

Incentives at Work:

Shaping Individual Behaviors and Institutional Outcomes in Public Policy

by

Dallin Overstreet

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Graduate Supervisory Committee:

Spiro Maroulis, Chair

Sian Mughan

Akheil Singla

ARIZONA STATE UNIVERSITY

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## ABSTRACT

This dissertation examines how incentives shape behavior and institutional outcomes across three distinct domains of American public policy. Through rigorous empirical analysis using quasi-experimental methods, the essays explore unemployment insurance (UI) income disregards, federal UI trust fund lending constraints, and local cannabis regulation in California. The first essay finds that increasing UI income disregards—the amount beneficiaries can earn before benefits phase out—improves rather than weakens trust fund solvency. This occurs as the "stepping stone" effect of facilitating transitions to full employment through part-time work outweighs any expansion in total claims. The second essay evaluates how states strategically responded to stricter federal criteria for accessing interest-free UI loans implemented in 2010. While the reform succeeded in improving overall trust fund solvency, states starting furthest below federal benchmarks disproportionately achieved compliance through benefit restrictions rather than revenue expansion, potentially undermining long-term program stability. The third essay investigates the adoption of retail cannabis ordinances by California cities following state legalization in 2016. Counter to expectations about tax revenue incentives driving adoption, cities with the greatest potential to capture tax revenue from external markets were no more likely to allow cannabis sales. Instead, regional networks appeared to strongly predicted adoption, with cities significantly more likely to adopt when a higher proportion of peers in their League of California Cities division had already done so. Together, these findings demonstrate that while fiscal incentives can effectively shape behavior, their impact is mediated by political constraints, institutional structures, and

networks. The dissertation contributes to scholarly understanding of fiscal federalism while offering practical insights for designing incentive-based policies across different levels of government.

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## CHAPTER 1

### INTRODUCTION

Across the American policy landscape, fiscal incentives serve as powerful levers for shaping the behavior of individuals, organizations, and governmental entities.

Policymakers rely on carefully calibrated incentives—tax breaks, program design parameters, and intergovernmental financing rules—to guide actions toward desired ends. Yet, designing these incentives is challenging. Not only must incentives spur desired behaviors among beneficiaries, but they must also align with complex fiscal and political realities at multiple levels of government. This dissertation explores how fiscal incentives influence policymaking, financial sustainability, and institutional decision-making by focusing on three distinct but thematically linked policy areas: unemployment insurance (UI) income disregards, federal lending constraints on UI trust funds, and local cannabis regulation in California.

The three essays, presented as standalone papers, are united by a core research question: How do incentives shape the behaviors of diverse actors in public policy and administration, and what are the implications for institutional outcomes? Each essay addresses this question in a different policy domain, but together they demonstrate how incentive structures operate in complex, multi-tiered environments to yield both intended and unintended consequences.

Essay One examines how a specific UI work incentive—the income disregard—affects state trust fund solvency. The income disregard allows unemployed individuals to earn a

specified amount before UI benefits begin to phase out. Economic theory suggests two opposing forces. On one hand, higher disregards may encourage claimants to accept part-time jobs and quickly transition to full-time employment (“steppingstone effect”), which would reduce claim durations and ease the strain on trust funds. On the other hand, more generous disregards could expand overall UI usage (“claimant effect”), increasing benefit payouts. Drawing on state-level panel data from 2012–2019 and using multiple quasi-experimental approaches, the essay finds that states that raised their UI income disregard saw a statistically significant improvement in trust fund solvency. These results suggest that, contrary to the fear that greater UI generosity inevitably strains public resources, higher disregards can encourage faster reemployment, ultimately yielding net fiscal benefits.

Essay Two turns from incentives that target individual UI claimants to incentives aimed at state policymakers managing UI trust funds. Following the Great Recession, the federal government tightened access to interest-free loans for states whose UI trust funds ran dry, with the goal of promoting stronger reserve-building and discouraging dependence on federal borrowing (moral hazard). Using several interrupted time series (ITS) model designs, the essay finds that while the 2010 federal reforms did improve overall state UI trust fund solvency, their success was partial and uneven. States that started furthest below the new solvency thresholds may have disproportionately relied on cutting UI benefits—rather than raising revenues—to meet federal guidelines. The study underscores how subnational governments strategically choose among policy levers in ways that fulfill technical requirements but may not fully mitigate long-term fiscal risk.

Essay Three moves away from UI systems to another regulatory domain where economic incentives and intergovernmental complexity collide: local adoption of recreational cannabis ordinances in California. Following voter-approved legalization in 2016, California's cities held broad discretion over whether to permit cannabis businesses. Using event history analysis on all 482 California cities from 2017 to 2023, the essay reveals two notable findings. First, regional networks strongly influence policy adoption. Municipalities in the study are far likelier to adopt if a growing share of their regional peers have done so, underscoring the power of policy learning and imitation in a novel, uncertain domain. Second, I find the size of a city's potential market opportunities from surrounding non-adopting jurisdictions has no influence on adoption status, contradicting the idea that cities leap at the chance to capture cannabis tax revenue from broader markets. Put differently, municipalities do not necessarily prioritize potential short-term economic gains when faced with regulatory, political, and administrative uncertainties; instead, they appear to rely more heavily on regional best practices and peer examples.

Taken together, these three essays highlight the diversity of incentives in American public policy and the varying ways they drive or fail to drive institutional change. Unemployment insurance programs, for instance, reveal a dynamic relationship between federal oversight and state-level autonomy—states respond to incentives, but often do so in ways that reflect local policy preferences. Local cannabis regulation, meanwhile, shows how peer networks can override even significant tax advantages, illustrating that political feasibility, legal risk, and learning from trusted peers can outweigh raw revenue considerations.

Methodologically, the dissertation highlights the value of quasi-experimental approaches—difference-in-differences, interrupted time series, and event history analysis—for disentangling the causal impact of policy changes amidst confounding variables and diverse contexts. Substantively, the findings speak directly to core issues of public administration and policy design. Whether the goal is to encourage reemployment through UI rules, strengthen state trust funds under federal constraints, or spur local adoption of new industries, the results show that incentive-based policies rarely work in isolation. They must be understood as part of a broader ecosystem of political institutions, administrative networks, and public pressures.

As policymakers continue to search for effective, data-driven strategies in areas like unemployment benefits, intergovernmental finance, and emerging industries such as cannabis, the studies in this dissertation offer both cautionary lessons and constructive insights. In many cases, well-designed incentives have the potential to produce desirable behaviors—but only if they are calibrated to account for local constraints, the presence of peer or professional networks, and the interplay of competing incentive structures across different tiers of government.

## CHAPTER 2

### THE IMPACT OF INCOME DISREGARD CHANGES ON STATE UNEMPLOYMENT INSURANCE TRUST FUND SOLVENCY

#### **Introduction**

Unemployment insurance (UI) serves as a crucial safety net for workers who experience temporary job loss, providing them with financial support during periods of uncertainty. The UI system was established in 1935 as part of the Social Security Act to mitigate the adverse effects of unemployment on workers, their families, and the economy. The UI system serves multiple purposes, such as stabilizing aggregate demand during recessions, reducing poverty and hardship among unemployed workers, facilitating job search and matching, and providing incentives for employers to avoid layoffs.

The UI system is funded by both state and federal taxes levied on employers. The state taxes are used to pay for regular UI benefits, which are determined by each state's laws and regulations. State UI tax rates vary depending on the solvency of the state UI trust fund and the experience rating of the employer, which reflects the employer's history of layoffs and UI claims. Federal UI taxes are used to pay for the administrative costs of the UI system, as well as for federal UI programs, such as extended benefits, emergency benefits, and disaster unemployment assistance.

In recent years, and especially during and following the COVID-19 pandemic, state UI trust funds have faced severe solvency challenges. Solvency is a measure of the ability of a state UI trust fund to pay benefits during periods of high unemployment without borrowing from the federal government or other sources. According to the U.S.

Department of Labor's UI Trust Fund Solvency Report (2023), only 16 states had trust fund balances above the recommended minimum solvency standard at the end of 2022. Moreover, 5 states still had outstanding federal loans totaling over \$28 billion as of January 2023. These loans carry interest charges and may impact tax rates for employers in borrowing states, increasing the strain on state UI tax revenues and trust fund solvency.

The partial UI system is a component of the UI system that provides benefits to workers who work less than full-time due to lack of work or other reasons. This differs from the regular UI system in that it allows workers to combine earnings from part-time work with reduced UI benefits, rather than losing their eligibility for benefits if they work at all. With the partial UI system, state programs aim to encourage reemployment by requiring workers to search for work and accept suitable job offers. However, some workers may face difficulties finding full-time work or may prefer to work part-time for various reasons, such as family responsibilities, health issues, or skill mismatch. For these workers, the UI system offers partial unemployment benefits, which allow them to supplement their earnings from part-time work with reduced UI benefits.

A key aspect of the partial UI system is the income disregard policy, which allows claimants to earn a certain amount of income from part-time work without affecting their unemployment benefits. The level of income disregard varies across states, with some states setting a specific dollar amount that is disregarded for all claimants while others disregard a specific percentage of earnings. These different incentive structures created in each state through the income disregard can have implications for the average duration of claims and the number of UI claimants.

This paper investigates the relationship between changes in the income disregard and trust fund solvency in the United States. Specifically, it aims to answer the question of whether increases in income disregard positively impact the solvency of state UI trust funds. The paper develops several hypotheses linking changes in income disregard to trust fund solvency through two mechanisms, including the steppingstone effect and changes in the number of insured unemployed.

To test these hypotheses, the paper employs an empirical analysis using state-level data on income disregard policies and trust fund solvency measures between 2012 and 2019. The analysis uses multiple estimation techniques, including several difference-in-differences (DiD) models and robustness checks to ensure the validity of the results. The primary finding of the paper is that increases in the income disregard positively impact trust fund solvency, a result that is consistent across different model specifications and outcome measures. This finding supports the notion that raising income disregard could be a viable policy option for improving trust fund health.

By providing empirical evidence on the relationship between income disregard and trust fund solvency, this paper makes several key contributions to the literature. First, it offers new evidence that increasing income disregard can strengthen trust fund solvency, adding to the limited prior research on this specific policy lever. The results suggest raising income disregard could be a viable option for policymakers seeking to improve trust fund health. Second, the analysis reveals the importance of considering the income disregard relative to average wages, as higher ratios amplify the positive effects on solvency. This finding provides guidance on how the income disregard policy should be designed for

maximum impact. Third, the paper indicates most states may not have surpassed the optimal phase-out point hypothesized by Munts (1970), pointing to the need for further study of the labor market implications. Finally, by highlighting the data limitations using state aggregates, the paper sets the stage for future work leveraging individual-level data to better understand labor market dynamics and heterogeneity in effects across subgroups.

## **Literature Review**

This literature review examines studies relevant to the topic area of UI trust fund solvency and changes to the income disregard. I begin by outlining the funding structure of the UI program at both the state and federal levels. I then move to a discussion of several explanations for the general trend in insolvency among US state UI systems. I explain shifts in the taxing schemes and their implications for solvency. I detail the effects of the economic and political environments that UI programs operate in on solvency levels. The literature review then moves to a discussion of the effect of changes to benefit generosity and the UI income disregard on solvency levels.

### *UI Program Structure*

Unemployment insurance (UI) is a social insurance program that provides income support to workers who experience involuntary job loss and satisfy certain eligibility criteria, such as active job search. UI operates as a federal-state partnership, and it is financed by payroll taxes imposed on employers at both levels of government (US Department of Labor, n.d).

The Federal Unemployment Tax Act (FUTA) authorizes the federal government to levy a 6 percent tax on the first \$7,000 of each employee's annual earnings. However, employers can receive credits for up to 5.4 percentage points of FUTA taxes paid to states that comply with federal UI standards, resulting in an effective FUTA tax rate of 0.6 percent, or a maximum of \$42 per worker. The FUTA revenues are deposited into the federal UI trust fund, which covers the administrative costs of UI programs, loans to state UI funds, and 50 percent of extended benefits during periods of elevated unemployment (US Department of Labor, n.d.).

States and territories also collect their own UI taxes from employers, with different rates and wage bases across jurisdictions. State UI taxes (SUTA) are collected in state-level UI trust funds, where each state has a dedicated account for paying regular UI benefits.

States can access loans from the federal fund if their own reserves are depleted, but they have to repay them with interest or face higher FUTA taxes for their employers. States can also obtain private loans to replenish their reserves.

The UI taxes collected by both the federal and state governments are earmarked for financing UI benefits and related administrative expenses. They cannot be diverted for other uses, such as general revenue or other social welfare programs. The UI taxes are also nonrefundable to employers or employees, regardless of whether they claim or receive any UI benefits (US Department of Labor, n.d.).

### *Trust Fund Solvency*

One of the main challenges facing the UI system is the inadequate funding of state UI trust funds, which has been examined by various scholars. To measure UI trust fund

solvency, scholars have used several measures, with the Average High-Cost Multiple (AHCM) and the Reserve Ratio being the most common (e.g. Goss & Knudsen, 1999; Guo & Johnston, 2021; Lachowska et al. 2020; Vroman & Woodbury, 2014). The AHCM is defined as the ratio of a state's trust fund balance divided by total wages paid in the state over the past 12 months to the average annual UI benefit payments in its three highest-cost years from the past (Unemployment Insurance Data, n.d.). This measure gives a rough estimate in terms of how many years a state could remain solvent if a recession were to start. Similarly, the Reserve Ratio is defined as the ratio of a state's trust fund balance and the total wages paid in a state over the past 12 months. To address shortfalls in the solvency of state trust funds in terms of AHCM and the Reserve Ratio, scholars have examined various aspects of the UI program, including tax schemes, the effects of economic and political environments of each state, and benefit generosity.

### *Tax Schemes*

The U.S. UI tax system was originally designed to operate on a forward-funded basis, meaning that states were expected to accumulate reserves in their UI trust funds during periods of economic growth and then use those funds to pay benefits to unemployed workers during periods of economic downturn and recession (Goss & Knudsen, 1999). State UI tax rates were designed to reflect the experience rating of employers, meaning that employers who had more layoffs and UI claims would pay higher taxes than employers who had fewer layoffs and claims. For the first four decades of the program's existence, most states followed this forward-funded framework. However, since the late 1970s, many states have shifted to a pay-as-you-go approach, keeping UI taxes

artificially low when the economy is healthy instead of building adequate trust fund reserves for a potential recession (Stone & Chen, 2014).

This approach undermines the optimal functioning of the UI system, which would require taxing employers enough to cover the expected expenditures during an economic downturn. Taxing too much would impose an unnecessary burden on the economy and be politically unpopular, while taxing too little would lead to insolvent trust funds that cannot smooth consumption during a recession and increased reliance on federal loans. In recent decades, it seems that the political incentive to keep tax rates low has prevailed over the fiscal responsibility to fund state trust funds adequately.

#### *Economic and Political Environment*

The economic environment in which UI programs operate have important effects on solvency as well. Long periods of high unemployment rates have had devastating effects on trust fund solvency (Vroman, 2012). For example, during the Great Recession and subsequent years, 35 state UI programs depleted their UI trust funds, necessitating federal involvement through financial loans to the programs (Weiner, 2012). Since this time, states have struggled to climb back to recommended levels of solvency by DOL, especially with the immense strain created by the Covid-19 Pandemic on the UI system (Lachowska & Vroman, 2020; Rhodes, 2021). However, the general insolvency of state trust funds is not only a recent phenomenon, but one that has plagued UI systems for decades. State trust funds have shown a decline in solvency measures since 1947, with clear dips in solvency when unemployment levels increase (Goss & Knudsen, 1999).

The political environment in which UI programs operate in are not inconsequential either. Smith and Wenger (2013) found that states with more solvent trust funds tend to offer higher average and maximum weekly UI benefit amounts to their claimants. This suggests that states with less solvent trust funds may have a weaker commitment to providing adequate income support to unemployed workers, instead depending on federal loans to cover their UI obligations when needed. Patashnik (1997) argued similarly, asserting that solvency issues within the UI system were largely political constructs and a consequence of budgetary politics. As such, the failure to tax sufficiently during economic booms may not only be driven by political constraints but also by ideological differences among states with respect to the importance of UI as a social insurance program.

#### *UI Benefit Generosity*

Scholars have examined how the generosity of unemployment insurance (UI) benefits impacts program insolvency. More generous UI benefits can reduce incentives to search for jobs, leading to longer unemployment spells and additional strain on trust funds, but may also enable better job matches. Specifically, Krueger and Meyer (2002) find that higher benefits decrease the opportunity cost of unemployment, discouraging job search effort. However, Schmieder, von Wachter, and Bender (2012) show that generous benefits allow the unemployed to be more selective, leading to better post-unemployment wages despite longer joblessness. Additionally, Johnston and Mas (2018) demonstrate UI policy changes like shortened benefit duration prompt broader labor market responses. Firms post more vacancies and hire more when potential UI duration decreases. Thus, UI

generosity impacts not just recipients but overall employment levels. While some dispute the link between UI generosity and unemployment (Sjoberg, 2000; Van Ours & Vodopivec, 2008), most scholars agree the program's presence and generosity increase unemployment on average (Krueger & Mueller, 2010; Filiz, 2017; Boone et al. 2021). Abundant evidence suggests more generous benefits create disincentives leading to longer unemployment, straining trust fund solvency, though with possible labor market benefits. Tightening generosity could improve solvency but may have broader impacts. Researchers have sought to discover the 'optimal' UI benefit design that balances these work disincentives. A central focus has been characterizing how benefits should evolve over the course of an unemployment spell to maintain job search incentives while providing insurance. For example, Baily (1978) shows that optimal benefits should decline over time but at a decreasing rate. This reflects the lower opportunity cost of prolonged unemployment. Hopenhayn and Nicolini (1997) similarly find optimal benefits decline, especially for workers with high reemployment probabilities who face greater moral hazard. They argue an optimal policy can be implemented through a sequence of benefits based on spell length. However, Chetty (2008) qualifies these results by incorporating liquidity constraints. He shows that with severe moral hazard, benefits should spike up initially then sharply decline to maintain incentives, unlike the smooth decline in Baily work. Mitman and Rabinovich (2015) build on Baily's findings but show optimal benefits should be mildly procyclical, rising in booms and falling in recessions, to balance stimulus and incentives over the business cycle.

The literature indicates optimal benefits should generally decline over time, but the precise trajectory depends on factors like worker characteristics, labor market conditions, and the degree of moral hazard (Landais et al., 2018). Recent work provides guidance on how to tailor the schedule to achieve the optimal balance between insurance and incentives. These studies highlight the importance of designing UI programs that provide insurance while preserving incentives for the unemployed to actively seek work.

### *The Income Disregard*

The literature on optimal UI design focuses on balancing insurance and incentives for fully unemployed workers. However, a related subprogram called partial unemployment insurance (partial UI) faces similar tradeoffs for workers with reduced hours. Partial UI provides benefits to workers experiencing a significant earnings loss due to reduced hours or intermittent work while job searching (US Department of Labor, n.d.). It aims to mitigate income loss and maintain work incentives by allowing workers to combine part-time earnings and UI benefits. However, partial UI benefits are subject to an income disregard rule, which determines how much of part-time earnings are deducted from the UI benefit. Typically, the disregard allows keeping a percentage or dollar amount of earnings without affecting benefits, before reducing benefits based on earnings above the threshold (US Department of Labor, n.d.). The income disregard aims to balance income support and work incentives for partially unemployed workers, similar to the broader optimal UI literature.

The income disregard threshold in partial UI faces concerns over potential distortionary effects on the labor market by altering work incentives. Specifically, researchers have

examined the impact on the duration of UI claims and strategic working behavior by recipients. Evidence suggests workers respond to the disincentives created by more generous partial UI terms, including higher income disregards. For example, Holen and Horowitz (1974) found increased average weekly benefits and maximum allowable earnings under partial UI led to longer UI durations. Similarly, Le Barbanchon (2016) showed claimants reduce work hours in response to lower benefit reductions from higher disregards. Workers strategically minimize hours to maximize earnings while preserving benefits.

This strategic behavior can increase partial UI usage given expanded eligibility. AitBihiOuali, Bargain, and Joutard (2017) exploited a reform in France that reduced the partial UI eligibility threshold, finding increased entry into jobs just below the new threshold to qualify for benefits. Overall, existing research indicates higher income disregards may prolong claims and encourage strategic employment responses to maximize benefits. This suggests disregards can distort incentives and impact the duration of joblessness among partial UI recipients.

However, at odds with these and other scholars, Kyrya (2010) found that partial unemployment had a positive effect on the return to work, facilitating workers in their transitions between full-time jobs. Kyrya examined workers who took up temporary part-time work between jobs in Finland, and these workers tended to find full employment more quickly than those that did not. Similarly, de Graaf-Zijl et al (2011) found that UI claimants that found temporary part-time work had shorter durations of full unemployment spells, transitioning to full-time work faster than those that did not obtain

part-time work. This type of behavior, known as the steppingstone effect, can serve as a steppingstone to regular employment and speed up the transition from unemployment to full employment. Encouraging unemployed individuals to gain temporary employment and take up partial UI benefits by increasing the amount claimants can earn may lead to lower average durations of claims.

While extensive research examines optimal UI benefit design, less attention focuses on the optimal partial UI income disregard threshold. The disregard faces a similar tradeoff - higher thresholds better insure income but potentially disincentivize work. However, empirical evidence on the optimal disregard level is limited. Muntz (1970) argued the weekly disregard should be 1.5-1.75 times the weekly benefit level to limit work disincentives, but this claim lacked empirical support. Moreover, setting the disregard too high risks transforming partial UI into a wage subsidy program by allowing claimants to earn near-full wages while receiving benefits. This could incentivize fewer work hours and more claims, straining trust funds. The optimal income disregard likely balances returning to work versus disincentivizing additional hours. It might modestly increase claimants, but it would reduce the average duration of claims by maintaining work incentives.

This paper contributes to the literature on UI trust fund solvency and the income disregard by exploring how increasing the amount of income that UI recipients can earn without losing benefits affects the financial stability of the UI system. These findings build on the literature, analyzing the positive and negative implications of the steppingstone and claimant effects on trust fund solvency. Additionally, it may provide

evidence that most states have yet to increase their income disregards beyond Munts' (1970) hypothesized optimal benefit phase-out point. These findings could serve as additional guidance to scholars and policymakers attempting to address the growing concern of UI trust fund insolvency in the US by examining changes to a less explored policy within the UI program.

### **Theory and Hypotheses**

In this section I aim to elaborate on my primary hypothesis. This hypothesis examines the impact of increases in a state's income disregard on the solvency of the trust fund. The potential effect of this change is underpinned by two counteracting influences: the positive steppingstone effect and the negative claimant effect. Both of these effects will be explained in depth in this section, building upon the insights gained from the existing body of literature.

The steppingstone effect, characterized by its positive influence on trust fund solvency, comes into play when changes are made to the income disregard. Such alterations may provide an incentive for a faster return to part-time work, given that it raises the threshold at which a claimant's benefits start to taper off. By increasing the phase-out point, we essentially increase the sum of income an individual can earn, which would be a combination of their UI benefit and the income they earn from part-time labor. The result is a heightened incentive to rejoin the workforce. This could create a faster transition to full-time employment, subsequently reducing the average duration of claims, increase wages and tax revenue, and alleviating the financial burden on the trust fund.

On the other side of the spectrum, the claimant effect exists, which could potentially exert a negative influence on the trust fund. An increase in the income disregard expands the potential earnings a claimant can receive by making a claim, making the UI system marginally more appealing to unemployed individuals. This could potentially include those who might not have considered claiming benefits previously. Consequently, a surge in the number of claimants could occur following an increase in the income disregard.

The interplay between these two effects – the steppingstone effect leading to a decrease in the average duration of claims, and the claimant effect causing a rise in the number of claimants due to an expanded eligibility pool – should form the crux of potential fluctuations in trust fund solvency. If the steppingstone effect prevails over the increase in claimants, state UI trust funds could potentially benefit from an increase in the income disregard. However, if the surge in the number of claimants outweighs the benefits created by the steppingstone effect, trust funds could face escalating pressure following increases in the income disregard.

While the relationships between these competing effects is complex, existing research on similar policy contexts suggests that the steppingstone effect may predominate over the claimant effect. Literature on the Earned Income Tax Credit (EITC) provides compelling evidence for this expectation through several established findings. First, programs that subsidize initial labor market entry have been shown to generate sustained earnings growth over time, as workers develop skills, gain experience, and build labor market attachments that continue to pay dividends well beyond the period of benefit receipt (Dahl, DeLeire, & Schwabish, 2009). Second, work-contingent benefits have

demonstrated substantial positive effects on labor force participation without meaningful negative impacts on hours worked among those already employed (Eissa & Hoynes, 2006). Third, benefit structures that gradually phase out as earnings increase, similar to the income disregard policy, have been found to increase earnings particularly at the lower end of the income distribution, encouraging greater work effort rather than work reduction (Chetty, Friedman, & Saez, 2013). Finally, research specifically examining partial unemployment benefits shows they effectively function as stepping stones to regular employment rather than prolonging benefit dependency (Kyyrä, 2010).

Collectively, these findings suggest that increasing the income disregard would likely accelerate transitions to fuller employment through the steppingstone effect, potentially outweighing any negative effects from increased program participation through the claimant effect. Moreover, research on the EITC indicates that work-encouraging subsidies can generate substantial fiscal benefits through increased tax revenue and reduced reliance on benefit programs (Bastian & Jones, 2021). This suggests that increased income disregards might partially offset their implementation costs through similar mechanisms – as recipients increase their labor market participation and earnings, they contribute more in taxes and may require less support from social programs, improving overall trust fund solvency.

Several confounding influences that vary across states and time will be accounted for in the analysis so as to observe the true causal effect of the increases in the income disregard on solvency. These include considerations that are consistent with the existing literature, such as the unemployment rate, workforce size, average weekly wages, average weekly

benefit amounts, UI taxable wage bases, UI tax rates, eligibility requirements, and demographic factors. Additionally, a large policy change happening throughout the US during this time frame that could potentially confound the estimates of this analysis was the Affordable Care Act (ACA) and Medicaid expansion by states. Buchmueller et al. (2021) tested whether states that expanded Medicaid through the ACA experienced a subsequent increase in unemployment. While they found no evidence of increased unemployment, I control for states that expanded Medicaid eligibility using a dummy variable indicating if a state chose to expand coverage.

### **Data and Methodologies**

The data used in this analysis comes primarily from the Department of Labor (DOL). The DOL collects data from the individual US states with respect to their respective UI programs on a weekly, monthly, quarterly, and annual basis. These reports are published online and are publicly available (Unemployment Insurance Data, n.d.). The DOL reports on statistics such as state unemployment rates, the number of benefit claims experienced by states during a specific time frame, states' taxing schemes, and state UI trust fund solvency measures, among many others.

To assess the solvency of state UI trust funds, this analysis employs quarterly data from the DOL for the period 2012-2019. The data covers all U.S. states, with the key variable of interest being a measure of trust fund solvency used by the DOL, the Average High-Cost Multiple (AHCM). The AHCM is defined as the ratio of a state's trust fund balance divided by total wages paid in the state to the average annual UI benefit payments in its

three highest-cost years from the past (Unemployment Insurance Data, n.d.). Equation (1) shows this calculation more clearly:

$$\frac{\text{Trust Fund Balance} / \text{Total Wages Paid in State}}{\text{Average Annual UI Benefit Payments in 3 Highest Cost Years}} \quad (1)$$

The DOL recommends for states to maintain minimally solvent trust funds, the AHCM ratio should be at least 1.0. In essence, a ratio of 1 indicates that a state would be able to pay benefits at a historically high rate without receiving additional revenue for one year. However, most states tend to fall far short of this recommendation at any given time. This period was chosen because it represents a relatively stable economic phase, following the recovery from the 2008 financial crisis and preceding the Covid-19 Pandemic, which ensures that the denominator of the AHCM remains constant over time. Other outcome variables of interest include quarterly measures of the average duration of claims, the number of claimants claiming benefits, and the average weekly benefit payment. Additionally, an alternative measure of trust fund solvency, the reserve ratio, is used to demonstrate the robustness of my estimates. The reserve ratio is equal to the numerator of the formula used to calculate the AHCM.

The main explanatory variable of interest is the income disregard. The income disregard is the amount of earnings a UI benefit claimant may earn while still qualifying for his full benefit amount. States have alternative structures to their income disregard, with some states specifying a specific amount of earnings that will be disregarded, while others a percentage of a claimant's wages that will be disregarded. Additionally, some states taper

benefit reductions more slowly than others, with some states reducing a claimant's benefit amount dollar for dollar past the income disregard while others may reduce the benefit by 70 cents per dollar. This makes the measurement and comparison of income disregards between states and the magnitude of changes somewhat difficult.

To address this difficulty, I use two measures to identify changes in a state's income disregard. The first is calculated as the difference between the amount of earnings in partial employment that reduces a claimant's benefit amount to zero dollars and the maximum benefit amount a claimant may receive in a state. Calculating the income disregard in this manner standardizes it across states and allows me to observe marginal changes, although it does mask some of the differences between states in terms of their calculations of the income disregard. An alternative measure of changes to a state's income disregard used is a binary indicator, indicating whether a change to a state's income disregard occurred during any given quarter. During this time frame, 10 states changed their income disregard to some degree while 33 states or territories experienced no changes to their income disregard. However, of the 10 that changed their policy, Michigan, Montana, and North Carolina either had an ambiguous change to their income disregard or decreased it. Because of the uncertainty of the change or the decrease in the income disregard, I have dropped those states from the sample. This leaves 7 treated states and 33 control states or territories. Table 1 shows descriptive statistics for relevant variables for this paper.

*Difference-in-differences with Binary Treatment Variable*

To estimate the causal effect of the increase in the income disregard on the outcome measures of interest, I start with a difference-in-differences (DiD) model. For this model, I use the binary treatment variable that indicates if a state increased their income disregard, as shown in equation (2).

$$Y_{it} = C + \beta^{bin}T_{it} + \delta X_{it} + \alpha_t + \gamma_i + \epsilon_{it} \quad (2)$$

Where  $Y_{it}$  is the trust fund solvency ratio for state  $i$  at time  $t$ ,  $T_{it}$  is a dummy variable that indicates whether state  $i$  increased their income disregard at time  $t$ ,  $\beta^{bin}$  is the coefficient of interest which measures the average effect of any increase in the income disregard on trust fund solvency,  $X_{it}$  is a vector of control variables,  $\alpha_t$  is a time fixed effect that captures any common factors that affect all units at time  $t$ ,  $\gamma_i$  is a state fixed effect that captures any unobserved characteristics of state  $i$  that do not vary over time, and  $\epsilon_{it}$  is the error term.

Table 1. Descriptive Statistics

| VARIABLES                                          | (1)<br>N | (2)<br>mean | (3)<br>sd | (4)<br>min | (5)<br>max |
|----------------------------------------------------|----------|-------------|-----------|------------|------------|
| Average High-Cost Multiple                         | 1,280    | 0.797       | 0.617     | 0          | 2.510      |
| Reserve Ratio                                      | 1,280    | 0.0358      | 0.0323    | 0          | 0.192      |
| Income Disregard                                   | 1,280    | 145.2       | 87.37     | 0          | 466        |
| Income Disregard Increased                         | 1,280    | 0.0984      | 0.298     | 0          | 1          |
| Average Duration of Claims (Past 12 Months)        | 1,280    | 15.45       | 2.454     | 9          | 22.30      |
| Total Claimants Compensated                        | 1,280    | 48.40       | 75.00     | 0.842      | 573.8      |
| Total Benefits Paid (000)                          | 1,280    | 175,021     | 269,057   | 2,992      | 1,829,000  |
| Average Weekly Benefit                             | 1,280    | 327.8       | 70.44     | 196.8      | 599.0      |
| Underemployment Rate                               | 1,280    | 3.721       | 1.124     | 1.500      | 7.500      |
| Unemployment Rate                                  | 1,280    | 5.034       | 1.791     | 1.600      | 12.30      |
| Average Weekly Wage                                | 1,280    | 953.7       | 185.2     | 678        | 1,742      |
| Average Tax Rate on Taxable Wages (Past 12 Months) | 1,280    | 2.345       | 1.167     | 0.430      | 6.660      |
| Taxable Wage Base                                  | 1,280    | 18,101      | 11,052    | 7,000      | 52,700     |
| Trust Fund Balance (000)                           | 1,280    | 773,340     | 931,623   | 0          | 5,055,000  |
| Log (Weeks Compensated)                            | 1,280    | 12.42       | 1.226     | 9.175      | 15.70      |
| Maximum Benefit                                    | 1,280    | 438.2       | 111.0     | 247        | 794        |

### *Difference-in-Differences with Continuous Treatment Variable*

The second approach I use to estimate the effect of increases in the income disregard on trust fund solvency is a difference-in-differences model with a continuous treatment variable. I use the first measurement of the income disregard mentioned in the prior section that standardizes the income disregard across states. This approach allows me to exploit the variation in income disregard levels and timing across states and over time, and to account for unobserved state and year-fixed effects. The continuous treatment variable is the amount of earnings that a UI claimant can disregard without affecting their weekly benefit amount, which varies by state and year. This model is estimated in equation (3).

$$Y_{it} = C + \beta^{cont} ID_{it} + \delta X_{it} + \alpha_t + \gamma_i + \epsilon_{it} \quad (3)$$

Where  $Y_{it}$  is the trust fund solvency ratio for state  $i$  at time  $t$ ,  $ID_{it}$  is the continuous treatment variable that measures the dollar amount of income disregard for state  $i$  at time  $t$ ,  $\beta^{cont}$  is the coefficient of interest which measures the average effect of a one dollar increase in the income disregard on trust fund solvency,  $X_{it}$  is a vector of control variables,  $\alpha_t$  is a time fixed effect that captures any common factors that affect all units at time  $t$ ,  $\gamma_i$  is a state fixed effect that captures any unobserved characteristics of state  $i$  that do not vary over time, and  $\epsilon_{it}$  is the error term.

While the continuous treatment specification offers several advantages over the binary approach—such as accommodating cases where all units have some level of treatment (as most states have income disregards greater than \$0) and allowing for treatment effect

heterogeneity across different values of the treatment variable (Callaway & Sant'Anna, 2021)—both standard two-way fixed effects DiD models still face important methodological limitations when treatment timing varies across units. Goodman-Bacon (2021) demonstrates that with staggered treatment adoption, the two-way fixed effects estimator represents a weighted average of all possible two-group/two-period DiD estimators, with some comparisons potentially using already-treated units as controls. De Chaisemartin & D'Haultfœuille (2020) further show that when treatment effects are heterogeneous across time or units, some of these weights can be negative, potentially biasing the overall treatment effect estimate. The standard DiD approaches also rely on a parallel trends assumption that may be violated in settings with complex treatment patterns. To address these methodological concerns and provide robust causal estimates, I employ two advanced difference-in-differences methods that specifically account for heterogeneity in treatment timing and effects: Synthetic Difference-in-Differences and the Sant'Anna-Calloway approach.

### *Synthetic Difference-in-Differences*

To provide additional robustness to my findings, I use a synthetic difference-in-differences (SDiD) model following Arkhangelsky et al. (2021). This model builds on the insights of the synthetic control method and the staggered difference-in-differences method to account for both time-varying and unit-specific unobserved confounders. Unlike the staggered DiD model, the SDiD model does not require a large number of treated and control units. Instead, the SDiD model constructs a synthetic control group for each treated unit using a data-driven weighting scheme, and compares the outcomes

of the treated units and their synthetic counterparts over time. This model is shown in equation (3).

$$Y_{it} = \beta W_{it} + \gamma_i v_t + \delta X_{it} + \epsilon_{it} \quad (4)$$

Where  $Y_{it}$  is the trust fund solvency ratio for state  $i$  at time  $t$ ,  $W_{it}$  is a dummy variable indicating if a state  $i$  is treated in time  $t$ ,  $\beta$  is the average treatment effect of an increase in the income disregard,  $\gamma_i$  is a vector of latent time factors,  $v_t$  is a vector of latent unit factors,  $X_{it}$  is a vector of control variables, and  $\epsilon_{it}$  is the error term.

The SDiD estimator improves on the traditional difference in differences (DiD) model by matching on the observed trends of the outcome measure instead of relying on the parallel trends assumption. This way, it ensures (if computationally possible) that the treated and synthetic groups have similar pre-treatment trajectories. I use the aggregation schemes proposed by Arkhangelsky et al. (2021) to summarize the overall effect of the policy change using a binary treatment indicator.

The SDiD estimator also accounts for potential endogeneity and selection bias, which can arise from the non-random assignment of policies and unobserved heterogeneity across units. It constructs a synthetic control unit for each treated unit by using a weighted average of control units that have similar pre-treatment characteristics and trends. It also exploits the variation in treatment timing across units to capture dynamic treatment effects and improve the precision of the causal estimate.

The SDiD estimator is based on a latent factor model that allows for both observed and unobserved confounders to affect the outcome. The latent factor model decomposes the

outcome measure into two additive parts: one from the common latent factors that capture the common structure or patterns of the data, and one from the idiosyncratic components that capture the unit-specific deviations from the common factors. The SDiD estimator estimates the latent factors and their loadings by using a low-rank matrix approximation method, without requiring a large number of potential control units or a common pre-treatment period. Results can be seen in Columns 5 and 6 of Table 2 for these estimates.

#### *Sant'Anna-Callaway Difference-in-Differences*

Lastly, I employ the Sant'Anna and Callaway DiD method, which addresses some of the limitations of the standard DiD approach (Callaway & Sant'Anna, 2021). This method explicitly accounts for heterogeneity in treatment timing by estimating group-time average treatment effects ( $ATT(g,t)$ ), which represent the treatment effect for group  $g$  at time  $t$ :

$$ATT(g, t) = E[Y_t(g) - Y_t(0) | G = g] \quad (5a)$$

Where  $ATT(g,t)$  is the treatment effect for units first treated in period  $g$  (the cohort) observed at time  $t$ ,  $Y_t(g)$  is the potential outcome at time  $t$  for units treated at time  $g$ ,  $Y_t(0)$  is the potential outcome had units never been treated, and  $G=g$  indicates the group of units first treated in period  $g$ . These group-time effects can then be aggregated into meaningful summary parameters using appropriate weights:

$$\theta_S = \sum_{g \in \mathcal{G}} \sum_{t \geq g} w_{g,t} \cdot ATT(g, t) \quad (5b)$$

Where  $w_{g,t}$  are weights used to aggregate the group-time effects into an overall treatment effect. The Sant'Anna and Callaway DiD method allows for multiple treatment groups and staggered treatment adoption, making it suitable for analyzing complex policy changes with varying implementation dates across states. This approach addresses key limitations of traditional two-way fixed effects models: it never uses already-treated units as controls for later-treated units and avoids the negative weighting problem that can arise when treatment effects are heterogeneous over time. Additionally, this method provides estimates of the average treatment effect for different subgroups and over different time periods, offering a more detailed understanding of the heterogeneous impacts of income disregard changes on trust fund solvency. Columns 7 and 8 of Table 2 show these estimates.

## **Results**

Table 2 shows estimates for the four methods mentioned above for the main outcome variable of interest, the AHCM, and the alternative measure of trust fund health, the reserve ratio. The results are positive and statistically significant at least at the 10% significance level in all eight models. Models 1 and 2 estimate the marginal average treatment effect on the treated (ATT) states that increased their income disregard between 2012-2019 on the solvency outcome measures, using the continuous measure of changes in states' income disregard amounts. Alternatively, Models 3 through 8 estimate the total ATT on these states using the binary measure of changes in states' income disregard amounts. The coefficient on the total effect size of the policy change on the AHCM are quite similar across the 4 models, averaging about 0.4. However, the effect size on

AHCM for the Sant'Anna-Calloway DiD estimator comes in at about double the average size of the others at 0.636 and is statistically significant at the 1% level. The coefficient on the total effect size of the policy change on the Reserve Ratio is more uniform across models, averaging about 0.0188. Figures 1 and 2 show the results of the SDiD models graphically while figures 3 and 4 show the results of the Sant'Anna-Calloway model.

Table 2. The Effect of Increases in Income Disregard on Solvency

|                                | Continuous Treatment DiD         |               | Binary Treatment DiD             |                  | Synthetic DiD                    |                  | Sant'Anna-Calloway DiD           |                  |
|--------------------------------|----------------------------------|---------------|----------------------------------|------------------|----------------------------------|------------------|----------------------------------|------------------|
|                                | (1)                              | (2)           | (3)                              | (4)              | (5)                              | (6)              | (7)                              | (8)              |
|                                | Average<br>High-Cost<br>Multiple | Reserve Ratio | Average<br>High-Cost<br>Multiple | Reserve<br>Ratio | Average<br>High-Cost<br>Multiple | Reserve<br>Ratio | Average<br>High-Cost<br>Multiple | Reserve<br>Ratio |
| Income Disregard<br>(Total)    |                                  |               | 0.289*                           | 0.0135*          | 0.362**                          | 0.0211*          | 0.636***                         | 0.0219***        |
|                                |                                  |               | (0.159)                          | (0.00733)        | (0.183)                          | (0.0101)         | (0.197)                          | (0.0111)         |
| Income Disregard<br>(Marginal) | 0.00326***                       | 0.000136***   |                                  |                  |                                  |                  |                                  |                  |
|                                | (0.000743)                       | (0.0000394)   |                                  |                  |                                  |                  |                                  |                  |
| Observations                   | 1280                             | 1280          | 1280                             | 1280             | 1280                             | 1280             | 1280                             | 1280             |

Note: Control variables used in each model include the unemployment rate, maximum weekly benefit amounts, average weekly benefit amounts, benefit eligibility stringency, average weekly wages, amount of covered employment, insured unemployment rate, average tax rate on the taxable wage base, the taxable wage base, Medicaid expansion, and several demographics measures, including age and race.

Note: Robust standard errors in parentheses for Models 1-4. Bootstrap standard errors in parentheses for Models 5-8.

\* p<0.10, \*\* p<0.05, \*\*\* p<0.01

The results from Table 2 appear to confirm my hypothesis that a change in the income disregard affects trust fund solvency. For two different measures of trust fund health and changes in the income disregard and across four different model specifications, I obtain similar, positive, and statistically significant estimates for the effect of an increase in a state's income disregard. Additionally, after adjusting the estimated marginal effect of models 1 and 2 by multiplying the average increase in the Income Disregard amount for treated states by the coefficients, the results are strikingly similar to the binary treatment indicator models. Interpreting these results, the increases implemented by these seven states from 2012-2019 appear to have increased their AHCM by 0.4 on average, all else equal. This increase means that these states on average would remain solvent for roughly 4 months longer if a recession were to start due to their adjustments to their income disregard amounts.

These results indicate that the increase in the income disregard in the treated states created a positive impact on trust fund solvency. Figures 4 through 7 provide graphical representations of the effect sizes of the policy changes for the SDiD and Sant'Anna-Calloway DiD estimators. All four of these graphs are particularly helpful in ensuring the parallel trends assumption holds, with visual inspection of these figures showing relatively parallel trends and diagnostic tests further providing confirmation.

Additionally, figure 7 presents point estimates with confidence intervals for both the pretreatment and posttreatment time periods. Based on these figures, the full effect of the increases in the income disregard appears to phase in over several quarters, with the full

impact not being felt for several years after implementation. At this point, the treatment effect appears to then stabilize.

These graphs also show the observed effects may be driven by four of the treated states. Oregon and Vermont appear experienced the effect more significantly with statistical significance in both model types while Minnesota and Alabama only appear to have discernible effects in the Sant'Anna-Calloway estimates. These differences in effect sizes may be due to whether the steppingstone effect or the claimant effect won out in each state.

Descriptive evidence appears to support the idea that the positive steppingstone effect may have overpowered the negative claimant effect in these four states. Figure 8 shows trends in the average duration of claims across treated states compared to those in the control group. Oregon, Vermont, and Alabama have somewhat noticeable departures from prior trends post-treatment and decrease relative to the control group, while Minnesota is less clear. Oregon quite clearly shows a large decrease in the average duration of claims after the intervention for several years in comparison to the control group. In the eight quarters following the policy change, Oregon's average duration dropped 10.3% compared to the control group's decrease of only 5.4% during the same time period. Alabama, Minnesota, and Vermont all similarly exhibit at least one sharp drop in the average duration of claims in their charts several years post-treatment at the same period the effect size is at the maximum effect. Similarly, figure 9 shows trends in the number of claimants by treated state compared to those in the control group. These charts are less remarkable, lacking any strong indication that the claimant effect was

significant in any of these states with no noticeable increases in claimants paid post-treatment.

### *Why the Heterogeneity in Effect Size?*

The difference in effect size of treatment by state deserves additional explanation. As can be seen in Figures 4 through 7, several states appear to be driving the results of an increase in trust fund solvency. Some heterogeneity is to be expected, given the difference in treatment intensity between states. While Vermont increased their income disregard by about \$300, Alabama only increased theirs by about \$70. Accordingly, Vermont's effect size was quite large while Alabama's was small and less statistically significant.

Additional heterogeneity may be explained by the different structures of the states' income disregard policies. As mentioned before, some states tie their income disregards to certain percentages of wages earned while others set absolute dollar limits. Similarly, states have differing speeds of benefit phase-outs, with some states reducing the weekly UI benefit dollar for dollar beyond the income disregard threshold while others reduce it by 50 cents per dollar.

Lastly, considering a state's ratio of income disregard to average weekly wage may help explain residual heterogeneity in treatment effect size between treated states. The ratio of the income disregard to the average weekly wage in a state should have a direct impact on a potential UI claimant's decision to claim benefits. Because states vary quite dramatically in terms of average earnings, an income disregard of \$200 in a high-earnings

state may not have the same draw to part-time workers as it would in a low-earnings state. With a higher percentage of the average weekly wage available to them if they were to work part-time without reducing their UI benefit amount, a claimant becomes more and more incentivized to take up a part-time job to maximize his income.

As a specific example, prior to Vermont's increase in their income disregard in the third quarter of 2014, the state's ratio of income disregard to average weekly wage was 15.88%, with an income disregard equivalent to \$127.50 and an average weekly wage of \$803. After the increase, this ratio jumped to 53.96%, with an income disregard equivalent to \$436 and an average weekly wage of \$808. In stark contrast, Alabama increased their income disregard to average weekly wage ratio from 1.83% to 10.72% by increasing their income disregard from \$15 to \$83.33. As shown in figures 1 and 3, Vermont experienced a sharp increase in solvency after the increase, while Alabama experienced a small to negligible increase in solvency post-increase.

While I control for a state's average weekly wage in models 1 through 8, inserting this ratio as the independent variable of interest in the model further illuminates this effect. Table 3 shows these results. A one-percentage-point increase in the income disregard to average weekly wage ratio produced a 0.0276 unit increase in the AHCM, all else constant ( $p < 0.01$ ). However, model 2 in Table 3 shows a more quadratic relationship between the ratio and trust fund solvency, with the squared ratio obtaining statistical significance at the  $p < 0.01$  level while the linear relationship loses significance. This may help in explaining why Oregon and Vermont benefitted so much from their increase in the income disregard relative to the other treated states.

Table 3. Effect of the Income Disregard to Average Weekly Wage Ratio on Solvency

|                                                              | (1)<br>Average High-<br>Cost Multiple | (2)<br>Average High-<br>Cost Multiple |
|--------------------------------------------------------------|---------------------------------------|---------------------------------------|
| Income Disregard to Average Weekly Wage Ratio                | 0.0283***<br>(0.00315)                | -0.0002<br>(0.0045)                   |
| (Income Disregard to Average Weekly Wage Ratio) <sup>2</sup> |                                       | 0.00046***<br>(0.00009)               |
| Observations                                                 | 1280                                  | 1280                                  |

Note: Control variables used in each model include the unemployment rate, maximum weekly benefit amounts, average weekly benefit amounts, benefit eligibility stringency, average weekly wages, amount of covered employment, insured unemployment rate, average tax rate on the taxable wage base, the taxable wage base, Medicaid expansion, and several demographics measures, including age and race.

Note: Robust standard errors in parentheses.

\* p<0.10, \*\* p<0.05, \*\*\* p<0.01

### *Goodman-Bacon Decomposition*

While the statistical significance of the four separate model specifications for both outcome measures of trust fund solvency gives credibility to the reliability of these estimates, several other robustness checks exist to ensure they are not biased. One such robustness check is the Goodman-Bacon decomposition test, which can be used to assess the validity of the parallel trends assumption in difference-in-differences (DiD) analyses (Goodman-Bacon, 2021).

The parallel trends assumption is a key requirement for DiD estimation, as it posits that, in the absence of the treatment, the treated and control groups would have followed the same trend over time. The similarity of the results between models, especially with the SDiD model, help to assuage these concerns, but the Goodman-Bacon decomposition test helps to verify this assumption by breaking down the overall DiD estimate into a

weighted average of two-way fixed effects estimates for each possible pair of treated and control groups. By examining these individual contributions to the overall estimate, researchers can identify potential deviations from the parallel trends assumption and ensure that the main results are not driven by a specific pair of groups or time periods.

Figure 10 shows the graphical results of the Goodman-Bacon Decomposition on my main outcome measure of interest, the AHCM. The results show three types of two-group comparisons: a group treated later serving as the control group for an earlier treatment group; a group treated earlier serving as the control group for a later treatment group; and a group which never receives the treatment serving as the control group.

The chart presents the weights assigned to each group when computing the DiD estimate. Intuitively, the group that receives the highest weight in the estimate is the one with the conventional control group, which is comprised of states that are never treated. This group accounts for approximately 82.2% of the weight, with a coefficient of 0.289. The remaining two groups collectively contribute around 17.8% to the average treatment effect, with coefficients marginally higher and lower than that of the first control group. The similarity in the coefficients suggests that the choice of control unit construction does not significantly influence the estimate. Furthermore, it indicates that the standard control group is not overshadowed by a less justifiable control group, thereby reinforcing the reliability of the overall DiD estimate.

Implementing the Goodman-Bacon decomposition test helps ensure that the estimates derived from the four separate model specifications are reliable and unbiased. By conducting this robustness check, the estimates calculated earlier have been given a more

comprehensive and accurate assessment of the relationship between income disregard changes and trust fund solvency, ultimately contributing to more informed policy discussions and decisions.

### **Discussion of Findings**

The empirical analysis conducted in this paper offers valuable insights into the relationship between changes in income disregard and trust fund solvency. The evidence uncovered in this paper lends support to the theoretical framework presented earlier. In this section, I discuss how the empirical results reconcile with the proposed theory and highlight areas where the evidence may pique several additional new questions.

The primary finding of this paper is that increases in income disregard positively impact trust fund solvency. Across different model specifications and outcome measures, the analysis consistently shows that states that increased their income disregard between 2012-2019 experienced a statistically significant improvement in their trust fund health. Specifically, these states increased their Average High-Cost Multiple (AHCM) by 0.4 on average, implying that they could remain solvent for about 4 months longer in the event of a recession. This result is in line with the theoretical expectation that increasing income disregard would bolster trust fund solvency.

The increases in the income disregard by treated states appear to have reduced the average duration of claims while having negligible effects on the number of claimants claiming UI benefits. Four of the states appear to have been more strongly affected by the policy changes, with Vermont and Oregon being the two most strongly impacted. While

the absolute size of the increase in the income disregard appears to have an effect on solvency, it also is important to consider the ratio of the income disregard to the average weekly wage. States with higher ratios seem to benefit more significantly from such increases, perhaps driven by the incentives unemployed workers face when claiming benefits and considering part-time employment. This effect seems to grow faster as the ratio increases as well.

An implication of these findings could be that US states have yet to surpass Munts' (1970) hypothetical optimal phase-out point of UI benefits. While Munts theorized, without empirical evidence, of an optimal phase-out point of UI benefits between 1.5 and 1.75 times the weekly benefit amount awarded to each claimant, Vermont appears to have increased their solvency levels by increasing their phase-out point from about 0.4 to about 1.4. It is possible, perhaps, if Vermont were to increase their income disregard much more, they may begin to negatively affect solvency, given the trade offs discussed. Additional research should to be done to understand more about the optimal phase-out point given it appears no state has yet reached this point. Additionally, if the steppingstone effect is outweighing the negative claimant effect as this analysis appears to show, it may be worthwhile to investigate if the increase in solvency is due in part to increased tax revenues from increased employment.

### *Limitations*

This analysis is not without limitations. While I believe I have addressed many of the potential issues by providing multiple estimates using various empirical models, outcome measures, and robustness checks, several words of caution are in order.

One issue, as explained before, is the measurement of income disregard. Due to the decentralized nature of the US UI program, each state calculates their income disregard differently, with some basing it on a portion of wages earned and the maximum state benefit amount, others as a portion of claimants' weekly benefit amount, and others as a pre-determined dollar amount. Additionally, some states phase out weekly benefit amounts at different rates once the income disregard has been hit. This makes comparing income disregards across states somewhat difficult. The two measures of the income disregard increases both appear to yield similar and statistically significant results and hold up to multiple robustness checks. However, it is possible that measurement error may be introducing bias into these estimates.

Another potential issue is the level of aggregation of the data. The data in use for this analysis is aggregated at the state level on a quarterly basis. This level of aggregation may be clouding the true effect of increases in the income disregard on the various measures of interest to this study. For example, the average duration of claims is measured as the total claims compensated divided by the total first payments made over a 12-month time period. While this may be a legitimate measurement of the average duration of claims, the smoothing of average durations may make it more difficult to observe the true effect, leading to less obvious departures in trends in average duration of claims from the control group.

To better understand the labor market dynamics and the potential increase in part-time work among benefit claimants a different dataset may be needed. Specifically, individual-level data could provide valuable insights into the behavior of both employers and

employees in response to changes in the income disregard. Individual-level data would allow researchers to track changes in employment status, wages, and benefit claims of each individual, offering a more nuanced understanding of the labor market adjustments and their implications for trust fund solvency. By examining the patterns of part-time work and their relationship with changes in income disregard, the role of total wages and tax revenue received by each state's unemployment insurance (UI) system can be better understood.

Moreover, individual-level data would help researchers to identify potential heterogeneity in the impact of income disregard changes across different subgroups of the population, such as by age, race, education, or income levels. While many of these variables are already controlled for in this analysis, they are controlled for in an aggregated manner, potentially masking their true effects. This additional layer of analysis could reveal important distributional consequences of income disregard changes and help policymakers design more targeted and effective interventions. Future research could explore this alternative explanation by examining the prevalence of part-time work among benefit claimants and its relationship with changes in income disregard. This additional analysis would provide a more comprehensive understanding of the factors influencing trust fund solvency and help in identifying the most effective policy interventions.

## **Conclusion**

This paper contributes to the understanding of the relationship between changes in income disregard and trust fund solvency by offering empirical evidence that supports the

positive impact of increasing income disregard on trust fund health. The primary finding of the paper is that states that increased their income disregard between 2012-2019 experienced a statistically significant improvement in their trust fund solvency, as measured by the Average High-Cost Multiple (AHCM) and the Reserve Ratio.

These results appear to be driven by the steppingstone effect dominating the claimant effect. In other words, the decrease in the average duration of claims outweighs the potential increase in the number of new UI claimants, leading to a decreased strain on trust funds. Additional findings include the importance of considering the ratio of a state's income disregard to their average weekly wage. Although nominal increases in the income disregard appear to have a positive effect on solvency, this effect may be amplified the higher the income disregard to average weekly wage ratio climbs. As of 2019, it appears that no state has surpassed the Munts' hypothetical optimal phase-out point of UI benefits due to part-time earnings, although I did not attempt to identify empirically where the optimal point may lie.

Alternative explanations for the observed increase in trust fund solvency, related to the potential increase in part-time work among benefit claimants, highlight the importance of considering labor market dynamics and their impact on trust fund solvency. Future research could benefit from using individual-level data to better understand the labor market adjustments and their implications for trust fund solvency, as well as to identify potential heterogeneity in the impact of income disregard changes across different subgroups of the population.

By providing evidence on the relationship between income disregard and trust fund solvency, this paper contributes to the ongoing policy debate on unemployment insurance and the effectiveness of policy interventions in ensuring the financial health of trust funds. The findings suggest that increasing income disregard could be a viable policy option for improving trust fund solvency, but further research on the underlying mechanisms is necessary to inform the design of targeted and effective policy interventions.

## CHAPTER 3

### FEDERAL CARROTS, STATE CHOICES: STRATEGIC RESPONSES TO UI TRUST FUND SOLVENCY REQUIREMENTS

#### **Introduction**

The design of federal-state programs presents a fundamental theoretical puzzle: how can federal oversight effectively counteract moral hazard while preserving state autonomy? This challenge is particularly noteworthy when federal backstops create incentives for states to underfund programs, knowing the federal government will prevent system collapse. While scholars have developed rich theoretical frameworks explaining these incentive problems, we need more systematic evidence about which federal policy tools successfully modify state behavior and how states strategically respond to federal reform attempts.

This paper advances both theoretical and empirical understanding of moral hazard mitigation in federal-state programs through an analysis of a significant unemployment insurance (UI) reform. The UI system provides an ideal setting for examining these dynamics: states maintain significant autonomy over program design while operating under federal oversight, creating multiple channels through which they can respond to federal requirements. Moreover, states' ability to access federal loans during downturns may discourage adequate trust fund maintenance during economic expansions, exemplifying the moral hazard problems inherent in federal backstops.

I take advantage of a 2010 U.S. Department of Labor reform that altered the incentive structure for state UI financing. The reform implemented stricter criteria for states to qualify for interest-free federal advances, requiring them to meet specific funding goals, including an Average High-Cost Multiple (AHCM) of at least 1.0 and maintaining consistent tax effort. This policy change creates a natural experiment for examining how states respond when federal oversight attempts to enforce fiscal discipline through specific, measurable criteria.

This study makes three primary contributions. First, it provide a rigorous empirical analysis about the effectiveness of using federal lending criteria to influence state fiscal behavior. While previous research has documented states' tendency to underfund UI trust funds (Vroman & Woodbury, 2014; Stone & Chen, 2014), I demonstrate how specific federal policy tools can modify this behavior. Second, this study extends theoretical understanding of moral hazard in federal systems by showing how states strategically choose among multiple policy levers when responding to federal requirements. Third, the study reveals important limitations in current approaches to moral hazard mitigation, showing how states can achieve technical compliance while potentially undermining broader policy goals.

Using a dataset tracking all state UI trust funds from 1990 to 2019, I use several interrupted time series analysis approaches. This design allows us to address two key questions:

1. How effective was the federal policy change in improving state UI trust fund solvency?

2. Through what policy mechanisms did states choose to meet the new federal requirements, and what do these choices reveal about the reform's effectiveness in addressing moral hazard?

The findings reveal a complex pattern of state responses that has important implications for both theory and policy design. While the reform appears to have succeeded in improving overall trust fund solvency, less solvent states may have achieved compliance through benefit restrictions rather than expanded revenue bases. This pattern suggests that current approaches to moral hazard mitigation may be incomplete: states can meet federal requirements while maintaining underlying fiscal vulnerabilities that federal oversight aims to address. These results demonstrate how the specific mechanisms states use to achieve compliance can be as important as compliance itself.

The paper proceeds as follows. Section II synthesizes three relevant literature streams—moral hazard in federal systems, UI financing structure, and state strategic responses to federal oversight—to develop specific hypotheses about state behavior. Section III presents my empirical strategy, explaining how the 2010 policy change provides clean identification of reform effects. Section IV details our dataset and methodology. Section V presents the results on both overall policy effectiveness and specific state response mechanisms. Section VI explores implications for theory and policy design in federal-state programs. Section VII concludes with directions for future research.

## **Literature Review**

The relationship between federal oversight and state autonomy in the U.S. UI system provides an ideal setting for examining how states respond to federal attempts to address moral hazard in federal-state programs. This literature review synthesizes three interconnected research streams that inform my theoretical framework: (1) moral hazard in federal systems, (2) institutional constraints and path dependence in state policy choices, and (3) strategic state responses to federal oversight.

### Moral Hazard in Federal Systems: The Fundamental Challenge

Moral hazard often emerges in federal systems when federal guarantees or backstops reduce subnational governments' incentives for fiscal discipline. This dynamic appears across various policy domains, from banking regulation (Acharya et al., 2011) to disaster assistance (Goodspeed & Haughwout, 2012) to education funding (Baicker & Gordon, 2006).

Early scholars of fiscal federalism identified the inherent tensions in this balancing act (Oates, 1972; Musgrave, 1971). The UI system attempts to maintain local administration while providing national standards and support, but like other federal-state programs with implicit guarantees, it creates incentives for states to underfund their programs, knowing the federal government will ultimately prevent system collapse. Rodden (2006) identifies this as a broader pattern where subnational governments exploit their position within federal structures, taking calculated risks based on expectations of federal intervention. Maroulis et al. (2010) further demonstrate how these complex intergovernmental dynamics produce policy outcomes that emerge from interactions across multiple governance levels with conflicting incentives.

The evolution of the federal role in UI financing illustrates how moral hazard intensifies as federal backstops expand. The federal government's growing function as lender of last resort has fundamentally altered state incentives for trust fund management—similar to how federal deposit insurance transformed bank regulation and state behavior (Dam & Koetter, 2012). This role has expanded significantly since 2001, with borrowing reaching unprecedented levels during the 2008-2009 Great Recession and COVID-19 pandemic (Albrizio et al., 2017). While this backstop ensures benefit payments during severe downturns (Vroman, 2012) and supports countercyclical policy goals (Hastings & Saris, 2012; Wandner, 2019), it simultaneously creates moral hazard problems by potentially diminishing states' incentives to maintain robust reserves during economic expansions.

This moral hazard challenge motivated the Department of Labor's 2009-2010 reforms to UI loan access criteria. Announced in September 2010 and implemented in 2014, these changes marked a significant attempt to address moral hazard through policy design by establishing specific "funding goals" that states had to meet to qualify for interest-free advances:

1. Achievement of an Average High Cost Multiple (AHCM) of at least 1.0 in one of the five years preceding the year in which an advance was taken
2. Maintenance of tax effort in the years between meeting the solvency criterion and taking an advance

Prior to this reform, states could receive interest-free loans if they repaid borrowed funds before the end of the fiscal year ("cash-flow" loans), allowing them to manage temporary

funding gaps without incurring interest charges. The 2010 reform added more stringent requirements for accessing interest-free loans of any kind, representing a significant shift in the financial incentives for state UI trust fund management. States that failed to meet the new AHCM requirements would face interest charges on all loans, including those repaid within the same fiscal year, making trust fund solvency a more pressing concern for state policymakers. During the Great Recession, states borrowed over \$47 billion from the federal government, much of it in the form of credit loans that accrued interest. For larger states, these interest charges could amount to tens of millions of dollars annually, while even smaller states faced interest payments in the millions. This financial pressure created strong incentives for states to maintain solvent trust funds.

These changes parallel similar efforts in other domains where federal agencies have attempted to condition aid on state behavior, such as education policy (McDermott, 2007) and environmental programs (Keiser & Shapiro, 2019). The reforms' focus on specific solvency metrics reflects a broader trend in federal oversight, where agencies increasingly use quantitative benchmarks to influence state behavior. The DOL's rationale stemmed from growing concerns about both the sustainability of the UI system and the misalignment of incentives created by the existing interest-free borrowing provision (Employment and Training Administration, 2009).

### UI System Structure and State Policy Tools

The UI system's structure reflects the delicate balance between federal oversight and state autonomy inherent in fiscal federalism. States maintain significant discretion in program design and administration while operating within federal parameters, creating multiple

channels through which they can respond to federal requirements. This flexibility manifests primarily through two key policy levers:

### *Tax Policy and Revenue Generation*

State tax policy choices reflect embedded institutional preferences. The system's dual tax structure, combining federal (FUTA) and state (SUTA) components, allows states considerable latitude in setting employer tax rates and taxable wage bases. As documented by Vroman and Woodbury (2014), state approaches vary widely, with taxable wage bases ranging from the federal minimum of \$7,000 to over \$50,000 in some states. This variation reflects not just different economic conditions but enduring institutional arrangements and policy preferences.

The evolution of state tax policies reveals a significant shift in UI financing philosophy, with many states moving from forward-funding to pay-as-you-go approaches since the late 1970s (Stone & Chen, 2014). This shift exemplifies how institutional arrangements can sometimes work against federal policy goals, as states respond to short-term political pressures rather than long-term fiscal stability concerns.

Tax policy choices extend beyond just setting rates and bases. States also determine the structure of their tax schedules, including how quickly rates adjust to trust fund conditions and how tax burden is distributed across different types of employers. These choices significantly impact both the stability of trust fund financing and the political feasibility of maintaining adequate reserves.

States also maintain significant discretion over experience rating systems, where employers with more layoffs pay higher taxes. Anderson and Meyer (1993) show how experience rating attempts to internalize the social costs of unemployment, but its effectiveness depends crucially on state design choices. Vroman and Woodbury (2014) note that incomplete experience rating can lead to implicit subsidies from low-layoff to high-layoff industries, potentially undermining fiscal stability.

The degree of experience rating varies significantly across states, reflecting different approaches to balancing employer incentives with other policy goals. Some states maintain aggressive experience rating systems that quickly adjust tax rates based on layoff patterns, while others opt for more gradual adjustments that may provide less immediate feedback to employers.

Research by Topel (1990) suggests that stronger experience rating could reduce layoffs and improve economic efficiency, but institutional and political constraints often lead states to maintain systems that don't fully charge employers for layoff costs.

### *Benefit Structure*

Apart from managing inflows into the UI trust fund, states also control outflows of benefit payments. States vary significantly in terms of their benefit generosity, with some states replacing considerable proportions of prior weekly earnings through unemployment compensation while others provide minimal support. States control benefit generosity through multiple dimensions including:

- Maximum weekly benefit amounts and replacement rates

- Duration of benefits
- Eligibility criteria and qualification requirements

Crucially, Smith and Wenger (2013) identified a coupling mechanism where states with more generous benefits often maintain higher trust fund balances. This finding challenges simplistic trade-off narratives and suggests that benefit and funding decisions reflect deeper institutional orientations rather than merely technical responses to immediate conditions. The persistence of this coupling, even when controlling for political and economic factors, suggests that these patterns represent fundamental policy preferences that shape how states respond to federal incentives.

#### Institutional Constraints and Path Dependence in State Policy Choices

The institutional contexts shaping UI policy choices are heavily influenced by interest group configurations. Lachowska et al. (2020) document how business interests typically lobby for lower taxes while labor groups advocate for more robust funding and benefits. This pattern reflects broader dynamics where interest group competition establishes enduring policy equilibria (Grossman & Helpman, 1999; Berry & Berry, 1990). States with stronger labor unions consistently maintain higher taxable wage bases and more solvent trust funds, demonstrating how these institutional arrangements create stable policy orientations that resist short-term pressures.

Interstate competition adds another institutional constraint. Besley and Case (1992) demonstrate how states benchmark their policies against neighboring jurisdictions, creating regional patterns of policy design. Similar to dynamics observed in welfare

benefits (Bailey & Rom, 2004) and corporate taxation (Chirinko & Wilson, 2017), this competitive pressure often works against federal goals of maintaining robust reserves by incentivizing states to minimize employer costs.

These path-dependent policy preferences create enduring approaches to UI financing that persist across economic and political cycles. As my theoretical framework will elaborate, these preferences establish trajectories that constrain and channel states' strategic responses to federal requirements.

### Strategic State Responses to Federal Oversight

Given these institutional constraints, states develop strategic approaches to federal requirements that accommodate both federal demands and local policy preferences.

Research on federalism demonstrates that federal tools rarely generate uniform responses across states. Instead, effectiveness is mediated by existing institutional frameworks and policy orientations.

### *Institutional Mediation of Federal Influence*

Volden's (2006) analysis of policy diffusion shows how states filter federal requirements through established policy approaches, generating responses that maintain institutional continuity. This mediation effect helps explain why, despite facing identical federal requirements, states pursue different implementation strategies that reflect their institutional contexts (Rigby & Haselswerdt, 2013). In the UI context, this suggests that federal solvency requirements will generate different response mechanisms across states based on their established preferences.

### *Strategic Compliance and Policy Innovation*

Research on policy implementation reveals that states often find creative ways to meet federal requirements without fully aligning with broader policy goals. Studies of other federal-state programs, from No Child Left Behind (Manna, 2010) to environmental regulation (Konisky & Woods, 2012), demonstrate how states can achieve technical compliance while potentially undermining broader objectives. Similarly, research on tax expenditure limitations shows how requirements are often manipulated to achieve technical compliance without practical effect on targeted outcomes (Shumberger & Singla, 2022).

Early work on UI trust fund management (Guo & Johnston, 2021) suggests similar dynamics, with states potentially meeting solvency requirements through methods that don't fully address underlying fiscal vulnerabilities. This strategic compliance behavior reflects what Volden (2006) identifies as policy innovation in federal systems, where states experiment with different approaches to meeting federal requirements while maintaining local policy preferences.

The UI system provides a particularly rich setting for examining these dynamics because states have multiple policy levers available. This allows observation of not just whether states respond to federal incentives but how they choose among different response options based on their institutionalized preferences. The system structure gives states significant discretion in program design and administration while operating within federal parameters, creating multiple channels through which they can respond to federal requirements.

## Research Gaps and Contribution

While existing literature provides valuable insights into moral hazard in federal systems and state responses to federal oversight, several important gaps remain. First, while we know that states respond strategically to federal incentives, we lack detailed evidence about how they choose among different policy tools when multiple options are available. The UI system, with its various policy levers and recent federal reforms, provides an ideal setting for examining these choices. Second, while moral hazard in federal-state programs is well-documented, we have limited evidence about the effectiveness of specific federal policy tools in addressing it. The 2010 UI reform offers a unique opportunity to evaluate how changes in federal lending criteria influence state behavior and whether such reforms effectively promote more responsible trust fund management. Third, existing research has not fully explored how states' responses to federal solvency requirements may vary based on their political, economic, and institutional characteristics. Understanding this heterogeneity is crucial for designing effective federal policies that can work across diverse state contexts.

This study addresses these gaps by providing a comprehensive empirical evaluation of how states responded to the 2010 federal reforms. By examining both whether states met the new requirements and how they chose to do so, this study contributes to broader theoretical debates about the effectiveness of federal tools for addressing moral hazard in federal-state programs. The findings have implications beyond UI policy, offering insights relevant to other areas where federal policies aim to influence state fiscal behavior, from pension fund regulation (Chaney et al., 2002) to municipal bond markets

(Gao et al., 2019). The timing of this study is also relevant given the growing importance of federal backstops in state program stability. Just as federal guarantees have become increasingly crucial in areas like disaster response (Michel-Kerjan, 2010) and financial regulation (Acharya et al., 2011), the federal role in UI has expanded significantly. Understanding how states respond to federal attempts to maintain program solvency while preserving state autonomy has become increasingly important for both theory and policy.

### **Theory and Hypotheses**

Building on the literature reviewed above, I develop a theoretical framework for understanding how states respond to federal attempts to enforce fiscal discipline in UI trust fund management. The 2010 DOL policy change provides an ideal setting to examine these dynamics, as it represents a clear federal intervention that maintains state autonomy while attempting to reshape incentives through specific, measurable criteria.

My theoretical framework is based on three interconnected premises derived from the literature. First, states operate as strategic actors with distinct underlying policy preferences that persist over time. This premise draws on institutional theory showing how subnational governments develop enduring approaches to policy that reflect deeper commitments rather than just immediate political or economic conditions (Rodden, 2006; March & Olsen, 1984). In UI financing specifically, these preferences manifest through established patterns of trust fund management that remain relatively stable despite changes in partisan control or economic circumstances.

Second, when federal oversight tools attempt to modify state behavior, their effects are mediated by these existing preferences, creating varied implementation patterns across states. Federal requirements rarely transform fundamental state orientations but instead generate strategic adaptations that accommodate both federal mandates and states' ongoing policy priorities (Volden, 2006; McDermott, 2007; Conlan, 2006). This mediation effect explains why identical federal requirements often produce different implementation approaches across states (Rigby & Haselswerdt, 2013).

Third, the strength of states' responses to federal incentives is mediated by both the economic value of compliance and their institutional capacity to adjust. This premise draws from implementation research showing how state-specific constraints and incentives shape responses to federal initiatives. These factors include administrative capacity (Manna, 2010), interest group configurations (Grossman & Helpman, 1999), and economic conditions that determine both the benefits of compliance and available policy options. In UI policy specifically, these constraints manifest through varying abilities to raise taxes, modify benefits, or maintain reserves during economic expansions. The coupling mechanism identified by Smith and Wenger (2013) suggests that states' approaches to solvency will reflect their broader institutional orientations toward the UI system, creating systematic patterns in how they balance tax policy, experience rating, and benefit structure adjustments.

The interaction of these premises suggests that state responses to federal oversight will reflect a balance between the uniform pressure of federal incentives and each state's distinct policy preferences and institutional constraints. Importantly, states are expected

to comply with federal requirements through mechanisms that reinforce rather than contradict their underlying policy approaches, creating systematic variation in how they achieve technical compliance.

Applied to the 2010 DOL policy change, these premises suggest two primary predictions. First, the financial incentives created by the differential between interest-free and interest-bearing loans should generate a general improvement in trust fund solvency measures as states respond to clear economic incentives. Even states previously resistant to maintaining higher balances face significant pressure to comply, as the cost differential can materially impact budgets. This leads to my first hypothesis:

*H1: Federal Incentive Effectiveness:*

*The 2010 federal policy change will lead to an overall improvement in state UI trust fund solvency, demonstrating that federal lending criteria can influence state fiscal behavior.*

While the uniform incentives should produce a common directional response, the literature suggests that states will achieve compliance through mechanisms that align with their pre-existing policy preferences. Initial solvency levels represent meaningful policy choices that persist independently of partisan control, economic conditions, or other contextual factors (Thelen, 1999). These choices reflect deeply embedded institutional preferences rather than merely technical responses to immediate conditions (March & Olsen, 1984).

This perspective is supported by empirical evidence from Smith and Wenger (2013), who identified a coupling mechanism where states with more generous benefits often maintain

higher trust fund balances, while less solvent states typically feature lower benefit generosity. Importantly, this relationship persists even after controlling for political and economic factors, suggesting that these patterns represent fundamental, enduring preferences in UI system design rather than merely reflecting partisan politics or economic conditions. Research on state budgeting by Wildavsky (1988) further reinforces this understanding, demonstrating how fiscal management approaches reflect fundamental values and priorities that persist across electoral cycles and economic conditions. Solvency levels thus serve as reliable indicators of enduring state policy orientations rather than circumstantial responses. When faced with federal requirements for improved solvency, states are likely to pursue compliance through mechanisms that reinforce these existing policy preferences rather than fundamentally altering their approach to UI system management (Pierson, 2000).

This interaction between federal requirements and established state preferences generates my second set of hypotheses about strategic response variation:

*H2: Strategic Response Variation:*

*H2a (Benefit Reductions): States with preferences for lower trust fund solvency (those starting further below the solvency threshold) will respond by decreasing benefit generosity to improve trust fund solvency.*

*H2b (Revenue Expansions): States with preferences for higher trust fund solvency (those starting closer to the solvency threshold) will respond by increasing tax revenues to improve trust fund solvency.*

States with significant solvency deficits have consistently demonstrated a preference for prioritizing low employer costs over robust reserves and benefit security. When faced with federal requirements, these states will likely continue this approach by further restricting benefits rather than expanding revenue capacity. Their institutional path dependence constrains their response options, making benefit reductions the path of least resistance given their established policy orientation. Conversely, states closer to or beyond compliance have demonstrated stronger preferences for system stability, countercyclical effectiveness, and fiscal preparedness. These states will likely reinforce this approach through revenue-based solutions that build long-term solvency. Their historical commitment to maintaining robust trust funds creates a path-dependent trajectory that makes revenue expansions a more natural response to federal solvency requirements.

### **Data and Methodology**

To test the hypotheses about state responses to the 2010 federal policy change, I construct a panel dataset combining unemployment insurance program data with state political and economic indicators. The primary data source is the U.S. Department of Labor (DOL), which collects detailed information from state-administered UI programs. The dataset spans from 1990 to 2019, providing 21 years of pre-treatment and 9 years of post-treatment observations across all 50 states.

The DOL data captures several key dimensions of state UI systems that are central to my analysis. To measure the primary outcome of interest—trust fund solvency—I use the Average High-Cost Multiple (AHCM) and the Reserve Ratio, which are specific metrics

targeted by the 2010 federal policy change. The AHCM represents how many years a state could pay historically high levels of unemployment benefits using current reserves without collecting additional revenue; a value of 1.0 indicates a state could pay one year of benefits at the average of their three highest payout years. The Reserve Ratio, calculated as the trust fund balance divided by total wages paid in covered employment, provides a standardized measure of a state's reserves relative to their economy's size. Together, these metrics offer complementary views of trust fund adequacy: AHCM captures resilience against historical worst-case scenarios, while the Reserve Ratio indicates current fiscal capacity relative to economic activity. To examine the mechanisms through which states achieve solvency improvements, I track three additional outcomes: benefit generosity (measured by the ratio of average weekly benefits to average weekly wages) and revenue generation (measured by average employer tax rates and taxable wage bases).

This panel structure allows us to implement two empirical strategies that map directly to my hypotheses. First, to test H1's prediction about overall policy effectiveness, I use an interrupted time series (ITS) analysis. This approach leverages the long pre-treatment period to establish baseline trends in trust fund solvency, then examines both immediate level changes and gradual slope changes following the policy intervention. The ITS framework is particularly well-suited for evaluating H1 because it can distinguish between temporary responses and sustained changes in state behavior.

The ITS model specification is:

$$Y_{it} = \beta_0 + \beta_1 \text{Time}_t + \beta_2 \text{Post}_t + \beta_3 (\text{Time}_t \times \text{Post}_t) + \delta X_{it} + \alpha_i + \varepsilon_{it} \quad (1)$$

In this equation,  $Y_{it}$  is the outcome measure (AHCM or Reserve Ratio) for state  $i$  at time  $t$ ;  $Time_t$  is a continuous time variable representing the baseline trend;  $Post_t$  is a binary indicator equal to 1 for periods after policy intervention (either 2010 or 2014 depending on specification);  $(Time_t \times Post_t)$  is the interaction term between time and the post-intervention indicator, capturing the change in trend after intervention;  $X_{it}$  is a vector of control variables including political factors (party control of state government, partisan composition of legislatures) and economic indicators (unemployment claims, wage levels);  $\alpha_i$  represents state fixed effects that account for time-invariant state characteristics; and  $\varepsilon_{it}$  is the error term.

This specification allows us to estimate three key parameters of interest:  $\beta_1$  captures the pre-intervention trend in trust fund solvency,  $\beta_2$  measures the immediate level change after the federal policy announcement/implementation, and  $\beta_3$  represents the change in slope (trend) after intervention, which captures sustained policy effects.

Second, to examine the heterogeneous responses predicted by H2, I complement the standard ITS approach with a moderated interrupted time series (ITS) design. While the standard ITS analysis provides evidence on the overall effectiveness of the federal policy change, the moderated ITS allows us to investigate how states with different policy preferences—as reflected in their pre-reform trust fund positions—chose different mechanisms to respond to the federal requirements.

The moderated ITS model specification is:

$$Y_{it} = \beta_0 + \beta_1 \text{Time}_t + \beta_2 \text{Post}_t + \beta_3 (\text{Time}_t \times \text{Post}_t) + \beta_4 Z_i + \beta_5 (Z_i \times \text{Post}_t) + \beta_6 (Z_i \times \text{Time}_t) + \beta_7 (Z_i \times \text{Time}_t \times \text{Post}_t) + \delta X_{it} + \alpha_i + \varepsilon_{it} \quad (2)$$

In this equation,  $Y_{it}$  is the outcome measure (benefit generosity or revenue generation strategies) for state  $i$  at time  $t$ ;  $\text{Time}_t$  is a continuous time variable measuring years from the start of the study period;  $\text{Post}_t$  is a binary indicator equal to 1 for periods after the 2010 policy change;  $Z_i$  is the moderator variable representing each state's pre-reform trust fund solvency level, measured as a state's pre-reform AHCM, where states with lower pre-reform AHCM values having demonstrated preferences for lower trust fund solvency, while those with higher pre-reform AHCM values have shown preferences for higher solvency;  $(Z_i \times \text{Post}_t)$  captures the differential immediate effect of the policy based on initial solvency levels;  $(Z_i \times \text{Time}_t \times \text{Post}_t)$  captures how the policy's slope effect varies by initial solvency;  $X_{it}$  is a vector of control variables;  $\alpha_i$  represents state fixed effects that account for time-invariant state characteristics; and  $\varepsilon_{it}$  is the error term.

This specification allows for the estimation of several key parameters of interest.  $\beta_5$  captures how states' immediate responses to the federal policy varied based on their pre-reform trust fund positions, while  $\beta_7$  shows how the longer-term trajectory of responses differed by initial solvency levels. These coefficients enable testing of hypotheses H2a and H2b, which predict that states with preferences for lower trust fund solvency will primarily respond through benefit restrictions, whereas states with preferences for higher trust fund solvency will rely more on revenue expansion.

The identification strategy for this model builds upon the standard ITS approach but extends it to account for effect heterogeneity across states with different baseline characteristics. This treatment effect modeling approach offers several strengths: it directly captures how policy responses vary based on pre-existing state characteristics, allows for more nuanced analysis than standard ITS, and can identify meaningful subgroups with similar response patterns (Rekkas et al., 2020). By interacting the policy indicators with the continuous measure of pre-reform solvency, we can examine how the magnitude and nature of responses varied systematically across states with different revealed preferences for UI system management. However, this approach is not without limitations. Treatment-covariate interaction models can be susceptible to overfitting, particularly with limited sample sizes; may have reduced statistical power compared to main effects models; and face challenges in validating counterfactual outcomes (Rekkas et al., 2020).

To address these limitations and provide a more transparent examination of heterogeneous effects, I complement the moderated ITS approach with stratified ITS analyses. Specifically, I estimate separate standard ITS models for two distinct subgroups: states that were solvent ( $AHCM \geq 1$ ) at the announcement of the policy change, and those that were insolvent ( $AHCM < 1$ ). This stratified approach has several advantages: it makes heterogeneous responses more immediately interpretable, reduces model complexity by eliminating three-way interactions, and serves as a robustness check for the moderated ITS findings. By comparing policy effects across these two clearly

defined groups, we can more directly assess whether states with different trust fund positions employed different strategic responses to the federal requirements.

Table 4 presents descriptive statistics for the key variables.

Table 4. Descriptive Statistics

| VARIABLES                                                | (1)<br>N | (2)<br>Mean | (3)<br>SD   | (4)<br>Min | (5)<br>Max    |
|----------------------------------------------------------|----------|-------------|-------------|------------|---------------|
| AHCM                                                     | 1,617    | 0.815       | 0.620       | 0          | 2.790         |
| Reserve Ratio                                            | 1,617    | 1.409       | 1.149       | -0.380     | 6.200         |
| Year End Taxable Wage Base                               | 1,617    | 14,473      | 9,045       | 7,000      | 62,500        |
| Average Employer Tax Rate                                | 1,617    | 0.723       | 0.367       | 0.100      | 2.690         |
| Benefit to Wage Ratio                                    | 1,617    | 0.356       | 0.0641      | 0.180      | 0.577         |
| Wages Paid in Covered Employment During Year (thousands) | 1,617    | 90,020,000  | 124,100,000 | 2,722,000  | 1,235,000,000 |
| Weeks Compensated for All Unemployment                   | 1,617    | 2,754,000   | 4,524,000   | 53,170     | 91,360,000    |
| Average Duration of Claims                               | 1,617    | 14.86       | 5.703       | 6.500      | 211.2         |
| Party of State Governor                                  | 1,617    | 0.456       | 0.493       | 0          | 1             |
| Party in Control of State Legislature                    | 1,617    | 0.493       | 0.452       | 0          | 1             |
| Proportion of State Senate that is Democrat              | 1,617    | 0.499       | 0.184       | 0.0645     | 1             |
| Proportion of State Senate that is Republican            | 1,617    | 0.497       | 0.185       | 0          | 0.935         |
| Proportion of State House that is Democrat               | 1,617    | 0.506       | 0.172       | 0.0806     | 0.922         |
| Proportion of State House that is Republican             | 1,617    | 0.489       | 0.173       | 0.0784     | 0.919         |
| Democrats have Control of Legislature and Governor       | 1,617    | 0.231       | 0.422       | 0          | 1             |
| Republicans have Control of Legislature and Governor     | 1,617    | 0.297       | 0.457       | 0          | 1             |
| Ease of Obtaining Benefits                               | 1,617    | 1.224       | 0.885       | 0.153      | 8.802         |
| Number of States                                         | 49       | 49          | 49          | 49         | 49            |

To isolate the causal effect of the federal policy change, I include a set of controls for state-specific economic and political factors that might influence UI system outcomes. Economic controls include total wages paid in covered employment, average duration and count of unemployment claims, which capture underlying labor market conditions. I also control for the ease of obtaining benefits, measured as the ratio of claims awarded to total claims filed, to account for variation in state-specific administrative practices. Political controls include indicators for party control of state government (governor's party and legislative majority), as well as more granular measures of partisan composition in state legislatures (proportion of Democrats and Republicans in both houses). These political variables are particularly important given that changes to UI systems often require legislative action and may reflect partisan policy preferences. All specifications include state-fixed effects to control for time-invariant state characteristics and year-fixed effects to account for national economic conditions and policy changes that affect all states similarly. This rich set of controls, combined with the panel structure and dual empirical strategy, helps ensure that the estimated treatment effects capture the causal impact of the 2010 federal policy change rather than other concurrent changes in state conditions.

## **Results**

We begin by examining the first hypothesis that the 2010 federal policy change would lead to an overall improvement in state UI trust fund solvency. Using an interrupted time series (ITS) approach, I test this hypothesis using both the Average High Cost Multiple

(AHCM) and Reserve Ratio as measures of trust fund solvency, with analyses centered on both the policy announcement (2010) and implementation (2014) dates.

Table 5. Interrupted Time Series Models for Effects on Trust Fund Solvency

|                                | (1)<br>AHCM<br>(2010) | (2)<br>AHCM<br>(2014) | (3)<br>Reserve<br>Ratio<br>(2010) | (4)<br>Reserve<br>Ratio<br>(2014) |
|--------------------------------|-----------------------|-----------------------|-----------------------------------|-----------------------------------|
| Time                           | -0.063***<br>(0.017)  | -0.055***<br>(0.016)  | -0.104***<br>(0.029)              | -0.097***<br>(0.027)              |
| Post                           | 0.011<br>(0.061)      | 0.157**<br>(0.065)    | -0.128<br>(0.102)                 | 0.260***<br>(0.096)               |
| Time × Post                    | 0.054***<br>(0.017)   | 0.050***<br>(0.018)   | 0.113***<br>(0.032)               | 0.115***<br>(0.036)               |
| Party of Governor              | 0.130*<br>(0.073)     | 0.130*<br>(0.074)     | 0.179*<br>(0.107)                 | 0.180*<br>(0.108)                 |
| Party of State Legislature     | 0.266**<br>(0.105)    | 0.265**<br>(0.105)    | 0.272<br>(0.180)                  | 0.277<br>(0.181)                  |
| % of Senate that is Democrat   | -0.893<br>(0.869)     | -0.948<br>(0.872)     | -0.632<br>(1.766)                 | -0.821<br>(1.768)                 |
| % of Senate that is Republican | -0.491<br>(0.843)     | -0.564<br>(0.840)     | -0.362<br>(1.739)                 | -0.556<br>(1.726)                 |
| % of House that is Democrat    | -0.820<br>(1.198)     | -0.915<br>(1.230)     | 0.536<br>(3.505)                  | 0.520<br>(3.605)                  |
| % of House that is Republican  | -0.290<br>(1.161)     | -0.356<br>(1.205)     | 0.694<br>(3.553)                  | 0.730<br>(3.657)                  |
| Dems Control Leg and Gov       | -0.076<br>(0.073)     | -0.072<br>(0.072)     | -0.081<br>(0.107)                 | -0.078<br>(0.105)                 |
| Reps Control Leg and Gov       | 0.042<br>(0.078)      | 0.046<br>(0.078)      | 0.124<br>(0.129)                  | 0.131<br>(0.129)                  |
| UI Weeks Compensated (Log)     | -0.619***<br>(0.104)  | -0.600***<br>(0.111)  | -0.754***<br>(0.162)              | -0.745***<br>(0.168)              |

|                            |                     |                     |                      |                      |
|----------------------------|---------------------|---------------------|----------------------|----------------------|
| Average Duration of Claims | -0.031**<br>(0.015) | -0.030**<br>(0.015) | -0.063***<br>(0.020) | -0.066***<br>(0.019) |
| Wages Paid (Log)           | 0.350***<br>(0.110) | 0.334***<br>(0.112) | 0.264<br>(0.196)     | 0.265<br>(0.202)     |
| Average Weekly Wage (Log)  | 1.028**<br>(0.455)  | 0.867*<br>(0.444)   | 1.337*<br>(0.746)    | 1.198*<br>(0.706)    |
| Ease of Benefits (Log)     | 0.207***<br>(0.043) | 0.174***<br>(0.046) | 0.185**<br>(0.075)   | 0.154*<br>(0.080)    |
| Constant                   | 0.326<br>(3.167)    | 1.366<br>(3.053)    | 1.639<br>(5.423)     | 2.553<br>(5.371)     |
| <i>N</i>                   | 1470                | 1470                | 1470                 | 1470                 |

Standard errors in parentheses

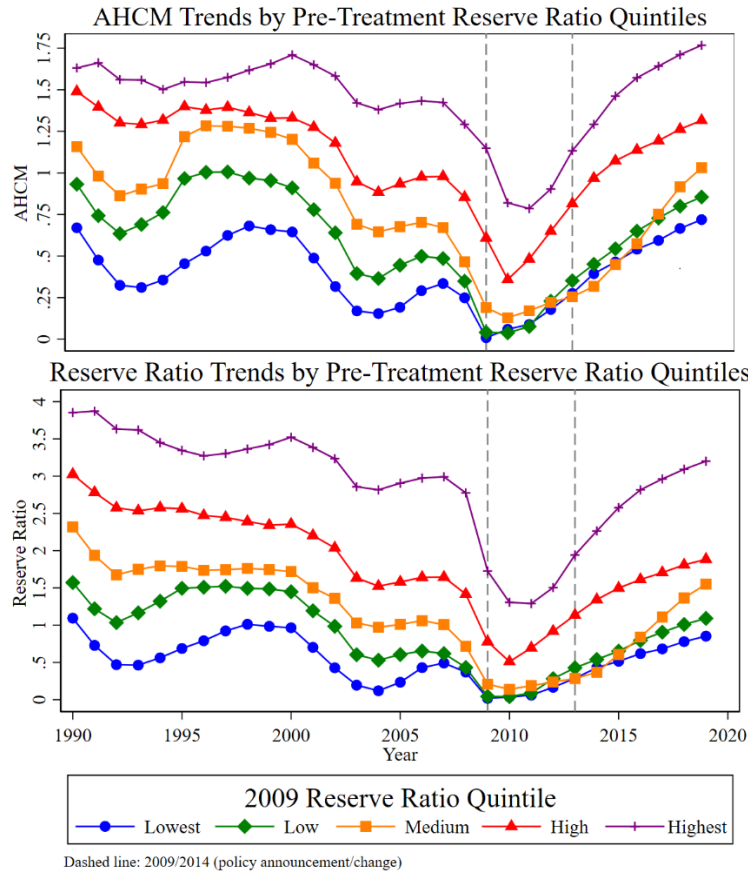
\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

The results provide strong support for H1. Across all specifications, I observe significant negative pre-intervention trends (Time), indicating declining trust fund solvency before the policy change. However, this pattern reverses following the intervention, with all models showing significant positive changes in post-intervention trends (Time x Post). The immediate effect of the policy (Post) varies by timing: while I find no significant immediate effect using 2010 as the treatment year (the announcement year), there are significant positive immediate effects when using 2014 (the implementation year), suggesting that the full impact of the policy materialized as implementation approached.

The magnitude of these effects is substantial in practical terms. Prior to the intervention, states' trust fund solvency was declining at a rate of 0.063 AHCM units per year (using the 2010 specification) or 0.055 AHCM units per year (using the 2014 specification). The policy change not only reversed this negative trend but led to positive growth, with post-

intervention increases of 0.054 and 0.050 AHCM units per year respectively. This represents a net positive swing of approximately 0.117 AHCM units per year in the 2010 specification and 0.105 units per year in the 2014 specification. To put these effects in context, given that the DOL recommends a minimum AHCM of 1.0 for adequate trust fund solvency, these improvements represent meaningful progress toward that goal. The 2014 specification also shows an immediate level increase of 0.157 AHCM units, suggesting that states made substantial adjustments as the implementation deadline approached. Similar patterns emerge in the Reserve Ratio specifications, with the policy reversing pre-intervention declines of about 0.10 percentage points per year to achieve post-intervention increases of approximately 0.11 percentage points per year, representing a substantial improvement in states' ability to meet their UI obligations.

Figure 1. Changes in UI Trust Fund Solvency Trends



Descriptive evidence in Figure 1 further supports H1, showing a clear reversal of the pre-2010 downward trend in trust fund solvency. The data reveal a marked improvement following the policy change, with many states achieving solvency levels by 2019 that hadn't been seen since the 1990s.

To test my second hypothesis regarding heterogeneous responses based on states' policy preferences (H2), I first present descriptive evidence. Figure 2 presents fiscal management trends grouped by pre-treatment reserve ratio quintiles, providing evidence that supports my theoretical framework before using more formal statistical tests.

Figure 2. Changes in Fiscal Management of UI Trust Funds

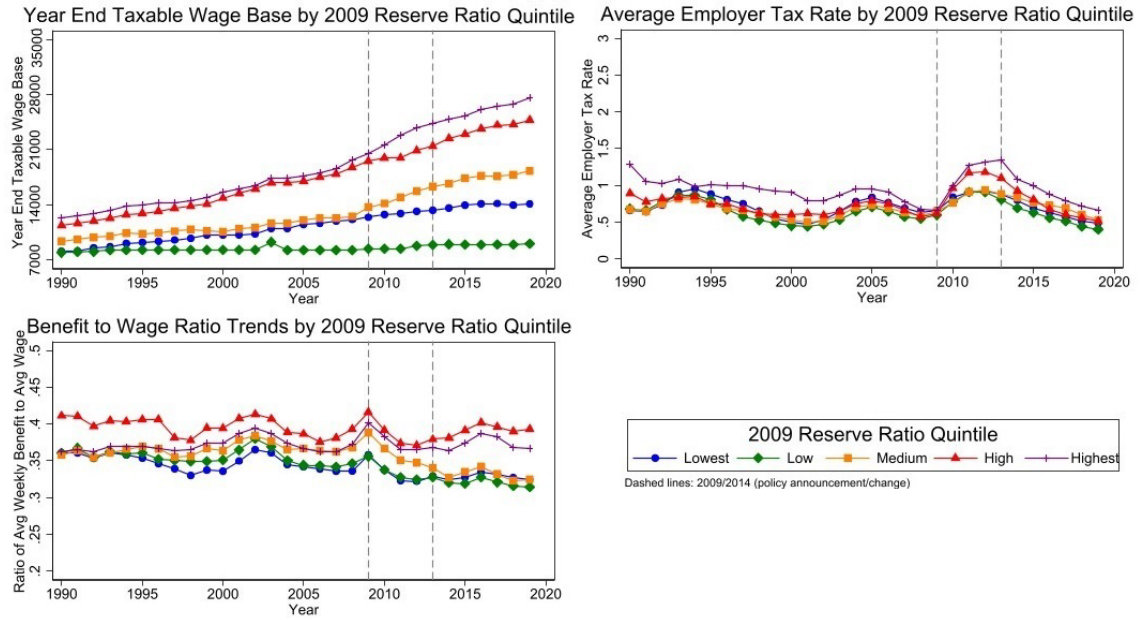


Table 6. Heterogeneous Effects of Federal UI Reform: Moderated vs. Stratified ITS Analysis

|                                       | (5)<br>Benefit-to-<br>Wage Ratio<br>Moderated | (6)<br>Solvent    | (7)<br>Less Solvent  | (8)<br>Taxable<br>Wage Base<br>Moderated | (9)<br>Solvent             | (10)<br>Less Solvent |
|---------------------------------------|-----------------------------------------------|-------------------|----------------------|------------------------------------------|----------------------------|----------------------|
| Post ( $\beta_2$ )                    | 0.011**<br>(0.004)                            | 0.015<br>(0.011)  | 0.008*<br>(0.004)    | 766.709<br>(546.940)                     | 3,843.942***<br>(1122.765) | 626.986<br>(562.452) |
| Post x Time ( $\beta_3$ )             | -0.005***<br>(0.001)                          | -0.002<br>(0.001) | -0.005***<br>(0.001) | 97.240<br>(71.728)                       | 570.759***<br>(177.421)    | 130.826*<br>(68.196) |
| 2009 AHCM ( $\beta_4$ )               | 0.002<br>(0.006)                              |                   |                      | -281.186<br>(794.147)                    |                            |                      |
| Post x 2009 AHCM ( $\beta_5$ )        | 0.000<br>(0.008)                              |                   |                      | 2,397.505**<br>(1019.065)                |                            |                      |
| Post x Time x 2009 AHCM ( $\beta_7$ ) | 0.003***<br>(0.001)                           |                   |                      | 358.356***<br>(130.477)                  |                            |                      |
| Observations                          | 1,617                                         | 363               | 1,254                | 1,617                                    | 363                        | 1,254                |
| Adjusted R-squared                    | 0.389                                         | 0.164             | 0.412                | 0.625                                    | 0.771                      | 0.444                |

All models include state fixed effects, cluster-robust standard errors, and control variables. The moderated models correspond to equation (2) where  $\beta_5$  captures the differential immediate effect of the policy based on initial solvency levels, and  $\beta_7$  captures how the policy's slope effect varies by initial solvency.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

These visualizations reveal two key patterns. First, states exhibit persistence in their relative positions across fiscal management dimensions despite shared economic cycles. States grouped by their 2009 reserve ratio quintile largely maintained their relative rankings throughout the 1990-2019 period. While all quintiles experienced similar cyclical patterns during economic downturns, the stratification remained notably stable—higher-solvency states consistently maintained higher taxable wage bases and more generous benefits compared to lower-solvency states, in line with the findings of Smith and Wenger (2013).

Second, following the 2010 policy intervention, this stratification became even more pronounced as states responded to federal incentives while adhering to their established approaches to trust fund management. The taxable wage base panel shows higher-solvency states increasingly expanding their tax bases after the policy change, while lower-solvency states made minimal adjustments. Simultaneously, the benefit-to-wage ratio panel reveals lower-solvency states reducing benefit generosity more aggressively than higher-solvency states. These divergent patterns across quintiles suggest that pre-reform solvency levels indeed capture meaningful differences in states' policy orientations.

To formally examine the mechanisms through which states achieved trust fund improvements, I analyze changes in taxable wage bases and benefit-to-wage ratios using both moderated and stratified ITS approaches. Table 6 presents results that support H2's prediction that states with different policy preferences for trust fund management responded through different mechanisms.

For benefit-to-wage ratios, the moderated ITS results (Model 5) show a significant immediate increase following the reform ( $\beta_2 = 0.011$ ,  $p < 0.05$ ), followed by a significant decline over time ( $\beta_3 = -0.005$ ,  $p < 0.01$ ). The most revealing finding comes from the Post  $\times$  Time  $\times$  2009 AHCM interaction term ( $\beta_7 = 0.003$ ,  $p < 0.01$ ), indicating that states with higher pre-reform solvency experienced less reduction in benefit generosity over time. This supports H2b, as states with preferences for lower solvency (lower AHCM) relied more on benefit restrictions. The stratified models confirm this pattern, showing that the negative time trend after reform is stronger and more significant for less solvent states ( $\beta_3 = -0.005$ ,  $p < 0.01$ ) compared to solvent states, where the decline is smaller and not statistically significant ( $\beta_3 = -0.002$ ,  $p > 0.10$ ).

For taxable wage bases, the moderated ITS results (Model 8) reveal heterogeneity in revenue responses. While the overall immediate effect of reform is positive but not significant, the Post  $\times$  AHCM interaction ( $\beta_5 = 2,397.50$ ,  $p < 0.05$ ) indicates that states with higher pre-reform solvency implemented substantially larger immediate increases in their taxable wage bases. Similarly, the Post  $\times$  Time  $\times$  AHCM interaction ( $\beta_7 = 358.35$ ,  $p < 0.01$ ) shows that these states also implemented larger annual increases in taxable wage bases over time. The stratified models powerfully confirm this pattern, with solvent states showing dramatic increases in taxable wage bases both immediately after reform ( $\beta_2 = 3,843.942$ ,  $p < 0.01$ ) and in the years following ( $\beta_3 = 570.759$ ,  $p < 0.01$ ), while less solvent states show much smaller and largely non-significant increases.

The magnitude of these heterogeneous effects is substantial. For each unit increase in pre-reform AHCM, states implemented approximately \$2,398 larger immediate increases in

their taxable wage base, followed by an additional \$358 increase each year after reform. Given that the average taxable wage base in the sample was \$14,473, the cumulative differences between high and low solvency states represent meaningful divergence in revenue policy. Similarly, the three-percentage point difference in trend effects on benefit-to-wage ratios between high and low solvency states (0.3% per year) represents a significant divergence in benefit policy over time.

Together, these results provide strong evidence for both hypotheses. The federal policy reform succeeded in improving overall trust fund solvency (H1), but states' responses varied systematically based on their pre-reform solvency level (H2). States with lower pre-reform AHCM levels relied more heavily on benefit restrictions over time, while states with preferences for higher pre-reform AHCM levels implemented substantially larger expansions to their revenue base. This pattern suggests that the federal policy intervention channeled state responses along paths consistent with their established policy preferences rather than fundamentally altering those preferences.

## **Discussion and Conclusion**

This analysis of state responses to the 2010 federal UI trust fund solvency requirements reveals complex dynamics in how states navigate federal oversight while maintaining policy autonomy. The results paint a nuanced picture of both the effectiveness and limitations of federal tools for promoting fiscal discipline in state-federal programs.

The findings demonstrate clear success in the federal policy's primary goal: improving overall trust fund solvency. The significant reversal of negative pre-intervention trends

following the 2010 policy change suggests that well-designed federal incentives can effectively modify state behavior, even in programs where states maintain substantial autonomy. This improvement manifested gradually, with stronger effects visible by 2014, indicating that states needed time to adjust their fiscal frameworks to meet the new requirements.

However, examining how states achieved these improvements reveals important limitations in the policy's effectiveness. The analysis reveals systematic differences in how states chose to achieve compliance. Less solvent states may have relied on benefit restrictions rather than robust revenue expansions, suggesting that political and institutional constraints led states to choose adjustment mechanisms that might undermine long-term program stability. This pattern raises concerns about the sustainability of these improvements, as benefit restrictions may leave programs less resilient to future economic downturns while failing to address underlying funding inadequacies.

These findings have significant implications for policy design. First, they suggest that federal oversight mechanisms need to consider not just whether states meet requirements, but how they achieve compliance. The tendency of less solvent states to rely on benefit restrictions rather than revenue expansion indicates that future reforms may need to incorporate specific requirements about the mechanisms of compliance, not just the outcomes. This might include minimum standards for both benefit levels and revenue generation, preventing states from achieving solvency purely through benefit cuts.

Second, the varying effectiveness across states with different initial conditions suggests that a one-size-fits-all approach to federal requirements may be suboptimal. Future policies might benefit from graduated requirements or targeted assistance for states starting from particularly weak positions, helping them build the institutional capacity needed for sustainable improvements.

### *Limitations*

Several important limitations of this study should be acknowledged. The moderated interrupted time series (ITS) approach faces specific methodological challenges identified by Rekkas et al. (2020). Their review points out that treatment effect modeling methods like my moderated ITS can be vulnerable to overfitting, particularly when treatment effect modifiers aren't well-established. While I address this concern by using a theoretically-grounded moderator (pre-reform AHCM) and including extensive controls for political, economic, and UI system characteristics, the potential for overfitting remains. Additionally, Rekkas and colleagues note these models may have lower statistical power than main effects models, potentially affecting detection of subtle heterogeneous effects. My complementary stratified approach helps mitigate this concern by testing for differences between clearly defined state groups, though at the cost of reduced sample size in the solvent states subgroup. Both approaches assume that pre-reform AHCM adequately captures state policy preferences, which, while theoretically justified, may not fully reflect the multidimensional nature of these preferences. Finally, as with any quasi-experimental design, causal inference depends on the assumption that observed post-reform changes were attributable to the policy intervention rather than

concurrent factors. Although I include an extensive set of time-varying controls to account for political, economic, and institutional changes during the study period, some unobserved time-varying confounders may remain. Despite these limitations, the consistent patterns across both modeling approaches strengthen confidence in the findings regarding heterogeneous state responses to federal policy.

Next, my analysis ends in 2019 due to the unprecedented disruptions caused by the COVID-19 pandemic to state UI programs, preventing us from observing how the reform's impact influenced state resilience during a major economic crisis. This timing limitation is particularly significant given that the 2020 crisis represented precisely the type of systemic shock that UI trust fund reforms were meant to address. Understanding how different compliance mechanisms (benefit restrictions versus revenue expansion) affected states' ability to handle the pandemic's unprecedented demands would provide valuable insights into the long-term implications of these policy choices.

Alternative explanations for these findings warrant careful consideration. The role of organized interests, particularly the relative strength of business and labor groups, might be more fundamental in driving differences in state approaches to trust fund management than this analysis suggests. States with stronger labor unions might have been better positioned to resist benefit cuts, while those with more powerful business lobbies might have faced greater resistance to tax increases. The controls for political factors may not fully capture these interest group dynamics, particularly given the challenge of measuring interest group influence consistently across states and time.

The interpretation of state responses as strategic behavior based on their pre-reform solvency level may also oversimplify the complex political economy of UI policy decisions. What appears as strategic response to federal incentives might instead reflect deeper institutional constraints or path dependencies. For instance, states' tendency to favor benefit restrictions over revenue expansion could stem from structural features of their political institutions, such as supermajority requirements for tax increases or strong executive budget authority, rather than conscious strategic choices.

Administrative capacity presents another alternative explanation for the findings. States with more professional legislatures, larger bureaucratic staffs, and more sophisticated fiscal management systems might have been better positioned to implement revenue-based solutions, while states with limited capacity may have defaulted to simpler benefit restrictions out of necessity rather than choice. This capacity constraint hypothesis is particularly relevant given the technical complexity of UI financing and the expertise required to design and implement effective revenue reforms.

Policy diffusion effects offer yet another alternative framework for understanding these results. The clustering of similar response strategies among neighboring states might reflect policy learning and imitation rather than independent strategic responses to federal incentives. States might have adopted approaches that proved successful in neighboring jurisdictions, leading to regional patterns in compliance mechanisms that my analysis attributes to strategic behavior.

These limitations and alternative explanations suggest the need for caution in interpreting these results and point to important areas for future research. While my findings provide

strong evidence of policy effects, the interactions of institutional constraints, interest group politics, and administrative capacity in shaping state responses warrant further investigation.

### *Conclusion*

These findings contribute to broader theoretical understanding of federal-state programs in several ways. They demonstrate how federal oversight tools can effectively influence state behavior while highlighting important constraints on their effectiveness. The analysis reveals how states can achieve technical compliance while potentially maintaining underlying vulnerabilities, suggesting that the classic principal-agent problems in federal-state programs may require more sophisticated oversight mechanisms than currently employed.

Future research could extend these findings in several directions. Studies incorporating data from the COVID-19 period could examine how pre-pandemic policy choices affected state program resilience during the crisis. Research examining specific administrative processes and implementation mechanisms could provide deeper insight into how states achieve compliance. Additionally, comparative analysis with other federal-state programs could help identify more general patterns in how states respond to federal oversight efforts.

In conclusion, this study demonstrates both the potential and limitations of federal policy tools for addressing moral hazard in federal-state programs. While federal lending criteria can successfully influence state behavior, ensuring that this influence translates into

sustainable program improvements requires careful attention to both the mechanisms of compliance and the varying constraints faced by different states. As policymakers continue to deal with maintaining robust federal-state programs, understanding these dynamics will become increasingly important for designing effective policy interventions that promote both fiscal discipline and program stability.

## CHAPTER 4

### REGIONAL NETWORKS, ECONOMIC INCENTIVES, AND LOCAL POLICY DIFFUSION: EVIDENCE FROM RETAIL MARIJUANA ORDINANCES IN CALIFORNIA CITIES

#### **Introduction**

Policy diffusion, the process by which policy choices in one jurisdiction affect those in others, is a central concern in political science and public policy research (Shipan and Volden, 2012). While scholars have extensively examined this phenomenon at the state and national levels, the dynamics of policy diffusion among local governments remain underexplored, particularly in policy areas where economic incentives may significantly influence adoption decisions. This study aims to fill this gap by investigating both institutional and economic mechanisms of local policy diffusion, using the case of retail marijuana ordinances in California following the 2016 legalization of recreational use as a lens through which to examine broader diffusion processes.

California's legalization of recreational marijuana through Proposition 64 in 2016 created an ideal setting to study local policy diffusion from its inception. The state's decision to grant significant discretion to cities and counties in determining whether and how to allow cannabis businesses within their jurisdictions set the stage for a natural experiment in policy diffusion. This decentralized approach provides two compelling research opportunities. First, California's regional municipal landscape, which includes formal networks like the League of California Cities divisions as well as informal relationships

among neighboring jurisdictions, allows us to examine how various regional peer relationships may influence policy learning and adoption. Second, variation in adoption timing across jurisdictions enables us to study how economic factors—particularly the potential to capture tax revenue from residents of neighboring non-adopting jurisdictions—influence municipal policy decisions. This context allows us to examine how local governments balance regional influences, policy learning from multiple sources, and economic considerations when responding to a new policy domain.

Our study addresses several key questions in the policy diffusion literature: (1) How do regional peer networks influence the spread of policies among local governments? (2) To what extent do economic incentives influence municipal adoption of retail policies? These questions are particularly relevant in the context of retail marijuana ordinances, where cities must weigh potential financial benefits against the uncertainties and risks associated with a new policy domain.

To investigate these questions, we have constructed a dataset tracking policy adoption for all 482 incorporated California cities from 2017 to 2023. This novel dataset enables us to analyze diffusion dynamics as they unfold over time, offering insights into both the initial spread of policies and their longer-term patterns. Our study uses Cox proportional hazards models with distinct measures to capture different aspects of policy diffusion, examining both the effects of regional peer relationships and drive-time distances as well as the influence of potential revenue generation on adoption decisions.

Our research makes several contributions to the literature on policy diffusion and local government decision-making. First, it provides a comprehensive analysis of municipal-

level policy diffusion, examining all incorporated cities in California over a seven-year period following recreational marijuana legalization. This dataset enables us to track how policies spread across diverse localities from their initial adoption opportunity, offering unique insights into the early stages of local policy diffusion. Second, we identify patterns suggesting regional peer relationships influence policy adoption among municipalities, contributing to our understanding of how information sharing may occur in new policy domains. Third, we advance understanding of how economic factors influence local policy diffusion through a nuanced examination of both internal and external market dynamics. Local governments' responsiveness to economic incentives reveal that larger internal markets appear more influential in adoption decisions than the potential for broader regional revenue generation, even when such opportunities are substantial. Finally, our methodological approach addresses a key challenge in studying municipal-level policy diffusion. While traditional diffusion studies at the state or national level typically rely on contiguous neighbors as their measure of proximity, this approach proves inadequate for municipal analysis, particularly in non-urban areas where cities may lack adjacent neighbors. Our use of structured regional groupings like League of California Cities divisions and drive-time distances provides an innovative framework for measuring potential peer relationships at the municipal level, offering a methodological template for future studies of local policy diffusion.

The remainder of this paper proceeds as follows. Section II reviews relevant policy diffusion literature and provides background on the case of marijuana policy in California. Section III develops our theoretical framework and hypotheses regarding

policy diffusion mechanisms. Section IV describes our data and empirical strategy for studying diffusion patterns. Section V presents our main results on policy diffusion dynamics. Section VI discusses the implications of our findings for policy diffusion theory and concludes.

## **Literature Review**

Understanding how and why policies spread across jurisdictions is a central concern in political science and public policy research. Policy diffusion—the process by which policy choices in one jurisdiction affect those in others—has been observed across various levels of government, from cities (Frederickson, Johnson, & Wood, 2004; Ji & Tate, 2021) and counties (Mitchell, 2016) to states (Berry & Berry, 1990; Gray, 1973; Matisoff, 2008; Volden, 2006) and even nations (Simmons & Elkins, 2004; Towns, 2012; Kern et al, 2023). While this phenomenon has been extensively studied at higher levels of government, our understanding of local policy diffusion remains limited, particularly regarding how economic incentives shape adoption decisions. This study examines diffusion dynamics at the municipal level, using retail marijuana ordinances in California cities as a lens to explore how both institutional networks and economic factors shape local policy adoption. This context is particularly valuable due to the significant discretion granted to local governments, the complex challenge of regulating a substance that remains federally illegal, and the substantial economic implications for municipal revenue generation.

### *Mechanisms of Policy Diffusion*

Early studies on policy diffusion, notably Walker (1969), established the foundational understanding that policy adoption decisions are interdependent across jurisdictions. Subsequent research has identified four primary mechanisms underlying policy diffusion: learning, imitation, competition, and coercion (Sabatier & Weible, 2014; Shipan & Volden, 2008). The first three mechanisms are particularly relevant for understanding how local marijuana policies might spread among California cities.

Learning occurs when policymakers adopt policies after observing their outcomes in other jurisdictions (Volden, 2006; Gilardi, 2010). This mechanism is especially crucial in areas of high uncertainty, such as marijuana regulation, where cities must navigate complex regulatory and public safety challenges. Through learning, cities can analyze the economic benefits, public health impacts, and implementation challenges faced by early adopters to inform their own decisions (Nicholson-Crotty & Carley, 2016). Hannah and Mallinson (2018) demonstrated this effect at the state level, finding that positive outcomes in neighboring states increased the likelihood of medical marijuana law adoption.

Imitation represents a distinct mechanism where jurisdictions adopt policies implemented by perceived leaders or peers, often without rigorous evaluation of outcomes (Jansa et al, 2019; Meseguer, 2005; Walker, 1969). This mechanism is driven by legitimacy-seeking behavior, where policies are adopted because they are considered appropriate or normatively desirable. In the municipal context, smaller cities often imitate the regulatory approaches of larger or more prestigious municipalities, leading to regional policy clusters (Shipan & Volden, 2012; Mallinson, 2021a). Shipan and Volden (2008)

illustrated this dynamic in their study of anti-smoking ordinances, where smaller cities followed the lead of larger municipalities.

Competition emerges as a diffusion mechanism when jurisdictions adopt policies in response to pressures created by their neighbors' choices (Berry & Berry, 1990; Baybeck, Berry & Siegel, 2011). This mechanism often manifests through policy adjustments aimed at maintaining economic competitiveness or preventing the loss of business activity across borders (Case, Rose, & Hines Jr, 1993; Brueckner & Saavedra, 2001). At the local level, this competitive pressure can be particularly acute given municipalities' reliance on local business activity and their need to maintain attractive policy environments relative to neighboring jurisdictions.

While coercion represents another recognized diffusion mechanism, it is less central to understanding local marijuana policy adoption, where cities maintain significant discretion over their regulatory choices. The first three mechanisms—learning, imitation, and competition—often correlate with geographic proximity, as neighboring jurisdictions typically share information more readily and face similar pressures (Mooney, 2001). However, recent research suggests that the influence of proximity is more nuanced, with policy diffusion shaped by various factors beyond mere geographic closeness.

### *Evolution of Policy Diffusion Research*

The study of policy diffusion has evolved significantly over time, moving from simple proximity-based models to more nuanced frameworks that consider multiple channels of influence. Early research emphasized the "neighbor effect," arguing that policies spread

primarily among geographically contiguous jurisdictions due to ease of communication, shared media markets, and cultural similarities (Berry & Berry, 1990; Mooney, 2001; Walker, 1969). This perspective was grounded in Tobler's First Law of Geography, which states that "everything is related to everything else, but near things are more related than distant things" (Tobler, 1970).

Contemporary scholarship has moved beyond this strict focus on geographic proximity, recognizing that jurisdictions often look to non-proximal peers when adopting policies. These choices are influenced by factors such as shared political ideology, socioeconomic characteristics, and participation in professional networks (Boehmke & Witmer, 2004; Gilardi & Füglistner, 2008; Gilardi, 2010; Desmarais, Harden, & Boehmke, 2015). For example, Gilardi (2016) demonstrated that policymakers are more likely to learn from jurisdictions with similar ideological orientations or policy environments, regardless of physical distance. This shift has led to increased attention on institutional proximity as a key factor in policy diffusion.

Some scholars have also examined how economic incentives interact with traditional diffusion mechanisms. While earlier studies often assumed that potential economic benefits would accelerate policy adoption, particularly in areas like tax policy or business regulation, emerging research suggests a more complex relationship (Shipan & Volden, 2008). Cities may be less responsive to economic opportunities when policies involve significant uncertainty or controversy, even when significant tax revenue generation is possible (Mallinson, 2021a). This finding challenges assumptions about the primacy of

economic competition in driving policy innovation and suggests the need to examine how market opportunities interact with other diffusion mechanisms.

### *Economic Incentives in Local Policy Diffusion*

While policy diffusion research has traditionally focused on learning, imitation, and competition as key mechanisms, the unique fiscal constraints and opportunities facing local governments necessitate special attention to economic incentives in municipal policy adoption. Local governments, unlike their state and federal counterparts, operate under strict budget constraints and rely heavily on local revenue generation, making economic considerations particularly salient in their policy decisions (Peterson, 1981).

Research has shown that local law enforcement and judicial systems often act strategically to improve their financial position, targeting specific groups to increase revenue (Mughan, 2021; Mughan & Singla, 2023). This economic dimension becomes especially important when studying the diffusion of policies with clear revenue implications.

Revenue generation opportunities represent significant economic incentives shaping local policy decisions. Research has shown that jurisdictions often strategically design policies to maximize tax bases, including the potential to attract consumers from neighboring areas, especially in areas like sales taxes and tourism-related fees (Mclure, 1967; Agrawal, 2015; Afonso and Moulton, 2024). This revenue-generating strategy is most effective where geographic barriers or policy restrictions create opportunities to serve as retail destinations for neighboring jurisdictions (Thompson & Rohlin, 2012). Studies of cross-border shopping have documented how differences in retail regulations can create

substantial revenue opportunities for strategically positioned jurisdictions (Fox, 1986; Goolsbee et al., 2010).

While economic incentives related to market expansion are well-documented in established policy areas, their influence on adoption decisions for new areas like local marijuana ordinances remains less understood. In the context of retail marijuana ordinances, cities might view early adoption as an opportunity to expand their tax base by serving both residents and consumers from neighboring non-adopting jurisdictions (Firth et al., 2019). However, research on local retail policy adoption suggests that the promise of economic gains may not always override the cautious approach typically seen in policy diffusion, particularly when policies involve controversial or federally regulated substances (Mallinson, 2021b).

The potential for significant revenue generation through expanded market access creates unique dynamics in local policy diffusion that may not be present at other levels of government. Cities must weigh these economic opportunities against various other factors, including implementation costs, regulatory challenges, and potential political opposition (Singla, Stritch, and Feeney, 2018). This calculation becomes particularly complex when dealing with controversial policies where the revenue potential is high but uncertainty about policy outcomes remains significant. Understanding how these economic considerations interact with geographic and institutional factors is crucial for explaining patterns of local policy adoption.

#### *Local-Level Policy Diffusion and Spatial Dynamics*

Local governments play a crucial role in policy diffusion, particularly in policy areas where municipalities have significant discretion, such as marijuana regulation (Shipan & Volden, 2008; Mitchell, 2018). Despite their importance as sites of policy innovation, the extent to which diffusion processes vary among local jurisdictions remains underexplored (Ji & Tate, 2021). Understanding local-level diffusion is essential because municipalities operate under different constraints and opportunities compared to states, facing unique pressures from constituents and stakeholders.

Research indicates that while geographic proximity matters in local policy diffusion, its influence varies considerably based on local contexts and the presence of regional leaders or policy champions (An, Butz, & Mitchell, 2022; Ji & Tate, 2021). For instance, Ji and Tate (2021) found that the impact of neighboring jurisdictions on municipal climate change policy adoption varied significantly across metropolitan areas, demonstrating spatial heterogeneity in diffusion processes. The complexity of local policy diffusion is further heightened by factors such as intergovernmental relationships, local political climates, and resource availability (Zhang & Yang, 2008; Feiock & West, 1993). These variations suggest that simple proximity-based models may be insufficient for understanding local policy diffusion.

#### *Role of Regional Leaders and Networks*

Regional leaders—typically larger cities with greater resources and administrative capacity—can significantly shape policy diffusion by serving as models for other jurisdictions (Lutz, 1987; Shipan & Volden, 2012). These cities often pioneer policy innovations and influence both neighboring and non-proximal municipalities through

mechanisms of learning and imitation (Ji & Tate, 2021). Professional associations represent one of multiple channels through which this influence may flow, creating structured opportunities for local governments to interact and potentially share information about policies and programs.

In the California context, municipal officials participate in various formal and informal networks that may facilitate information sharing. The League of California Cities (LoCC) represents one such network structure, with its organization into 16 regional divisions creating geographically defined groupings that provide opportunities for city officials to interact (League of California Cities, 2021a). While these divisions constitute just one of many potential channels for policy information exchange, they offer a methodologically useful way to identify potential peer relationships among municipalities facing similar regional challenges.

Municipal officials exchange information through multiple pathways, including formal association meetings, informal professional relationships, shared consultants, and direct observation of neighboring jurisdictions' experiences. The LoCC divisions host periodic events where policy discussions may occur, including sessions on economic development strategies and public safety initiatives (League of California Cities, 2021b, 2021c). These structured interactions represent one potential environment where policy learning might take place, alongside numerous other formal and informal channels through which municipal officials gather information about innovative policies.

The influence of regional leaders often extends through various communication networks simultaneously, as cities look to successful peers for guidance when facing uncertainty in

novel policy areas (Frederickson, Johnson, & Wood, 2004). This dynamic highlights the importance of considering both geographic proximity and institutional relationships when studying patterns of policy diffusion. Using the LoCC divisions as an empirical measure provides a structured approach to identifying potential peer networks while acknowledging the complex web of relationships through which policy information flows among local governments.

### *Methodological Approaches in Policy Diffusion Research*

The evolution of policy diffusion research has been accompanied by methodological challenges, particularly in measuring and analyzing how policies spread between jurisdictions. Event history analysis, especially Cox proportional hazards models, has emerged as a valuable tool for studying the timing of policy adoption (Berry & Berry, 1990; Box-Steffensmeier & Zorn, 2001; Jones & Branton, 2005; Bromley et al, 2016). However, these models require careful consideration of how to measure relationships between jurisdictions, traditionally operationalized through geographic contiguity or proximity measures.

This measurement challenge is particularly acute when studying policy diffusion at the municipal level. While state-level analyses can rely on clearly defined contiguous neighbors (Berry & Berry, 1990; Butz et al, 2015), this approach becomes problematic for municipal studies, especially in non-urban areas where cities may be geographically isolated or lack adjacent neighbors (Mitchell, 2018). Some scholars have addressed this by developing spatial econometric approaches that incorporate more nuanced measures of

geographic relationships (Anselin, 1988; Fotheringham et al, 2002; Berry & Baybeck, 2005; LeSage & Pace, 2009).

Network analysis offers another promising approach, recognizing that policy diffusion occurs through various channels beyond geographic proximity (Desmarais, Harden, & Boehmke, 2015; Karch, 2007; Maroulis, 2016). This is particularly relevant for studying how professional associations and formal institutional networks influence policy adoption. For example, Ji and Tate (2021) demonstrated how institutional relationships can shape diffusion patterns independently of geographic proximity. However, there remains a need for methodological approaches that can better capture both the formal networks and spatial relationships that influence municipal policy adoption, particularly in contexts where traditional contiguity measures may be insufficient.

### **Theory and Hypotheses**

Building on the policy diffusion literature discussed, we now develop a theoretical framework specifically tailored to the adoption of retail marijuana ordinances among California cities. This framework emphasizes the role of regional networks in facilitating policy diffusion through learning and imitation mechanisms.

The legalization of recreational marijuana in California presents a unique policy environment characterized by high uncertainty and complexity. In such contexts, we argue that the influence of regional networks on policy adoption is likely to be particularly strong. The regulation of recreational marijuana is a new policy area for most cities, creating a high degree of uncertainty about potential outcomes, implementation

challenges, and best practices. In such situations, theory suggests that policymakers are more likely to rely on the experiences and decisions of their peers to inform their own choices. This reliance on peer experiences is further amplified by the legal ambiguity surrounding marijuana regulation. Despite state-level legalization, the federal illegality of marijuana creates a complex legal landscape. This ambiguity increases the perceived risks associated with adoption, potentially making cities more reliant on the choices of their regional peers when making decisions.

The potential for significant local impact further underscores the importance of regional influences in this policy area. Retail marijuana ordinances have the potential to significantly affect local communities in terms of public health, safety, and economic outcomes (Mughan, 2024). The high stakes involved may encourage cities to closely observe and learn from the experiences of similar jurisdictions within their region. This learning process likely occurs through multiple channels, with cities potentially drawing insights from peers that share similar demographic, economic, and cultural characteristics. Cities within the same League of California Cities division often share regional characteristics and face comparable challenges, potentially enhancing the perceived relevance of their peers' experiences. These similarities may increase the likelihood that cities will view the policy choices of their regional counterparts as applicable to their own contexts.

Information exchange among municipalities occurs through various formal and informal mechanisms. Structured interactions through regional associations like the League of California Cities represent one potential channel, while informal professional networks,

shared consultants, direct observation, and media coverage constitute others. These multiple pathways for communication can collectively contribute to the dissemination of information about policy outcomes, implementation strategies, and challenges faced by early adopters. The League's divisional structure provides a methodologically useful way to identify potential peer relationships when examining patterns of policy adoption, while recognizing that information likely flows through multiple channels simultaneously.

Given these factors, we expect that the regional networks will play a significant role in shaping city-level decisions about retail marijuana ordinances. Specifically, we anticipate that as more cities within a regional division adopt these ordinances, the likelihood of adoption for other cities in the same division will increase. This expectation is based on two primary mechanisms: cumulative learning and normative pressure that spurs imitation. As more cities within a division adopt retail marijuana ordinances, the collective knowledge about implementation strategies, challenges, and outcomes grows. This expanding pool of relevant, localized information reduces uncertainty for potential adopters, making adoption more likely. Simultaneously, a higher proportion of adopters within a division may create a sense of inevitability or appropriateness around the policy. Cities may feel increasing pressure to conform to what is becoming a regional norm, even in the absence of concrete evidence about policy outcomes.

These mechanisms are expected to operate simultaneously and reinforce each other, leading to a positive relationship between the proportion of adopters in a region and the likelihood of adoption for any given city. This leads us to our central hypothesis:

*H1: The likelihood of a city adopting a permissive retail marijuana ordinance increases with the proportion of cities within its League of California Cities regional division that have already adopted such ordinances.*

This hypothesis reflects our expectation of a positive relationship between regional adoption rates and individual city adoption probabilities. We anticipate that the effect may be particularly strong in the early and middle stages of diffusion, as the initial uncertainty surrounding the policy is reduced and normative pressures build. However, we also recognize the possibility of diminishing returns at very high levels of regional adoption, as the marginal informational and normative value of each additional adopter may decrease.

Beyond the learning and imitation processes discussed in earlier sections, economic incentives play a crucial role in shaping the spread of retail marijuana ordinances at the local level. Local governments face unique fiscal pressures and opportunities that can significantly influence their policy decisions. This dynamic is particularly relevant in the context of retail marijuana ordinances, where municipalities can potentially generate substantial tax revenue through both resident and non-resident consumption. The opportunity for broader revenue generation becomes especially salient when a municipality is surrounded by non-adopting jurisdictions with sizable populations that lack local access to legal marijuana. In such circumstances, adopting an ordinance could transform the city into a retail destination, potentially creating new revenue streams through both resident and non-resident consumption (McLure, 1967). In such

circumstances, adopting an ordinance could transform the city into a retail destination, creating a new revenue stream through non-resident consumption.

From this reasoning, we derive our second hypothesis:

*H2: The greater a city's economic incentive for broader market access, the more likely the city is to adopt a permissive retail marijuana ordinance..*

This hypothesis reflects our expectation that economic incentives can influence adoption decisions. While we recognize that traditional diffusion mechanisms remain important, we argue that substantial economic opportunities may lead cities to adopt despite the uncertainties typically associated with early adoption of controversial policies. In other words, when the potential economic benefits are sufficiently large—whether from local consumer spending or attracting regional consumers—these financial incentives may override the cautious approach typically seen in policy diffusion.

## **Data and Methodology**

To test our hypotheses about the influence of regional networks and economic incentives on policy adoption, we examine the adoption of retail marijuana ordinances by California cities from January 2017 to December 2023. Our sample encompasses all 482 incorporated cities in California, offering a comprehensive view of local policy responses to the state's legalization of recreational marijuana use. We begin our study period in 2017, as this was the first full year after the passage of Proposition 64 in November 2016, which legalized recreational marijuana in California. This starting point allows us to capture the initial wave of policy responses following the change in state law.

The primary dependent variable is the adoption of a retail marijuana ordinance that allows the sale of recreational marijuana, coded as a binary variable where 1 indicates adoption and 0 indicates non-adoption. We manually collected this data from each city's municipal code, ensuring accuracy and completeness in tracking policy changes over time.

While policy diffusions studies with units of analysis at the state or national level typically use measures such as the percentage or count of geographically contiguous jurisdictions that have adopted a policy, such a measure doesn't always make sense when examining local jurisdictions, especially in rural areas. To capture regional policy diffusion effects of learning and imitation at the local level, we constructed two key independent variables. The first, Percentage of Division Adopters, represents the percentage of cities within a given city's California League of Cities division that have adopted retail marijuana ordinances at each time point. It is calculated as the number of adopting cities in the division divided by the total number of cities in the division, multiplied by 100. The second variable, Cumulative Adopters, represents the count of cities within a given city's division that have adopted retail marijuana ordinances at each time point. Using both measures allows us to ensure our results are robust, with both common measures used in the policy diffusion literature.

To test our economic incentives hypothesis, we employ two complementary measures that capture both internal and external market potential. First, we use a city's labor force size to represent its internal market capacity. Second, we construct a broader market opportunity measure that calculates the total labor force of all non-adopting cities within

a 30-minute drive time from each potential adopting city. This approach reflects our understanding that policymakers likely consider multiple economic factors when evaluating retail marijuana ordinances: their existing resident customer base, the regulatory landscape of nearby municipalities, and the potential to serve consumers from surrounding areas who lack local access. Higher values of our market opportunity measure indicate greater revenue generation potential, particularly when a city is positioned near large, non-adopting populations within convenient traveling distance. Both measures are recalculated at each time point to account for changes in adoption status and population characteristics, creating time-varying covariates that capture the evolving economic landscape facing each jurisdiction.

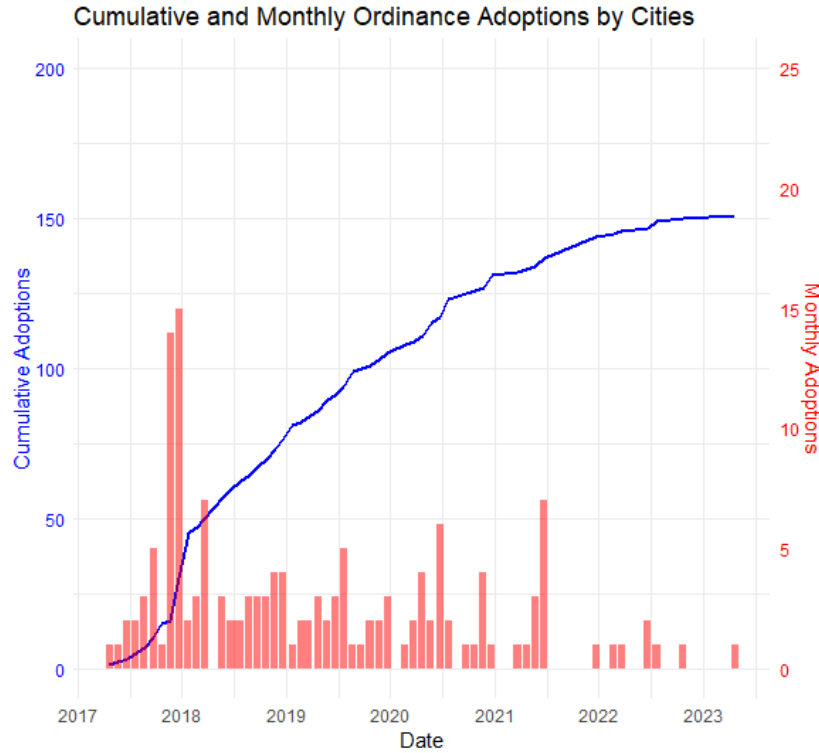
We incorporate control variables selected to account for factors that might influence both regional diffusion patterns and market-driven adoption decisions. Demographic variables that could affect a city's policy preferences include male-female percentages of the population, median age, and race/ethnicity percentages (White, Black, Native American, Hispanic, Asian, and Pacific Islander). Political factors, which may affect a city's receptiveness to peer influence and policy innovation, include the percentage of registered voters affiliated with the Democratic party and whether the county has adopted a retail marijuana ordinance. Economic indicators that could impact market-based adoption decisions include the percentage of the population with a high school diploma, unemployment rates, labor force size, and debt per capita. These controls help isolate the effects of our key variables of interest - regional peer influence and economic incentives - from other factors that might affect adoption decisions.

We sourced our data from several reputable databases. Demographic data came from the U.S. Census Bureau's American Community Survey (2020 5-year estimates). Economic indicators were obtained from the California Department of Finance and Bureau of Labor Statistics. Election results and party registration data were sourced from the California Secretary of State. Geographic data was derived from the U.S. Census Bureau TIGER/Line Shapefiles. Information on marijuana ordinance adoption was manually collected from city municipal codes and official websites. Our final dataset is structured as a panel, with each city having multiple observations over time (one per month from January 2017 to December 2023). This structure allows us to capture changes in both the dependent and independent variables over time. Table 1 presents descriptive statistics for the variables used in this analysis.

Table 1. Descriptive Statistics

| Variable                         | N      | Mean    | Std. Dev. | Min   | Max       |
|----------------------------------|--------|---------|-----------|-------|-----------|
| LoC Division Percent Adopted     | 38,240 | 21.78   | 16.83     | 0     | 66.67     |
| LoC Division Cumulative Adopters | 38,240 | 7.31    | 4.87      | 0     | 18        |
| External Market Size             | 35,760 | 233,248 | 336,430   | 0     | 2,668,600 |
| Labor Force                      | 35,760 | 35,573  | 112,691   | 100   | 2,175,800 |
| Unemployment Rate (%)            | 35,760 | 6.08    | 4.48      | 0     | 45.6      |
| County Adopted at Time           | 38,240 | 0.2     | 0.04      | 0     | 1         |
| Female (%)                       | 36,560 | 0.51    | 0.03      | 0.29  | 0.62      |
| Median Age (years)               | 36,560 | 39.34   | 7.04      | 25.7  | 74.9      |
| White (%)                        | 36,560 | 0.46    | 0.21      | 0.08  | 0.89      |
| Black (%)                        | 36,560 | 0.04    | 0.05      | 0     | 0.39      |
| Native (%)                       | 36,560 | 0.02    | 0.01      | 0     | 0.09      |
| Asian (%)                        | 36,560 | 0.13    | 0.15      | 0     | 0.72      |
| Islander (%)                     | 36,560 | 0       | 0.01      | 0     | 0.07      |
| High School or GED (%)           | 36,560 | 21.06   | 8.13      | 2.2   | 43.48     |
| Democratic Party (%)             | 38,240 | 44.3    | 11.43     | 15.52 | 74.55     |

Figure 1 shows the pattern of adoption over time, beginning in 2017 and ending in 2023.



To analyze the diffusion of retail marijuana ordinances, we use an event history analysis framework, specifically a Cox proportional hazards model. This approach is well-suited for studying policy diffusion as it allows us to examine the timing of adoption while accounting for right-censoring in the data (Berry & Berry, 1990; Shipan & Volden, 2008). The Cox model is particularly appropriate for our study because it does not require us to specify the baseline hazard function, allowing for flexibility in the underlying time dependence of policy adoption.

The Cox proportional hazards model is specified as follows:

$$h(t) = h_0(t) * \exp(\beta_1 X_1 + \beta_2 X_2 + \dots + \beta_k X_k)$$

Where  $h(t)$  is the hazard rate of adoption at time  $t$ ,  $h_0(t)$  is the baseline hazard function, and  $X_1, X_2, \dots, X_k$  are the independent variables with their respective coefficients  $\beta_1, \beta_2, \dots, \beta_k$ .

...,  $\beta_k$ . The hazard rate can be interpreted as the probability of a city adopting a retail marijuana ordinance at time  $t$ , given that it has not yet adopted one.

One of the key advantages of the Cox model is its ability to handle time-varying covariates. Many of our independent variables, such as the percentage of adopters in a city's division, change over time. The Cox model allows us to incorporate these dynamic factors, providing a more accurate representation of the policy environment at each point in time. To account for potential correlation in adoption decisions among cities within the same California League of Cities division, we cluster our standard errors at the division level. This approach allows for within-division correlation while maintaining independence between divisions.

## Results

Table 2 reports results from several Cox proportional hazards models to examine the factors influencing the adoption of retail marijuana ordinances by California cities. We present our findings organized around our first hypothesis.

Table 2. Estimates of Policy Diffusion and Economic Incentives

| Variable                               | (1)                 | (2)                 |
|----------------------------------------|---------------------|---------------------|
| LoC Division Percent Adopted           | 0.038***<br>(0.009) |                     |
| LoC Division Cumulative Adopters (log) |                     | 0.752***<br>(0.249) |
| External Market Size                   | -0.026<br>(0.019)   | -0.036<br>(0.026)   |
| Labor Force (log)                      | 0.295***<br>(0.084) | 0.235***<br>(0.037) |
| Unemployment Rate (%)                  | 0.062***<br>(0.026) | 0.036*<br>(0.020)   |

|                        |                     |                     |
|------------------------|---------------------|---------------------|
| County Adopted at Time | 1.252***<br>(0.218) | 1.117***<br>(0.267) |
| Female (%)             | 0.001<br>(0.023)    | -0.013<br>(0.024)   |
| Median Age (years)     | -0.025*<br>(0.014)  | -0.034*<br>(0.018)  |
| White (%)              | 0.029***<br>(0.008) | 0.035***<br>(0.010) |
| Black (%)              | 0.015<br>(0.009)    | 0.009<br>(0.010)    |
| Native (%)             | 0.100***<br>(0.029) | 0.157***<br>(0.040) |
| Asian (%)              | 0.002<br>(0.007)    | 0.000<br>(0.009)    |
| Islander (%)           | 0.132<br>(0.115)    | 0.196<br>(0.148)    |
| High School or GED (%) | 0.053***<br>(0.014) | 0.012***<br>(0.008) |
| Democratic Party (%)   | 0.040***<br>(0.008) | 0.049***<br>(0.008) |
| Observations           | 35,760              | 35,760              |
| Number of Events       | 153                 | 153                 |
| Concordance            | 0.748               | 0.723               |

Note: Robust standard errors clustered by League of California Cities division are shown in parentheses.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### *Hypothesis 1*

Our first hypothesis expected the likelihood of a city adopting a retail marijuana ordinance to increase with both the proportion and count of cities within its League of California Cities regional division that had already adopted such ordinances. We find strong support for this hypothesis across our models. This effect remains robust when controlling for various city characteristics (Model 1:  $\beta = 0.038$ ,  $p < 0.01$ ). The hazard

ratio indicates that for each percentage point increase in adoption within a city's division, the hazard of adoption increases by approximately 4%. Similarly, we find strong support when using the cumulative number of adopters (log-transformed) as our measure of division influence. The effect is positive and significant in the full model (Model 2:  $\beta = 0.754$ ,  $p < 0.01$ ). The magnitude of these coefficients suggests that a one-unit increase in the log of cumulative adopters substantially increases the hazard of adoption.

Several control variables showed consistent effects across our models. Labor force size demonstrated a positive and significant effect across all specifications, suggesting larger cities are more likely to adopt retail marijuana ordinances. The percentage of white residents generally showed a positive association with adoption. County-level adoption of retail marijuana ordinances consistently showed a large, positive, and significant effect across all models, indicating the importance of county-level decisions on city policies.

These results provide strong support for our theoretical framework emphasizing the role of regional networks in policy diffusion. The positive and significant effects across multiple measures of regional influence demonstrate the robust relationship between regional adoption patterns and individual city adoption decisions. This finding aligns with our theoretical expectations about the importance of regional networks in facilitating both learning and imitation processes, particularly in novel and complex policy areas like marijuana regulation.

To validate our Cox proportional hazards models, we conducted Schoenfeld residuals tests to assess the proportional hazards assumption (Grambsch and Therneau, 1994). In our initial test, we found that the assumption held for the model as a whole (global p-

value = 0.375) and for most individual variables. However, the unemployment rate variable showed a potential violation of this assumption ( $p = 0.064$ ), although it was not statistically significant at the conventional 0.05 level. All other variables in the model satisfied the proportional hazards assumption.

To address this potential issue and allow for a more flexible model, we followed the approach suggested by Box-Steffensmeier and Zorn (2001) and included an interaction term between the unemployment rate and time. This technique allows for time-varying effects of covariates that may violate the proportional hazards assumption. After this adjustment, we re-ran the Schoenfeld residuals test. The results showed that all variables, including the unemployment rate and its time interaction, now clearly satisfied the proportional hazards assumption. The global test also confirmed that the model as a whole met this assumption (global  $p$ -value = 0.77). The inclusion of this time-varying effect for unemployment did not materially change the results of our prior models. This adjustment ensures that our final model adequately accounts for any time-varying effects of unemployment on the adoption of retail marijuana ordinances, enhancing the robustness of our findings while maintaining the overall interpretation of our results.

### *Hypothesis 2*

Moving our focus to our second hypothesis, we begin by presenting descriptive statistics showing the differences between municipalities that were first to adopt in their League of Cities division, those that adopted later, and those that have yet to adopt such an ordinance.

| Table 3. Characteristics by Adoption Status (Mean Value) |                |         |         |
|----------------------------------------------------------|----------------|---------|---------|
| Variable                                                 | Adoption Order |         |         |
|                                                          | First          | Later   | Never   |
| External Market Size                                     | 135,141        | 120,378 | 260,384 |
| Total Market                                             | 271,849        | 258,043 | 388,485 |
| Labor Force                                              | 68,245         | 63,556  | 26,815  |
| Debt per Capita                                          | \$340          | \$1,505 | \$8,174 |
| Unemployment Rate                                        | 7.87           | 6.56    | 5.87    |
| Female %                                                 | 49.30          | 50.50   | 50.80   |
| Median Age                                               | 35.94          | 37.73   | 39.91   |
| White %                                                  | 39.80          | 46.40   | 46.80   |
| Black %                                                  | 5.10           | 4.90    | 3.50    |
| Native %                                                 | 2.00           | 2.00    | 1.60    |
| Asian %                                                  | 6.90           | 9.70    | 13.80   |
| Islander %                                               | 0.30           | 0.40    | 0.30    |
| HS/GED %                                                 | 24.17          | 22.76   | 20.48   |
| Democrat %                                               | 47.37          | 48.42   | 43.17   |

Table 3 reveals compelling patterns regarding the role of economic incentives in policy adoption timing. First adopters possess substantially larger internal markets, with an average labor force of 68,245 compared to never-adopters' 26,815. These early-moving municipalities also demonstrate greater financial stability (debt per capita of only \$340 compared to \$8,174 for never-adopters), younger populations (median age 35.94 versus 39.91), and more progressive political leanings (Democratic registration of 47.37% versus 43.17%). This profile suggests that early adopters are predominantly larger, financially secure cities with socio-political characteristics that might predispose them to innovation in such a policy area.

Contrary to theoretical expectations about market-driven adoption, cities with the greatest potential to serve consumers from neighboring jurisdictions are not leading adoption efforts. Never-adopters actually show the highest average external market size (260,384)

compared to first adopters (135,141). This counterintuitive finding suggests that the ability to generate revenue from regional consumers alone does not appear to drive early policy adoption, even when such economic opportunities are substantial.

The Cox proportional hazards models in Table 2 further reinforce these observations. Internal market size (labor force) demonstrates a strong positive and statistically significant relationship with adoption likelihood across both models (Model 1:  $\beta = 0.295$ ,  $p < 0.01$ ; Model 2:  $\beta = 0.235$ ,  $p < 0.01$ ). Meanwhile, external market opportunity shows a negative, though not statistically significant, relationship with adoption probability (Model 1:  $\beta = -0.026$ ; Model 2:  $\beta = -0.036$ ). This statistical evidence indicates that cities consider the size of their own resident consumer base when making adoption decisions, while the potential to serve regional markets does not appear to accelerate policy adoption.

These findings reveal a more nuanced relationship between economic incentives and policy adoption than our theoretical framework initially anticipated. Rather than seeing higher adoption rates among cities strategically positioned to maximize revenue, we find that internal market considerations and city characteristics appear influential while external markets are not. The characteristics of early adopters—typically larger municipalities with younger, more politically progressive populations—align with previous findings in the diffusion literature about municipal innovation. Meanwhile, the consistent significance of division-level adoption patterns across our models highlights the importance of regional peer relationships in the diffusion process. This pattern suggests that municipalities weigh potential economic benefits against various other

factors, including implementation complexities, regulatory uncertainties, and regional norms, when approaching controversial policy innovations. Adoption decisions likely reflect multiple considerations being evaluated simultaneously, with internal market dynamics and peer influence appearing more consequential than the potential to generate revenue from regional consumers.

## **Discussion and Conclusion**

This study set out to examine how regional networks influence policy diffusion at the municipal level, while also investigating how economic incentives affect adoption decisions when substantial revenue-generating opportunities exist. Using the adoption of retail marijuana ordinances in California cities as our empirical context, our research makes significant contributions to the literature on policy diffusion by enhancing our understanding of diffusion processes among local governments, an understudied level of analysis compared to state and national jurisdictions.

Our findings provide strong support for the importance of regional influence in facilitating local policy diffusion. Specifically, we found that the likelihood of a city adopting a retail marijuana ordinance increases with both the proportion and count of cities within its League of California Cities regional division that have already adopted such ordinances. This effect remains robust across various model specifications and different measures of regional influence, including controls for demographic, economic, and political factors. The consistency of these findings suggests that regional channels play a crucial role in municipal policy adoption decisions.

However, our investigation of economic incentives yields nuanced results that challenge conventional wisdom about local policy adoption. Our analysis reveals that the relationship between market opportunities and policy adoption differs from what we hypothesized. In both our models, cities with greater external market potential—those surrounded by large populations in non-adopting jurisdictions—did not show an increase in the likelihood of adoption, while cities with larger internal markets were significantly more likely to adopt. This suggests that the decision to adopt retail marijuana ordinances is complicated by several factors beyond simple economic calculations. Municipal officials must weigh potential financial benefits against the uncertainties inherent in implementing controversial policies, particularly in the absence of nearby examples to guide implementation. Without concrete examples from peer cities, leaders may hesitate to proceed in an uncharted policy environment, even when economic incentives are strong. The key consideration becomes whether the potential fiscal benefits are sufficient to justify undertaking the administrative complexities and political risks associated with a newly legalized industry.

These economic calculations are particularly complex at the municipal level, where budget constraints are often more binding than at state or federal levels. Cities must carefully evaluate whether the projected revenue from retail marijuana sales—including potential revenue from both resident and non-resident consumers—outweighs the costs and risks of early adoption. This assessment is especially critical given that early adopters face greater uncertainty about both implementation challenges and public response.

These findings suggest that the potential for revenue generation may play a less decisive

role in adoption decisions than traditional theories would predict. This pattern is particularly noteworthy given that early adopters in our data tend to be larger, financially stable municipalities with younger, more politically progressive populations, rather than those positioned to serve the broadest regional markets.

By examining both regional influence and economic incentives, our research expands our understanding of how institutional structures and market opportunities shape local policy diffusion. Our study demonstrates that regional networks create important channels for information sharing and peer influence that facilitate policy adoption, while potential revenue generation alone may not be sufficient to drive adoption of controversial policies. This finding aligns with and extends work by scholars such as Shipan and Volden (2008), who emphasize the importance of institutional channels for policy diffusion, while specifically illuminating how these processes operate at the municipal level.

Our analysis using multiple measures of regional influence provides robust evidence of these effects on policy adoption. The consistent positive effects across different measurement approaches strengthens our confidence in the role of regional networks in facilitating policy diffusion. This finding contributes to our understanding of how different channels of regional influence operate at the municipal level.

Our research also deepens our understanding of the role of vertical policy diffusion in the local government context. We find that the adoption of a permissive retail marijuana ordinance by counties significantly increases the likelihood of a city within that county adopting similar policies. This adds to the literature on multilevel governance and

highlights how local governments are influenced by higher-tier jurisdictions, even when no formal mandate is in place.

Beyond theoretical contributions, this study has significant implications for policymakers and regional professional associations. Our findings suggest that fostering regional cooperation and information exchange through professional networks can be a powerful tool for local governments seeking to adopt complex and uncertain policies. However, the null relationship between external market potential and adoption likelihood indicates that policymakers should not assume economic incentives alone will drive policy innovation, particularly in controversial policy areas.

#### *Limitations and Future Research*

Several limitations of our research warrant consideration and suggest promising avenues for future inquiry. A primary limitation is our focus on a single state and policy area. While California's approach to marijuana regulation offers a rich context for analysis, the state's unique political, economic, and regulatory landscape may limit the generalizability of our findings.

Our quantitative methodology, while providing robust statistical evidence, has inherent limitations in capturing the nuanced decision-making processes of local policymakers. The complexity of factors influencing policy adoption decisions may not be fully reflected in our models. Additionally, while our findings demonstrate strong relationships between regional adoption patterns and individual city adoption, establishing definitive causal relationships remains challenging within the constraints of our research design.

Another limitation relates to our use of League of California Cities divisions as our measure of regional peer networks. While we use these divisions primarily as a methodological approach to identify potential peer relationships, our research design cannot definitively determine which specific communication channels most significantly influence diffusion patterns. It is worth noting that there is substantial evidence the League actively facilitated information sharing about cannabis regulations—including dedicated listservs for information on local ordinances, statewide surveys to determine best practices, and numerous cannabis-focused roundtables and meetings (League of California Cities, 2021c). These activities lend credibility to our methodological approach of using LoCC divisions to study regional diffusion patterns, even as we acknowledge that policy information likely flows through multiple formal and informal channels simultaneously.

This study makes a significant contribution to our understanding of local policy diffusion dynamics by demonstrating the important role of regional influence in shaping municipal policy decisions, while also revealing the limitations of economic incentives in driving policy adoption. These findings not only advance theoretical discussions in public administration and policy studies but also offer practical insights for policymakers and professional associations seeking to navigate uncertain policy terrain.

## CHAPTER 5

### SUMMARY

This dissertation has explored how incentives shape behavior, decision-making, and institutional outcomes in multiple arenas of American public policy. In doing so, it has combined theoretical insights from economics, public administration and policy, and fiscal federalism to assess how diverse actors respond to changes in incentive structures. Three key findings emerge from the essays:

#### **1. Incentives for UI Claimants Can Enhance Solvency**

The first essay finds that increasing the income disregard in unemployment insurance, which many feared would raise costs by boosting overall claims, instead improves trust fund solvency in most states. The “steppingstone” effect—shortening claim durations through part-time work—appears to outweigh any expansion of the UI claimant pool. The policy implication is that *well-crafted incentives for unemployed workers need not compromise financial stability*. Indeed, moderate expansions in partial UI benefits may provide a pathway to stronger trust funds if they truly facilitate more rapid reemployment.

#### **2. Federal Constraints Work, but Implementation Varies**

The second essay shows that the federal government’s 2010 decision to tighten interest-free borrowing criteria for UI trust funds led to meaningful improvements in state-level reserve-building. Yet states that started in the worst solvency positions may have disproportionately met federal benchmarks by restricting

benefits, rather than expanding revenues. This “sticks without carrots” approach reduced moral hazard in the narrow sense of discouraging perpetual borrowing but did little to ensure sustainable financing across economic cycles. The policy lesson for federal-state programs is that *metrics-based oversight can induce compliance, but states facing political constraints often choose paths of least resistance, which may undercut the broader goals of program resilience.*

### **3. Local Adoption of Controversial Policies Hinges on Peer Networks More than Revenue**

The third essay investigates how California’s cities responded to the 2016 legalization of recreational cannabis. Counter to expectations that municipalities would be strongly driven by economic incentives to capture greater market shares, the analysis shows that cities with the biggest potential markets are *no more likely* than their peers to adopt a permissive marijuana ordinance. By contrast, higher adoption rates of peer municipalities in regional networks—particularly the League of California Cities divisions—strongly predicts adoption. This underscores the importance of *policy learning and imitation in new regulatory domains*, even when foregone revenue might be significant. Cities appear to rely on examples set by regional peers to navigate unknown risks and political sensitivities, rather than rushing to secure short-term tax windfalls.

Taken together, these findings highlight the fundamental complexity of using financial incentives to steer behavior. Although theory often assumes straightforward responses to price signals, each essay reveals how political context, institutional structures, and

administrative networks mediate incentive effects. In some cases (Essay One), an incentive can yield broad-based benefits that simultaneously enhance individual welfare and system solvency. In others (Essay Two), meeting a federally imposed target may prompt strategic compliance that achieves technical “success” but fails to achieve the deeper objective of broad financial sustainability. And in local governance (Essay Three), carefully structured revenue opportunities can be overshadowed by uncertainty, risk aversion, and peer-driven policy learning.

For scholars of fiscal federalism and multi-level governance, the dissertation demonstrates how subnational entities—from states administering federal programs to municipalities implementing state authorizations—adapt to incentives within a tapestry of constraints. Fiscal incentives do work, but they do not always operate in isolation. They intersect with political dynamics, from statehouse negotiations over UI to city council deliberations about cannabis. They interact with professional associations and policy networks that transmit ideas and reduce information costs. They also reflect differing balances between short-term political feasibility and longer-term administrative capacity.

For policymakers, the most salient lesson is that incentivizing desired behaviors requires more than setting the “right price.” It means anticipating strategic responses, understanding local constraints, and ensuring that complementary structures—like peer learning platforms or robust oversight—are in place. In the unemployment insurance context, states can redesign partial UI benefits to better incentivize reemployment while still preserving solvency. However, states need broad-based political consensus, not just federal guidelines, if they aim to sustain funding without displacing costs onto claimants.

In local cannabis regulation, state officials might reduce regulatory uncertainty, coordinate best-practice sharing, or offer technical assistance, thereby lowering the barriers that deter municipalities from adopting a potentially beneficial policy.

The dissertation also highlights the advantages of robust quasi-experimental methods in studying how policy incentives diffuse across jurisdictions. Approaches such as difference-in-differences, event history analysis, and interrupted time series can capture nuanced variation and bolster causal inferences, even amid the inherent messiness of real-world policymaking. Future work might apply these methods to other emerging industries (e.g., sports betting or cryptocurrency regulation) or to structural policy challenges (e.g., climate adaptation financing) where subnational variation can yield critical lessons.

Incentives are powerful but not omnipotent. They operate through layered governance structures, shaped by administrative capacity, political constraints, and networks of learning and diffusion. By examining UI income disregards, federal UI loan constraints, and local cannabis ordinances, this dissertation shines light on the conditions under which incentive policies succeed, the pathways states and localities choose to follow, and the reasons why even significant revenue opportunities may not always translate into rapid policy uptake. Through these findings, it underscores the importance of nuance, context, and rigorous evaluation when deploying fiscal incentives as tools for public policy.

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## APPENDIX A

### ADDITIONAL FIGURES

Figure 4. Synthetic DiD Treatment Effect Sizes - AHCM

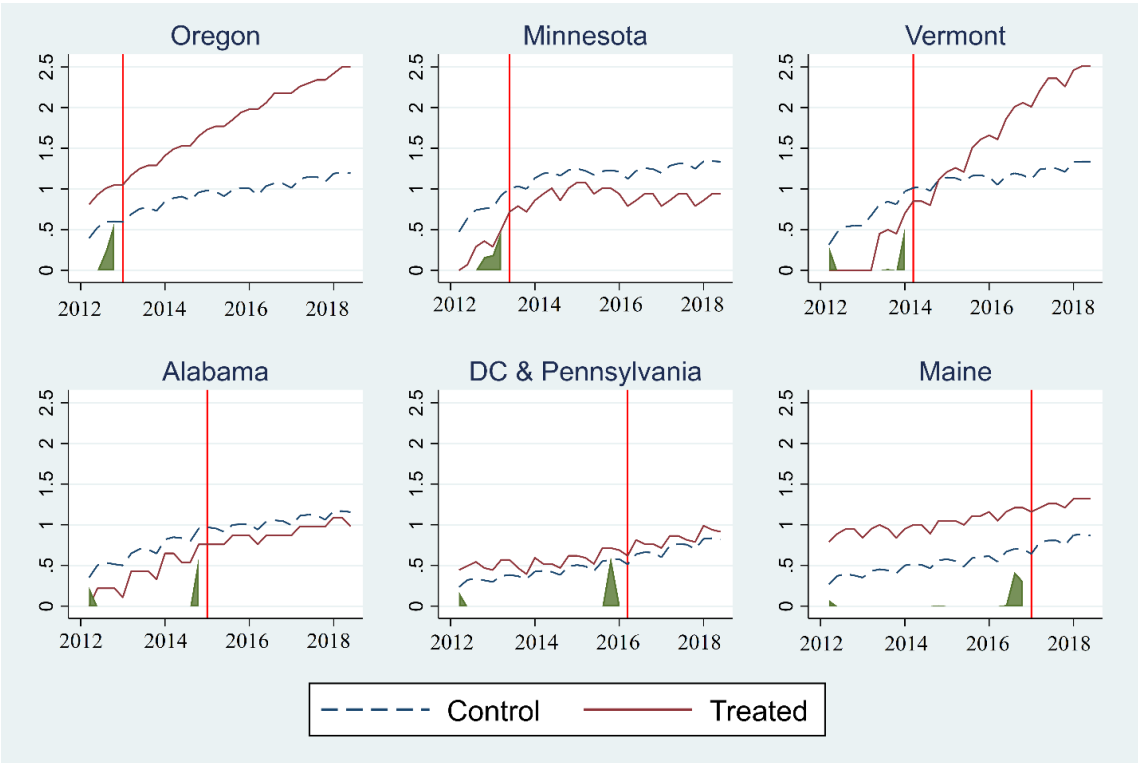


Figure 5. Synthetic DiD Treatment Effect Sizes – Reserve Ratio

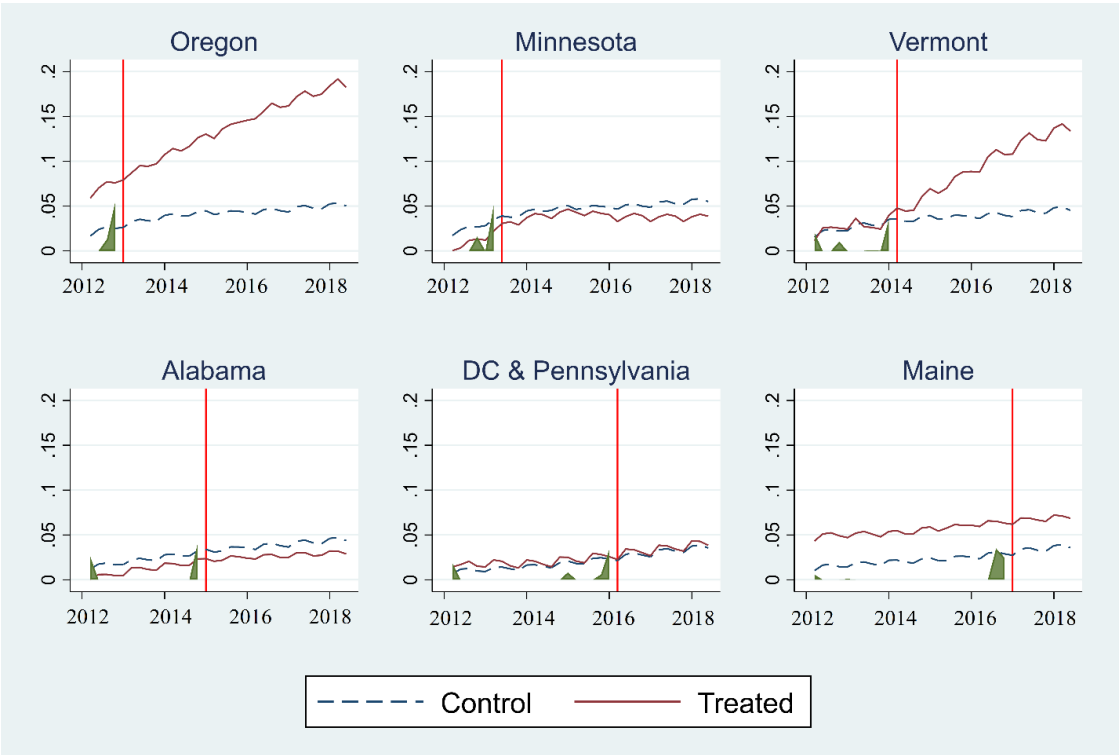


Figure 6. Sant’Anna-Calloway Treatment Effect Sizes by Treatment Timing Group - AHCM

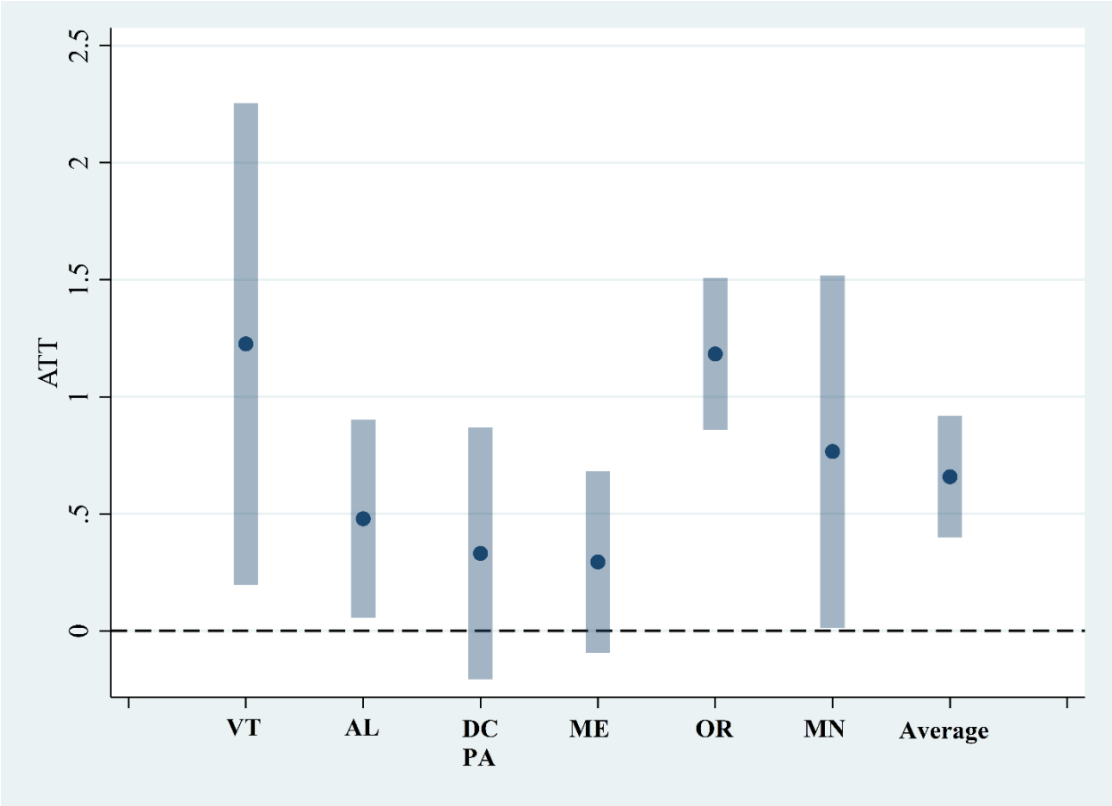


Figure 7. Sant’Anna-Calloway Treatment Effect Sizes by Time Period - AHCM

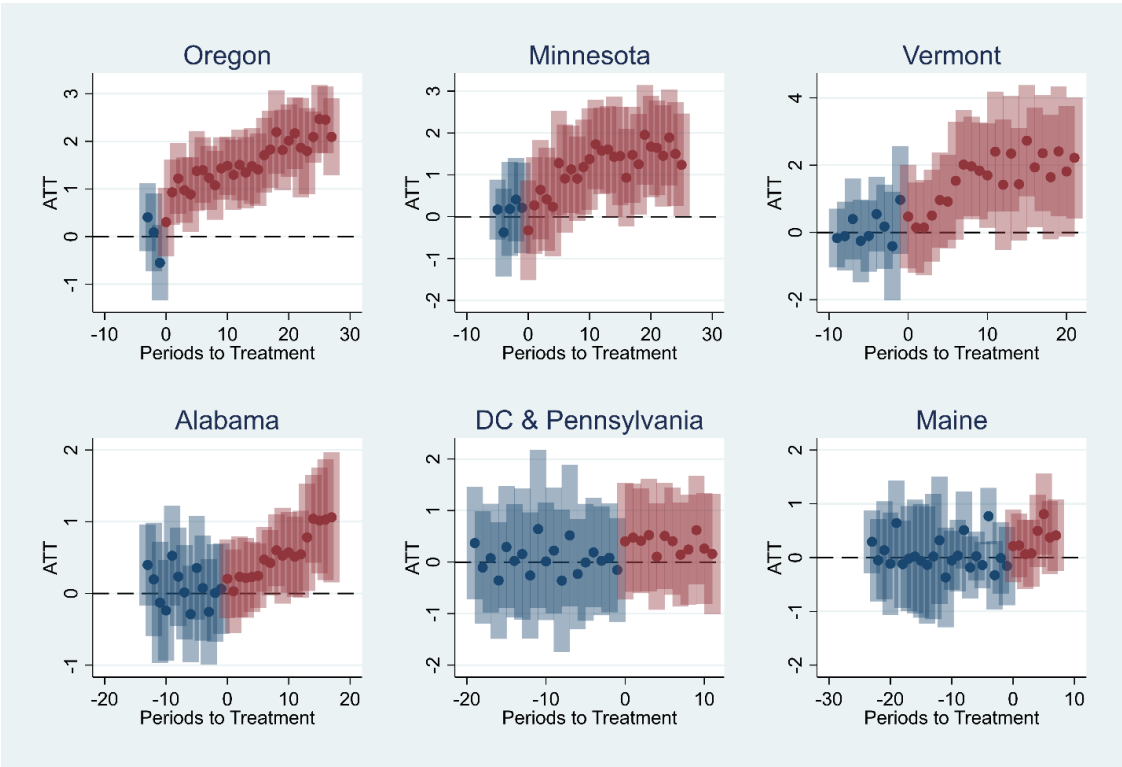


Figure 8. Trends in Average Duration of Claims

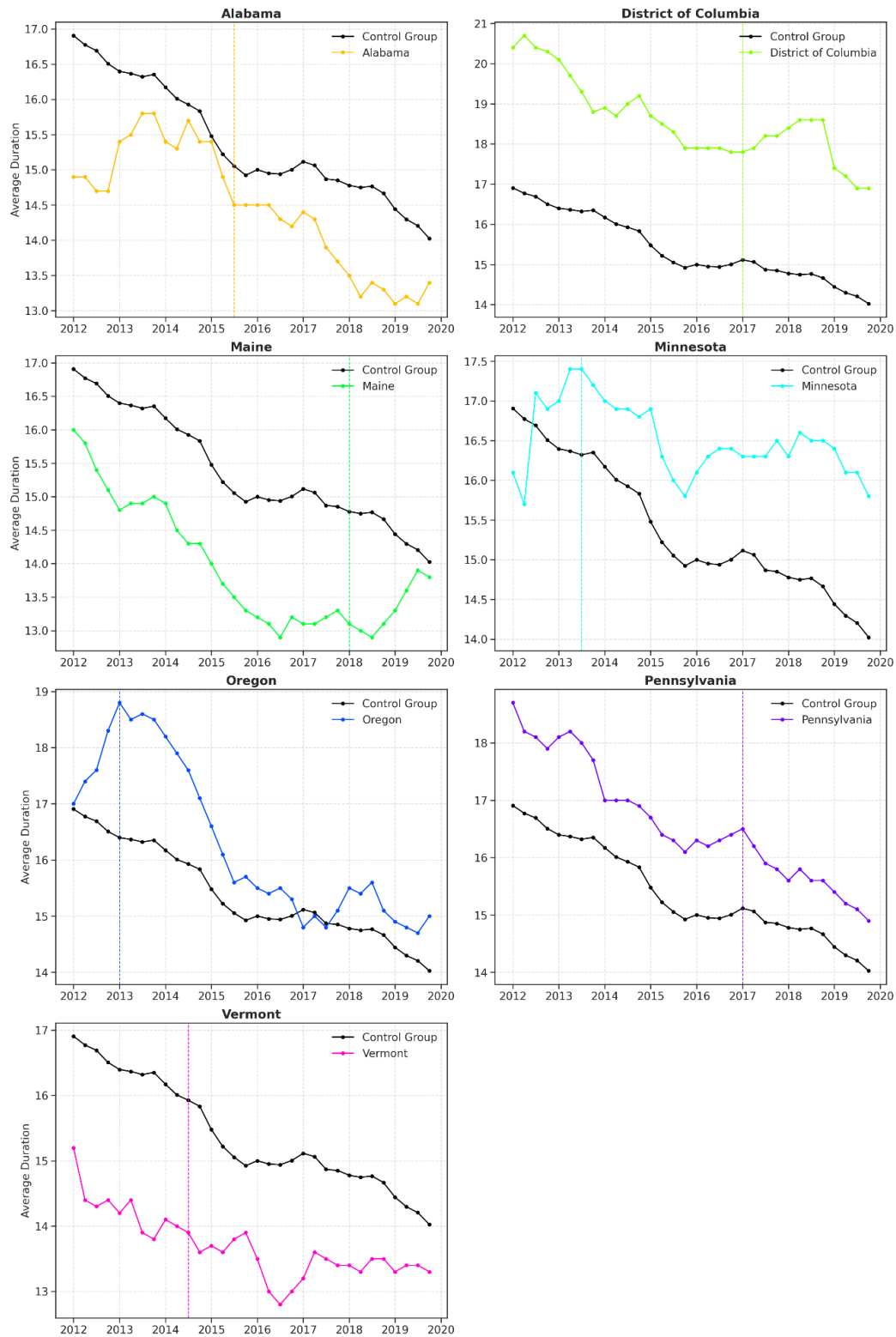


Figure 9. Trends in Claimants Paid

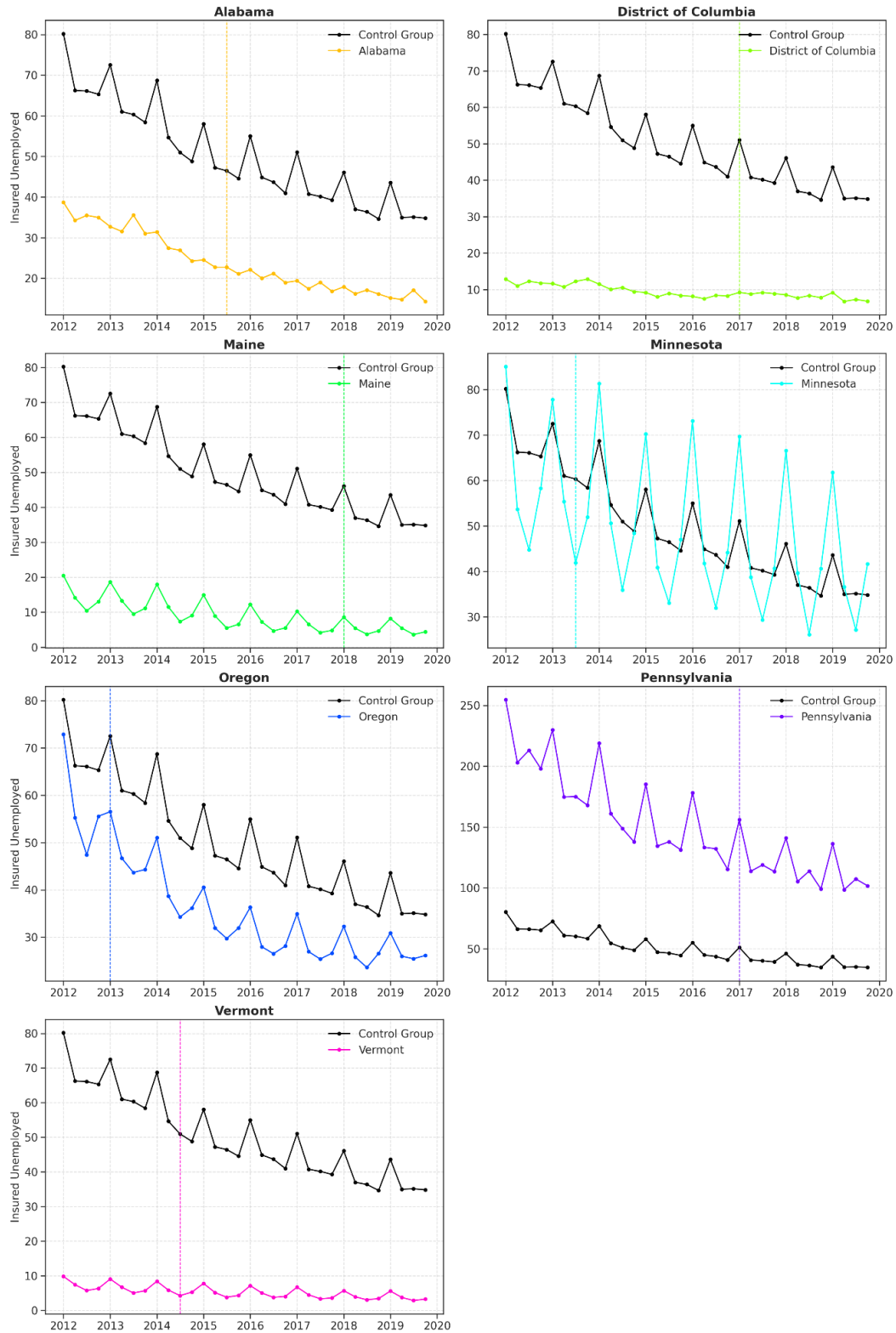


Figure 10. Goodman-Bacon Decomposition

