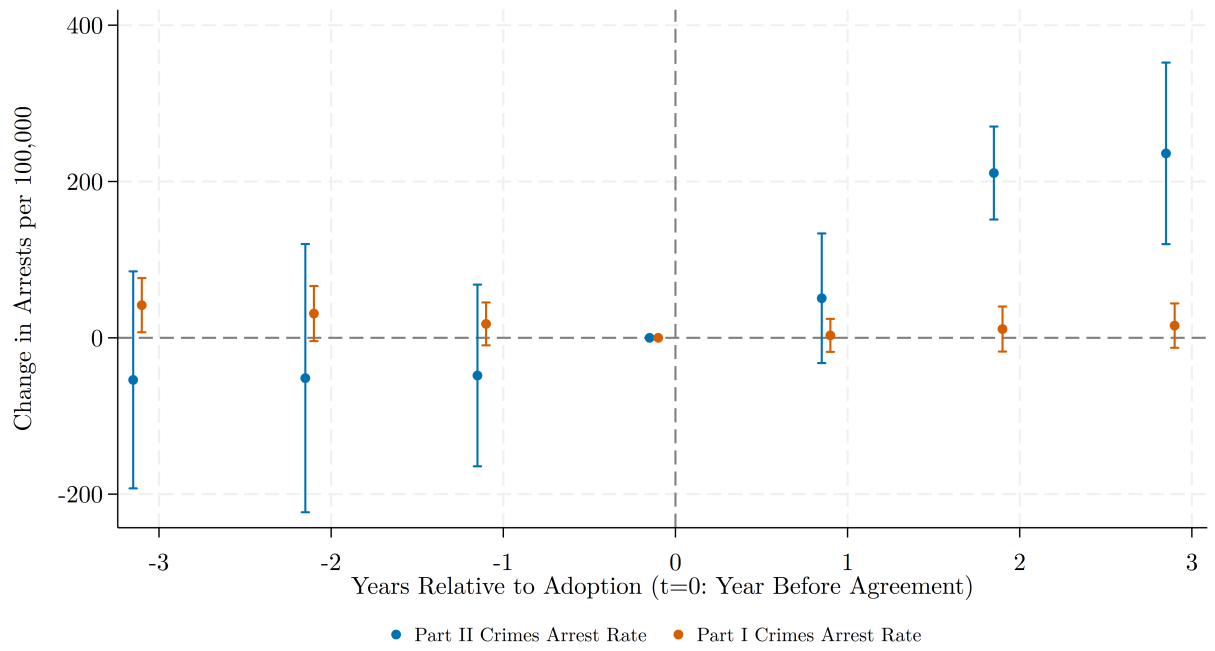


Figure 3: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Rate, DCDH

Table 3: The Impact of 287(g) Agreements on Arrest Rates, DCDH

	Part II Crimes Arrest Rate	Part I Crimes Arrest Rate
287(g)	166.509**	10.047
	(33.765)	(12.552)

** p<0.01, * p<0.05, + p<0.1.

V.B. Drivers of Effects

To understand the mechanisms behind the aggregate Part II increase, we examine specific offense categories.[11] Figure 4 (Table 4) shows results for drug-related arrests. Following adoption, drug arrests rise sharply by $t = 2$ and remain elevated through $t = 3$, with an average cumulative effect of 53 per 100,000 (11%). This increase is driven primarily by marijuana possession arrests (up 21 per 100,000, or 9%), with opioid possession arrests contributing a moderate, marginally significant increase as well (up 18 per 100,000, or 25%). This pattern is consistent with the discretion-reallocation channel: drug possession is most

11. Throughout this subsection, cited effect magnitudes refer to the average cumulative treatment effect (ATT) unless otherwise noted; the timing and persistence of effects discussed in the text are drawn directly from the event-study figures.

commonly detected during routine, officer-initiated encounters such as traffic stops, exactly the kind of low-level, discretionary activity 287(g) authority would be expected to intensify.

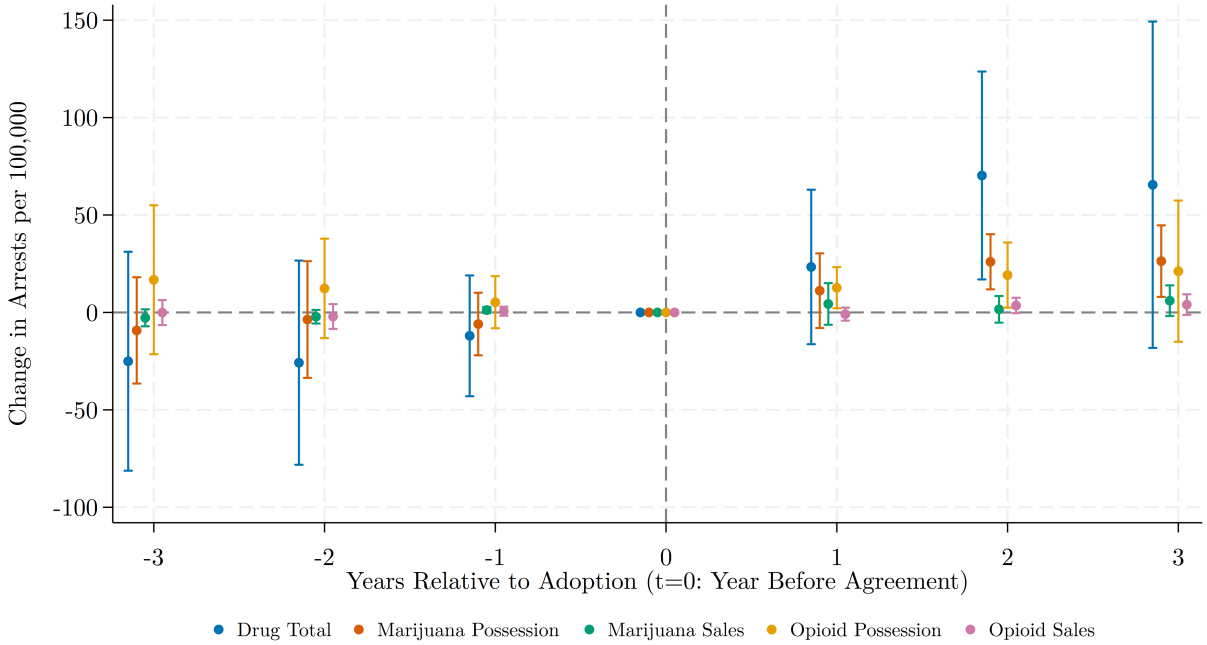


Figure 4: The Impact of 287(g) Agreements on Drug Arrests, DCDH

Table 4: The Impact of 287(g) Agreements on Drug Arrests, DCDH

	Drug Total	Marijuana Possession	Marijuana Sales	Opiate/Cocaine Possession	Opiate/Cocaine Sales
287(g)	53.280*	21.255**	4.013	17.758+	2.246
	(26.805)	(7.283)	(3.934)	(10.176)	(1.703)

** p<0.01, * p<0.05, + p<0.1.

Similarly, Figure 5 (Table 5) displays the results for different types of public order crimes. Several of these subcategories show statistically significant pre-treatment placebo effects. For DUI and liquor laws, these placebo estimates run in the opposite direction from the post-treatment increase, suggesting a change at adoption rather than a continuing pre-existing trend; drunkenness's placebo estimates run in the same direction as its post-treatment increase, warranting more caution in interpreting that specific result causally. Drunkenness, DUI, and liquor law arrests show the sharpest and most sustained increases, continuing to rise through $t = 3$ (drunkenness: up 19 per 100,000, or 11%; DUI: up 51 per 100,000, or 10%; liquor laws: up 14 per 100,000, or 8%). These alcohol-related offenses are typically detected through

officer-initiated encounters and involve substantial enforcement discretion, consistent with the same discretion-reallocation mechanism driving the drug arrest results above, though the pre-trend pattern noted above warrants additional caution in interpreting the drunkenness result specifically.

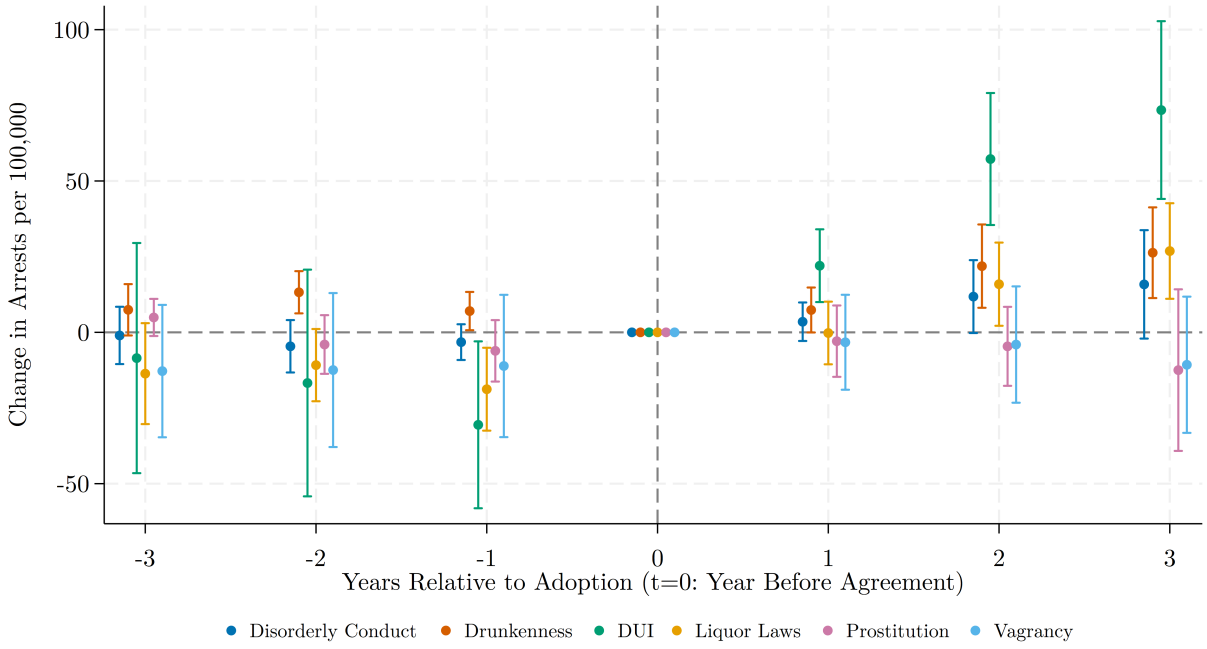


Figure 5: The Impact of 287(g) Agreements on Public Order Arrests, DCDH

Table 5: The Impact of 287(g) Agreements on Public Order Arrests, DCDH

	Disorderly Conduct	Drunkenness	DUI	Liquor Laws	Prostitution	Vagrancy
287(g)	10.426+	18.593**	51.118**	14.240**	−6.709	−6.044
	(5.882)	(5.376)	(9.873)	(4.534)	(8.745)	(9.733)

** p<0.01, * p<0.05, + p<0.1.

We compare the effects above with disaggregated categories of violent and property crime. Figure 6 (Table 6) and Figure 7 (Table 7) display these results for violent and property crime, respectively. Total assault arrests show a significant average cumulative increase (up 33 per 100,000, or 7%). Disaggregating by charge type, this increase appears concentrated in simple assault, which reaches significance at $t = 2$, while aggravated assault does not reach significance at any individual period, consistent with the greater discretion officers hold over simple, as opposed to aggravated, assault charges. Their average cumulative effects,

however, run the opposite way: aggravated assault's is marginally significant despite no single significant period, while simple assault's falls short of significance despite its significant $t = 2$ estimate, underscoring how imprecisely estimated each component is once split from the total. Murder arrests show no discernible change following adoption. Rape arrests increase significantly (up 0.6 per 100,000, or 9%), while robbery shows only a marginally significant increase (up 3.0 per 100,000, or 16%). However, offense rates for both crimes show no corresponding change (see Section V.C.2), and the aggregate Part I arrest effect across all violent and property crimes remains statistically insignificant, suggesting these specific increases reflect greater clearance of already-reported cases rather than either a change in underlying crime or a systematic pattern across serious violent offenses generally.

Within property crime, we find significant average cumulative effects on theft, vandalism, and stolen property (buying, receiving, or possessing) arrests. As with drug and public order offenses, these results are consistent with discretionary enforcement of low-level property crimes. Arson arrests also increase, though only marginally significantly at the average cumulative level, and offense data are unavailable for this crime, so we treat this result as more tentative than the other property crime findings. A county spillover analysis (see Section V.C.1) restricted to arson shows no significant increase among neighboring agencies, providing no evidence that this result reflects a broader increase in arson activity at the county level.

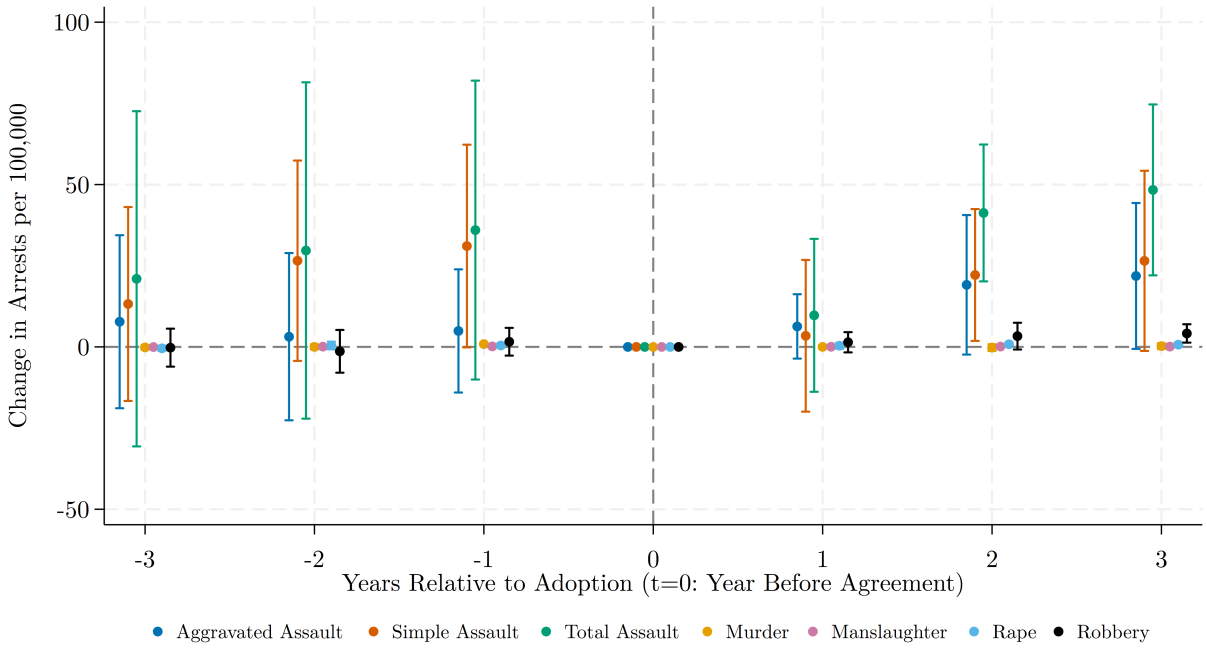


Figure 6: The Impact of 287(g) Agreements on Violent Arrests, DCDH

Table 6: The Impact of 287(g) Agreements on Violent Arrests, DCDH

	Aggravated Assault	Simple Assault	Total Assault	Murder	Manslaughter	Rape	Robbery
287(g)	15.825+	17.425	33.250**	0.017	0.089	0.634**	2.976+
	(8.956)	(11.141)	(10.059)	(0.349)	(0.158)	(0.225)	(1.578)

** p<0.01, * p<0.05, + p<0.1.

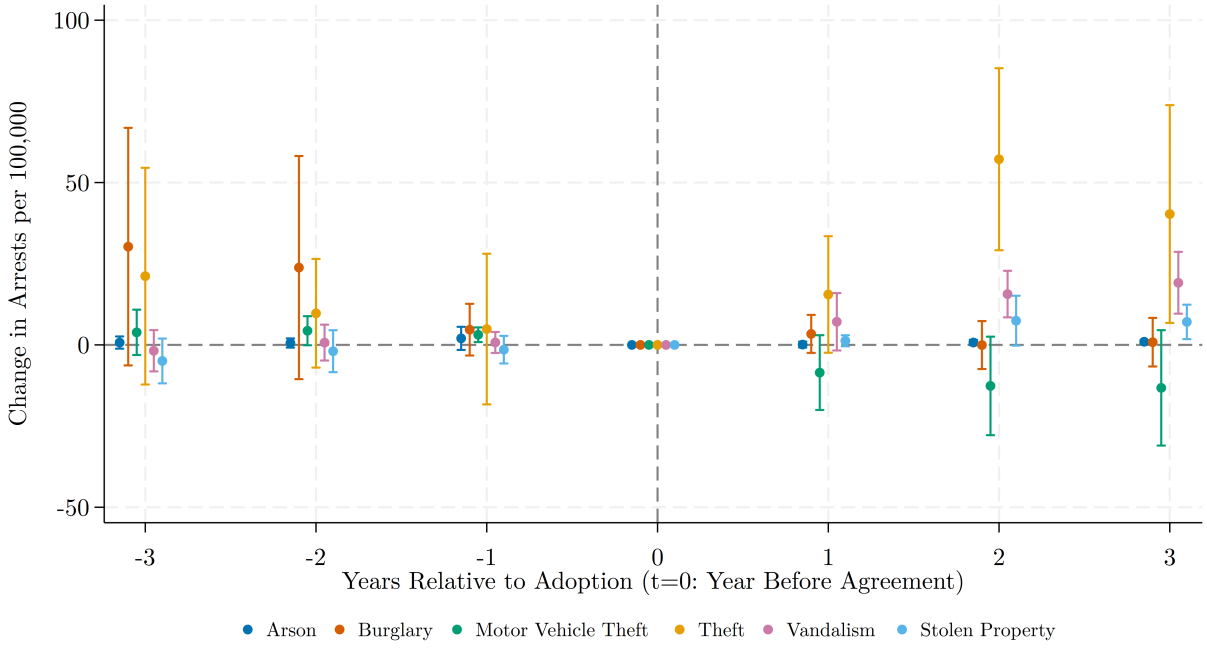


Figure 7: The Impact of 287(g) Agreements on Property Arrests, DCDH

Table 7: The Impact of 287(g) Agreements on Property Arrests, DCDH

	Arson	Burglary	Motor Vehicle Theft	Theft	Vandalism	Stolen Property
287(g)	0.614+	1.407	−11.515	37.825**	14.036**	5.291*
	(0.358)	(2.934)	(7.557)	(12.625)	(3.681)	(2.203)

** p<0.01, * p<0.05, + p<0.1.

Together, these disaggregated results point to a consistent pattern: the largest and most robust increases are concentrated in discretionary, officer-initiated offenses: drug possession, alcohol-related offenses, and low-level property crimes, while murder shows no discernible change. A smaller number of serious-crime categories show arrest increases, rape significantly and aggravated assault, robbery, and arson only marginally so, but these are isolated results

rather than a systematic pattern, are not accompanied by increases in offense rates where such data are available, and do not extend to the aggregate Part I measure. We turn next to directly testing whether these arrest patterns reflect genuine changes in enforcement, rather than underlying crime, using county spillover and offense rate comparisons.

V.C. Distinguishing Enforcement Changes from Crime Trends

It is critical to determine whether our results reflect changes in police behavior or changes in underlying crime. If 287(g) adoption were instead associated with an increase in low-level criminal activity itself, rising arrests would not indicate any intensification of enforcement.

V.C.1. County Spillover Analysis

Since Part II offense rates are not reported to the UCR, we cannot directly compare arrest and offense rates for these crimes. Instead, we take advantage of the fact that criminal activity typically operates at a broader geographic scale than an individual police jurisdiction, conducting a county spillover analysis. If our main results reflected genuine changes in underlying crime rather than changes in enforcement, untreated agencies in the same county as a 287(g) agency should experience similar arrest trends to treated agencies, since they police the same broader area affected by any change in crime. We implement this by designating all agencies in a treated county as "spillover treated," excluding the actual 287(g) agency itself.[12] Figure 8 (Table 8) displays the main category results. We find no statistically significant change in arrests among these spillover agencies, in contrast to our main results. A formal test shows the agency effect exceeds the spillover effect in 185 of 200 bootstrap resamples (Table 9).[13] These findings provide evidence that our main results reflect changes in enforcement by 287(g) agencies specifically, rather than underlying crime trends.

12. See Appendix C for additional DCDH specifications.

13. The two estimates come from separate regressions, so their standard errors cannot be combined directly. We instead recover the missing covariance from a cluster bootstrap that resamples states with replacement, treating each draw of a state as a distinct unit. The difference is positive in 185 of 200 resamples, though a nonparametric percentile interval does not exclude zero, reflecting a minority of resamples in which the thin spillover treatment group creates outlier estimates.

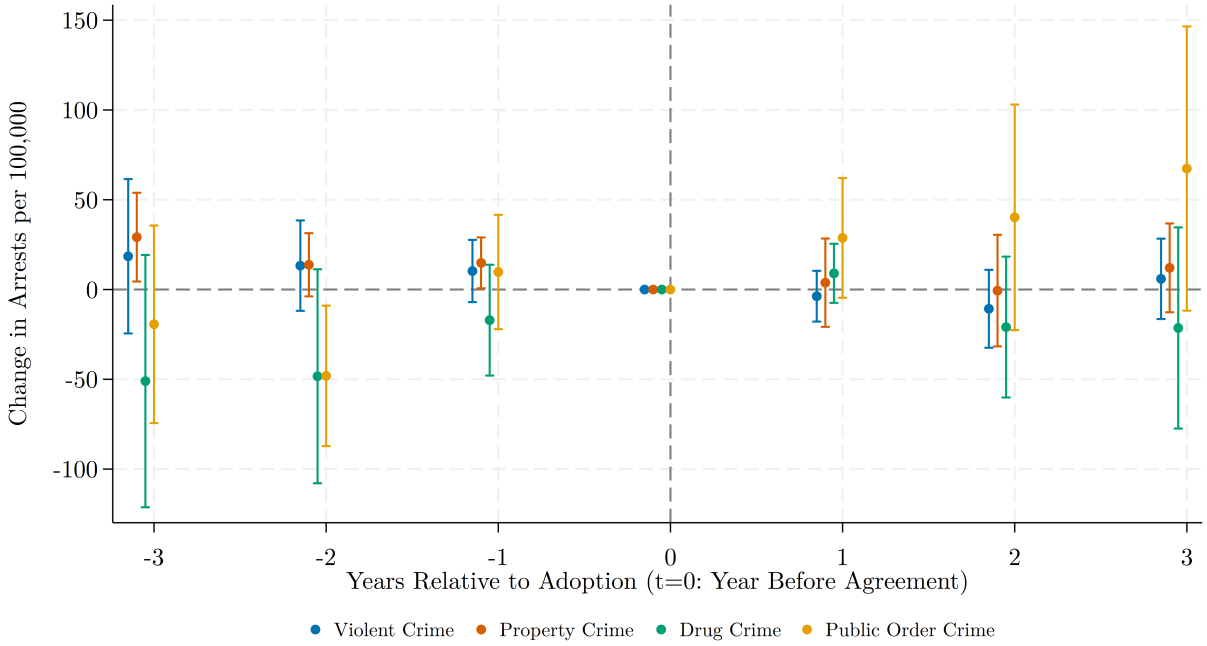


Figure 8: The Impact of 287(g) Agreements on Aggregated Arrests, DCDH (County Spillover)

Table 8: The Impact of 287(g) Agreements on Aggregated Arrests, DCDH (County Spillover)

	Violent Crime	Property Crime	Drug Crime	Public Order Crime
287(g)	−2.902	5.093	−10.939	45.583
	(9.583)	(12.999)	(17.039)	(28.119)

** p<0.01, * p<0.05, + p<0.1.

Table 9: The Impact of 287(g) Agreements on Part II Crimes Arrest Rate: Agency vs. County Spillover, DCDH

	Agency	County Spillover	Difference
287(g)	166.509**	57.915	108.593*
	(33.765)	(38.623)	(53.266)
95% CI	[100.3, 232.7]	[−17.8, 133.6]	[4.2, 213.0]
p-value	<0.001	0.134	0.041
Alternative inference			
Bootstrap percentile CI			[−147.3, 153.4]
Bootstrap SD			122.9 (p = 0.377)

** p<0.01, * p<0.05, + p<0.1.
The Agency and County Spillover columns report de Chaisemartin and D'Haultfœuille (2026)'s asymptotic standard errors, clustered by state. Because the two ATTs are estimated in separate regressions, their difference has no analytic standard error. The Difference column therefore combines the two reported standard errors using the Pearson correlation between the two estimates measured across the cluster bootstrap ($\rho = -0.079$, 200 replicates resampled by state). The bootstrapped difference has median 103.4, interquartile range [82.0, 123.8], and is positive in 92.5% of resamples. Its standard deviation (122.9) is inflated by a small number of resamples in which the spillover design becomes degenerate: the most extreme returns a spillover ATT of 1,454.6 against a point estimate of 57.9. The percentile interval and the standard-deviation result are both reported.

V.C.2. Offense Rates

To further test whether 287(g) adoption affects underlying crime, we re-estimate our main specification using offense rates as the outcome for the Part I crimes for which these data are available. Unlike arrest rates, offense rates capture crime reported to police, independent of any subsequent enforcement decision. Figure 9 (Table 10) and Figure 10 (Table 11) display these results for violent and property crime, respectively. We find no evidence that 287(g) adoption affects offense rates for murder, rape, robbery, burglary, or theft, with two exceptions: a modest increase in assault offenses, driven by simple assault, and a marginally significant decrease in motor vehicle theft offenses. The assault increase could reflect a change in officers' reporting behavior rather than a change in underlying assault: incidents that might previously have been resolved informally, without generating any formal record, may now be logged as simple assault offenses, consistent with 287(g) authority increasing officers' incentive to document minor encounters. The motor vehicle theft offense decline is likewise imprecisely estimated and, unlike the assault result, is not mirrored by any corresponding change in motor vehicle theft arrests (Table 7), so we do not interpret it as evidence of a systematic effect. Notably, the increase we observe in theft arrests is not accompanied by any

corresponding change in theft offenses, indicating that this arrest increase reflects greater enforcement activity rather than a rise in theft itself.

Together, the county spillover analysis and offense rate results support the same interpretation: 287(g) agreements change how police allocate enforcement resources toward discretionary, low-level arrests, rather than responding to any underlying change in crime.

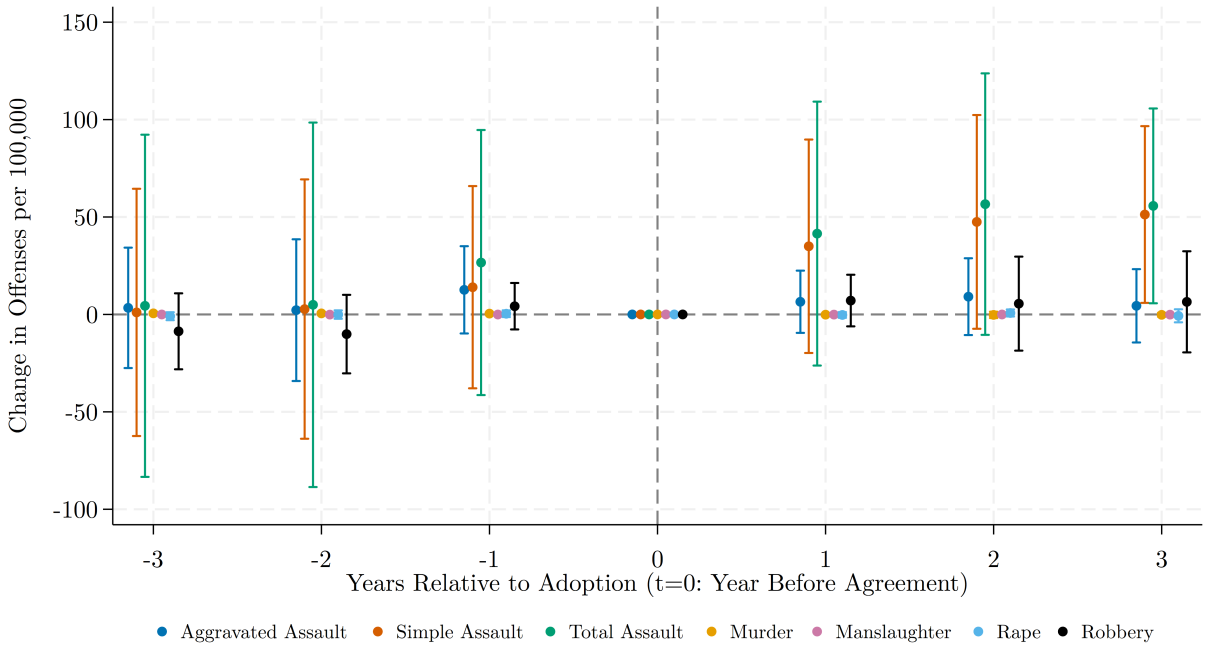


Figure 9: The Impact of 287(g) Agreements on Violent Offenses, DCDH

Table 10: The Impact of 287(g) Agreements on Index Violent Offenses, DCDH

	Aggravated Assault	Simple Assault	Total Assault	Murder	Manslaughter	Rape	Robbery
287(g)	6.757	45.272+	52.033+	−0.178	−0.067	−0.088	6.494
	(8.188)	(24.080)	(28.829)	(0.568)	(0.071)	(0.857)	(10.688)

** p<0.01, * p<0.05, + p<0.1.

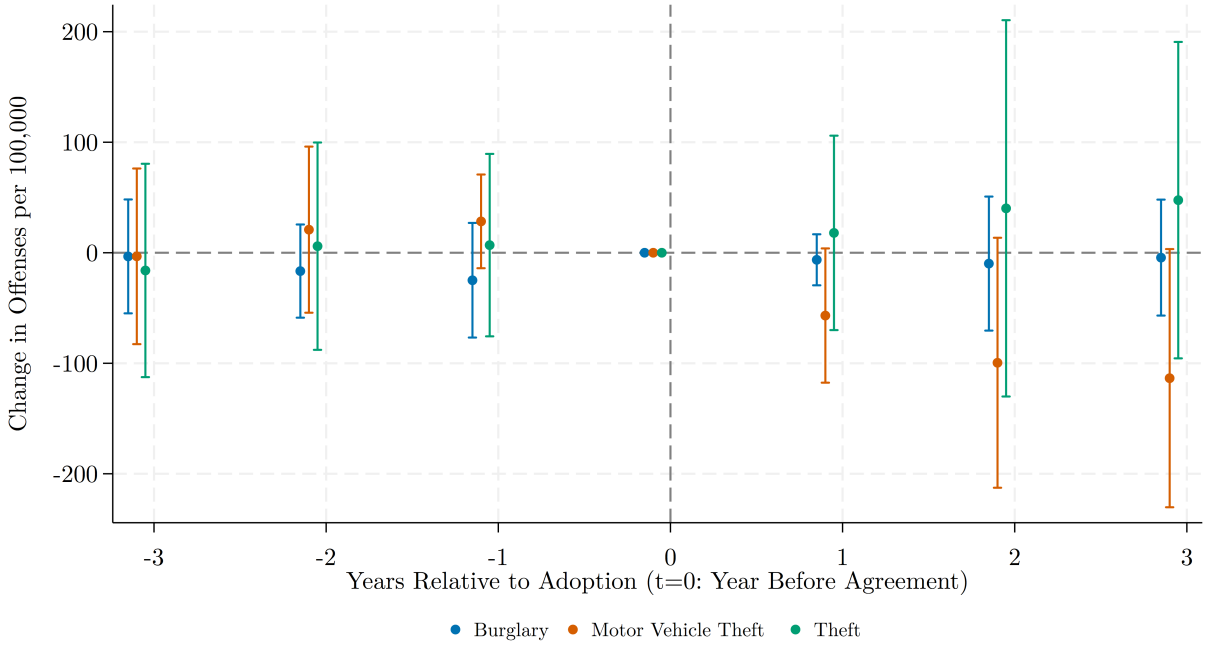


Figure 10: The Impact of 287(g) Agreements on Property Offenses, DCDH

Table 11: The Impact of 287(g) Agreements on Index Property Offenses, DCDH

	Burglary	Motor Vehicle Theft	Theft
287(g)	−6.930	−91.274+	35.745
	(22.152)	(49.882)	(66.399)

** p<0.01, * p<0.05, + p<0.1.

V.D. Robustness

To validate our main findings, we conduct a series of robustness checks. Because 287(g) adoption is non-absorbing and staggered, we present results using alternative estimators appropriate for this setting: Local Projections Difference-in-Differences (Dube et al. 2023), PanelMatch (Imai et al. 2023), and the Interactive Fixed Effects Counterfactual estimator (IFEct) (Liu et al. 2024). Table 12 presents these estimates.

LP-DiD reproduces a significant, growing increase in discretionary arrest rates by $t = 2$ and $t = 3$ (197.6 and 234.8, respectively), consistent with our main DCDH estimates, with insignificant pre-treatment coefficients. PanelMatch and IFEct do not replicate this positive effect; however, PanelMatch's own placebo test reveals a significant pre-existing trend, limiting our confidence in treating its null result as a genuine contradiction, and IFEct's estimates are imprecise at every horizon. For Part I crimes, all three estimators agree with our main finding

of no significant post-treatment effect. LP-DiD's pre-treatment coefficients are significant at $t = -3$ and $t = -2$, however, echoing the pre-trend violation already flagged in our main specification's placebo test. PanelMatch alone detects a marginally significant decline in Part I arrests at $t = 1$, but this does not persist. Overall, the evidence across specifications is most consistent with a genuine, discretionary increase in low-level (Part II) arrest activity following adoption, with no corresponding public safety benefit.

Table 12: Comparison of Treatment Effect Estimates: DCDH, LP-DiD, PanelMatch, and IFEct

Outcome	Period	DCDH	LP-DiD	PanelMatch	IFEct
Part II Crimes Arrest Rate	-3	-53.8 (70.8)	-30.2	182.4* (81.2)	-170.2 (131.6)
	-2	-51.6 (87.5)	-27.9	149.9+ (83.6)	-171.7 (125.7)
	-1	-48.2 (59.3)	-33.0	127.6 (78.0)	-133.2 (132.0)
	0		0.0		-78.3 (86.5)
	1	50.6 (42.3)	44.7	-75.3 (73.1)	-156.6 (123.7)
	2	210.8** (30.4)	197.6**	64.7 (98.5)	-45.5 (150.7)
	3	236.0** (59.3)	234.8**	125.3 (91.2)	-6.0 (105.3)
Part I Crimes Arrest Rate	-3	41.8* (17.7)	41.8+	13.9 (14.4)	-10.1 (10.9)
	-2	31.1+ (18.0)	33.5+	12.1 (11.3)	-9.7 (8.9)
	-1	17.8 (14.0)	19.0	-10.1 (19.8)	11.6 (15.8)
	0		0.0		-4.7 (9.8)
	1	3.1 (10.8)	2.4	-18.3+ (10.9)	-21.3 (14.5)
	2	11.2 (14.7)	10.4	-5.7 (13.3)	-12.9 (15.4)
	3	15.6 (14.5)	17.4	-0.6 (15.0)	-3.3 (17.4)

Standard errors in parentheses. ** p<0.01, * p<0.05, + p<0.1. "t=0" refers to the year before treatment. LP-DiD uses Dube et al. (2023) local projections for non-absorbing treatments. PanelMatch uses propensity score matching with 3 lags. IFEct uses interactive fixed effects with cross-validated factors.

Our main outcomes are arrest rates, which divide arrest counts by agency population. This creates a specific threat to our interpretation: since population is the denominator, any decline in population following 287(g) adoption, for instance if residents leave the jurisdiction out of fear of immigration enforcement, would mechanically raise the arrest rate even if the actual number of arrests, and thus true police behavior, had not changed at all. Our main results could therefore reflect a shrinking denominator rather than a genuine increase in enforcement. We test this directly by decomposing the rate into its two components: re-estimating our main specification using raw arrest counts (rather than rates), and separately examining log population itself as an outcome, in both cases without population weights. If our results were driven by this mechanical channel, we would expect to see population declines in treated agencies, alongside arrest count increases too small to explain the rate increases we observe. These results are presented in Figure 11 (Table 13) and Figure 12 (Table 14), respectively.

Instead, we find the opposite pattern from what the mechanical story predicts: arrest counts increase for Part II crimes (87.1, though not statistically significant), with null effects for Part I crimes, consistent with our main rate-based findings, while population itself shows a negative but statistically insignificant effect (−0.001, s.e. 0.005), an aggregate that combines a marginally significant increase at $t = 1$ with null-to-negative effects at $t = 2$ and $t = 3$ (Figure 12), rather than the decline the mechanical channel would require. This rules out the mechanical channel: a shrinking population cannot explain an increase we observe using raw counts, where population plays no role at all, and we find no evidence that population actually declined.

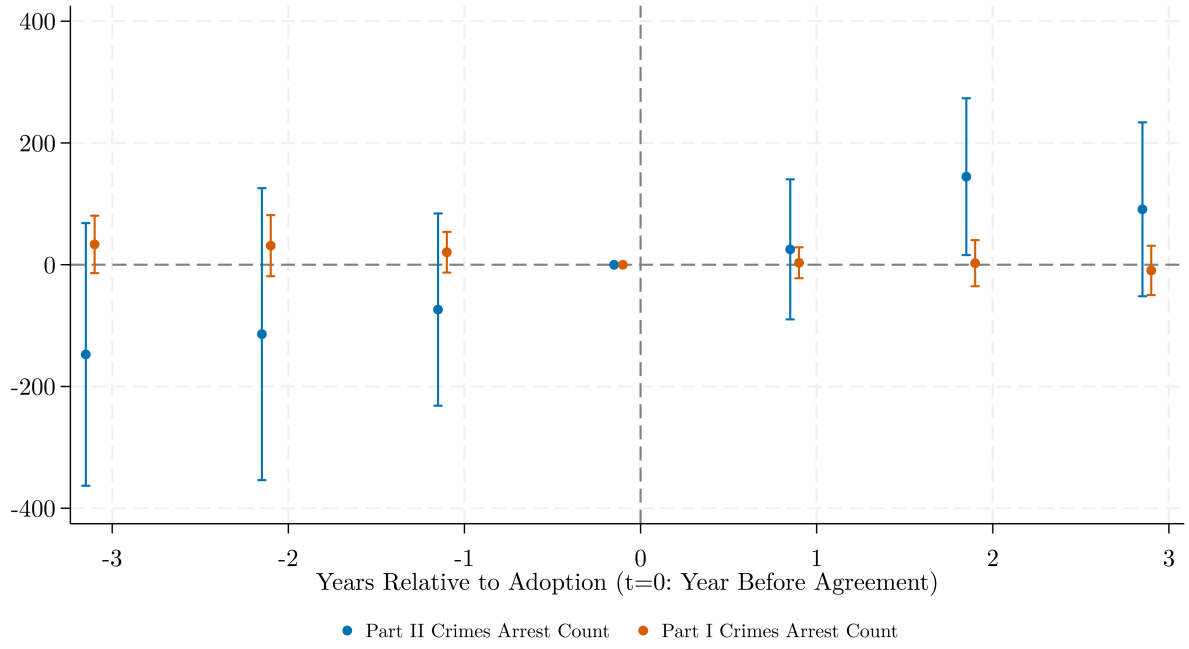


Figure 11: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Count, DCDH

Table 13: The Impact of 287(g) Agreements on Arrest Counts, DCDH

	Part II Crimes Arrest Count	Part I Crimes Arrest Count
287(g)	87.118	−1.153
	(57.076)	(17.201)

** p<0.01, * p<0.05, + p<0.1.

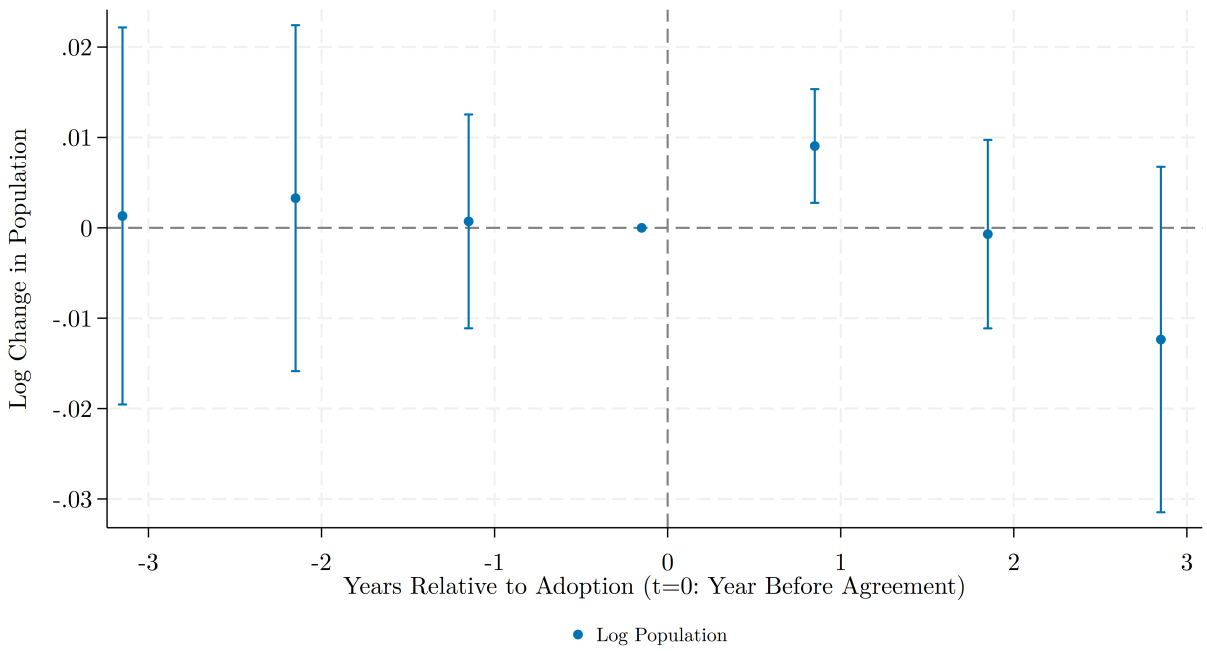


Figure 12: The Impact of 287(g) Agreements on Log Population, DCDH

Table 14: The Impact of 287(g) Agreements on Log Population, DCDH

	Log Population
287(g)	−0.001
	(0.005)

** p<0.01, * p<0.05, + p<0.1.

We examine whether our results are robust to expanding the set of crimes analyzed.[14] Figure 13 (Table 15) shows that Part II arrest rates increase significantly, consistent with our main results. We also observe an increase in Part I arrest rates, driven primarily by theft arrests, the one added offense with substantial officer discretion; excluding theft, our main Part I result of no significant effect is unaffected.

14. This translates to including offenses against family and children, curfew, loitering, runaways, and suspicion in Part II crimes, and theft in Part I crimes.

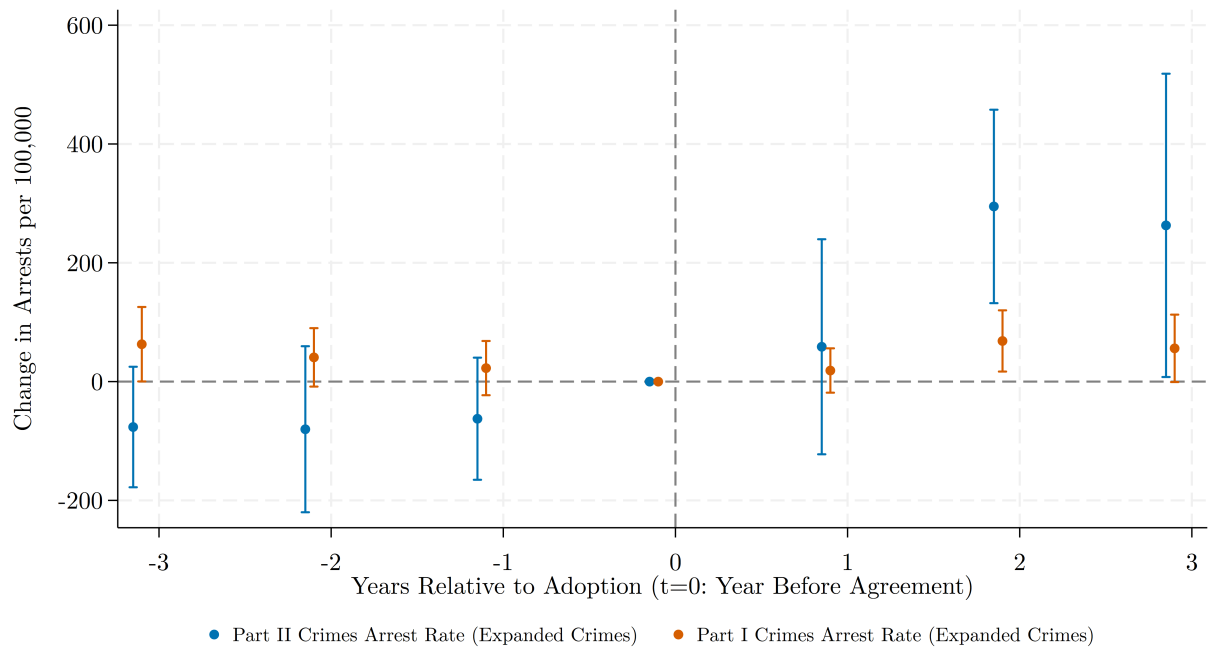


Figure 13: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Rate (Expanded Crimes), DCDH

Table 15: The Impact of 287(g) Agreements on Expanded Crime Definitions, DCDH

	Part II Crimes Arrest Rate (Expanded Crimes)	Part I Crimes Arrest Rate (Expanded Crimes)
287(g)	206.316*	47.871*
	(92.299)	(23.922)

** p<0.01, * p<0.05, + p<0.1.

Finally, we test whether these results are specific to 287(g)'s particular institutional design, which directly grants immigration enforcement authority to individual officers, or whether they reflect a more general response to local-federal immigration cooperation. We re-estimate our main specification using Secure Communities adoption as the treatment variable, controlling for 287(g) status, with results shown in Figure 14 (Table 16). Unlike 287(g), Secure Communities produces no significant effects on the Part II arrest rate at any horizon, and only a slight, isolated decrease in the Part I arrest rate at $t = 3$. This asymmetry is consistent with our discretion-reallocation channel specifically, rather than immigration enforcement cooperation broadly: Secure Communities operates centrally, through fingerprint sharing between jails and ICE, without granting individual officers any new legal authority. Its null effect on discretionary arrests therefore reinforces that 287(g)'s effects operate through

the mechanism we identify, the direct grant of enforcement discretion to local officers, rather than through immigration enforcement cooperation of any kind.

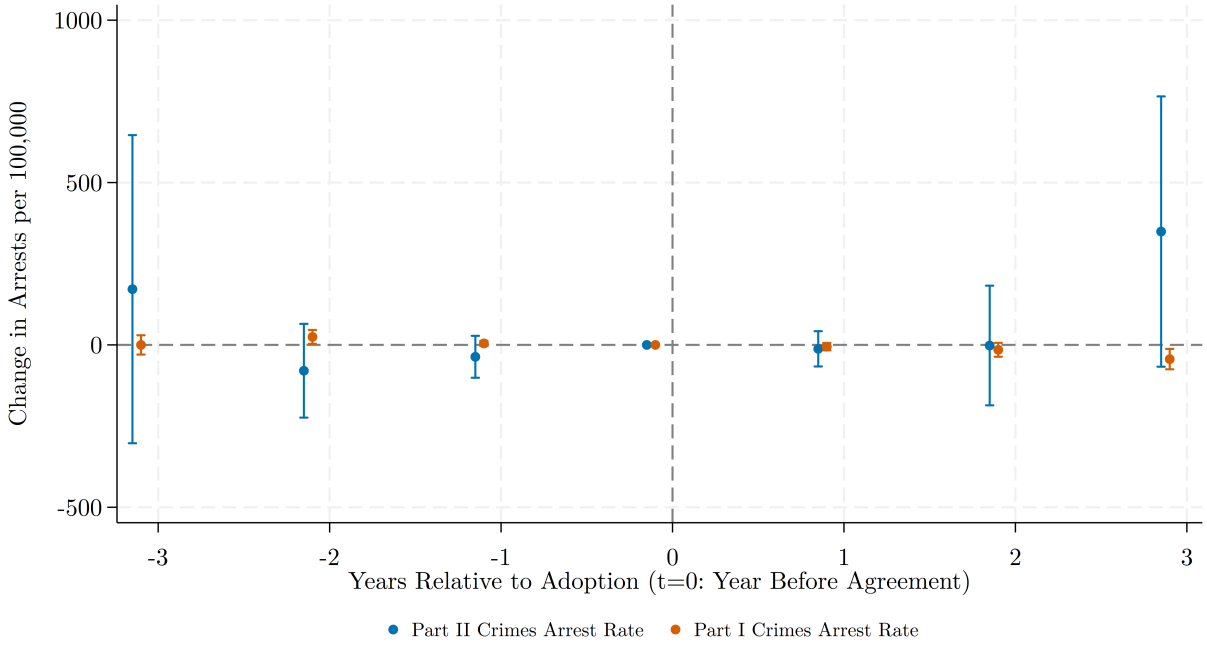


Figure 14: The Impact of Secure Communities on Part I and Part II Crimes Arrest Rate, DCDH

Table 16: The Impact of Secure Communities on Arrest Rates, DCDH

	Part II Crimes Arrest Rate	Part I Crimes Arrest Rate
Secure Communities	43.516	−14.379+
	(72.292)	(8.315)

** p<0.01, * p<0.05, + p<0.1.

VI. Discussion

The evidence presented here indicates that 287(g) agreements do not make communities safer. They do, however, change how police allocate their attention: following adoption, Part II arrests increase by approximately 7%, driven primarily by drug and public order offenses, while serious violent and property crime remains unchanged. By examining both arrests and offenses for Part I crimes and conducting a spillover analysis, we isolate changes in police

behavior from changes in underlying crime, and find no evidence that 287(g) agreements deter crime.

These results support the discretion-reallocation channel over a traditional deterrence account. Even though the expected costs of crime increased under 287(g), we find no evidence of reductions in serious crime. Instead, we observe increases in arrests for discretionary offenses without corresponding changes in underlying offense rates. The effects emerge and persist beginning at $t = 2$, suggesting institutional adaptation rather than immediate policy response. This delayed pattern is consistent with 287(g) agreements gradually reshaping priorities as officers learn to integrate immigration screening into routine police activity.

Our findings indicate that 287(g) agreements are not achieving their stated goal of reducing violent and dangerous crime. Instead, we find evidence of police targeting low-level offenders, and potentially escalating minor incidents into formal arrests. This pattern is starkest for the most discretionary arrests, consistent with 287(g) authority incentivizing officers to pursue low-level stops that may yield immigration violations. More serious violent and property crimes, by contrast, show no evidence of change in either arrests or offenses, indicating that 287(g) does not improve the apprehension of dangerous offenders.

Our identification strategy addresses two main threats to interpreting these results causally: non-random selection into 287(g) adoption, and the possibility that our results reflect changing crime rather than changing enforcement. We address the first through our county-level controls and event-study design. We address the second through our county spillover analysis, which finds no corresponding change in arrests among untreated agencies sharing a county with a 287(g) agency. We confirm this by looking at available offense rates, finding no corresponding increase in reported offense rates. One might alternatively hypothesize that victims increase their reporting to police following 287(g) adoption, rather than police increasing enforcement; however, this explanation is difficult to reconcile with our findings, since the largest increases occur in exactly the offenses where officer discretion, not victim reporting, determines whether an arrest occurs, such as drug and public order offenses.

Our findings extend the literature on police discretion, showing how a federal enforcement mandate can reshape local police priorities even absent any changes in underlying crime. Our results are consistent with 287(g) creating incentives for pretextual stops, as officers seek opportunities to identify immigration-related offenses during otherwise routine encounters. This pattern echoes broader concerns about the effectiveness of order-maintenance policing strategies that prioritize low-level enforcement (Harcourt and Ludwig 2006). We also contribute to the literature on immigration enforcement and crime, providing the first modern, nationwide causal evidence that a local-federal immigration collaboration program does not reduce crime. This null effect is consistent with evidence that immigrants, including

undocumented immigrants, offend at lower rates than the native-born population (Abramitzky et al. 2024; Texas Department of Public Safety 2024).

This analysis carries clear policy implications. Claims that 287(g) agreements enhance public safety are unsubstantiated by empirical evidence. Instead, agreements redirect enforcement toward minor offenses without any corresponding reduction in serious crime. Local agencies considering adoption should weigh this shift toward discretionary enforcement against the program's opportunity and resource costs. At best, these agreements are ineffective at their stated goal; at worst, they redirect officer attention toward low-level, discretionary enforcement while creating a platform for potential discriminatory policing. The current unprecedented expansion of 287(g) agreements proceeds without empirical support for public safety benefits. Additionally, the concentration of these increases in discretionary offenses such as marijuana possession, where racial disparities in enforcement are well-documented, raises concern that 287(g) could exacerbate existing inequities in the criminal justice system, though we do not analyze racial disparities directly in this paper. Policymakers should recognize that claims of enhanced public safety lack empirical support, and weigh the shift toward discretionary enforcement against claimed benefits when considering adoption.

Our analysis has several limitations. We cannot directly measure community-level responses, such as changes in trust or cooperation. Reporting gaps in the UCR also required us to restrict our sample based on consistent reporting, which may limit generalizability to agencies with less reliable reporting histories. Future research could extend this work by examining racial and ethnic disparities in 287(g) enforcement directly, by analyzing officer-level behavior change, and by studying how communities respond to this type of enforcement over time.

Overall, we find that 287(g) agreements do not enhance public safety. Rather, they redirect police attention toward discretionary enforcement of low-level offenses, diverting resources from serious crime investigation while creating openings for discriminatory policing. As these agreements proliferate and federal immigration authority becomes more deeply intertwined with local policing, understanding their effects on communities becomes increasingly urgent. Our evidence indicates not that 287(g) agreements make communities safer, but that they intensify enforcement of minor offenses where officer discretion is greatest.

Disclosures

Funding

This project received no external funding. All research-related expenses were supported by Stanford University.

Conflicts of Interest

The authors declare no conflicts of interest.

Institutional Review Board Status

The Stanford University Institutional Review Board has determined that this study does not involve human subjects as defined in 45 CFR 46.102(e) and therefore does not require full review by the Institutional Review Board. This determination was made on July 14, 2026.

Data Availability

All code, detailed documentation, and the directory structure required to reproduce every table and figure in the paper will be deposited in an appropriate repository upon publication. Until the public release, the code package is available from the authors at their discretion and upon reasonable request.

References

Abramitzky, Ran, Leah Boustan, Elisa Jácome, Santiago Pérez, and Juan David Torres. 2024. "Law-Abiding Immigrants: The Incarceration Gap between Immigrants and the US-Born, 1870–2020." *American Economic Review: Insights* 6 (4): 453–471.

American Civil Liberties Union. 2026. *ICE is Rapidly Expanding Dangerous 287(g) Agreements with Local Police.* https://www.aclu.org/news/immigrants-rights/ice-expanding-287g-agreements-police.

American Immigration Council. 2025. *The 287(g) Program: An Overview.* https://www.americanimmigrationcouncil.org/fact-sheet/287g-program-immigration/.

Baicker, Katherine, and Mireille Jacobson. 2007. "Finders Keepers: Forfeiture Laws, Policing Incentives, and Local Budgets." *Journal of Public Economics* 91 (11-12): 2113–2136.

Becker, Gary S. 1968. "Crime and Punishment: An Economic Approach." *Journal of Political Economy* 76 (2): 169–217.

Bernstein, Hamutal, Paola Echave, Heather Koball, and Joseph Stinson. 2017. "State Immigration Policy Resource." *Urban Institute* (May).

Bureau of Economic Analysis. 2025. *Annual Personal Income by State.*

Bureau of Justice Statistics. 2018. *Law Enforcement Agency Identifiers Crosswalk, United States, 2012.* V2. https://www.icpsr.umich.edu/web/ICPSR/studies/35158.

Bureau of Labor Statistics. 2025a. *Consumer Price Index for All Urban Consumers.*

Bureau of Labor Statistics. 2025b. *Local Area Unemployment Statistics.*

Capellan, Joel A., and Evan T. Sorg. 2022. "Do Local-Federal Immigration Enforcement Agreements Reduce Crime? A Nationwide Evaluation Of The Crime Reduction Benefits of Section 287(g) of the United States Immigration and Nationality Act." *US Department of Justice.*

Capps, Randy. 2011. "Delegation and Divergence: A Study of 287(g) State and Local Immigration Enforcement." *Migration Policy Institute.*

Cho, Sungwoo, Felipe Gonçalves, and Emily Weisburst. 2024. "The Impact of Fear on Police Behavior and Public Safety." *Review of Economics and Statistics,* 1–45.

Cook, Philip J. 1980. "Research in Criminal Deterrence: Laying the Groundwork for the Second Decade." *Crime and Justice: An Annual Review of Research* 2:211.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review* (September): 2964–96.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2026. "Difference-in-Differences Estimators of Intertemporal Treatment Effects." *Review of Economics and Statistics,* 1–18.

Department of Homeland Security. 2012. *Archived: FY 2012: ICE announces year-end removal numbers, highlights focus on key priorities and issues new national detainer guidance to further focus resources | ICE.* https://www.ice.gov/news/releases/fy-2012-ice-announces-year-end-removal-numbers-highlights-focus-key-priorities-and, December.

Donato, Katharine M., and Leslie Ann Rodríguez. 2014. "Police Arrests in a Time of Uncertainty: The Impact of 287(g) on Arrests in a New Immigrant Gateway." *American Behavioral Scientist* 58 (13): 1696–1722.

Donohue, John J., Matthew Benavides, Amy L. Zhang, and Alex Oktay. 2024. "Beyond Bruen: Can Firearm Training Replace Local Discretion in Concealed Carry Permitting?" *NBER Working Paper Series*, no. 33240. National Bureau of Economic Research.

Dube, Arindrajit, Daniele Girardi, Òscar Jordà, and Alan M. Taylor. 2023. "A Local Projections Approach to Difference-in-Differences." *NBER Working Paper Series*, no. 31184. National Bureau of Economic Research.

Forrester, Andrew, and Alex Nowrasteh. 2018. Do Immigration Enforcement Programs Reduce Crime? Evidence from the 287(g) Program in North Carolina. Cato Institute.

Goncalves, Felipe, and Steven Mello. 2026. "Police Discretion and Public Safety." *Journal of Political Economy Microeconomics* (forthcoming).

Goodman-Bacon, Andrew. 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics* 225 (2): 254–277.

Harcourt, Bernard, and Jens Ludwig. 2006. "Broken Windows: New Evidence from New York City and a Five-City Social Experiment." *University of Chicago Law Review* 73 (1).

Imai, Kosuke, In Song Kim, and Erik H. Wang. 2023. "Matching Methods for Causal Inference with Time-Series Cross-Sectional Data." *American Journal of Political Science* 67 (3): 587–605.

IPUMS NHGIS. 1990. *1990 Census County Codebook.* Nhgis0002_ds123_1990_county_codebook.txt.

Kaplan, Jacob. 2025a. *Crime Data Tool Crosswalk.*

Kaplan, Jacob. 2025b. *Jacob Kaplan's Concatenated Files: Uniform Crime Reporting (UCR) Program Data: Arrests by Age, Sex, and Race, 1974-2024.* https://doi.org/10.3886/E102263V16.

Kaplan, Jacob. 2025c. *Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2024.* https://doi.org/10.3886/E100707V22.

Liu, Licheng, Ye Wang, and Yiqing Xu. 2024. "A Practical Guide to Counterfactual Estimators for Causal Inference with Time-Series Cross-Sectional Data." *American Journal of Political Science* 68 (1): 160–176.

Makowsky, Michael D., and Thomas Stratmann. 2009. "Political Economy at Any Speed: What Determines Traffic Citations?" *American Economic Review* 99 (1): 509–527.

Malone, Andrew. 2025. *What are 287(g) Agreements and How Do They Fuel Trump's Mass Deportations?* https://www.aclund.org/news/what-are-287g-agreements-and-how-do-they-fuel-trumps-mass-deportations/.

Miles, Thomas J., and Adam B. Cox. 2014. "Does Immigration Enforcement Reduce Crime? Evidence from Secure Communities." *The Journal of Law and Economics* 57 (4): 937–973.

MIT Election Data and Science Lab. 2018. *County Presidential Election Returns 2000-2024.* V20. https://doi.org/10.7910/DVN/VOQCHQ.

Nagin, Daniel S., Robert M. Solow, and Cynthia Lum. 2015. "Deterrence, Criminal Opportunities, and Police." *Criminology* 53 (1): 74–100.

National Immigration Forum. 2025. *Explainer: Training Under the Revived 287(g) Task Force Model.* https://forumtogether.org/article/explainer-training-under-the-revived-287g-task-force-model/.

Nunez, Cesar D. Vargas, Sakshina Bhatt, Basil F. Seif, Fernando S. Mendoza, David D. Laitin, and Asad L. Asad. 2026. "Reconsideration of Secure Communities rollout reveals preemptive local-federal cooperation in immigration enforcement." *Proceedings of the National Academy of Sciences* 123 (15): e2510928123.

Office of the President. 2007. *President Bush's Plan For Comprehensive Immigration Reform.* https://georgewbush-whitehouse.archives.gov/stateoftheunion/2007/initiatives/immigration.html.

Rushin, Stephen, and Griffin Sims Edwards. 2021. "An Empirical Assessment of Pretextual Stops and Racial Profiling." *Stanford Law Review* 73.

Smith, Brad W., Kenneth J. Novak, James Frank, and Christopher Lowenkamp. 2005. "Explaining Police Officer Discretionary Activity." *Criminal Justice Review* 30 (3): 325–346.

Texas Department of Public Safety. 2024. *Undocumented Immigrant Offending Rate Lower Than U.S.-Born Citizen Rate | National Institute of Justice.*

U.S. Census Bureau. 1999. *US Intercensal County Population Data by Age, Sex, Race, and Hispanic Origin.* https://www.nber.org/research/data/us-intercensal-county-population-data-age-sex-race-and-hispanic-origin. Provided by NBER.

U.S. Census Bureau. 2000. *TIGER/Line Shapefiles.*

U.S. Census Bureau. 2010. *TIGER/Line Shapefiles.*

U.S. Census Bureau. 2011. *Intercensal Estimates of the Resident Population for the United States, Regions, States, and Puerto Rico: April 1, 2000 to July 1, 2010.*

U.S. Census Bureau. 2012. *Intercensal Estimates of the Resident Population by Five-Year Age Groups, Sex, Race and Hispanic Origin for States and the United States: April 1, 2000 to July 1, 2010.*

U.S. Census Bureau. 2019. *Annual Estimates of the Resident Population for the United States, Regions, States, and Puerto Rico: April 1, 2010 to July 1, 2019.*

U.S. Census Bureau. 2020a. *Annual State Resident Population Estimates for 6 Race Groups (5 Race Alone Groups and Two or More Races) by Age, Sex, and Hispanic Origin: April 1, 2010 to July 1, 2019.*

U.S. Census Bureau. 2020b. *TIGER/Line Shapefiles.*

U.S. Census Bureau. 2024a. *Annual Estimates of the Resident Population for the United States, Regions, States, District of Columbia, and Puerto Rico: April 1, 2020 to July 1, 2024.*

U.S. Census Bureau. 2024b. *Small Area Income and Poverty Estimates.*

U.S. Census Bureau. 2025a. *Annual State Resident Population Estimates for 6 Race Groups (5 Race Alone Groups and Two or More Races) by Age, Sex, and Hispanic Origin: April 1, 2020 to July 1, 2024.*

U.S. Census Bureau. 2025b. *TIGER/Line Shapefiles.*

U.S. Census Bureau. *Profile of Selected Social Characteristics: 2000.* U.S. Census Bureau. Accessed on 18 June 2026. https://data.census.gov/table/DECENNIALDPSF32000.DP2.

U.S. Census Bureau. *Selected Social Characteristics in the United States.* U.S. Census Bureau. Accessed on 18 June 2026. https://data.census.gov/table/ACSDP5Y2014.DP02?g=610XX00US06025.

U.S. Immigration and Customs Enforcement. 2025. https://www.ice.gov/identify-and-arrest/287g.

Appendix A — Data Panel Construction

1. Treatment Variables

We construct a comprehensive agency-year panel of 287(g) agreements from 2000–2024 using two primary sources. Agreement data from 2000–2020 comes from the Urban Institute's State Immigration Policy Resource (Bernstein et al. 2017), which provides agency-level information on active agreements, agreement type, and start and end dates; agreements coded as "Joint" in these data are classified as both Task Force Model and Jail Enforcement Model, per guidance from the Urban Institute. We extend coverage through 2024 by scraping ICE's archived web pages using the Wayback Machine (U.S. Immigration and Customs Enforcement 2025), capturing snapshots at intervals throughout the year. Where a scraped agreement's recorded signing date precedes 2020 but the agreement is not yet reflected in the Urban Institute data for those years, we extend its treatment status backward to that signing year. We define an agency as treated if it held an active 287(g) agreement at any point during a given year. Certain agencies are excluded from our analysis. State-wide agencies generally lack corresponding jurisdiction identifiers in the UCR crime data and cannot be matched (e.g., the Arizona Department of Public Safety). Exceptions are agreements administered by a state's primary patrol or police agency (Colorado's and Minnesota's Departments of Public Safety, and Delaware's and New Mexico's Departments of Corrections), which we re-associate with that agency's ORI identifier. However, county sheriff's offices are included as distinct agencies separate from municipal police departments within the same county, consistent with the reporting structure of the UCR. Similarly, agreements administered by a specific detention facility rather than a general-purpose law enforcement agency (e.g., the Prince William-Manassas Regional Adult Detention Center) are attributed to the county sheriff's office that operates that facility, since UCR reports crime data at the agency level rather than the facility level. Finally, we identified and corrected a small number of data-entry errors in the Urban Institute data by cross-referencing the original ICE Memoranda of Agreement, including agreements attributed to the wrong state (e.g., Caldwell County, recorded as Georgia, is in North Carolina) and transposed signing dates. 287(g) agreements operate under three models varying in scope and discretionary authority. Table A1 summarizes key differences. The Task Force Model (TFM) allows officers to perform immigration enforcement during routine patrol duties, granting the broadest discretionary authority. The Jail Enforcement Model (JEM) allows for interrogating and identifying immigrants in state and local custody, and placing them into the custody of ICE. The Warrant Service Officer Model (WSO) is most limited, allowing local police to execute pre-existing ICE administrative warrants within correctional

facilities (American Immigration Council 2025; Malone 2025; National Immigration Forum 2025). Because of the lack of treated agencies in comparison to all agencies in the United States, our main analysis does not distinguish between types of agreements.

Table A1: 287(g) Agreement Descriptions

Type	Description	Training Required	Years Active in Sample
Task Force Model (TFM)	Allows state and local law enforcement to make immigration arrests during routine police enforcement.	Four weeks in-person	2002–2012
Jail Enforcement Model (JEM)	Gives law enforcement power to identify and interrogate immigrants in local jails.	Four weeks in-person	2005–2024
Warrant Service Officer (WSO)	Permits local law enforcement to serve and execute administrative immigration warrants on individuals in custody of an agency's jails.	Eight hours in-person	2019–2024

2. Outcome Variables

Our primary outcomes are arrest rates and offense rates, constructed from the FBI's Uniform Crime Reporting (UCR) program. We use Jacob Kaplan's concatenated versions of two UCR datasets: the Arrests by Age, Sex, and Race (ASR) dataset and the Offenses Known and Clearances by Arrest (Return A) dataset (Kaplan 2025b, 2025c). Arrests measure police enforcement activity, specifically the number of individuals taken into custody. Offenses measure reported crime, which are incidents known to police either through victim reports or officer observation. This distinction is critical to our analysis: if 287(g) affects police behavior without changing underlying crime, arrests may increase while offenses remain stable, allowing us to isolate the enforcement changes from crime changes. Following established practices for UCR data (Donohue et al. 2024), we address well-documented reporting inconsistencies. We restrict our sample to agencies reporting at least nine months of data per year and at least 15 years of data in the 2000–2024 period. We calculate arrest and offense rates per 100,000 population using agency population estimates provided in the UCR data. We match crime data to treatment data using agency names. The UCR data provides unique Originating Agency Identifiers (ORIs) for each agency, but the agreement data uses agency names without standardized ORIs. Because agency names vary across datasets (e.g., 'City of Austin Police Department' vs. 'Austin Police' or 'Austin PD'), we manually correct common discrepancies to ensure consistent matching. All matches for treated agencies were manually verified for accuracy. This name-based matching allows us to assign ORIs from the UCR data to each agency in the agreement data, which we then use to map to county-level controls. Unmatched agencies in the agreement data are either present in the raw UCR data but excluded due to

insufficient reporting or state-level agencies without UCR jurisdiction codes. Any agencies in the UCR data that were not matched are marked as untreated. The UCR categorizes crimes into two groups based on severity and data availability. Part I crimes include serious offenses: murder, manslaughter, aggravated assault, rape, robbery, burglary, motor vehicle theft, larceny-theft, and arson.[15] For these crimes, the UCR collects both arrest and offense data. Part II crimes include all other offenses. These include simple assault, forgery, fraud, embezzlement, stolen property, vandalism, weapons crimes, prostitution, sex offenses other than rape, drug offenses, gambling, offenses against family and children, DUI, liquor laws, disorderly conduct, vagrancy, curfew and loitering, runaways, drunkenness, suspicion, and all other offenses (excluding traffic violations). We exclude from our main analysis offenses against family and children (which are often handled by specialized units), curfew, loitering, and runaway offenses (which apply only to juveniles), suspicion arrests (which represent arrests without formal charges), and all other offenses excluding traffic (an unspecified, heterogeneous residual category). The UCR collects offense data for only a small subset of Part II categories (e.g., simple assault), too incomplete to construct a meaningful Part II offense aggregate, so our offense-based analysis is restricted to Part I crimes. Beyond these individual crime rates, we construct several composite outcome measures used throughout our analysis. We sum the Part II crime rates into a single Part II Crimes Arrest Rate and the Part I crime rates into a Part I Crimes Arrest Rate, each expressed per 100,000 population. From these we calculate the Part II Crimes Arrests as Share of Total Arrests, the share of combined Part I and Part II arrests that are Part II arrests, and the Difference in Part II and Part I Arrest Rates, the Part II rate minus the Part I rate. As a robustness check, we construct expanded versions of these totals that additionally include offenses against family and children, curfew and loitering, runaway, suspicion, and all other (excluding traffic) arrests in the Part II total, and larceny-theft in the Part I total. We also construct standardized versions of the Part I and Part II arrest rates, expressed in standard deviations from each agency's own pre-treatment baseline mean, calculated using years in which the agency had neither a 287(g) agreement nor Secure Communities in effect. This allows us to compare effect sizes across outcomes with very different natural scales, since Part II crimes are far more common than Part I crimes. Finally, we construct arrest-per-offense ratios for Part I crimes, dividing the number of arrests by the number of offenses known to police for the same crime in the same agency-year. Because offense data are unavailable for arson, we exclude arson and include larceny-theft, for which offense data are available, in this Part I crime set.

15. Larceny-theft (commonly called theft) is one of the most common crimes. While technically Part I, we exclude it from our primary analysis in Table 2, Figure 3, and Table 3. However, we include it as an individually reported category in Table 7 and Figure 7 for completeness.

This ratio outcome captures enforcement intensity conditional on reported crime, separately from the raw rate measures, which reflect both enforcement intensity and underlying crime levels.

3. Control Variables

3.1. CPI

The yearly CPI data are derived from the Bureau of Labor Statistics (BLS). These data are specifically "All items in U.S. city average, all urban consumers, not seasonally adjusted." These data only vary by year, not by county. CPI is not directly used as a control, but rather used to calculate Real Per Capita Personal Income.

3.2. Demographics

Demographic data are taken from the U.S. Census Bureau. Specifically, these are population estimates done by the Census as of July 1st of the given year, broken down by age, sex, and race. We use these data to create the proportion of black and Hispanic individuals in a population. The underlying Census data separates race into white, black, American Indian/Alaska Native, and Asian/Pacific Islander categories, plus (starting with the 2000 Census) a "Two or More Races" category, each further separated by Hispanic and non-Hispanic origin; we use only the black and Hispanic proportions as controls. In our analysis, we use the proportion of black individuals, proportion of Hispanic individuals, the year-over-year percent change in the proportion of Hispanic individuals, proportion of individuals aged 10–19, proportion of individuals aged 20–29, and proportion of individuals aged 30–39.

3.3. Foreign-Born Population

Foreign-Born population data are taken from the U.S. Census Bureau. We use the 1990 Census to create 1999 values, and the 2000 Census to create values for 2000–2004 data, linearly interpolating when necessary. For years 2005–2024, we use the 5-year American Community Survey estimates (ACS 2009 for 2005–2009, ACS 2014 for 2010–2014, ACS 2019 for 2015–2019, and ACS 2024 for 2020–2024). From these data we construct the foreign-born rate (foreign-born population per 100,000 residents) and its year-over-year percent change, both of which are included as controls.

3.4. Population Density

We take county-level population from the U.S. Census Bureau, and the shape files from the U.S. Census Bureau's TIGER/Line Shape files. We use the 2000, 2010, 2020, and 2025 county-level shape-files to create consistent land area size over time, using the most recent land area size for each county. We calculate population density as people per square kilometer.

3.5. Poverty Rate

Poverty rates are taken from the Census Bureau, specifically the Small Area Income and Poverty Estimates (SAIPE). We are specifically using the "All ages in Poverty, Rate Estimate". Each county's poverty rate is directly calculated by the Census Bureau.

3.6. Real Per Capita Personal Income (RPCPI)

Annual Personal Income is taken from the Bureau of Economic Analysis' (BEA) county-level data. The BEA directly calculates per capita personal income. This was converted from a nominal to real value by dividing this metric by the CPI from the BLS.

3.7. Republican Vote Share

County Presidential Election returns are from the MIT Election Data and Science Lab. We calculate the Republican Presidential vote share as the number of votes for the Republican candidate in the general election divided by the total number of votes. Since elections occur every four years, the data each year is the Republican presidential vote share of the most recent election.

3.8. Secure Communities

Data on the roll-out of Secure Communities is provided from the replication package of Nunez et al. (2026) in "Reconsideration of Secure Communities rollout reveals preemptive local-federal cooperation in immigration enforcement." Their publicly available dataset recorded the date every county implemented the Secure Communities program. Secure Communities was suspended nationwide from 2014–2016 and reinstated in 2017; we code our Secure Communities control as 0 for all counties during the suspension period and as 1 for all counties from 2017 onward, regardless of each county's original implementation date.

3.9. Unemployment Rate

Data on unemployment rate is compiled from the Bureau of Labor Statistics. Each county's non-seasonally adjusted unemployment rate for each year is directly calculated by the Bureau of Labor Statistics. Louisiana counties' unemployment rates for 2005 and 2006 are missing due to disruptions from Hurricane Katrina; we linearly impute these values from each county's 2004 and 2007 rates.

Appendix B — TWFE Results

1. Arrests

Table B1: The Impact of 287(g) Agreements on Violent Arrests, TWFE

	Aggravated Assault	Simple Assault	Total Assault	Murder	Manslaughter	Rape	Robbery
287(g)	0.693	−37.846*	−37.153*	−0.382	−0.072	−0.312	1.359
	(11.578)	(15.296)	(15.359)	(0.280)	(0.087)	(0.487)	(1.878)
N	210 999	210 999	210 999	210 999	210 999	210 999	210 999
Adj. R^2	0.70	0.72	0.75	0.35	0.12	0.36	0.74
Negative Weights	0.02%	0.02%	0.02%	0.02%	0.02%	0.02%	0.02%
Min. SD (Sign Reversal)	1.9	105.8	103.9	1.1	0.2	0.9	3.8

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

Table B2: The Impact of 287(g) Agreements on Property Arrests, TWFE

	Arson	Burglary	Motor Vehicle Theft	Theft	Vandalism	Stolen Property
287(g)	−0.290	−14.116	−6.854	−6.762	4.933+	6.935**
	(0.194)	(10.180)	(6.676)	(16.130)	(2.458)	(2.525)
N	210 999	210 999	210 999	210 999	210 999	210 999
Adj. R^2	0.16	0.62	0.58	0.38	0.62	0.54
Negative Weights	0.02%	0.02%	0.02%	0.02%	0.02%	0.02%
Min. SD (Sign Reversal)	0.8	39.5	19.2	18.9	13.8	19.4

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

Table B3: The Impact of 287(g) Agreements on Drug Arrests, TWFE

	Drug Total	Marijuana Possession	Marijuana Sales	Opiate/Cocaine Possession	Opiate/Cocaine Sales
287(g)	87.267**	18.979**	6.205*	2.454	3.566
	(25.842)	(6.416)	(2.692)	(5.810)	(2.129)
N	210 999	210 999	210 999	210 999	210 999
Adj. R^2	0.52	0.58	0.40	0.59	0.63
Negative Weights	0.02%	0.02%	0.02%	0.02%	0.02%
Min. SD (Sign Reversal)	244.0	53.1	17.4	6.9	10.0

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

Table B4: The Impact of 287(g) Agreements on Public Order Arrests, TWFE

	Disorderly Conduct	Drunkenness	DUI	Liquor Laws	Prostitution	Vagrancy
287(g)	1.322	5.585	10.020	21.131	−3.655	7.583
	(6.950)	(8.682)	(25.165)	(12.982)	(5.778)	(4.668)
N	210 999	210 999	210 999	210 999	210 999	210 999
Adj. R^2	0.68	0.67	0.62	0.58	0.58	0.38
Negative Weights	0.02%	0.02%	0.02%	0.02%	0.02%	0.02%
Min. SD (Sign Reversal)	3.7	15.6	28.0	59.1	10.2	21.2

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

2. Offenses

Table B5: The Impact of 287(g) Agreements on Offenses, TWFE

	Index Violent Crime	Index Property Crime
287(g)	−10.924	−66.046
	(26.442)	(104.067)
N	280 187	280 190
Adj. R^2	0.85	0.80
Negative Weights	0.00%	0.00%
Min. SD (Sign Reversal)	33.0	199.5

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

Table B6: The Impact of 287(g) Agreements on Violent Offenses, TWFE

	Aggravated Assault	Simple Assault	Total Assault	Murder	Manslaughter	Rape	Robbery
287(g)	−25.742	−31.389	−57.130	−0.561*	−0.008	−2.502	17.886*
	(22.396)	(36.863)	(56.080)	(0.248)	(0.030)	(2.449)	(8.737)
N	280 187	280 190	280 190	280 190	280 190	280 190	280 190
Adj. R^2	0.78	0.78	0.82	0.67	0.07	0.59	0.87
Negative Weights	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
Min. SD (Sign Reversal)	77.8	94.8	172.6	1.7	0.0	7.6	54.0

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

Table B7: The Impact of 287(g) Agreements on Property Offenses, TWFE

	Burglary	Motor Vehicle Theft	Theft
287(g)	−11.612	−35.878	−18.556
	(25.391)	(39.141)	(60.821)
N	280 190	280 190	280 190
Adj. R^2	0.79	0.78	0.75
Negative Weights	0.00%	0.00%	0.00%
Min. SD (Sign Reversal)	35.1	108.4	56.1

All models include agency and year fixed effects and are weighted by agency population, with standard errors clustered by state shown in parentheses. County-level controls (see Appendix A) are included in every specification. ** p<0.01, * p<0.05, + p<0.1.
"Negative Weights" and "Min. SD (Sign Reversal)" report the de Chaisemartin and D'Haultfœuille (2020) TWFE weight diagnostic: the share of the total weight attached to negative weights, and the minimal standard deviation of treatment effect heterogeneity across treated (g,t) cells compatible with the reported coefficient and the ATT being of opposite signs. A low value means the coefficient's sign is not robust to modest treatment effect heterogeneity.

Appendix C — DCDH Results

1. Aggregated Arrests

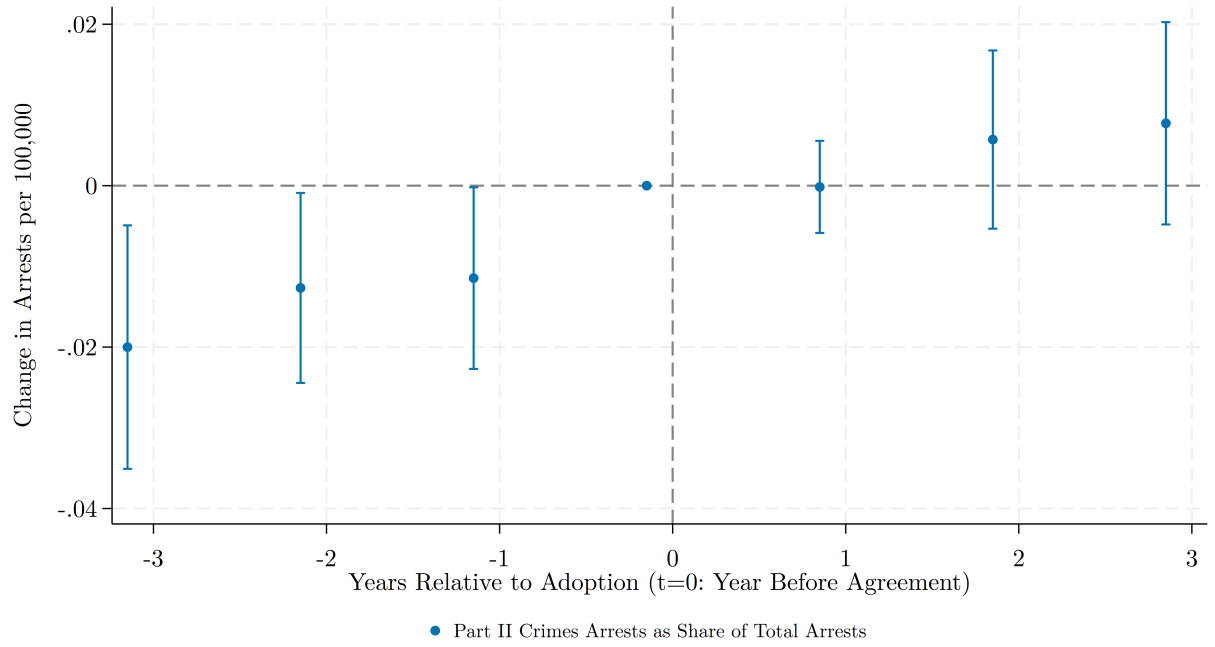


Figure C1: The Impact of 287(g) Agreements on Part II Crimes Arrests as Share of Total Arrests, DCDH

Table C1: The Impact of 287(g) Agreements on Part II Crimes Arrests as Share of Total Arrests, DCDH

	Part II Crimes Arrests as Share of Total Arrests
287(g)	0.004
	(0.005)

** p<0.01, * p<0.05, + p<0.1.

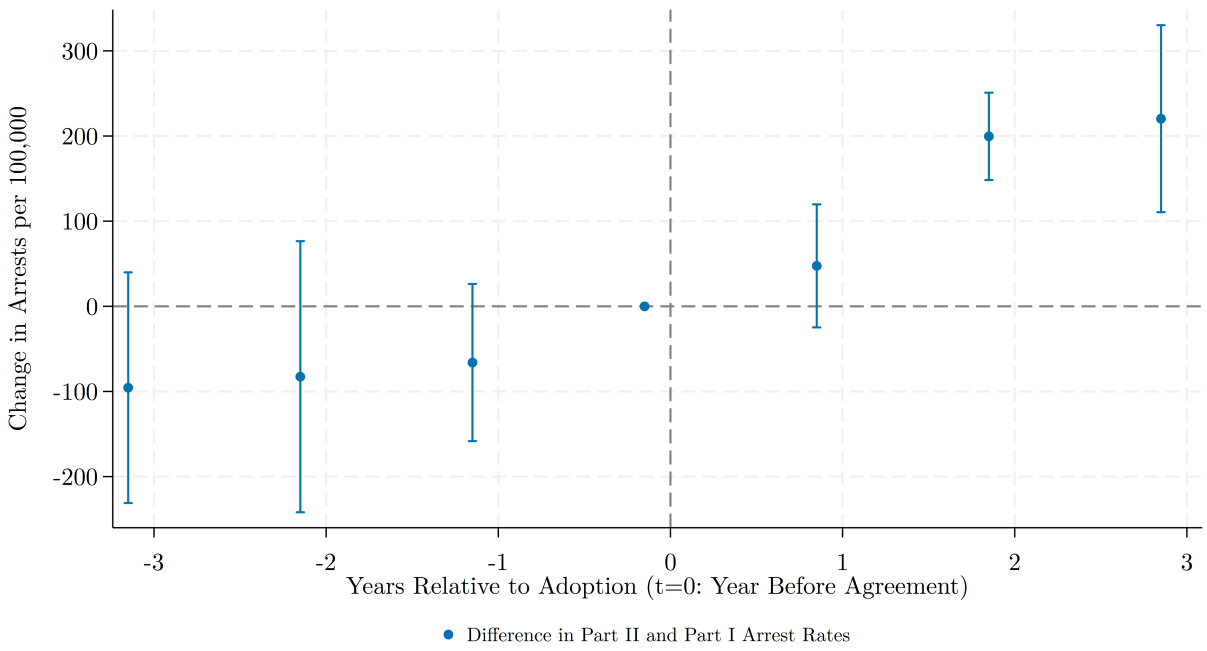


Figure C2: The Impact of 287(g) Agreements on Difference in Part II and Part I Arrest Rates, DCDH

Table C2: The Impact of 287(g) Agreements on Difference in Part II and Part I Arrest Rates, DCDH

	Difference in Part II and Part I Arrest Rates
287(g)	156.462**
	(31.070)

** p<0.01, * p<0.05, + p<0.1.

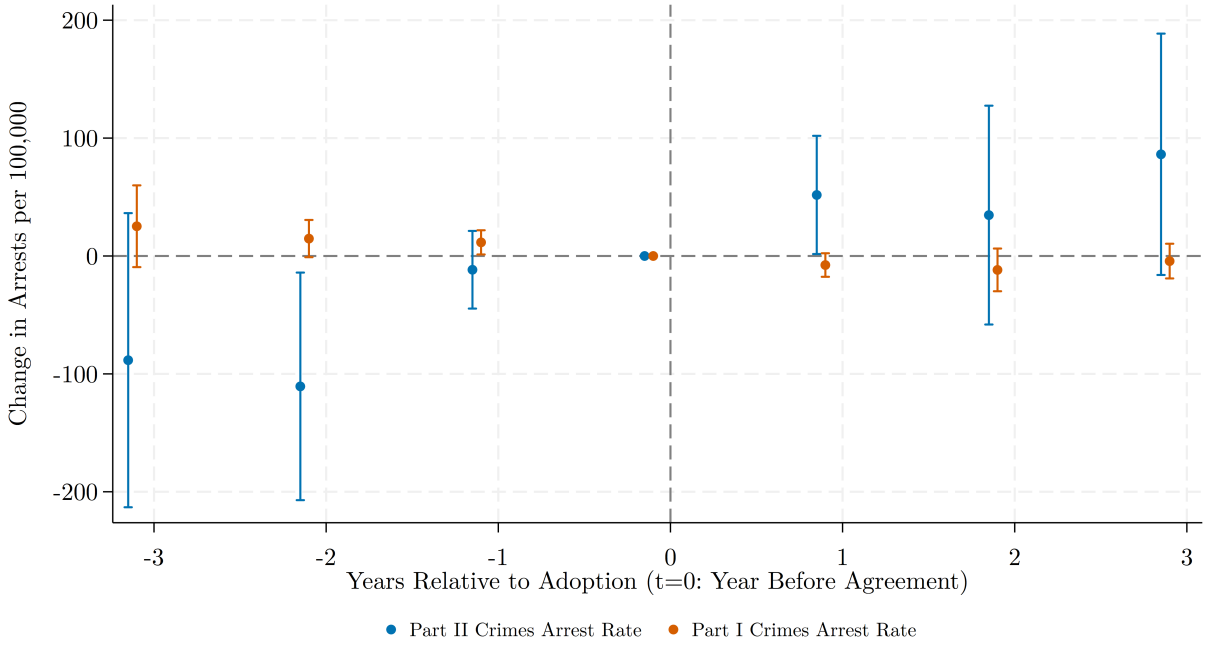


Figure C3: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Rate, DCDH (County Spillover)

Table C3: The Impact of 287(g) Agreements on Arrest Rates, DCDH (County Spillover)

	Part II Crimes Arrest Rate	Part I Crimes Arrest Rate
287(g)	57.915	−8.020
	(38.623)	(7.067)

** p<0.01, * p<0.05, + p<0.1.

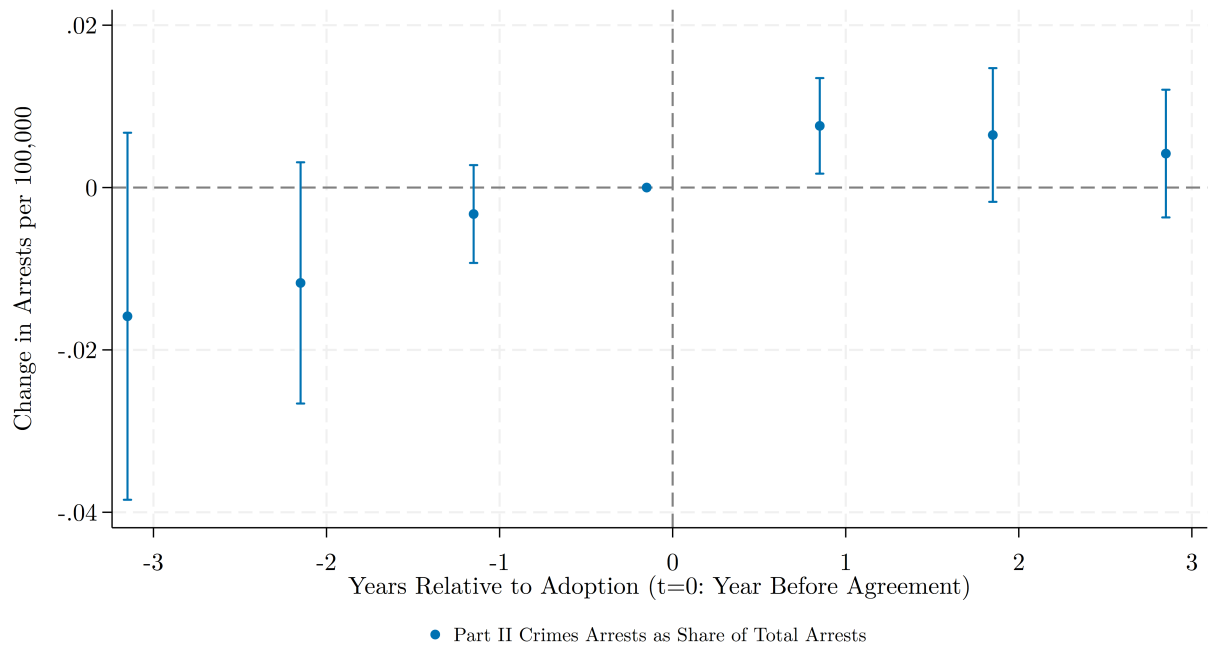


Figure C4: The Impact of 287(g) Agreements on Part II Crimes Arrests as Share of Total Arrests, DCDH (County Spillover)

Table C4: The Impact of 287(g) Agreements on Part II Crimes Arrests as Share of Total Arrests, DCDH (County Spillover)

	Part II Crimes Arrests as Share of Total Arrests
287(g)	0.006+
	(0.003)

** p<0.01, * p<0.05, + p<0.1.

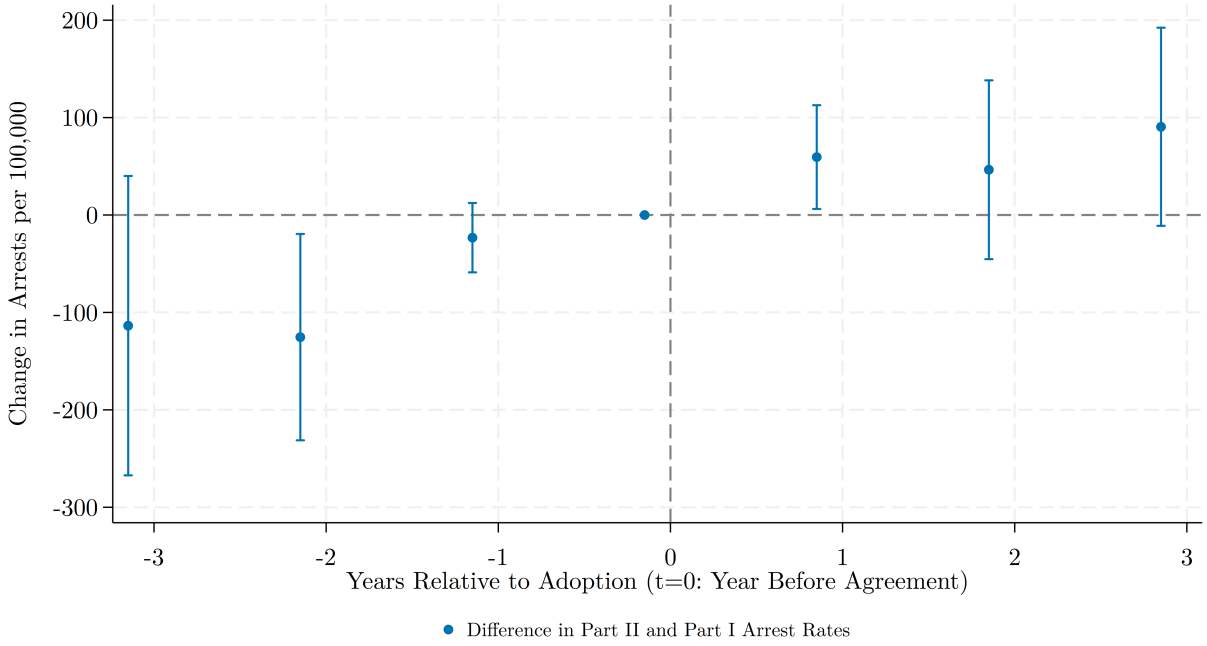


Figure C5: The Impact of 287(g) Agreements on Difference in Part II and Part I Arrest Rates, DCDH (County Spillover)

Table C5: The Impact of 287(g) Agreements on Difference in Part II and Part I Arrest Rates, DCDH (County Spillover)

	Difference in Part II and Part I Arrest Rates
287(g)	65.936+
	(39.049)

** p<0.01, * p<0.05, + p<0.1.

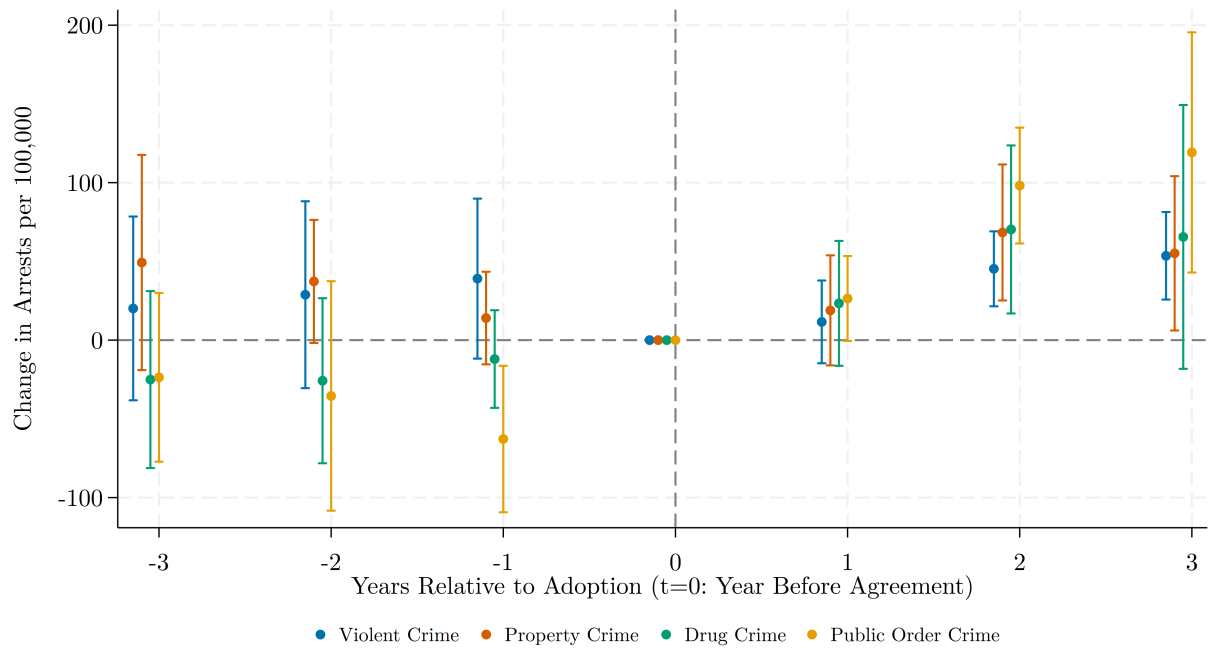


Figure C6: The Impact of 287(g) Agreements on Aggregated Arrests by Crime Type, DCDH

Table C6: The Impact of 287(g) Agreements on Aggregated Arrests, DCDH

	Violent Crime	Property Crime	Drug Crime	Public Order Crime
287(g)	36.966**	47.658*	53.280*	81.624**
	(10.934)	(20.664)	(26.805)	(22.075)

** p<0.01, * p<0.05, + p<0.1.

2. Aggregated Offenses

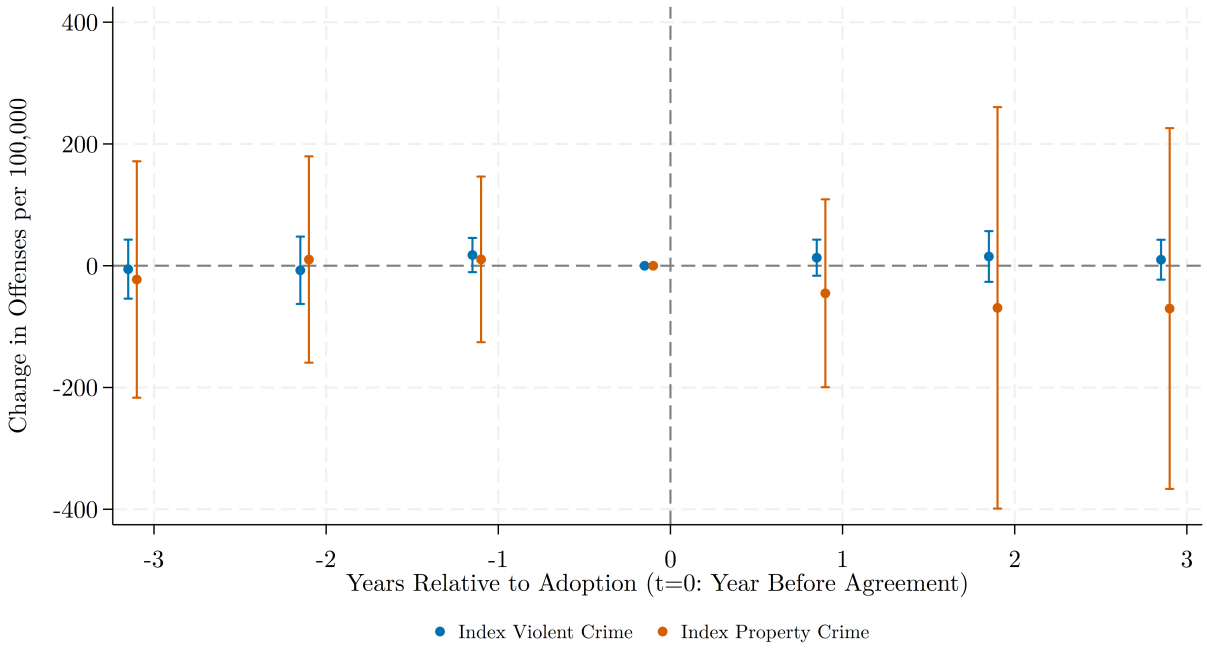


Figure C7: The Impact of 287(g) Agreements on Aggregated Offenses, DCDH

Table C7: The Impact of 287(g) Agreements on Aggregated Offenses, DCDH

	Index Violent Crime	Index Property Crime
287(g)	12.917	−62.459
	(17.307)	(131.601)

** p<0.01, * p<0.05, + p<0.1.

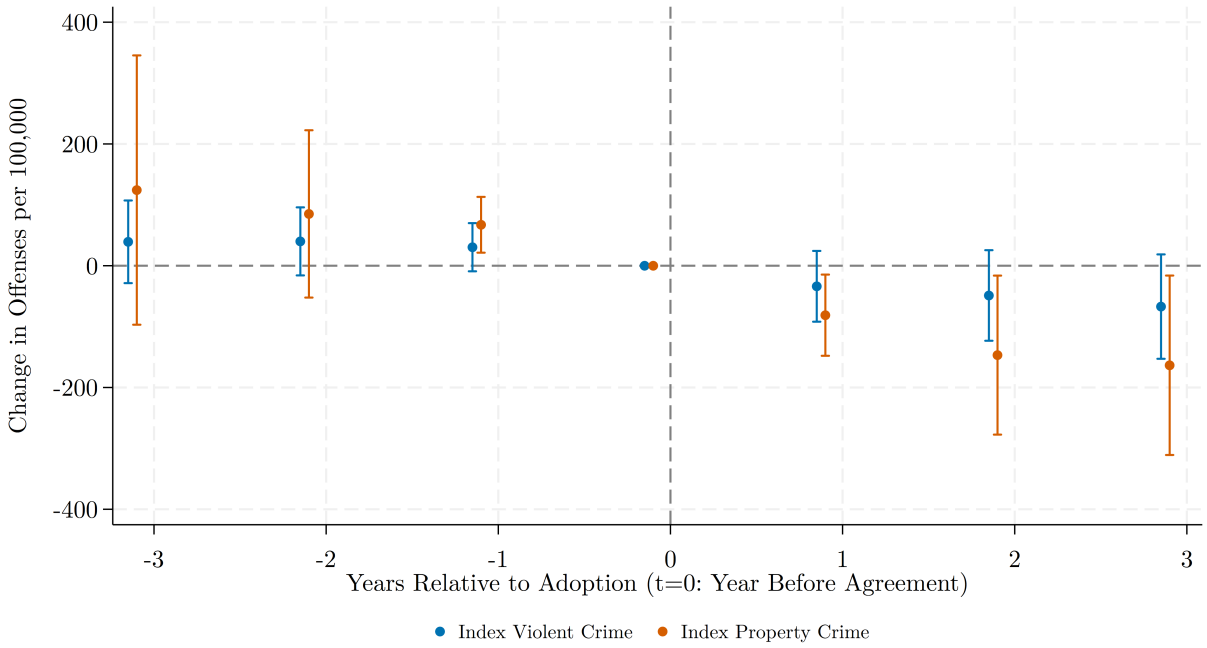


Figure C8: The Impact of 287(g) Agreements on Aggregated Offenses, DCDH (County Spillover)

Table C8: The Impact of 287(g) Agreements on Aggregated Offenses, DCDH (County Spillover)

	Index Violent Crime	Index Property Crime
287(g)	−50.265	−131.345*
	(37.299)	(55.917)

** p<0.01, * p<0.05, + p<0.1.

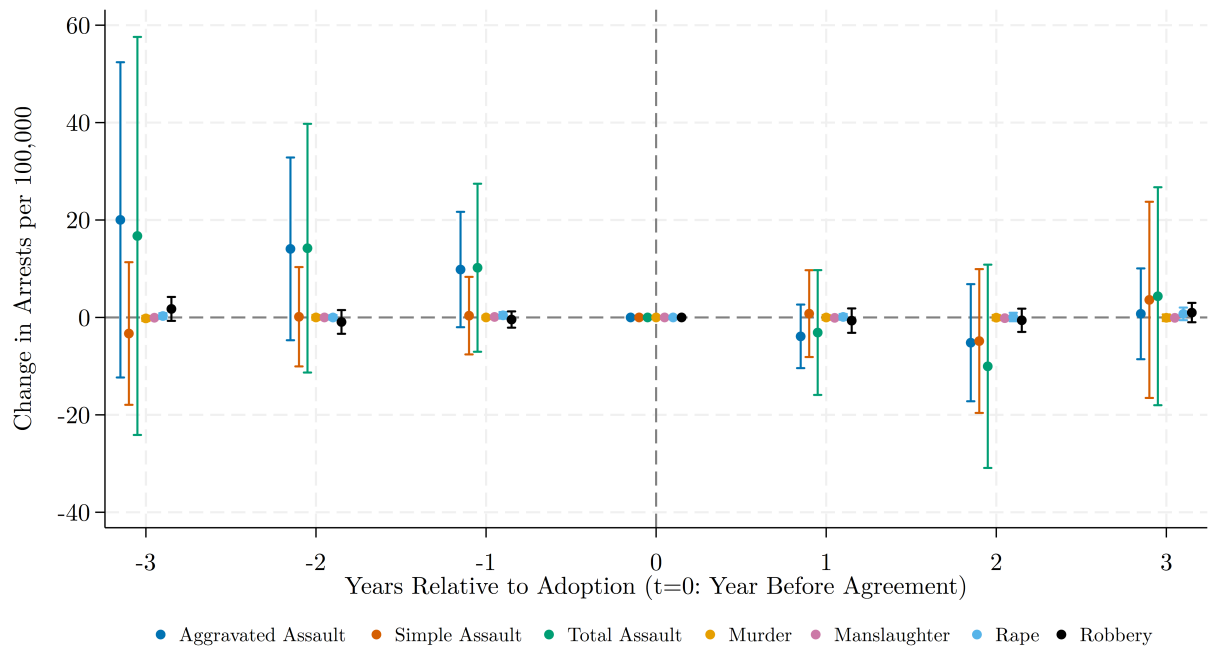


Figure C9: The Impact of 287(g) Agreements on Violent Arrests, DCDH (County Spillover)

Table C9: The Impact of 287(g) Agreements on Violent Arrests, DCDH (County Spillover)

	Aggravated Assault	Simple Assault	Total Assault	Murder	Manslaughter	Rape	Robbery
287(g)	−2.824	−0.155	−2.979	−0.040	−0.101	0.310	−0.092
	(4.612)	(7.289)	(9.290)	(0.260)	(0.067)	(0.432)	(0.944)

** p<0.01, * p<0.05, + p<0.1.

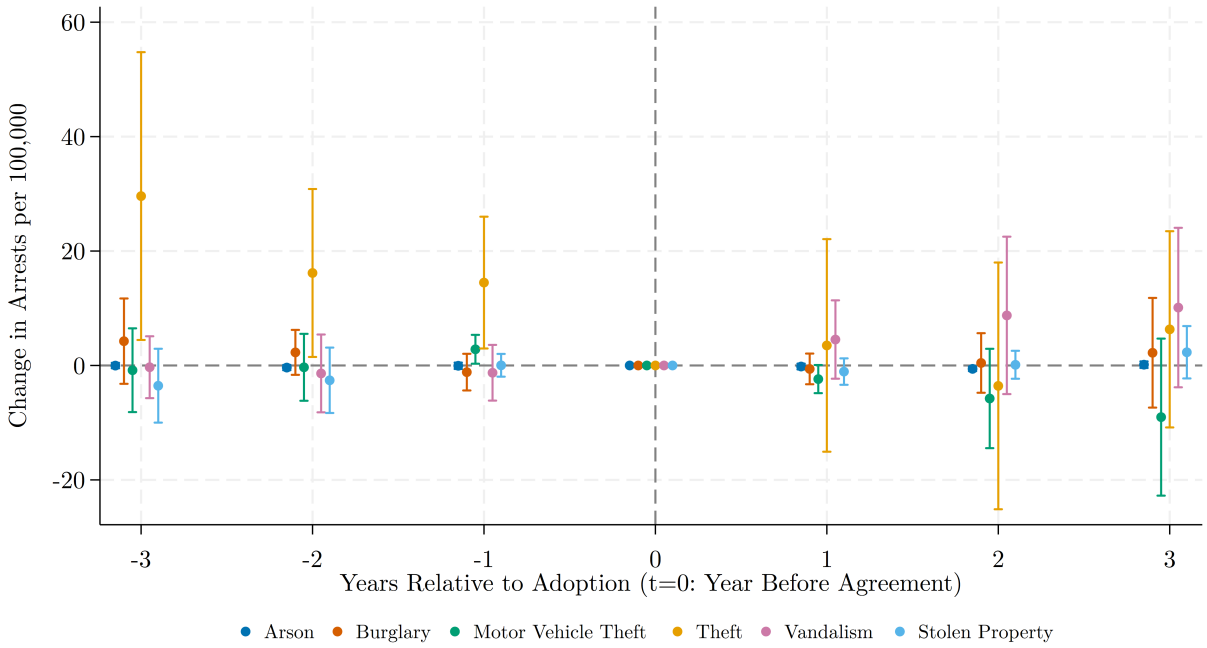


Figure C10: The Impact of 287(g) Agreements on Property Arrests, DCDH (County Spillover)

Table C10: The Impact of 287(g) Agreements on Property Arrests, DCDH (County Spillover)

	Arson	Burglary	Motor Vehicle Theft	Theft	Vandalism	Stolen Property
287(g)	−0.214	0.670	−5.729	2.102	7.828	0.436
	(0.179)	(2.815)	(4.148)	(9.044)	(5.778)	(1.341)

** p<0.01, * p<0.05, + p<0.1.

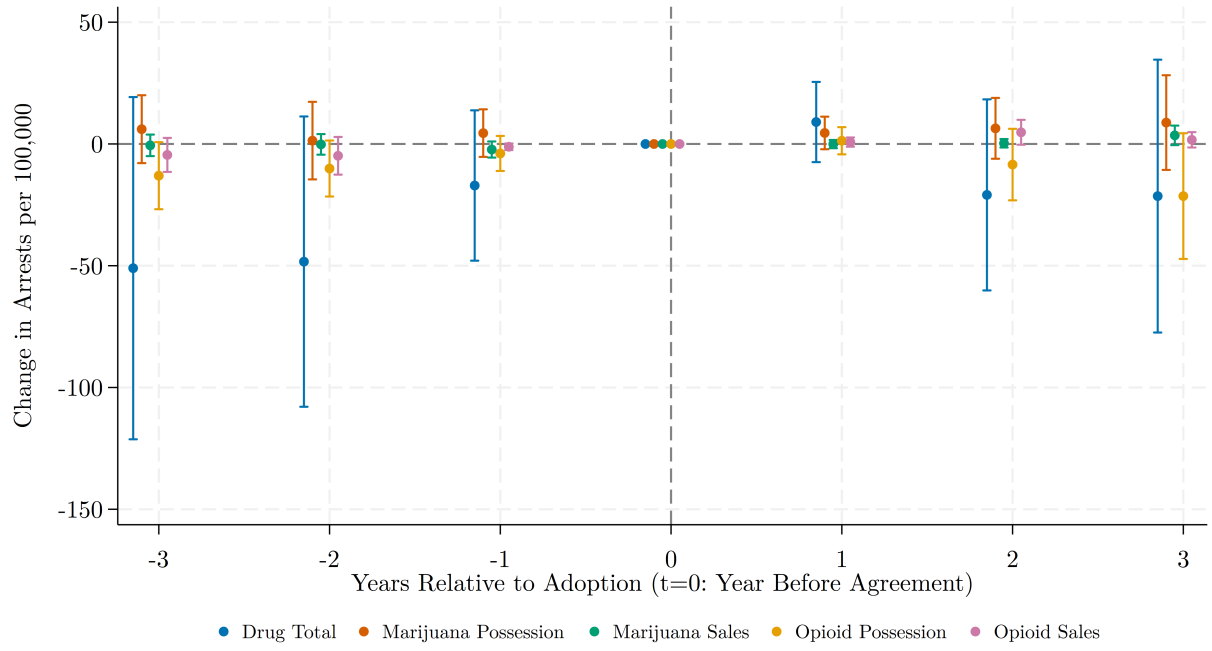


Figure C11: The Impact of 287(g) Agreements on Drug Arrests, DCDH (County Spillover)

Table C11: The Impact of 287(g) Agreements on Drug Arrests, DCDH (County Spillover)

	Drug Total	Marijuana Possession	Marijuana Sales	Opiate/Cocaine Possession	Opiate/Cocaine Sales
287(g)	−10.939	6.597	1.238	−9.441	2.425+
	(17.039)	(6.057)	(0.772)	(7.683)	(1.328)

** p<0.01, * p<0.05, + p<0.1.

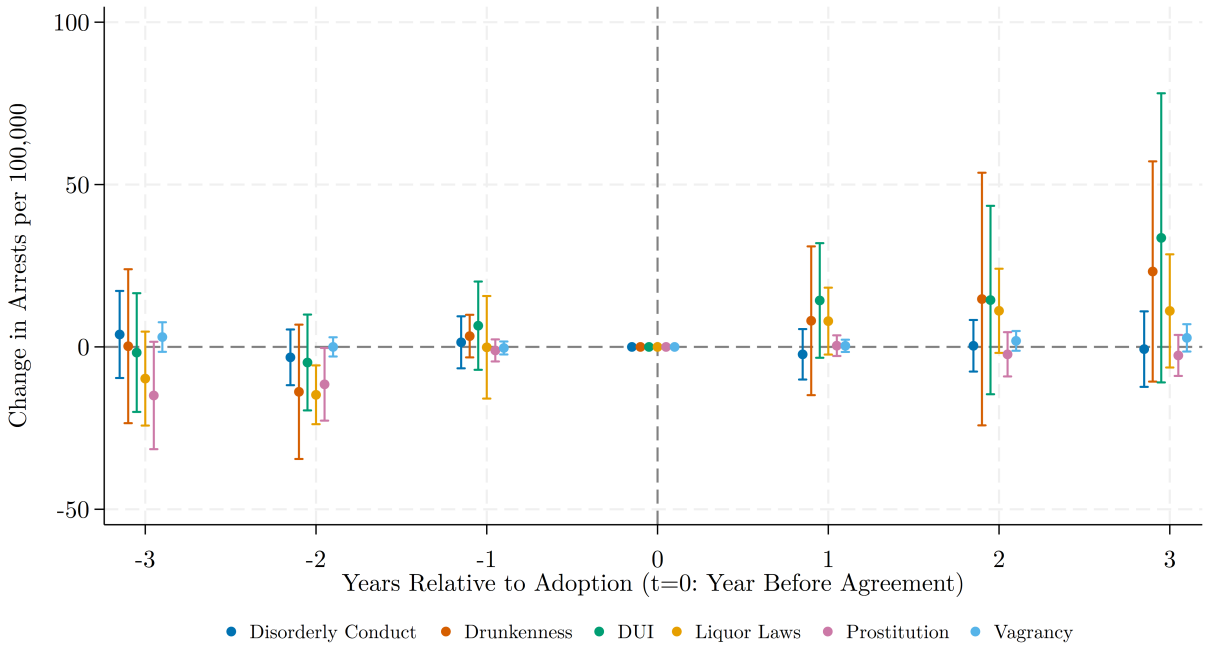


Figure C12: The Impact of 287(g) Agreements on Public Order Arrests, DCDH (County Spillover)

Table C12: The Impact of 287(g) Agreements on Public Order Arrests, DCDH (County Spillover)

	Disorderly Conduct	Drunkenness	DUI	Liquor Laws	Prostitution	Vagrancy
287(g)	−0.894	15.367	20.841	10.112+	−1.497	1.655
	(4.133)	(15.744)	(14.671)	(5.244)	(2.433)	(1.456)

** p<0.01, * p<0.05, + p<0.1.

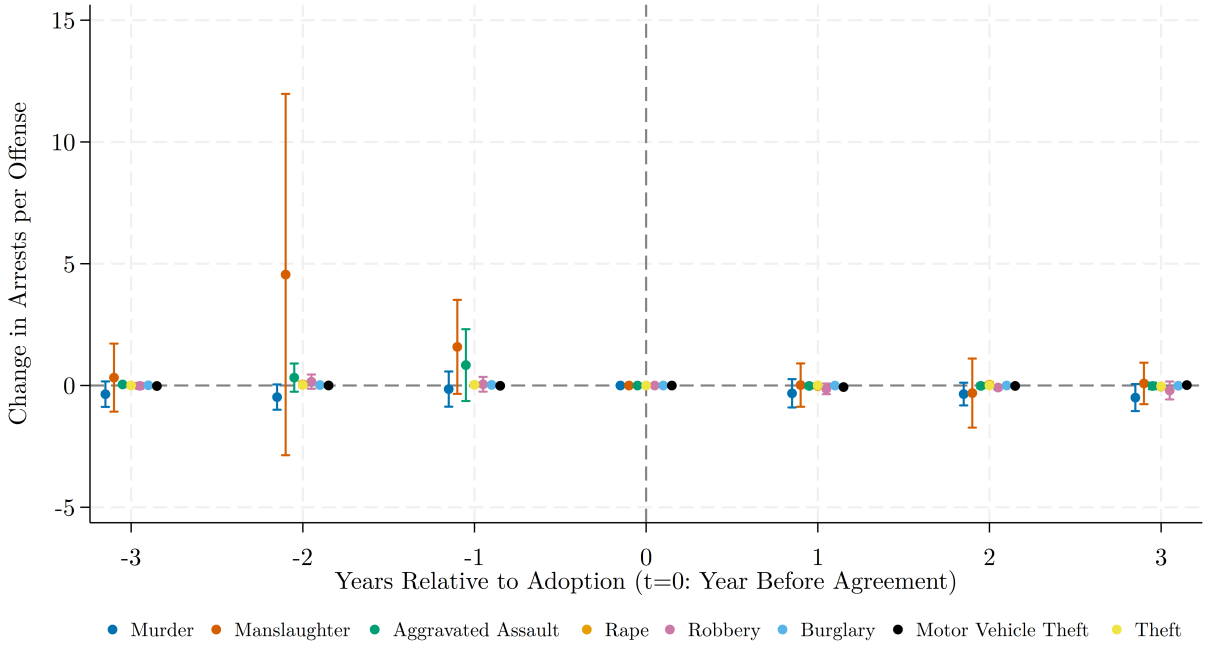


Figure C13: The Impact of 287(g) Agreements on Arrests per Offense Ratio, DCDH

Table C13: The Impact of 287(g) Agreements on Arrests per Offense, DCDH

	Aggravated Assault	Murder	Manslaughter	Rape	Robbery	Burglary	Motor Vehicle Theft	Theft
287(g)	−0.017	−0.394	−0.081	−0.012	−0.141	−0.003	−0.021	−0.005
	(0.056)	(0.249)	(0.449)	(0.027)	(0.102)	(0.010)	(0.018)	(0.010)

** p<0.01, * p<0.05, + p<0.1.

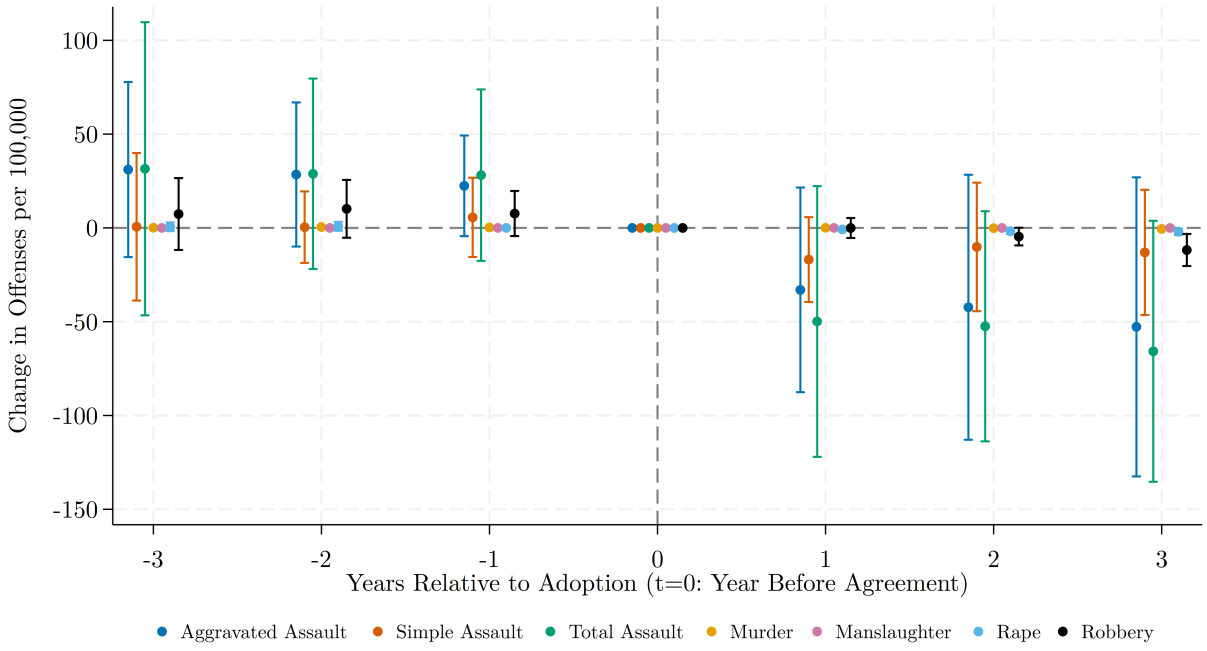


Figure C14: The Impact of 287(g) Agreements on Violent Offenses, DCDH (County Spillover)

Table C14: The Impact of 287(g) Agreements on Index Violent Offenses, DCDH (County Spillover)

	Aggravated Assault	Simple Assault	Total Assault	Murder	Manslaughter	Rape	Robbery
287(g)	−43.043	−13.550	−56.571+	−0.207	−0.006	−1.541*	−5.470+
	(35.069)	(12.014)	(34.163)	(0.259)	(0.043)	(0.727)	(2.897)

** p<0.01, * p<0.05, + p<0.1.

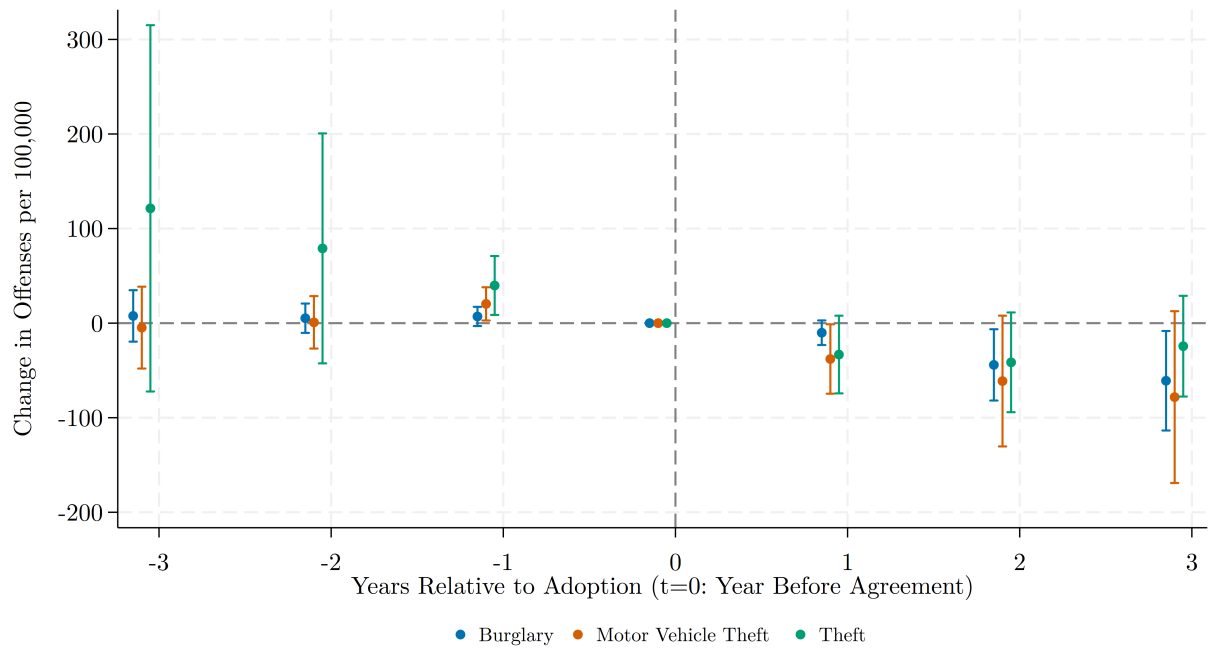


Figure C15: The Impact of 287(g) Agreements on Property Offenses, DCDH (County Spillover)

Table C15: The Impact of 287(g) Agreements on Index Property Offenses, DCDH (County Spillover)

	Burglary	Motor Vehicle Theft	Theft
287(g)	−38.458*	−59.499+	−33.388
	(16.287)	(33.272)	(22.460)

** p<0.01, * p<0.05, + p<0.1.

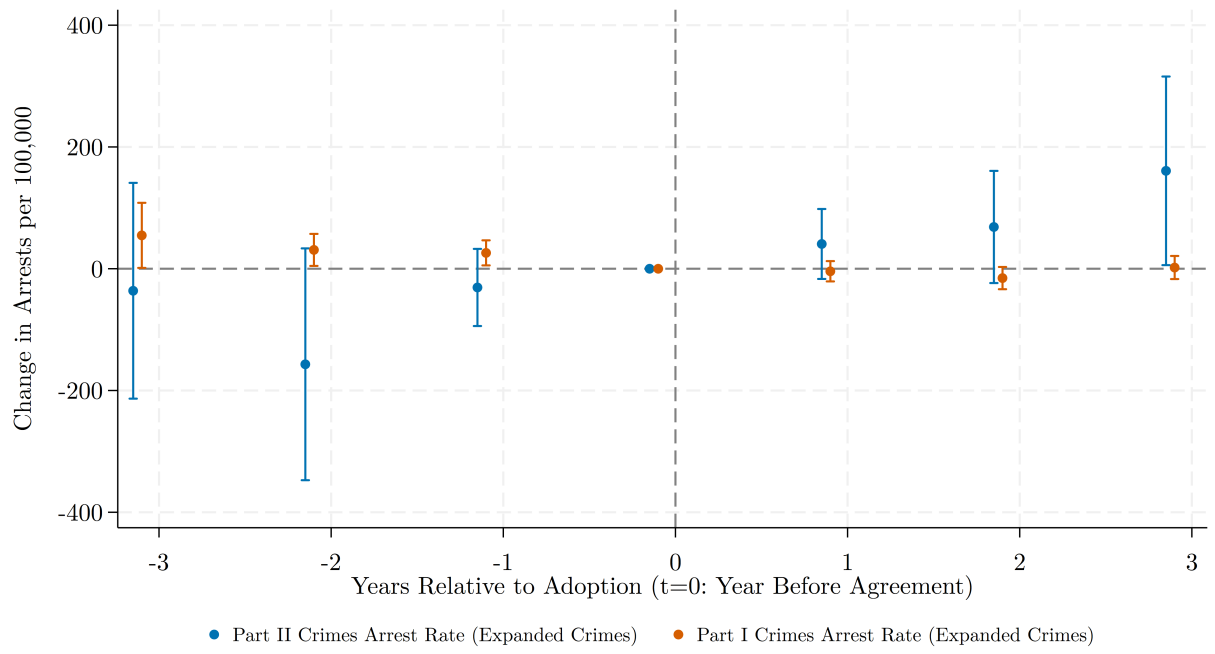


Figure C16: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Rate (Expanded Crimes), DCDH (County Spillover)

Table C16: The Impact of 287(g) Agreements on Expanded Crime Definitions, DCDH (County Spillover)

	Part II Crimes Arrest Rate (Expanded Crimes)	Part I Crimes Arrest Rate (Expanded Crimes)
287(g)	90.026+	−5.918
	(46.586)	(7.944)

** p<0.01, * p<0.05, + p<0.1.

3. Robustness: SD-Standardized Arrest Rates

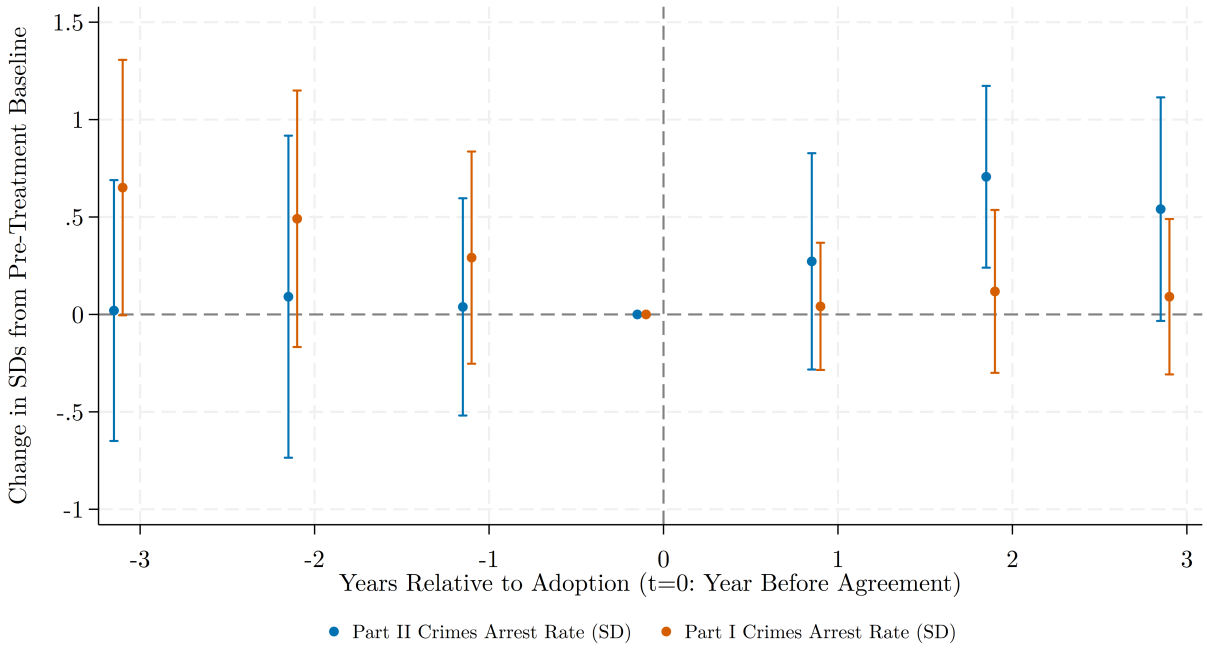


Figure C17: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Rate (SD), DCDH

Table C17: The Impact of 287(g) Agreements on SD-Standardized Outcomes, DCDH

	Part II Crimes Arrest Rate (SD)	Part I Crimes Arrest Rate (SD)
287(g)	0.509*	0.084
	(0.234)	(0.184)

** p<0.01, * p<0.05, + p<0.1.

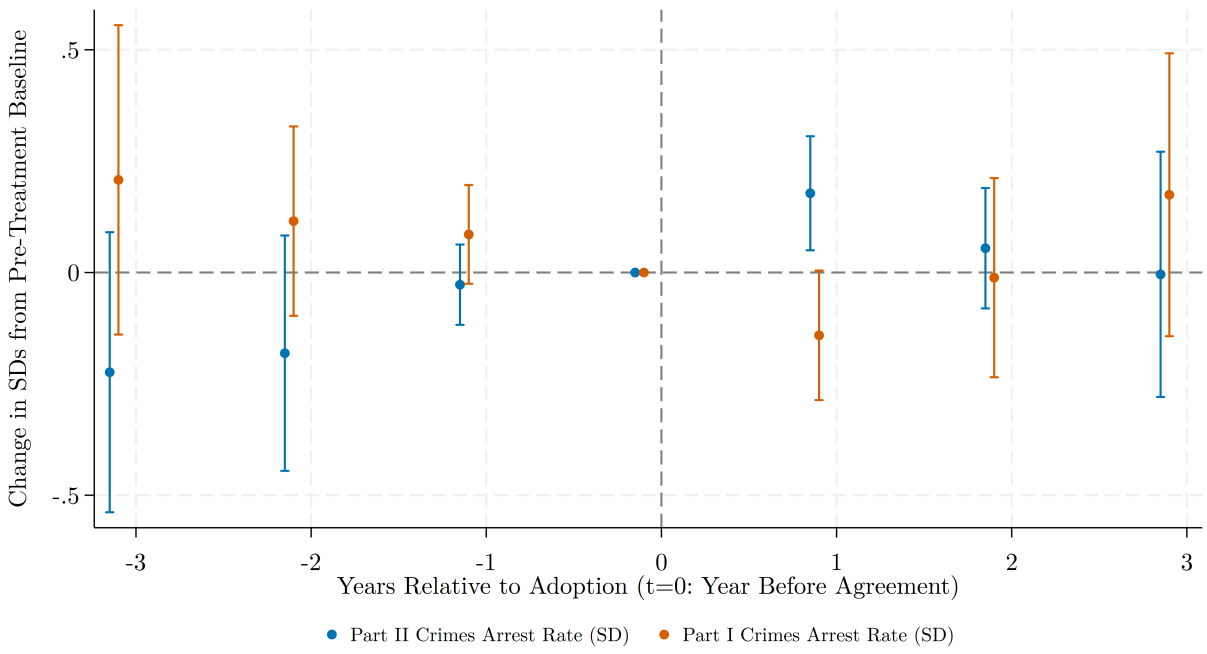


Figure C18: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Rate (SD), DCDH (County Spillover)

Table C18: The Impact of 287(g) Agreements on SD-Standardized Outcomes, DCDH (County Spillover)

	Part II Crimes Arrest Rate (SD)	Part I Crimes Arrest Rate (SD)
287(g)	0.078	0.005
	(0.060)	(0.100)

** p<0.01, * p<0.05, + p<0.1.

4. Robustness: Arrests per Offense Ratio

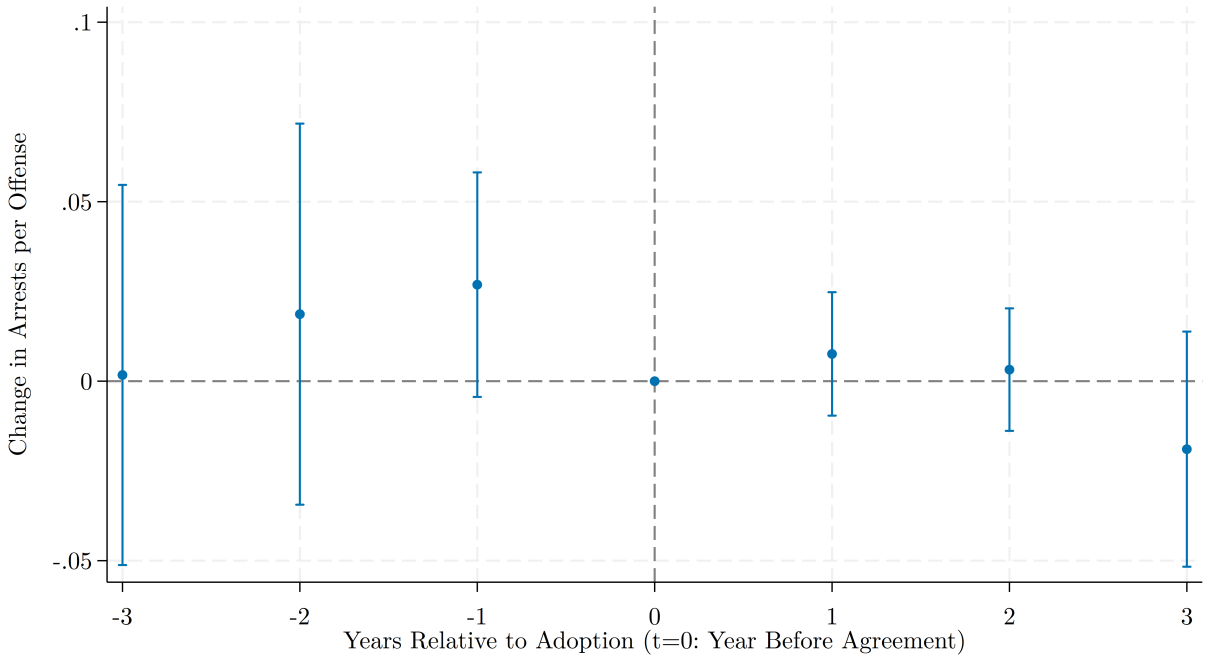


Figure C19: The Impact of 287(g) Agreements on Part I Crimes Arrests per Offense, DCDH

Table C19: The Impact of 287(g) Agreements on Arrests per Offense Ratio, DCDH

	Part I Crimes Arrests per Offense
287(g)	−0.003
	(0.009)

** p<0.01, * p<0.05, + p<0.1.

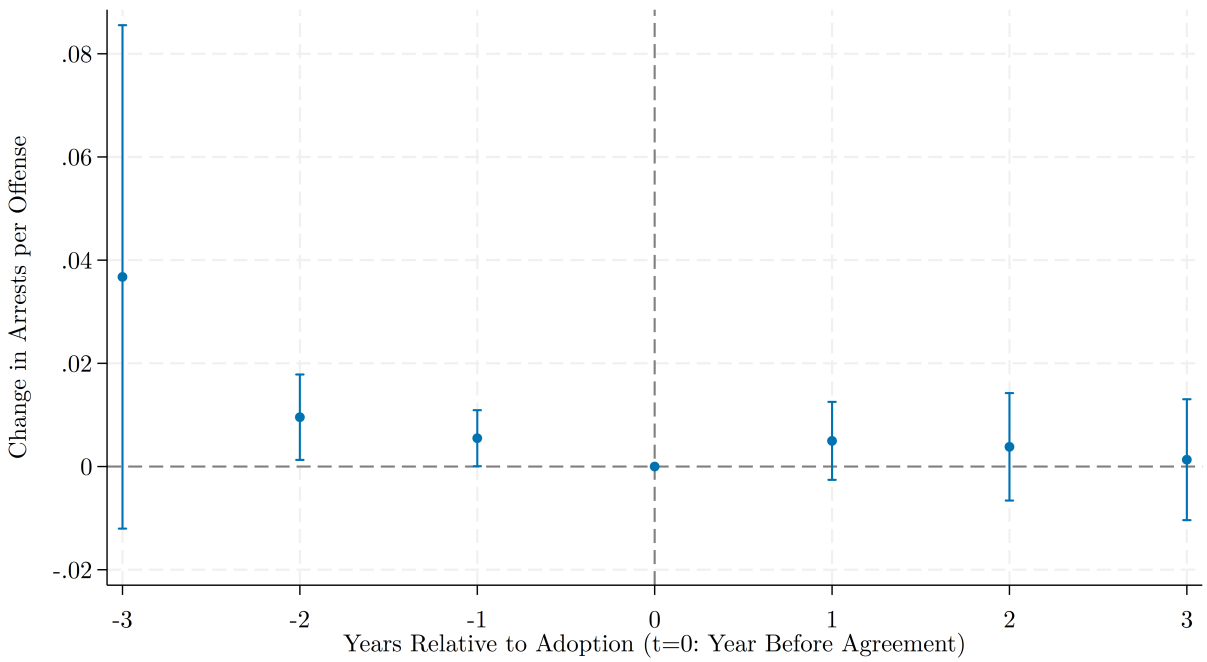


Figure C20: The Impact of 287(g) Agreements on Part I Crimes Arrests per Offense, DCDH (County Spillover)

Table C20: The Impact of 287(g) Agreements on Arrests per Offense Ratio, DCDH (County Spillover)

	Part I Crimes Arrests per Offense
287(g)	0.003
	(0.004)

** p<0.01, * p<0.05, + p<0.1.

5. Robustness: Population Mechanism Check

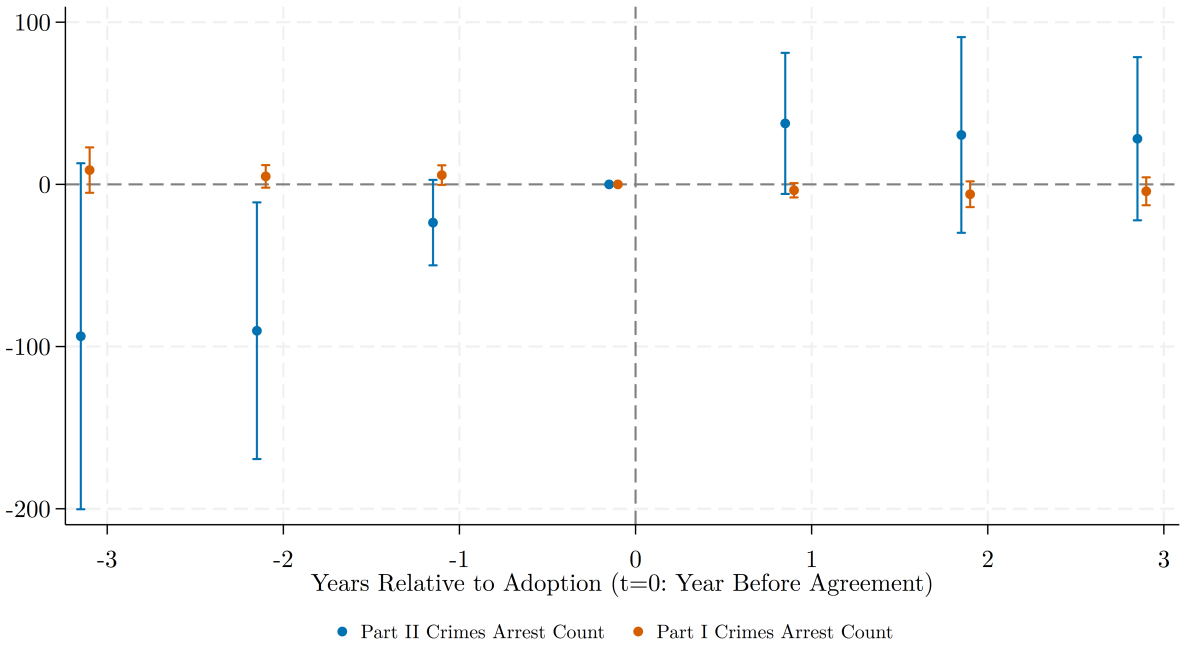


Figure C21: The Impact of 287(g) Agreements on Part I and Part II Crimes Arrest Count, DCDH (County Spillover)

Table C21: The Impact of 287(g) Agreements on Arrest Counts, DCDH (County Spillover)

	Part II Crimes Arrest Count	Part I Crimes Arrest Count
287(g)	32.427	−4.705
	(24.204)	(3.340)

** p<0.01, * p<0.05, + p<0.1.

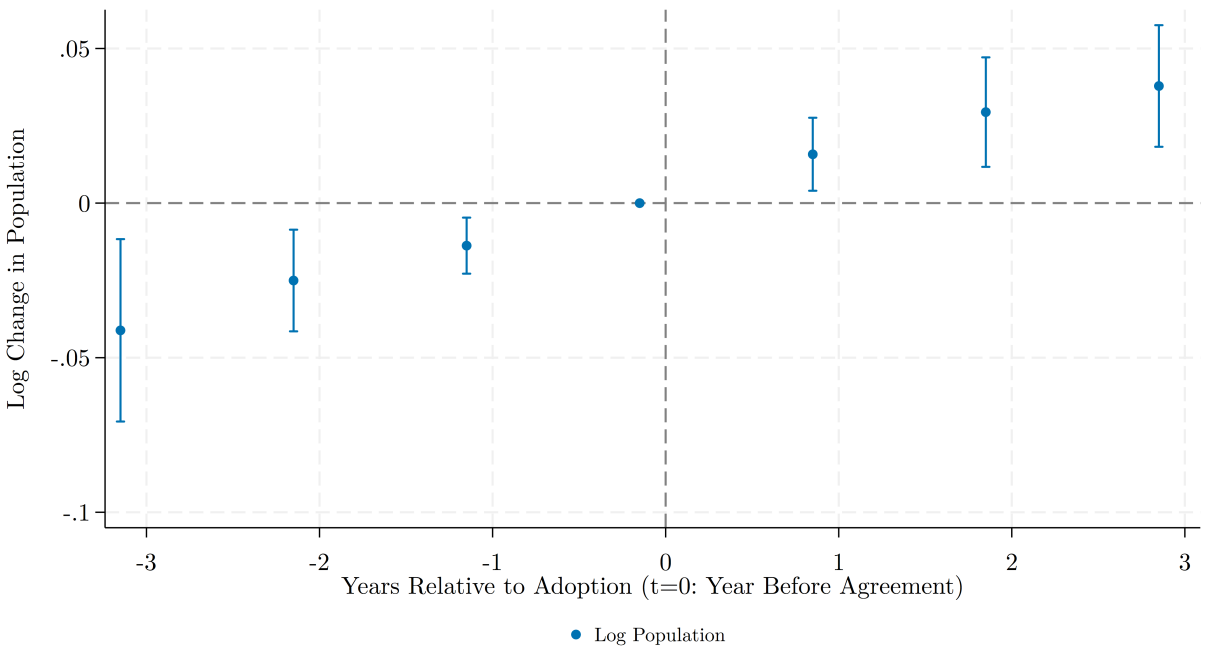


Figure C22: The Impact of 287(g) Agreements on Log Population, DCDH (County Spillover)

Table C22: The Impact of 287(g) Agreements on Log Population, DCDH (County Spillover)

	Log Population
287(g)	0.028**
	(0.008)

** p<0.01, * p<0.05, + p<0.1.