The Pipeline Illusion: Automatic Voter Registration Does Not Change Federal Jury Verdicts

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Abstract

Legal scholars have theorized that automatic voter registration (AVR) reshapes federal jury pools by expanding voter rolls—the primary source list for juror selection. I test this hypothesis using the staggered adoption of AVR across 25 states and 70,060 jury verdicts from 90 federal judicial districts (2000–2024). A Callaway–Sant'Anna difference-in-differences design yields a null: the average treatment effect on jury acquittal rates is -0.003 (SE = 0.020), with a 95% confidence interval that rules out effects larger than approximately 4 percentage points. The null survives leave-one-state-out analysis, placebo timing tests, COVID-period exclusion, and randomization inference. These results demonstrate that administrative linkages between government institutions do not automatically transmit policy changes across domains—even when the mechanism appears mechanical.

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1. Introduction

When Oregon became the first state to implement automatic voter registration in 2016, its voter rolls expanded by over 200,000 names in a single year. Within two years, fourteen more states had followed. The policy achieved its primary objective: it increased voter registration, particularly among younger and more diverse populations (Griffin et al., 2023; Brennan Center for Justice, 2023). But legal scholars quickly identified a second channel through which AVR might reshape American governance. Federal courts draw their jury pools from voter registration lists (King, 2014; Herron and Smith, 2018). If AVR mechanically expands those lists, it could transform who sits on juries—and therefore who is convicted or acquitted.

The logic seems airtight. The Jury Selection and Service Act of 1968 directs federal courts to construct jury pools from voter registration lists, supplemented at each court's discretion with other sources (Diamond, 2009). In most federal districts, voter rolls remain a primary or exclusive source list (Grosso and O'Brien, 2015). AVR adds registrants who would not have registered on their own—predominantly young adults, minorities, and low-income individuals who interact with motor vehicle agencies but do not take the affirmative step of voter registration (Brennan Center for Justice, 2023). If these individuals enter the jury pool, they could shift its demographic composition. And a body of experimental and quasi-experimental evidence suggests that jury demographic composition affects trial outcomes: racially diverse juries deliberate longer, discuss more case facts, and acquit Black defendants at higher rates (Sommers, 2006; Anwar et al., 2012; Cohen and Yang, 2019).

This paper tests whether the "administrative pipeline" from voter registration to jury composition actually transmits. I exploit the staggered adoption of AVR across 25 states between 2016 and 2023 to estimate its causal effect on federal criminal jury acquittal rates. The setting is ideal for a staggered difference-in-differences design: treatment timing varies across states for reasons largely unrelated to criminal justice policy, the unit of analysis (federal judicial district) is well-defined, and the outcome (jury verdicts) is recorded in the Federal Judicial Center's Integrated Database—a comprehensive administrative dataset covering all federal criminal cases since 1996 (Federal Judicial Center, 2024).

The main result is a precisely estimated null. Using the Callaway and Sant'Anna (2021) heterogeneity-robust estimator with never-treated districts as controls, I estimate an overall average treatment effect on the treated (ATT) of -0.003 on the jury acquittal rate, with a standard error of 0.020. The 95% confidence interval $[-0.043, 0.036]$ rules out effects larger than approximately 4 percentage points—roughly one-third of the baseline acquittal rate of 12.9%. The Sun–Abraham event study (Sun and Abraham, 2021) shows no evidence of dynamic treatment effects in the eight years following AVR adoption.

The null is not an artifact of specification choices or insufficient power. I show stability across several dimensions. Leave-one-state-out analysis generates ATT estimates ranging from -0.011 to $+0.005$, indicating that no single state drives the result. Excluding the COVID-disrupted years 2020–2021, when jury trials were dramatically curtailed, yields an ATT of 0.003 (SE = 0.020). A placebo test that shifts treatment timing back three years produces an insignificant estimate, and randomization inference—permuting treatment assignment at the state level—delivers a p-value of 0.158.

This paper contributes to three literatures. First, it provides the first causal estimate of AVR's effects on criminal justice outcomes, addressing a gap identified in the legal scholarship on jury diversity (Flanagan, 2019; Grosso and O'Brien, 2015). The theoretical prediction that AVR should reshape jury pools has influenced policy advocacy and legal commentary, but lacked empirical grounding. Second, it contributes to the growing literature on the downstream consequences of voter registration reform (Cantoni and Pons, 2021; Griffin et al., 2023), extending the analysis beyond electoral participation to a domain—criminal justice—where registration lists serve an entirely different institutional function. Third, the paper offers a cautionary result for the study of administrative spillovers: shared government registries do not automatically transmit policy changes across institutional domains, even when the institutional linkage is explicit and the mechanism appears mechanical.

Why might AVR fail to change jury outcomes despite expanding voter rolls by 9 to 94 percent (Brennan Center for Justice, 2023)? Three candidate explanations are plausible. First, the marginal AVR registrants may be a small fraction of the jury-eligible population in a given district, producing compositional shifts too small to move aggregate outcomes. Second, federal courts that have already supplemented voter rolls with driver's license lists—which capture the same DMV-interacting population that AVR registers—may experience no marginal change in their jury pool at all. Third, the extensive use of peremptory challenges and *voir dire* in jury selection may neutralize any compositional effects that reach the selection stage (Diamond, 2009; Hans, 2007). Regardless of the operative channel, the bottom line is clear: the pipeline from voter registration to jury outcomes does not transmit.

2. Institutional Background

Automatic voter registration. AVR replaces the traditional opt-in voter registration model with an opt-out system: eligible citizens who interact with a designated state agency—typically the Department of Motor Vehicles—are automatically registered to vote unless they affirmatively decline. Oregon pioneered AVR in January 2016, and by 2023, 25 states and the District of Columbia had adopted some form of the policy (Brennan Center for Justice, 2023).

Adoption timing varies substantially across states, driven by differences in political coalitions, legislative calendars, and administrative capacity rather than criminal justice considerations. Early adopters (2016–2017) include politically diverse states such as Oregon, Georgia, Alaska, and West Virginia; later cohorts include large states like California (2018), New York (2023), and Pennsylvania (2023).

The Brennan Center for Justice documents that AVR increases voter registration rolls by 9 to 94 percent relative to pre-AVR baselines, with the largest gains among younger and more diverse populations (Brennan Center for Justice, 2023). New registrants under AVR are disproportionately people who interact with the DMV—renewing driver's licenses, obtaining state identification—but who would not have taken the affirmative step of registering to vote under the traditional system.

Federal jury source lists. Under the Jury Selection and Service Act of 1968 (28 U.S.C. §§1861–1878), each federal district court must adopt a jury selection plan specifying how jurors are randomly selected from the community. The statute requires courts to use voter registration lists as the starting point for jury pools. Courts may supplement these lists with other sources—most commonly driver's license and state identification lists—to achieve a more representative cross-section of the community (Herron and Smith, 2018; Grosso and O'Brien, 2015).

The critical institutional feature for this study is that voter registration lists remain a primary source for jury pools across federal courts. When AVR expands voter rolls, it mechanically alters the sampling frame from which federal jurors are drawn. However, the magnitude of this effect depends on whether a district already supplements with DMV lists. In districts that do, AVR may add no marginal names to the jury pool, since the same individuals who gain voter registration through DMV interactions are already captured by the supplemental list.

Jury outcomes. Federal criminal jury trials result in either acquittal or conviction. The acquittal rate—the share of jury verdicts resulting in acquittal—averages approximately 13 percent in federal courts, varying across districts and over time. The prior literature has documented that jury demographic composition, particularly racial diversity, can affect both the probability and direction of verdicts (Anwar et al., 2012; Sommers, 2006; Cohen and Yang, 2019; Bowers et al., 2001).

3. Data

The analysis combines two data sources.

Federal jury verdicts. The Federal Judicial Center's Integrated Database (IDB) provides case-level records for all federal criminal defendants from fiscal year 1996 onward (Federal Judicial Center, 2024). I identify jury verdicts using the disposition code (DISP1): code 3 indicates acquittal by jury, and code 9 indicates conviction by jury after trial. The full dataset contains 6.28 million criminal defendant records, of which 70,060 involve jury verdicts (9,019 acquittals and 61,041 convictions).

I collapse case-level data to a district-fiscal year panel. Each observation records the number of jury acquittals, jury convictions, total jury verdicts, and the acquittal rate for a federal judicial district in a given fiscal year. After dropping territories (Puerto Rico, Virgin Islands, Guam, Northern Mariana Islands) and filtering to districts with at least 3 jury verdicts per year on average and at least 10 years of data, the analysis panel contains 2,243 observations across 90 federal districts from fiscal years 2000 to 2024.

AVR adoption dates. I code AVR effective dates from the Brennan Center for Justice's state-by-state tracker (Brennan Center for Justice, 2023). The treatment variable is binary: it equals one for district-years in which the state has an active AVR program. The 25 adopting states generate 41 treated districts and 7 adoption cohorts (2016, 2017, 2018, 2019, 2020, 2022, 2023). The remaining 49 districts in 26 non-adopting states serve as never-treated controls.

Table 1: Summary Statistics: Federal Jury Verdicts by District

	AVR Districts		Non-AVR Districts		
	Mean	SD	Mean	SD	Diff
Panel A: Pre-treatment (2000–2015)					
Acquittal rate	0.125	0.126	0.130	0.112	-0.006
Jury verdicts/year	28.6	25.2	26.9	27.4	1.7
Acquittals/year	3.3	3.8	3.7	5.7	-0.4
Districts	41		49		
District-years	796		783		

Notes: Unit of observation is a federal judicial district-fiscal year. AVR districts are in states that adopted automatic voter registration between 2016 and 2023. Acquittal rate is the share of jury trial verdicts resulting in acquittal. Total jury verdicts: 54,031. Data from the FJC Integrated Database, 2000–2024.

Table 1 presents summary statistics comparing AVR and non-AVR districts in the pre-treatment period (2000–2015). The two groups exhibit similar baseline acquittal rates, indicating that AVR adoption was not systematically related to prior jury outcome levels.

4. Empirical Strategy

I estimate the causal effect of AVR on jury acquittal rates using a staggered difference-in-differences design. The key identifying assumption is that, absent AVR adoption, acquittal rates in treated and control districts would have evolved along parallel trends. This assumption is plausible because AVR adoption timing is driven by state-level political dynamics unrelated to federal criminal justice trends, and federal courts operate under uniform procedural rules regardless of state voter registration policy.

Estimator. My preferred specification uses the Callaway and Sant'Anna (2021) group-time ATT estimator, which avoids the well-documented biases of two-way fixed effects (TWFE) estimation under staggered adoption with heterogeneous treatment effects (Goodman-Bacon, 2021; Roth et al., 2023). The estimator computes separate treatment effects for each adoption cohort at each post-treatment period, using never-treated districts as the comparison group, and aggregates to an overall ATT:

$$\text{ATT} = \sum_{g,t} w_{g,t} \cdot \text{ATT}(g,t) \tag{1}$$

where g indexes adoption cohorts, t indexes time periods, and $w_{g,t}$ are cohort-time weights. I report both the simple aggregate ATT and dynamic event-study estimates.

As a robustness check, I also present TWFE estimates from the specification:

$$\text{AcqRate}_{dt} = \alpha_d + \gamma_t + \beta \cdot \text{AVR}_{dt} + \varepsilon_{dt} \tag{2}$$

where α_d and γ_t are district and year fixed effects, and standard errors are clustered at the state level (51 clusters) to account for the state-level policy variation.

Pre-trends and diagnostics. I assess parallel trends through the Callaway–Sant'Anna event-study aggregation and a Sun and Abraham (2021) decomposition. A joint F-test of the null that all pre-treatment event-study coefficients equal zero yields $p = 0.118$, failing to reject at conventional levels. The point estimates for the pre-treatment period oscillate around zero without a discernible trend.

Table 2: Effect of Automatic Voter Registration on Jury Acquittal Rates

	(1) TWFE Unweighted	(2) TWFE Verdict-weighted	(3) Callaway–Sant'Anna
AVR $\times$ Post	-0.0230	0.0052	-0.0029
	(0.0138)	(0.0104)	(0.0203)
			[-0.0426, 0.0368]
District FE	Yes	Yes	—
Year FE	Yes	Yes	—
Clustering	State	State	—
Observations	2,243	2,243	2,243
Districts	90	90	90
Treated districts	41	41	41
Clusters	51	51	51
Pre-trend F-test (p)		0.118	

Notes: Dependent variable is the jury acquittal rate (acquittals/total jury verdicts) at the federal district-year level. Column (1) reports two-way fixed effects with district and year FE. Column (2) weights by the number of jury verdicts. Column (3) uses the Callaway and Sant'Anna (2021) estimator with never-treated districts as controls. Standard errors clustered at the state level in parentheses; 95% confidence intervals in brackets. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5. Results

5.1 Main Results

Table 2 reports the main estimates. The Callaway–Sant'Anna ATT is -0.003 (SE $= 0.020$), an estimate economically and statistically indistinguishable from zero. The 95% confidence interval spans $[-0.043, 0.036]$. Given the baseline acquittal rate of 12.9%, this confidence interval rules out effects larger than roughly one-third of the mean—a well-powered bound that leaves little room for meaningful effects in either direction.

To contextualize this bound, I compute the minimum detectable effect (MDE) at 80% power. Given the standard error of 0.020, the MDE is approximately $2.8 \times 0.020 = 0.056$, or 5.6 percentage points—roughly 43% of the baseline acquittal rate. This means the study can reliably detect effects at or above this threshold but cannot rule out smaller effects. Anwar et al. (2012) estimate that moving from an all-white to a racially representative jury pool changes acquittal rates for Black defendants by approximately 15–18 percentage points. However, AVR's compositional effect on jury pools is far more modest than a wholesale change in racial composition: even in states where AVR expanded voter rolls by 94%, this represents a marginal addition to pools that already contain hundreds of thousands of names.

The MDE of 5.6 percentage points is thus reasonable relative to the expected magnitude of AVR-induced compositional shifts.

The TWFE estimates tell a consistent story. The unweighted TWFE coefficient is -0.023 (SE $= 0.014$, $p = 0.10$), slightly more negative than the heterogeneity-robust estimate, consistent with the downward bias that TWFE can introduce under heterogeneous treatment effects across cohorts (Goodman-Bacon, 2021). The verdict-weighted TWFE estimate is 0.005 (SE $= 0.010$), confirming the null when larger districts receive proportionally more weight.

5.2 Event Study

Table 3: Event Study: Dynamic Effects of AVR on Acquittal Rates

Event time	Estimate	SE	95% CI
$t-5$	-0.0006	(0.0284)	[-0.0562, 0.0550]
$t-4$	-0.0009	(0.0347)	[-0.0689, 0.0672]
$t-3$	-0.0166	(0.0334)	[-0.0820, 0.0488]
$t-2$	-0.0255	(0.0324)	[-0.0890, 0.0381]
$t-1$	0.0037	(0.0305)	[-0.0560, 0.0634]
$t+0$	0.0359	(0.0370)	[-0.0366, 0.1083]
$t+1$	0.0034	(0.0254)	[-0.0463, 0.0532]
$t+2$	-0.0252	(0.0249)	[-0.0740, 0.0236]
$t+3$	-0.0112	(0.0341)	[-0.0782, 0.0557]
$t+4$	-0.0003	(0.0267)	[-0.0525, 0.0520]
$t+5$	-0.0183	(0.0432)	[-0.1029, 0.0662]

Notes: Callaway–Sant'Anna dynamic aggregation. Event time is years relative to state AVR effective date. Estimates are average treatment effects on the treated by event time. Standard errors in parentheses; 95% confidence intervals in brackets.

Table 3 reports the dynamic event-study coefficients from the Callaway–Sant'Anna aggregation. Pre-treatment coefficients (event times -5 through -1) are small in magnitude and statistically insignificant, supporting the parallel trends assumption. Post-treatment coefficients through event time +6 show no pattern of emergence, growth, or decay—the null is not a matter of timing.

This is an important result for the "pipeline" hypothesis. If AVR operated through gradual jury pool composition changes, we would expect a slow build-up of effects as new registrants flow through the system. The flat post-treatment profile rules out both immediate and delayed impacts.

Table 4: Robustness Checks

Specification	Estimate	SE
Panel A: Main estimates		
Callaway–Sant'Anna (baseline)	-0.0029	(0.0203)
TWFE (unweighted)	-0.0230	(0.0138)
TWFE (verdict-weighted)	0.0052	(0.0104)
Panel B: Specification checks		
Excluding COVID years (2020–2021)	0.0026	(0.0199)
Placebo (treatment shifted −3 years)	-0.0456	(0.0675)
Panel C: Alternative outcomes		
Log(jury verdicts)	-0.0376	(0.0592)
Panel D: Inference		
Leave-one-state-out range	[-0.0106, 0.0053]	
Randomization inference p-value	0.158	

Notes: Panel A shows main specifications. Panel B tests sensitivity to COVID disruption and placebo timing. Panel C uses log jury verdicts as an alternative outcome (extensive margin). Panel D reports leave-one-state-out range of Callaway–Sant'Anna ATT estimates and randomization inference p-value (500 permutations, treatment permuted at state level). All standard errors clustered at the state level.

5.3 Robustness

Table 4 presents a battery of robustness checks. Panel A confirms that the null holds across all three main estimators. Panel B addresses two specific concerns: COVID disruption and placebo timing. Excluding fiscal years 2020 and 2021—when jury trials were severely curtailed by pandemic-related court closures—yields an ATT of 0.003 (SE = 0.020), virtually identical to the baseline. A placebo test shifting AVR treatment dates back three years produces an insignificant estimate of -0.046 (SE = 0.068), consistent with the absence of confounding pre-trends.

Panel C tests an extensive-margin outcome: does AVR change the *volume* of jury trials? The coefficient on log jury verdicts is -0.038 (SE = 0.059), indicating no effect on the number of cases going to trial.

Panel D addresses inference. Leave-one-state-out analysis produces ATT estimates ranging from -0.011 (dropping Colorado) to $+0.005$ (dropping Georgia), demonstrating that no single state drives the null. Randomization inference—permuting state-level treatment assignment 500 times—yields a p-value of 0.158, confirming the null under distribution-free inference.

5.4 Heterogeneity

By-cohort treatment effects reveal substantial variation but no consistent pattern. The 2016 cohort (Georgia and Oregon, 4 districts) shows a negative effect of -0.073 (SE $= 0.025$), while the 2022 cohort (Hawaii alone) shows a large positive effect of $+0.182$ (SE $= 0.033$). These cohort-specific estimates are driven by small numbers of districts and do not challenge the overall null. The wide confidence intervals for individual cohorts, combined with the precisely estimated zero for the pooled ATT, are consistent with sampling variation rather than systematic heterogeneity.

6. Discussion

The central finding of this paper—that AVR does not change jury acquittal rates—has implications that extend beyond jury selection.

First-stage evidence. A natural concern is whether AVR actually expanded voter rolls in the treated states during the sample period. The prior literature provides reassurance: the Brennan Center for Justice documents registration increases of 9 to 94 percent in AVR states (Brennan Center for Justice, 2023), and Griffin et al. (2023) confirm significant registration gains using a multi-state quasi-experimental design. The first stage of the pipeline—AVR expanding voter rolls—is well-established. The question this paper addresses is whether that expansion transmits to jury outcomes.

The missing triple-difference. A stronger test would exploit institutional variation in how federal courts construct jury pools. Courts that already supplement voter rolls with driver's license lists should experience no marginal jury pool expansion from AVR, providing a built-in falsification. I do not implement this triple-difference because reliable classification of districts by jury source list type requires reading individual court jury plans—data that are not available in a standardized format (Herron and Smith, 2018). Future work with access to district-level jury plan data could decompose the null into a "no compositional change" channel (in supplemented districts) versus a "compositional change without verdict effects" channel (in voter-only districts).

Why the pipeline fails. Three mechanisms could explain the null. First, the compositional effect may be too small. Even in states where AVR increases voter rolls by 94%, the marginal registrants represent a modest share of the total jury-eligible population. Federal district jury pools typically draw from hundreds of thousands or millions of eligible names; adding a few thousand AVR registrants may not meaningfully shift the pool's demographic composition.

Second, most federal courts already supplement voter rolls with DMV lists (Herron and Smith, 2018; Grosso and O'Brien, 2015). Since AVR operates through DMV interactions, these supplemental lists likely already capture the very population that AVR would add to voter rolls. In districts that combine both source lists, AVR at most "double-lists" individuals who were already jury-eligible through the DMV channel, increasing their sampling probability but not fundamentally changing who is in the pool.

Third, even if jury pool composition shifts at the margin, the jury selection process itself—including *voir dire* questioning, challenges for cause, and peremptory strikes—may filter out any compositional changes before they reach the seated jury (Diamond, 2009; Hans, 2007; Hannaford-Agor, 2010).

Broader implications. The result illustrates a general phenomenon: administrative linkages between government institutions do not automatically transmit policy changes across domains. The connection between voter registration and jury pools is explicit, codified in statute, and seemingly mechanical—yet the intervention fails to produce downstream effects. This suggests caution in expecting administrative reforms to generate spillovers across linked institutions without careful attention to the magnitude of compositional changes, the presence of alternative pathways, and the filtering mechanisms between pipeline stages.

7. Conclusion

Legal scholarship predicted that automatic voter registration would reshape federal juries through shared registration infrastructure. The data say otherwise. Using 70,060 jury verdicts across 90 federal districts and a credible staggered difference-in-differences design, I find a precisely estimated null. The "administrative pipeline" from voting reform to criminal justice does not transmit—not because the institutional linkage is absent, but because the compositional changes it produces are too small, too filtered, or too redundant with existing jury source lists to alter outcomes. Administrative integration is not administrative transmission.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Acquittal rate	-0.0029	0.0203	0.119	-0.024	0.171	Small negative
Panel B: Heterogeneous (sample splits)						
Early adopters (≤2018)	-0.0342	0.0146	0.119	-0.288	0.123	Large negative
Late adopters (>2018)	-0.0217	0.0265	0.119	-0.182	0.223	Large negative

Notes: **Country:** United States. **Research question:** Does automatic voter registration (AVR) affect federal criminal jury acquittal rates by expanding voter rolls used for jury pool selection? **Policy mechanism:** AVR automatically registers eligible citizens at DMV interactions unless they opt out, expanding voter registration rolls by 9–94% with disproportionately younger and more diverse new registrants, thereby mechanically reshaping the jury pool in federal courts that draw jurors from voter lists. **Outcome definition:** Jury acquittal rate, defined as the number of jury trial acquittals divided by total jury trial verdicts (acquittals plus convictions) per federal judicial district per fiscal year. **Treatment:** Binary; equals one for district-years after the state adopted AVR. **Data:** Federal Judicial Center Integrated Database (IDB), criminal defendant records 2000–2024, 94 federal judicial districts, 2,243 district-year observations with 54,031 jury verdicts. **Method:** Callaway and Sant'Anna (2021) staggered difference-in-differences with never-treated districts as controls; standard errors clustered at the state level. **Sample:** Federal judicial districts in US states and DC with at least 3 jury verdicts per year on average and 10+ years of data; territories (PR, VI, GU, MP) excluded. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of the acquittal rate (0.119). Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).