Downstream Consequences of Criminal Justice Reform: Evaluating the Impact of Proposition 47 on California's Retail Sector

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Abstract

This paper evaluates the economic effects of California's Proposition 47, a 2014 criminal justice reform that reclassified certain nonviolent felonies—such as petty theft and drug possession—as misdemeanors. Using state-level panel data from 2008 to 2019 and multiple causal inference strategies—Difference-in-Differences, Synthetic Control, and Synthetic Difference-in-Differences—I estimate the policy's impact on larceny and downstream outcomes in the retail sector. I find that Prop 47 caused a significant increase in larceny per capita. Retail payroll and employment also appear to have modestly increased following the reform, while retail business density remained unchanged. A causal mediation analysis reveals limited evidence that increased theft explains the labor market effects, suggesting the existence of alternative mechanisms such as anticipatory hiring or security-related staffing changes. These findings are robust to multiple specifications but should be interpreted with caution given the omission of post-2019 data and the aggregation of urban and rural areas. Taken together, the results suggest that Prop 47 achieved its decarceration goals without generating substantial economic harm to the retail sector, although targeted firm-level or geographic effects may warrant further investigation.

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1 Introduction

The urgency of criminal justice reform has intensified as governments address the challenges of mass incarceration and its far-reaching social and economic consequences. Across the United States, policymakers, advocates, and researchers have increasingly questioned the efficacy and equity of punitive sentencing practices—particuarly for non-violent offenses. These concerns have prompted a wave of reforms aimed at reducing prison populations while maintaining public safety. Over the last decade, California has been spearheading criminal justice reform in the United States. California has historically hosted the nation's largest state prison system; in 2006, the state prison population reached a peak of 170,000 (West and Sabol, 2008), even though the system's maximum capacity was 80,000. With an approximate cost of $52,000 per inmate annually (Lofstrom and Raphael, 2016), taxpayers were bearing an outsized financial burden—particularly given that many inmates were low-level, nonviolent offenders—in an inefficient and unsustainable system.

The overcrowding of California's prison system ultimately resulted in a Supreme Court order mandating that the state reduces its prison population to meet with constitutional standards. In the landmark case Brown v. Plata, the Supreme Court ruled that prison overcrowding resulted in cruel and unusual punishment, thus violating the Eighth Amendment. The court ordered California to reduce it's prison population to 137.5% of design capacity within two years. California responded by rolled out a flurry of minor policies in attempt to resolve their overcrowding issue. The target was finally met following the passing of Proposition 47: the Safe Neighborhoods and Schools Act, on November 4, 2014. Proposition 47 (Prop 47) was designed to decrease the proportion of nonviolent, low-level offenders in the prison population. To accomplish this, the policy reduced the least significant petty theft and drug possession charges from felonies to misdemeanors. Petty theft offenses—including shoplifting, grand theft, receiving stolen property, and forgery—were subject to misdemeanor sentencing for amounts up to $950, a $550 increase. Prop 47 aimed to influence future convictions and sentencing while also granting individuals already incarcerated for crimes covered by the measure the ability to petition for resentencing. Importantly, it mandated a comprehensive review of criminal history and a proper risk assessment prior to resentencing to

prioritize public safety.

A significant component of Prop 47 was the planned reallocation of state savings. The anticipated reduction in the prison population was projected to save California millions of dollars annually. The Safe Neighborhoods and Schools Fund was established to reinvest these savings into programs designed to keep nonviolent offenders out of jail and prison. This included drug abuse and mental health treatment, victim services, and dropout prevention programs.

Initially, Prop 47 was viewed as a massive success; both incarceration rates and prison costs lowered. However, in recent years, Prop 47 has been blamed for an increase in crime across California—particularly in shoplifting. News outlets frequently claim that urban areas in California have experienced spikes in theft. For instance, Walgreens claims that "petty theft in [San Francisco] has gotten so out of control that it's had to close 17 of its stores" (Place, 2021); Wallace (2021) reports of brazen theft, regardless of security. The increase in theft has raised concerns about the economic impact on retail stores. Often operating on narrow profit margins, retail stores are more vulnerable to sustained losses from theft.

Although prior research on Proposition 47 has documented its effects on crime, incarceration, and racial disparities—with most studies finding no increase in violent crime but moderate increases in larceny—the broader economic implications of the policy remain underexplored. This paper narrows the broad scope of Prop 47 to focus on its implications for the retail sector: specifically, did the rise in theft undermine retail stores' ability to remain open? To investigate this question, I estimate the policy's effects on crime and business health outcomes and apply causal mediation analysis to assess whether larceny mediates the relationship between Prop 47 and various business health indicators. Understanding the economic consequences of Prop 47 offers valuable insights for policymakers considering similar reforms; it underscores the need to balance criminal justice objectives with the potential unintended effects on local economies and communities. This study contributes to the existing literature by extending the analysis one step further down the causal chain, examining a widely discussed but underexplored corollary of the policy.

The following sections of the paper are structured as follows. The next section details the

existing literature on Prop 47 and the impact of crime on businesses. Then, I split my analysis into two sections: Total Effects and Mediation. In each section, I explain the methodology, then analyze the results. The paper concludes with a discussion on the results, limitations, a reflection on Prop 47, and suggestions for further research.

2 Literature Review

Despite the magnitude of the debate surrounding Prop 47, research on the effects of Prop 47 remains relatively limited. The most influential studies (Bartos and Kubrin, 2018); (Bird et al., 2020); (Dominguez-Rivera et al., 2019); (Crodelle et al., 2021) agree that the policy had no impact on violent crime—aligning with its aim to reclassify nonviolent offenses while maintaining a focus on dangerous crime. However, every study found an increase in property crime and that the increase was mostly due to crimes directly impacted by the policy. Overall, while misdemeanor property crimes such as theft and drug possession have increased, the crime rate in California has remained steady since 2010, indicating that the broader public safety implications of Prop 47 are minimal.

Beyond crime, another body of contributions to the literature examines the efficacy of Prop 47 in achieving its broader goals. The policy successfully reduced incarceration and arrest rates (Bird et al., 2020); (Mooney et al., 2018) and immediately led to eight-figure annual state savings. (Taylor, 2016). It has also been associated with lower recidivism (Bird et al., 2020) and reductions in racial and ethnic disparities across various stages of the criminal justice process—including arrests, sentencing, and incarceration (MacDonald and Raphael, 2020); (Lofstrom et al., 2020); (Mooney et al., 2018). These disparities appear to have narrowed in part due to Prop 47's reclassification of drug offenses.

To examine the downstream impact of Prop 47, it is necessary to establish not only that the policy affected larceny rates, but also that crime—specifically theft—impacts business performance. Harbaugh et al. (2013) conducted economic experiments with young adults, manipulating both the potential reward for committing petty larceny and the penalties for being caught. They found that

the probability of theft increases when the amount that can be stolen rises and decreases when penalties are more severe. In the context of Prop 47, the increase in the felony threshold from $400 to $950 simultaneously raises the potential reward and lowers the expected penalty, mechanisms consistent with Harbaugh et al.'s findings.

In related work, Fe and Sanfelice (2022) investigate how crime affects businesses. Using granular location data, they find that each additional property crime incident near a business results in 1.13 fewer consumer visits the following month. A one-standard-deviation increase in property crime leads to a 12% decline in monthly visits per vendor. Notably, they identify theft as the primary subcategory of crime driving this effect. Considered in tandem, these studies provide a theoretical and empirical basis for expecting Prop 47 to increase petty theft, which in turn may reduce business activity through decreased consumer engagement.

Public opinion regarding Prop 47 is another facet of the literature worth noting. Garcia (2024) finds that Californians often associate a perceived increase in shoplifting with the policy, even in cases where data doesn't support a clear rise. This perception reflects ongoing debates about the real-versus-perceived impacts of criminal justice reforms. Despite concerns about retail theft, Garcia finds that most Californians still support Prop 47, recognizing its role in reducing incarceration and shifting the focus toward rehabilitation and alternative sentencing. Public attitudes appear to reflect a nuanced understanding of the trade-offs, balancing concerns over specific crimes with the broader benefits of systemic reform.

While most research focuses on crime and incarceration outcomes, relatively little is known about how Prop 47 may have indirectly affected economic outcomes, such as retail business health, through its impact on property crime.

3 Data

This study primarily uses the US Census Bureau's *Statistics of US Businesses* (SUSB) dataset. The SUSB provides state-level data, disaggregated by NAICS code, on: number of businesses,

employment, and annual payroll (millions). I supplement this with the Bureau of Economic Analysis' *GDP by State and Industry* dataset—providing retail sector GDP by state—and population data from the Census Bureau's *State Population Totals*. To measure crime outcomes, I use annual larceny data, from the FBI's *Crime Data Explorer*, which provides total observed larceny cases.

All outcome variables—business counts, employment, payroll, Sector GDP, and larceny incidents—are normalized on a per 1,000 population basis. This normalization is necessary because California differs substantially from other states in terms of both population and economic output; without such adjustments, raw figures would be misleading. I also transform employment, payroll, and sector GDP on a per establishment basis. This is to offer an alternative interpretation in the case of differing population growth trends. Additionally, I log each variable for increased interpretability. All datasets are publicly available and cited in the references section.

4 Total Effects: Crime and Business Outcomes

4.1 Methodology

The impact of increased larceny on retail business health is investigated through three outcomes: retail business density, retail payroll, and retail employment. Business density captures firm entry and exit dynamics and serves as a direct measure of sectoral presence. However, business density is unlikely to change much in the short run, as businesses may delay closure by drawing on financing or cutting costs to remain operational even when suffering financially. To complement this, I examine payroll and employment—more flexible margins of adjustment that businesses may alter in the short run in response to theft-related pressures. Often, firms reduce hiring or cut payroll expenditures before ultimately closing.

I begin by identifying the total effect of Prop 47 on both the mediator (larceny) and the business outcomes. While the literature consistently finds that Prop 47 increased theft-related crime (e.g., (Bartos and Kubrin, 2018); (Bird et al., 2020)), I replicate this effect within my own dataset to confirm alignment and internal consistency.

Selecting an appropriate control group presents a challenge: few states maintained a larceny–felony threshold near California's pre-Prop 47 level for an extended period. To address this, I proceed under the assumption that behavioral responses to theft laws are influenced more by salience of the policy than the precise threshold level. In other words, potential offenders may react more to the awareness of a reform than to its specific numerical cutoff. Holding all else constant, it is reasonable to suspect larceny will increase shortly after a policy update—when salience is at its height. Therefore, I construct a control group of 15 states[1] that did not alter their felony theft thresholds between 2008 and 2019.

I include two covariates: minimum wage and retail GDP per capita. Minimum wage levels vary significantly across the sample and are especially relevant in retail, which disproportionately employs low-wage labor. Payroll expenditures, in particular, are likely to be sensitive to wage floors. Retail GDP per capita proxies for the overall performance of the retail sector and captures demand-side conditions that may also influence firm outcomes.

For causal inference, I estimate total effects using three approaches: two-way fixed effects Difference-in-Differences (DiD), Synthetic Control (SCM), and Synthetic Difference-in-Differences (SDiD). For this research, DiD serves as a baseline specification; I use it to get a general idea of the impact Prop 47 had on each outcome. It also allows for event study plots, helpful for visualizing pre-trends. In this evironment, DiD is limited by two key shortcomings: (1) requiring the parallel trends assumption (2) lack of an optimally weighted donor pool.

To address these issues, I implement SCM[2], which constructs a weighted average of control units to form a synthetic California that closely matches the California on pre-treatment outcomes. Post-treatment, synthetic California acts as a counterfactual, representing California without the passage of Prop 47. A key advantage of SCM is that it explicitly builds a donor pool. However, it does so without two-way fixed effects. This means SCM does not control for unobserved heterogeneity across units or time, potentially allowing confounding factors to bias the results. Using

[1] The control states are: Arizona, Idaho, Maine, Michigan, Minnesota, New Jersey, New Mexico, New York, North Carolina, Pennsylvania, South Dakota, Vermont, West Virginia, Wisconsin, and Wyoming.

[2] Following the methodology of Abadie et al. (2010)

SCM also allows for placebo testing—where a control unit is treated instead of California—which tests for the uniqueness of the effect.

To calculate the total effect on each outcome, I primarily consider SDiD[3]. I employ SDiD because using DiD or SCM alone means forgoing either fixed effects or donor pool matching—whereas SDiD offers both. Unlike standard SCM, which performs best with a long pre-treatment period and relatively few treated units, SDiD is well-suited to shorter pre-treatment panels. This is particularly valuable in my setting, where I observe only seven pre-treatment and five post-treatment years. The method also relaxes the strict parallel trends assumption required by DiD, instead relying on a weighted balance of pre-treatment trends while still accounting for unit and time effects.

In addition to these strengths, SDiD offers robustness to moderate treatment endogeneity and better accommodates imbalanced panels. Berman and Israeli (2022) emphasize its advantage in contexts where descriptive counterfactuals are needed but clean identification is complicated by short panels or staggered treatment timing. For these reasons, I adopt SDiD as my preferred specification.

Regardless, I still report results from both DiD and SCM to triangulate the treatment effect. Each method relaxes a different identifying assumption, and convergence across the three approaches increases confidence in the results.

[3]See Arkhangelsky et al. (2021).

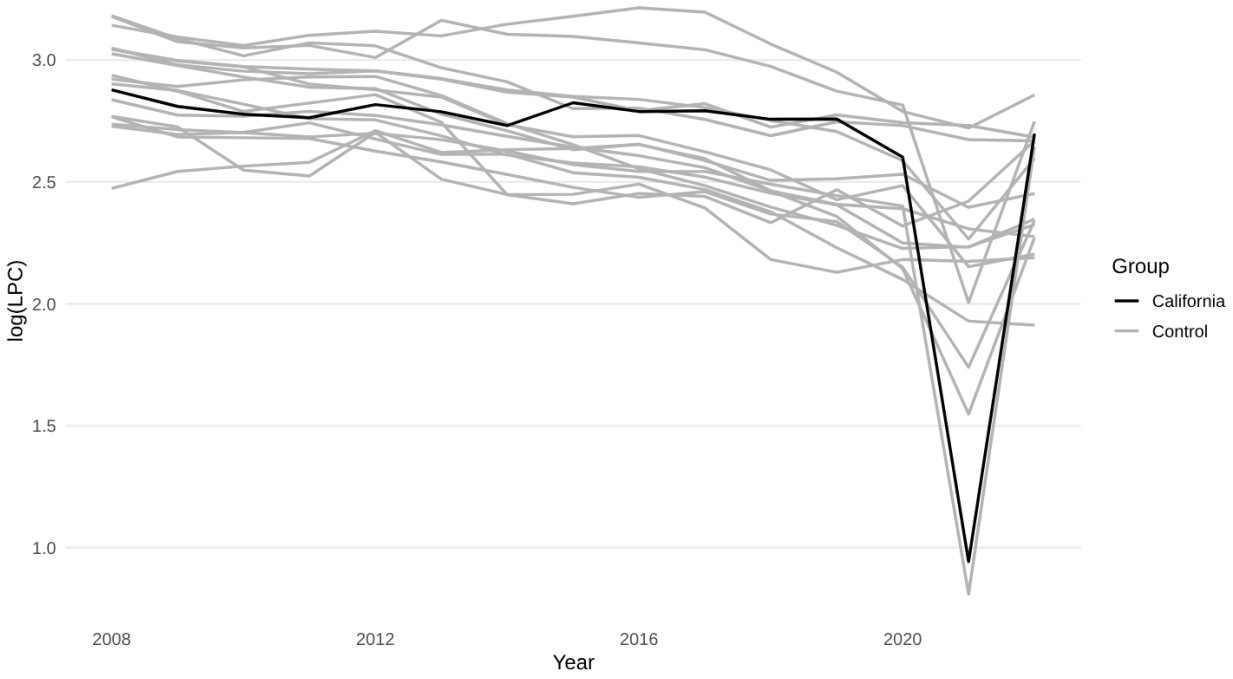


Figure 1: Log of Larceny Per Capita Over Time

I omit the years 2020 through 2022 from the analysis due to the substantial and heterogeneous disruptions caused by the COVID-19 pandemic. These years saw widespread lockdowns, stimulus-induced demand shifts, and sharp changes in enforcement and reporting practices—all of which affect crime and business outcomes independently of Prop 47. Although year and state fixed effects should capture national and local shocks respectively, my models were no longer significant due to the extreme variance within the time span. As depicted by Figure 1, larceny trends diverge sharply and erratically beginning in 2020, particularly in California, underscoring the distortion introduced by including these years. Restricting the analysis to 2008–2019 preserves a cleaner identification strategy, maintaining regular business cycles. Shrinking the post-treatment period also allows for an increase in donor pool units.

4.2 Effects on Larceny

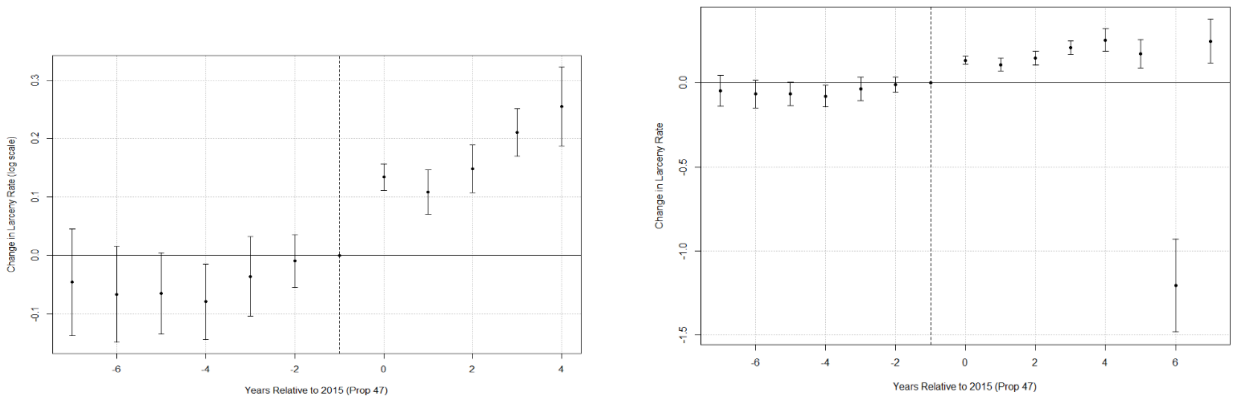
To assess whether Prop 47 increased larceny, I begin by estimating a two-way fixed effects DiD model. This specification compares changes in the log of larceny per capita between California and a control group of 15 states that did not change their felony theft thresholds from 2008 to 2019. The model includes state and year fixed effects, as well as log-transformed retail GDP per capita and minimum wage as covariates. Standard errors are clustered by state.

$$\log(\text{LPC}_{it}) = \alpha + \beta \cdot (\text{Treat}_i \times \text{Post}_t) + \log(\text{GDP\_PC}_{it}) + \log(\text{MW}_{it}) + \gamma_i + \delta_t + \varepsilon_{it} \qquad (1)$$

where $\log(\text{LPC}_{it})$ is the log of reported larceny per capita in state i at time t; $\text{Treat}_i \times \text{Post}_t$ is an interaction term equal to 1 for California in post-treatment years; $\log(\text{GDP\_PC}_{it})$ is the log of state-level retail GDP per capita; $\log(\text{MW}_{it})$ is the log of the state's minimum wage; γ_i and δ_t denote state and year fixed effects, respectively; and ε_{it} is the error term.

The parallel trends assumption—central to the DiD framework—requires that the treated and control units would have followed similar trajectories in the absence of treatment. To evaluate

this assumption, I estimate an event study specification. Figure 2 shows that in the pre-treatment period (2008–2014), all but one unit is statistically indistinguishable from zero, and no visual trend divergence is apparent. This provides support for the plausibility of the parallel trends assumption.



(a) Without COVID (b) With COVID

Figure 2: Event Study Results

I estimate two versions of the DiD model: one including the COVID-19 pandemic years (2020–2022), and one excluding them. As shown in Table 1, the results differ dramatically. When the COVID years are included, the treatment effect on larceny is small and statistically insignificant. In contrast, when limited to 2008–2019, the treatment effect is positive, large, and highly significant. This aligns with expectations, as the COVID-19 pandemic introduced sudden, state-specific shocks to crime, enforcement, and reporting that are orthogonal to Prop 47.[4] I therefore omit 2020–2022 from the main analysis.

Table 1: Larceny Per 1,000 Population

	Without Covid	With Covid
treatment × post	0.197***	0.047

*** p <0.001; Full Table: A.2

[4]temporary business closures, shifts in mobility patterns, and police resource reallocation may have suppressed or distorted larceny reporting during the pandemic years.

To address the limitations of DiD—including its reliance on only two groups and arbitrary treatment periods—I estimate the effect using SCM. This method constructs a weighted average of control states to approximate California's pre-treatment larceny trend. Following Abadie et al. (2010), I use lagged values of larceny per capita in 2008, 2010, and 2012 to inform the donor pool.[5]

SCM relies on the assumption that the synthetic control represents the untreated counterfactual for California, capturing shared, time-varying shocks. As shown in Figure 3, SCM reproduces California's pre-treatment trend well and shows a distinct post-treatment divergence. To validate these findings, I conduct placebo tests by applying the same treatment to each control unit. The resulting distribution of placebo effects confirms that California's post-treatment shift in larceny is statistically unique.[6]

Finally, I estimate the Synthetic Difference-in-Differences (SDiD) model. SDiD combines features of both SCM and DiD, leveraging pre-treatment weights while incorporating unit and time fixed effects. SDiD relaxes the parallel trends assumption and improves robustness to unobserved heterogeneity. Figure 3 shows that the SDiD estimates closely align with those from DiD and SCM, both in magnitude and timing.

Taken together, these three methods converge on the same conclusion: Prop 47 caused a statistically significant and idiosyncratic increase in larceny per capita in California relative to comparable states, consistent with the literature. The consistency across methods strengthens the credibility of this finding.

[5] Weights used to construct the synthetic control, as well as RMSPE, are reported in Table C.3.

[6] See Table A.1 for placebo test results.

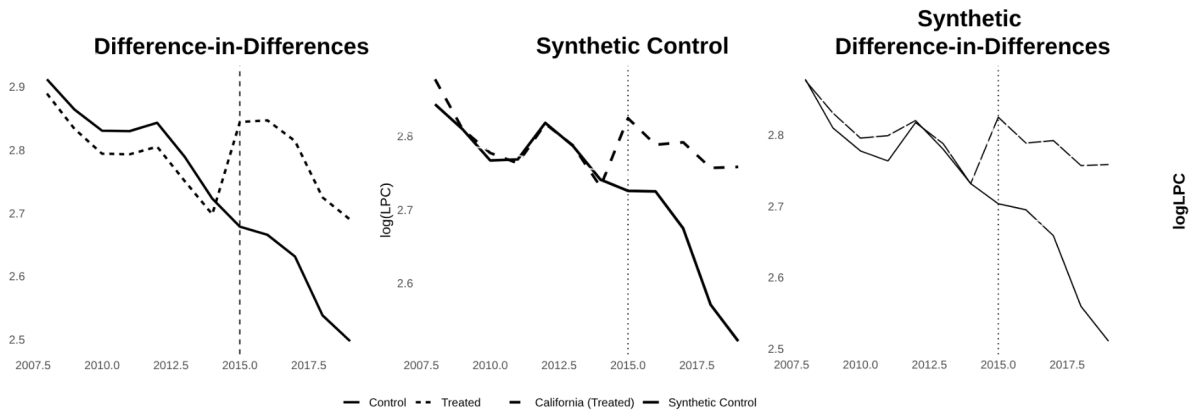


Figure 3: Larceny Per Capita Models

4.3 Effects on Retail Health

To evaluate how Prop 47 may have affected retail health, I use the same empirical strategy as for larceny: a two-way fixed effects DiD model, supplemented by SCM and SDiD. These outcomes—payroll, employment, and retail businesses density—represent different dimensions of firm behavior and labor demand. I estimate each on both a per capita and per establishment basis (excluding business count) to help disentangle compositional effects of population change.

In the SCM and SDiD models where business health is the outcome, I avoid including lagged larceny as a covariate, as it likely lies on the causal path between Prop 47 and retail outcomes. Including it would risk conditioning on a mediator, potentially biasing the estimated total effect. Instead, I include pretreatment covariates such as minimum wage and retail GDP per capita, which are not affected by the treatment. In DiD models, lagged outcomes and pretreatment averages are omitted due to collinearity with the state and year fixed effects.

Table 2: Difference-in-Differences Estimates for Each Outcome

Outcome	Metric	Estimate	Std. Error	P-Value	Significance
Payroll	Per Capita	0.045	0.009	0.000	***
Payroll	Per Establishment	0.030	0.013	0.036	*
Employment	Per Capita	0.027	0.005	0.000	***
Employment	Per Establishment	0.012	0.007	0.093	.
Business	Per Capita	0.015	0.011	0.185	

4.3.1 Retail Business Density

The DiD estimate for retail businesses per capita is small and statistically insignificant[7]. This result is expected given that firm exit does not normally happen in the short run—businesses will often delay closure through cost-cutting, loans, or contractual obligations. Although including data through the COVID period yields statistically significant effects, I attribute these findings primarily to pandemic-related shocks rather than Prop 47. Business closures during 2020–2022 likely reflect lockdowns, demand shifts, and labor disruptions, which makes causal interpretation in this context unreliable.

Furthermore, results from SCM and SDiD are mixed. The models trend in opposite directions post-treatment, weakening confidence in the existence of a stable effect. The placebo test for business per capita is also insignificant ($p = 0.33$). Taken together, there is little evidence of a clear impact of Prop 47 on retail business counts. Acknowledged in tandem, the evidence offers little support for a meaningful impact of Prop 47 on business density.

[7] See Table A.2 and Figure A.3b for estimates with COVID years included.

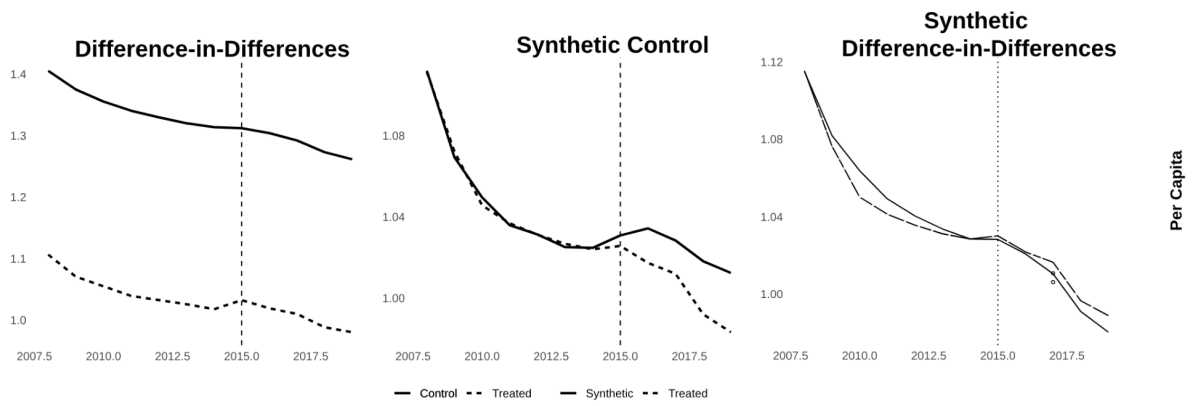


Figure 4: Business Per Capita Models

4.3.2 Payroll

In DiD models, both payroll per capita and payroll per establishment show positive and statistically significant effects, with p-values of 0.00 and 0.036, respectively. This suggests that following Prop 47, businesses may have increased wage spending—potentially in response to an elevated risk of shoplifting.

Both SCM and SDiD models support this trend. The SCM placebo p-value for per capita payroll is marginal ($p = 0.083$), while the per establishment result is much weaker ($p = 0.625$). Pre-treatment trends in the event study are largely flat, with only one year showing a statistically significant difference. This supports the credibility of the parallel trends assumption, although not as strongly as in the larceny analysis.

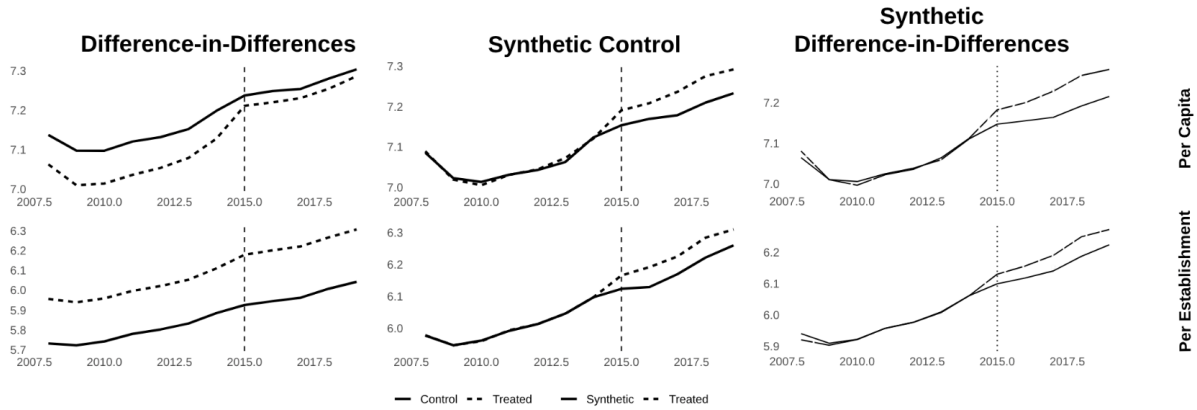


Figure 5: Payroll Models

4.3.3 Employment

DiD estimates show a significant increase in employment per capita (0.027, $p = 0.00$) and a weaker but still directionally consistent increase in employment per establishment (0.012, $p = 0.093$). Although the latter is only marginally significant, the alignment in signs suggests a genuine trend. However, the event study pre-treatment coefficients are more volatile than in the larceny model, underscoring the value of SCM and SDiD in this context.

SCM and SDiD estimates reinforce this finding, with strong upward trajectories post-treatment. The SCM placebo test for employment per establishment and per capita both yield a p-value of 0.00. These results suggest that the treatment effect is statistically distinguishable from spurious changes observed in control units.

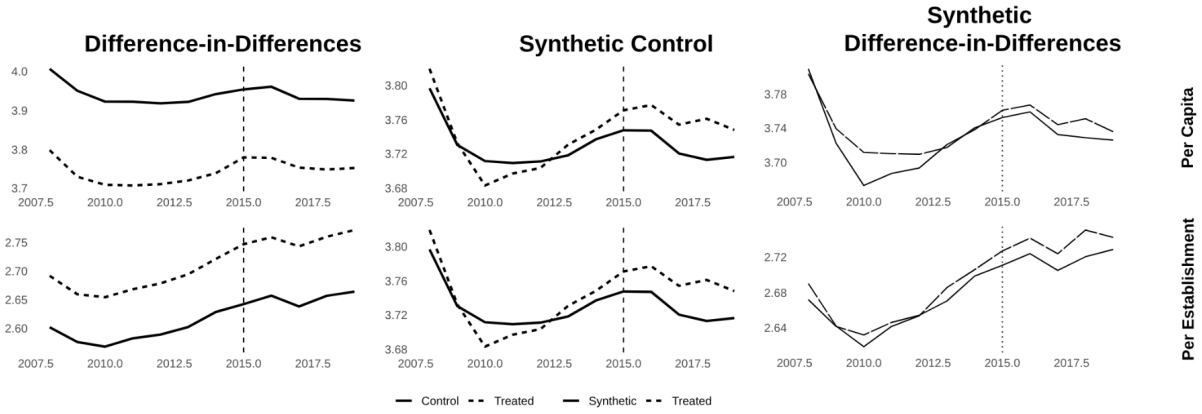


Figure 6: Employment Models

4.3.4 Summary

Overall, the results suggest that Prop 47 may have modestly increased payroll and employment in the retail sector—particularly through per capita measures. These effects are statistically significant in DiD models and supported by SCM and SDiD estimates. However, they do not appear to extend to changes in the business density, where the results are poor and placebo tests show no treatment effect. Interpretation is also complicated by noise in employment pre-trends and by the relatively weak placebo performance in some SCM estimates. As in the case of larceny, the most compelling evidence emerges where multiple methods converge on the same directional finding

and are reinforced by placebo and event study diagnostics[8].

5 Mediation Analysis

5.1 Methodology

This study investigates the causal pathway through which Prop 47 may have affected labor market outcomes in the retail sector. Specifically, the analysis seeks to distinguish between (1) the total effect of Prop 47 on outcomes such as payroll and employment, (2) the indirect effect that operates through changes in larceny, the Average Causal Mediation Effect (ACME), and (3) the direct effect, the Average Direct Effect (ADE), that operates independently of the mediator.

Let T denote the treatment (implementation of Prop 47), M the mediator (the log of reported larceny per capita), and Y business health indicator of choice. Covariates include the state's minimum wage and retail GDP per capita, both of which are assumed to be unaffected by Prop 47 and are included for adjustment.

Following the potential outcomes framework developed by Imai et al. (2010), the indirect and direct effects are formally defined as:

$$\text{ACME} = E[Y(T=1, M(T=1)) - Y(T=1, M(T=0))],$$
$$\text{ADE} = E[Y(T=1, M(T=0)) - Y(T=0, M(T=0))].$$

The sum of these two components yields the total effect of the treatment on the outcome.

Estimation proceeds through two regression models. First, the mediator model regresses larceny on treatment and covariates:

$$M_{it} = \alpha + \beta T_{it} + X_{it}\gamma + \varepsilon_{it},$$

[8]Results for models estimated with COVID data are provided in the appendix, along with covariate trends, event study figures, and SCM weights.

followed by the outcome model, which regresses the labor market outcome on both the treatment and the mediator:

$$Y_{it} = \delta + \theta T_{it} + \zeta M_{it} + X_{it}\eta + u_{it}.$$

Both models include state and year fixed effects to absorb unobserved heterogeneity and time-specific shocks. Standard errors are clustered by state. Inference is based on bootstrapped confidence intervals.

Identification of causal mediation effects requires the assumption of sequential ignorability, which has two conditions: (1) the treatment is independent of potential outcomes and potential mediators conditional on observed covariates, and (2) the mediator is independent of potential outcomes given treatment and covariates. While the first assumption has plausibility in the context of a policy shock like Prop 47, the second is stronger and untestable with observational data.

In addition, I estimate mediation models using both contemporary and lagged measures of the mediator. The use of a lagged mediator is motivated by several considerations. First, it reflects the temporal ordering of the causal pathway; economic outcomes such as payroll and employment may respond to changes in crime with a delay. Second, using a lagged mediator helps avoid potential simultaneity bias that may arise if business and crime respond to a common shock in the same period. Third, the lagged specification offers a more policy-relevant interpretation by modeling how theft increases in one year may influence business behavior in the next. Comparison of contemporary and lagged mediators also serves as a robustness check for the assumed timing of the mediation pathway.

5.2 Results

Table 3 presents the results of the mediation analysis, decomposing the total effect of Prop 47 on retail labor market outcomes into direct and indirect components. The total effects are consistently positive across all outcomes, ranging from 0.019 to 0.051 log units, and align in magnitude with the baseline DiD estimates reported previously.

Table 3: Mediation Analysis Results

Mediator	Outcome	ACME	ADE	Total Effect	Prop. Mediated	p-Value	Significance
Lagged	logPAY	-0.002	0.037	0.035	-0.058	0.636	
Not Lagged	logPAY	-0.001	0.031	0.030	-0.035	0.798	
Lagged	logPPC	-0.003	0.054	0.051	-0.054	0.342	
Not Lagged	logPPC	-0.005	0.050	0.045	-0.111	0.180	
Lagged	logEMPC	-0.008	0.044	0.036	-0.216	0.036	*
Not Lagged	logEMPC	-0.006	0.039	0.032	-0.199	0.248	
Lagged	logEMPE	-0.006	0.025	0.019	-0.314	0.076	.
Not Lagged	logEMPE	-0.006	0.039	0.032	-0.199	0.194	

The ACME is statistically significant only for lagged larceny mediating the effect on log employment per capita (logEMPC), with an estimated ACME of -0.008 ($p = 0.036$). This negative sign suggests that, conditional on treatment, higher lagged larceny is associated with a reduction in employment—consistent with a mechanism where increased theft discourages hiring. The corresponding ADE is 0.044, implying that the bulk of the total effect (0.036) operates directly, not through the larceny channel. The proportion mediated, in this case, is approximately -21.6%, indicating a possible suppression effect, where the indirect and direct effects work in opposite directions.

For all other outcome-mediator combinations, the ACME estimates are small in magnitude and not statistically significant. Notably, the ACME estimates for payroll (both per capita and per establishment) are close to zero and imprecisely estimated, suggesting limited evidence of mediation through larceny for wage-related outcomes.

The comparison between contemporary and lagged mediators reveals that lagged larceny generally produces larger—in absolute value—ACME estimates. This aligns with theoretical expectations: economic outcomes like employment are more plausibly affected by theft with a delay, rather than instantaneously. Among the few models with marginally significant results—such as lagged

larceny on log employment per establishment (logEMPE), $p = 0.076$—the direction is consistent with the logEMPC model, though the statistical strength is weaker.

To assess the robustness of the ACME to violations of sequential ignorability, a sensitivity analysis was conducted. The test is depicted visually in Figure 7. The analysis shows that the ACME for logEMPC is sensitive to modest violations of the sequential ignorability assumption. Specifically, the mediation effect becomes statistically indistinguishable from zero when the correlation between unobserved confounders in the mediator and outcome models exceeds approximately $\rho = \pm 0.2$. This finding does not invalidate the result, but suggests that the evidence for mediation is fragile and should be interpreted with caution.

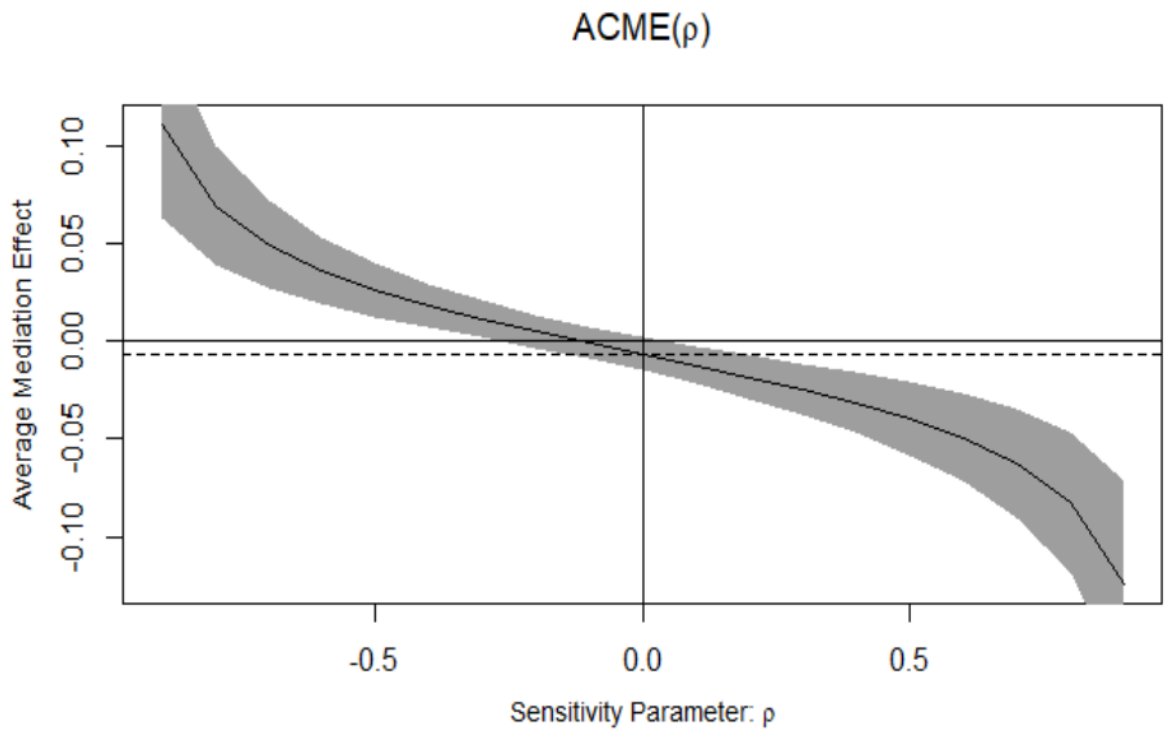


Figure 7: Sensitivity test on logEMPC (Lagged Mediator)

Across outcomes, there is no consistent evidence of mediation via larceny. The indirect effects are neither statistically significant nor robust for payroll or business outcomes, and the only suggestive pattern appears in employment models, particularly when the mediator is lagged. This

variation emphasizes the importance of testing for mediation with sensitivity tools and supports framing these results as exploratory rather than confirmatory.

6 Discussion

The absence of a statistically significant effect on retail business density is consistent with the idea that firm exit typically occurs over longer horizons. Businesses facing rising theft may pursue temporary solutions—such as reducing costs, securing loans, or reallocating staff—before making the difficult decision to shut down. This is particularly plausible in the post-2014 period, which coincided with broader economic recovery and resilience among retailers.

In contrast, the significant increases in payroll and employment suggest a compensatory response within the retail sector. Businesses may have responded to higher theft risk not by exiting, but by increasing investment in human capital through hiring—to increase in-store presence—and by raising wages—to incentivize effort. These findings are consistent with anecdotal evidence of retailers hiring in-house security staff, increasing hours for existing workers who take on deterrence roles, or bolstering floor presence to monitor theft. Indeed, there has been a 20% growth in private security guards across California, and some stores have installed physical barriers to deter theft.[9]

However, mediation analysis reveals that the pathway through larceny is not statistically robust. While employment per capita shows a marginally significant indirect effect through lagged larceny, this result is sensitive to small violations of the sequential ignorability assumption. No clear mediation effect is observed for payroll. This raises an important question: if Prop 47 significantly increased employment and payroll in the retail sector, and this effect is not mediated by larceny, what alternative mechanisms might explain it?

One possibility is that firms responded to the perceived threat of theft, not the true damages. The increased political and media discourse around Prop 47 may have shaped business decisions even before crime trends fully materialized. Retailers might have adopted precautionary labor strategies,

[9] See Schuster (2021)

such as expanding staffing for deterrence or security visibility. Alternatively, Prop 47 may have improved labor market stability by reducing incarceration rates among low-level offenders, thereby increasing the pool of available workers—particularly in communities with high historical imprisonment. Another possible explanation: increases in payroll may reflect better job classification or formalization, rather than genuine economic growth. Finally, it is plausible that Prop 47 had competing effects—simultaneously increasing theft (negative effect) and improving other institutional factors (positive effect)—yielding a net positive direct effect that masks the mediation.

The exclusion of the COVID-19 period (2020–2022) was necessary to avoid confounding from lockdowns, stimulus-driven demand shifts, and disruptions in enforcement and reporting. However, omitting these years limits the post-treatment period to just five years; suboptimal practice for evaluating slower-moving outcomes like business exit. Furthermore, much of the public discourse and law enforcement response to organized retail theft has intensified since 2020, including new felony provisions under Proposition 36 (2024) and $267 million in grants for retail crime enforcement[10]. This study may not fully capture the cumulative or lagged effects of such responses.

Additional limitations stem from data structure. State-level analysis aggregates urban and rural areas, potentially diluting treatment effects. Since retail theft is more concentrated in urban centers, control states with lower urban density may not serve as ideal counterfactuals. County- or city-level data could allow for more granular analysis and better alignment across treated and control units.

Future research should disaggregate businesses by type (e.g., chains vs. independents), size, or industry; consider private security hiring explicitly; and extend the analysis to incorporate post-2022 data as it becomes available. More nuanced designs—such as matching on county urbanization or using firm-level outcomes—may help isolate policy effects in the presence of substantial geographic and economic heterogeneity.

[10] See CA.gov (2025)

7 Conclusion

Importantly, this study's findings should not be interpreted in isolation. Prop 47 achieved its core policy goals: reducing incarceration, cutting correctional costs, and narrowing racial disparities in the criminal justice system. The absence of strong negative economic effects in the retail sector reinforces the idea that it is possible to decarcerate without inducing widespread economic harm.

Nonetheless, the economic effects of Prop 47 are not monolithic. This analysis aggregates across rural and urban counties and omits the COVID-19 pandemic years (2020–2022), which introduced severe disruptions to business operations, consumer behavior, and law enforcement practices. The shortened post-treatment window may limit the detection of long-term business dynamics, such as firm exits. Future research should incorporate county-level data and disaggregate businesses by size and industry to better understand how Prop 47 affected different types of firms and communities.

Since the end of this study period, California has introduced several new policy responses to address retail crime. Proposition 36, passed in 2024, reclassifies certain repeat offenses as felonies. The state has also invested $267 million in grants to enhance retail crime enforcement. Between 2023 and 2024, the California Highway Patrol reported over 6,900 retail-related theft arrests, and 88% of convictions for organized retail theft were felonies. These developments signal a shift in the policy landscape that may interact with or counterbalance the original effects of Prop 47.

In sum, while Prop 47 did not undermine the retail sector in any clear or sustained way, its full economic implications continue to evolve. This study contributes one empirical perspective to a broader policy debate—one that should weigh public safety, fiscal responsibility, and economic equity in tandem.

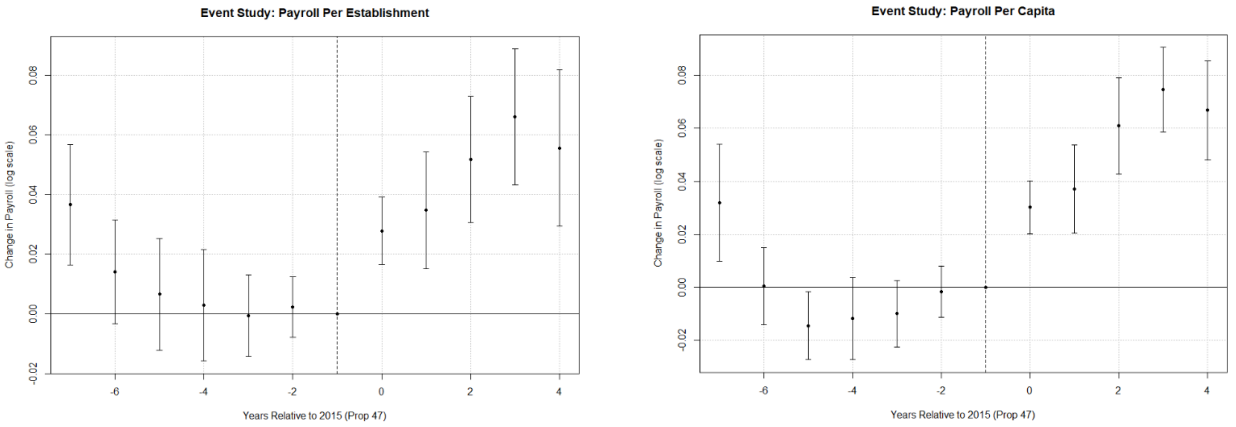
Appendix

A Robustness Checks

Table A.1: SCM Placebo Test Results

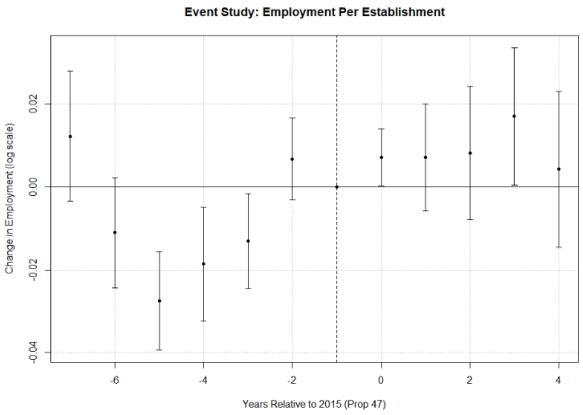
Outcome	Metric	Group Size	Estimate	SE	p-value	Significance
Larceny	Per Capita	8	0.1409	0.0274	0.0000	***
Business	Per Capita	12	-0.0187	0.0281	0.3333	
Payroll	Per Capita	12	0.0517	0.0417	0.0833	.
Payroll	Per Establishment	8	0.0543	0.1113	0.6250	
Employment	Per Capita	15	0.0334	0.0154	0.0000	***
Employment	Per Establishment	14	0.0471	0.0197	0.0000	***

Placebo p-values are based on control units with MSPE ≤ 5× that of California.

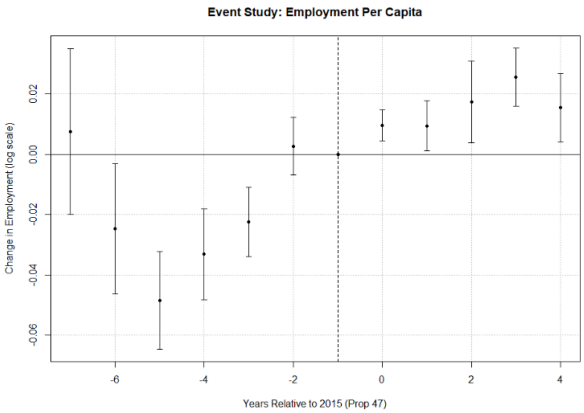


(a) Payroll Per Establishment (b) Payroll Per Capita

Figure A.1: Payroll Event Study Results

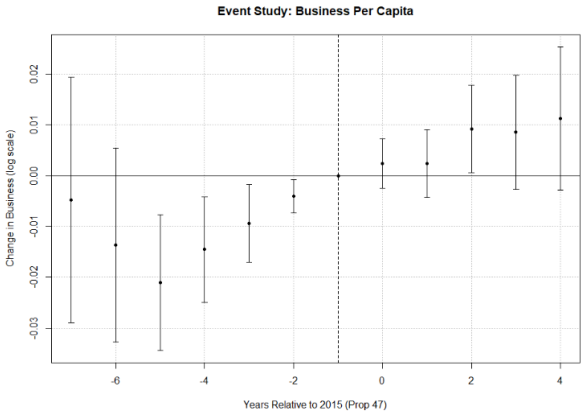


(a) Employment Per Establishment

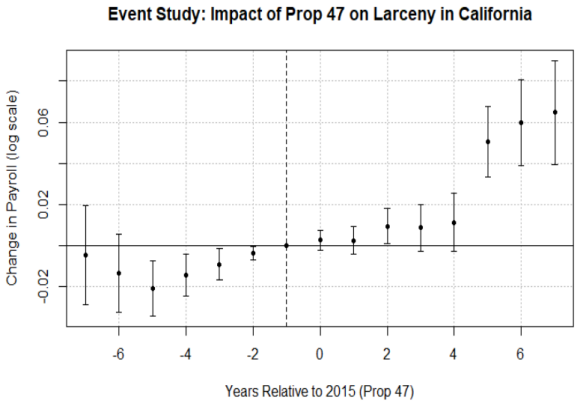


(b) Employment Per Capita

Figure A.2: Employment Event Study Results



(a) Without COVID



(b) With COVID

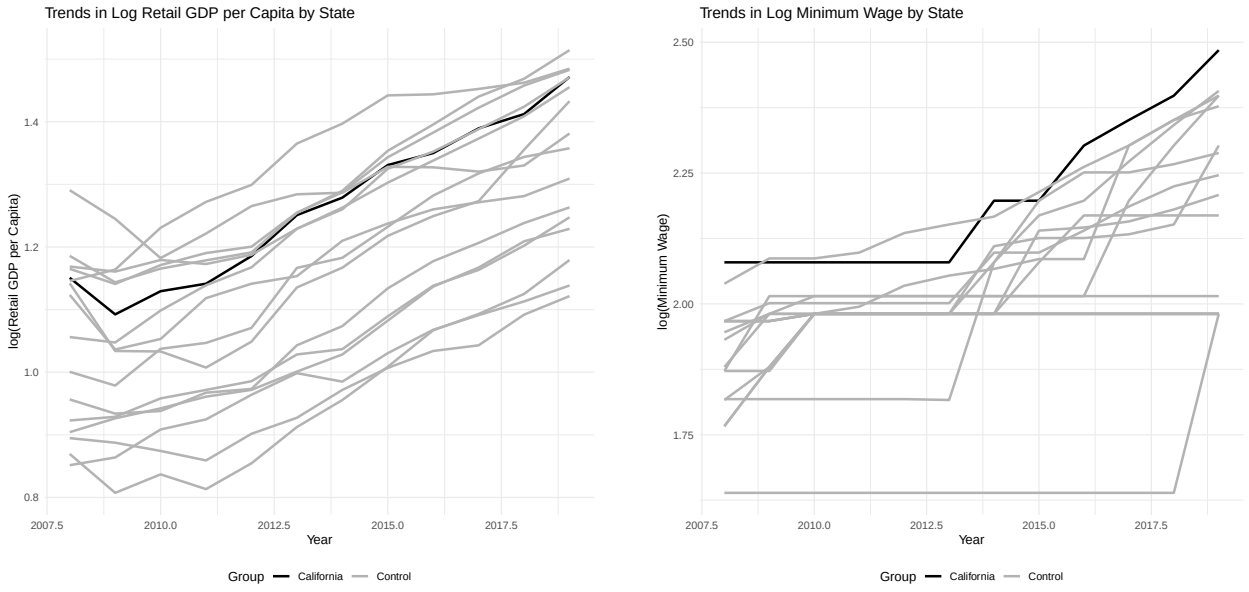
Figure A.3: Business Per Capita Event Study Results

Table A.2: Difference-in-Differences Estimates for Larceny and Retail Business Outcomes

		Larceny per Capita		Businesses per Capita	
		With COVID	Without COVID	With COVID	Without COVID
1.	`logMINWAGE`	0.067 (0.153)	0.176 (0.156)	-0.007 (0.035)	-0.025 (0.036)
2.	`logGPC`	-0.088 (0.412)	-0.010 (0.355)	0.131 (0.085)	0.199 (0.092)
3.	`treatment × post`	0.047 (0.035)	0.197*** (0.036)	0.032* (0.012)	0.015 (0.011)
4.	Num. Obs.	240	192	240	192
5.	R^2	0.761	0.891	0.990	0.993
6.	Adj. R^2	0.724	0.871	0.988	0.992
7.	Within R^2	0.003	0.124	0.089	0.138
8.	Within Adj. R^2	-0.011	0.108	0.075	0.122
9.	Std. Errors (clustered by state)	YES	YES	YES	YES

Standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B Additional Figures



(a) Log Retail GDP Per Capita Trends

(b) Log Minimum Wage Trends

Figure B.4: Covariate Trends Across States

C Synthetic Control Weights

Table C.3: Larceny SCM Donor Weights and RMSPE

State	Weight
Arizona	0.28
Idaho	0.03
Maine	0.02
Michigan	0.02
Minnesota	0.02
New Jersey	0.03
New Mexico	0.01
New York	0.04
North Carolina	0.01
Pennsylvania	0.03
South Dakota	0.25
Vermont	0.02
West Virginia	0.19
Wisconsin	0.02
Wyoming	0.02
Pre-treatment RMSPE	0.01
Post-treatment RMSPE	0.15
RMSPE Ratio	10.90

Table C.4: SCM Weights by State and Outcome

State	PPE	PPC	BPC	EMPC	EMPE
Arizona	0.0344	0.0357	0.0398	0.0329	0.0620
Idaho	0.0150	0.0349	0.0340	0.0387	0.0193
Maine	0.0089	0.0422	0.0477	0.0473	0.0000
Michigan	0.0132	0.0300	0.0294	0.0303	0.0149
Minnesota	0.0141	0.0331	0.0330	0.0323	0.0116
New Jersey	0.0185	0.0394	0.0400	0.0495	0.0268
New Mexico	0.0107	0.0255	0.0257	0.0211	0.0054
New York	0.0178	0.0374	0.0373	0.0432	0.0236
North Carolina	0.0098	0.0249	0.0247	0.0212	0.0058
Pennsylvania	0.0132	0.0305	0.0298	0.0311	0.0149
South Dakota	0.0058	0.2502	0.2550	0.2236	0.0141
Vermont	0.8134	0.0414	0.0470	0.0495	0.7673
West Virginia	0.0125	0.0300	0.0288	0.0319	0.0163
Wisconsin	0.0129	0.0300	0.0296	0.0291	0.0124
Wyoming	0.0000	0.3148	0.2980	0.3183	0.0056

Table C.5: SDiD Weights by State and Outcome

State	BPC	EMPC	EMPE	LPC	PPC	PPE
Arizona	0.105	0.046	0.000	0.135	0.126	0.113
Idaho	0.100	0.163	0.159	0.000	0.129	0.090
Maine	0.067	0.057	0.046	0.044	0.057	0.071
Michigan	0.090	0.061	0.046	0.042	0.000	0.057
Minnesota	0.000	0.068	0.089	0.081	0.043	0.000
New Jersey	0.088	0.122	0.118	0.047	0.059	0.057
New Mexico	0.079	0.000	0.000	0.107	0.099	0.087
New York	0.092	0.093	0.097	0.077	0.046	0.133
North Carolina	0.105	0.117	0.100	0.057	0.098	0.090
Pennsylvania	0.073	0.047	0.000	0.067	0.040	0.081
South Dakota	0.000	0.000	0.000	0.108	0.000	0.000
Vermont	0.000	0.000	0.048	0.000	0.050	0.000
West Virginia	0.000	0.000	0.065	0.095	0.000	0.000
Wyoming	0.040	0.061	0.068	0.044	0.117	0.057

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