Gig Workers and Mandated Benefits: Evidence from Washington Ride-share Regulation

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Abstract

Are legal rules requiring minimum benefits for gig workers effective in improving gig worker welfare? If independent contractor wages increase, how does their labor supply respond? To study these questions, I explore the effects of a recently enacted state-wide legislation in Washington mandating minimum transportation network company driver pay rates and paid sick leave on counties that did not previously have similar legislation. Relative to my constructed synthetic controls, I find that the law increased average nonemployer establishment earnings by about $1,191 and decreased the number of establishments by about 8 per 100,000 residents during the first year it was effective. These differences are associated with p-values of 0.08 and 0.21, respectively, under placebo testing. Additional descriptive analysis using household survey data suggests that the new legislation might have allowed those driving as a primary occupation to earn higher effective hourly wages, leading to similar yearly earnings with fewer hours of work.

Keywords: Ride-share Drivers, Gig Work Regulation, Synthetic Control

JEL Codes: J22, J31, J32, J38

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1 Introduction

Since their introduction, "Transportation Network Companies" (TNC) that operate by connecting drivers to passengers via a digitial platform (e.g. Uber, Lyft) have faced fewer regulations than traditional taxi companies which offer similar transportation services (Moorman, 2016). More recently, there has been a movement to address another aspect of TNC companies that exempts them from certain regulations: The status of their drivers as independent contractors. Many have advocated for these drivers to be instead classified as paid employees, which would subject them to additional benefits and protections, such as the minimum wage (Spring, 2024). This idea gained national attention in 2019 when California instituted its "ABC" standard for identifying independent contractors through Assembly Bill 5. Among other things, this would lead California to consider TNC drivers as full-time employees (Sumagaysay, 2024). The ride-share companies did not comply with the law, and in November 2020, California voters passed Proposition 22 (Bay City News, 2025). This law granted TNC companies an exemption from the ABC standard for their drivers, allowing them to still be treated as independent contractors by the state (Sumagaysay, 2024). In exchange for foregoing the rights afforded traditional employees, it guaranteed drivers certain benefits. These included minimum pay rates tied to the minimum wage and miles driven, a health insurance stipend for drivers with more than 15 hours a week in drive time, occupational accident insurance, and anti-discrimination protections (Ballotpedia, 2025).[1]

Legal efforts to mandate similar driver benefits as those contained in Proposition 22 soon followed in other states. In both Massachusetts and New York, TNC companies were sued by the state attorney general. Both of the resulting settlements included minimum pay standards and paid sick leave for TNC

[1] Proposition 22 itself faced challenges in court that were only fully resolved by the California Supreme Court in 2024 (Spring, 2024).

drivers. (Office of the Attorney General, 2024; Office of the New York State Attorney General, 2025).

On the other hand, the state of Washington took a different approach. Instead of voter initiatives or lawsuits, the state legislature passed two major laws granting a wide array worker protections and benefits. The first of these was House Bill 2076, which established minimum pay standards, paid sick leave, and industrial insurance coverage for TNC drivers, effective at the start of 2023 (Washington State Legislature, 2022b). The second law was House Bill 1570, which granted TNC drivers unemployment insurance benefits and introduced a new pilot program within the Paid Family Medical Leave program focusing specifically on TNC companies and their drivers. This program was to be introduced by July 2024, whereas the unemployment insurance benefits became effective immediately upon signing in July 2023. At the same time, the unemployment insurance part of the law also allowed TNC companies to be exempt from providing these UI benefits to drivers as long as they continued to provide them with similar opportunities for work as in the year they were separated from the job or if they became eligible to receive unemployment insurance benefits from another employer (Washington State Legislature, 2023).

1.1 Research Question

In light of the preceding discussion, this paper seeks to understand the following research question:

How do policies mandating minimum benefits impact the earnings and labor supply of independent contractors in the ride-share industry?

The ride-share market is an especially unique one to study in regards to mandated pay minimums because the ease of entry means that TNC drivers vary widely in how much they work and how they think of the job as a source of income. For example, in their December 2019 study of Seattle, Parrott and Reich (2020) found that 43.6% percent of surveyed drivers worked less than 20 hours a week. If these workers are only working as a way to be productive in their spare time, they might not be very reactive to the changes. On the other hand, while the proportion of workers driving full-time (32 hours or more) is smaller than in other markets, they also handle a much larger share of total trips. For example, in the aforementioned study, full-time drivers only accounted for 32.1% of all drivers, but served 54.9% of all trips (Parrott & Reich, 2020).[2] The results of the recent legislation could therefore still be very much affected if this group ends up being more sensitive to the changes, and it is not initially clear whether they would have opted to work more or less hours in response to the increased minimum pay rates (i.e. whether the income or substitution effect dominates), or how much of the paid sick leave they ended up utilizing.

Additionally, driver earnings are also determined by the customer demand in the market, and it is unclear whether the customers' demand will be sufficiently inelastic to support the earnings increases if the companies respond by passing on the cost increases to the customers. This is especially true if the increased minimums increase the number of hours drivers work or the number of drivers on the road. In fact, even before the passage of the bill, certain parties already argued there were too many drivers on the road for drivers to be able to earn a living with the number of available customers (see footnote 17 of Seattle Office of Labor Standards (2024)).

To address my research question, I use administrative tax data from the Census Bureau to determine how the earnings and number of establishments in Washington changed in the taxi and limousine industry (much of which is composed of TNC drivers) in 2023, the first year the mandated benefits of

[2] By comparison, drivers driving 20 hours or less only accounted for 18.9% of all trips (Parrott & Reich, 2020).

HB 2076 became effective.[3] I compare treated Washington counties with a synthetic control constructed from other states. The overwhelming majority of the establishments in the data are sole properietorships, the classification used for self-employed TNC drivers and taxi drivers.[4]

Using my chosen synthetic control method, I found that the law increased average receipts in the industry by about $1,191 per establishment in the year the law became effective among the Washington counties included in the synthetic control. I also found that it decreased the number of establishments by about 8 establishments per 100,000 state residents. While neither of these point estimates remain statistically significant at the common 5% level under placebo testing, the finding on average earnings comes fairly close with a p-value of 0.08.

I also supplement my synthetic control analysis with an analysis of household survey data. While this analysis is only suggestive, I still find that among Washington respondents who list taxi and limousine services as their primary industry, effective hourly self-employment pre-tax income increased by roughly 61% between the 2022 and 2023 surveys. While total earnings decreased slightly for this group, it was also accompanied with a decrease in hours. Taken together, one explanation consistent with the results would be the dominance of an income effect, with drivers able to trade off additional earnings for leisure due to increases in hourly rates. This is similar to the existing contentious academic literature on the existence of (short-term) "earning targets" in the taxi industry, which is the idea that a driver will stop working once having earned enough money for a certain period of time (generally, a day). The idea is discussed

[3] Unfortunately, I am not able to separate any labor market impacts of the unemployment insurance benefits that became effective midway through the year. However, given that TNC drivers can generally choose their hours, I expect TNC companies to generally be exempt from providing these benefits because the drivers should have ample opportunities to continue working. Additionally, to qualify for UI in Washington, a claimant must have worked for the company for at least 680 hours in the applicable year (roughly 13 hours a week) and either have quit for cause or be fired without cause (Washington State Legislature, 2023). Therefore, I do not expect high take-up of the new UI benefits among TNC drivers.

[4] See section 4.1 for a more detailed explanation.

in more detail in section 3. While the decreased hours and higher earnings among those working more hours could also reflect utilization of the paid sick leave benefits, the year-over-year decrease in average hours worked per week is greater than the number of paid sick leave hours the law allows.

The remainder of this paper is organized as follows. Section 2 provides additional detail on HB 2076. Section 3 gives an overview of the related literature. Section 4 discusses the data. Section 5 explains the empirical method used. Section 6 gives the main results. Section 7 provides additional sensitivity analysis using different sub-samples of the data to show the robustness of the main results to selection of the treated and control groups. Section 8 conducts additional descriptive analysis using survey data. Section 9 discusses potential mechanisms and avenues for future research.

2 HB 2076 Summary

HB 2076 established several new benefits for TNC drivers in Washington. The legislation specifically distinguishes minimum pay standards separately for cities with populations above and below 600,000. At the time of its passage, only Seattle was large enough to be included in the larger city category, (Washington State Legislature, 2022b). For cities over 600,000, the law mandated drivers to earn the larger of "59 cents per minute and $1.38 per passenger platform mile; or...a minimum of $5.17 per dispatched trip" (Washington State Legislature, 2022b). Notably, the 59 cents per minute and $1.38 per mile numbers were exactly the same as those already effective for Seattle due to its existing city policies for 2022 (Seattle Office of Labor Standards, 2024).[5] For cities under 600,000, the law mandated drivers to earn the larger of "34 cents per minute and

[5] Seattle had minimum pay rates beginning in 2021, and HB 2076 in large part can be seen as expanding much of what Seattle already established to the rest of the state. Seattle's local policy was replaced by the rules set forth in HB 2076 (Seattle Office of Labor Standards, 2024).

$1.17 per passenger platform mile; or...a minimum of $3 per dispatched trip" (Washington State Legislature, 2022b). These rates were designated to be used proportional to the time or distance driven in Seattle for trips that crossed the city's boundaries (Washington State Legislature, 2022a). Further, this minimum compensation was not allowed to include any tips the driver might receive from passengers, and the law requires the amounts to be adjusted upwards annually on January 1st of each year by the rate of increase in the minimum wage. This calculation is made each year on September 30th (Washington State Legislature, 2022a).[6]

In addition to pay minimums, the bill also granted ride-share drivers paid sick leave of 1 hour for every 40 hours worked, the same standard used for non-contractor employees in the state. The legislation allowed them to earn this after the first 90 hours worked on the platform, carry over 40 unused hours at the end of the year, and use at least four hours of paid sick leave at a time. Finally, the law required the state's Department of Labor and Industries to use the rates charged to taxicab companies when calculating premiums for ride-share sole proprietors choosing to purchase industrial insurance (workers' compensation coverage) (Washington State Legislature, 2022b).

3 Literature Review

As far as I am aware, my study is the first to specifically study the effects of this kind of TNC driver policy using a causal framework. While Seattle's Office of Labor Standards did issue a report analyzing the effects of its similar 2021-2022 policy, it only reported the before and after changes, rather than comparing those changes to any counterfactual. Further, interpretation of its effects were

[6] This includes September 2022. That is, the law was passed in early March of 2022, and the starting amounts specified by the law were required to be adjusted upwards by the increase in 2023's minimum wage before going into effect for the first time on January 1st (Washington State Legislature, 2022a).

complicated by the recency of the COVID-19 pandemic. Overall, however, it found that gross hourly pay was \$42 and \$26 in the final quarters of 2021 and 2022, respectively (Seattle Office of Labor Standards, 2024). By comparison, the initial study which proposed a minimum pay standard to Seattle had found gross hourly pay to be \$21.53 during among drivers surveyed in early December 2019 (prior to the policy), translating to \$9.73 after expenses. Its authors had noted that this was less than the local minimum wage of \$16.39 at the time (Parrott & Reich, 2020).

While this kind of policy is still relatively new among TNC drivers, there is a long literature dedicated to estimating the labor supply elasticities of taxi and TNC drivers in response to changes in hourly pay. The influential Camerer, Babcock, Loewenstein, and Thaler (1997) analyzed samples from New York City cabdrivers, finding mostly negative elasticities. Its authors suggested these findings could be explained by a form of reference-dependence (Kahneman & Tversky, 1979). That is, they presented the idea that taxi drivers (especially inexperienced ones) stop work after hitting a daily "income target" (Camerer et al., 1997). Agarwal, Diao, Pan, and Sing (2015) also found evidence consistent with income targeting among taxi drivers in Singapore using administrative data on 10,000 cabdrivers. By contrast, Farber (2015) studied the same question using all trips in New York City from 2009 to 2013, finding heterogeneity but generally positive elasticities. Farber (2015) noted that he used a much larger sample the ones used in previous work up to that point. He also found that individuals with lower elasticities are more likely to quit, while individuals who stay increase their elasticities with experience, a finding similar to Camerer et al. (1997)'s explanation of inexperienced drivers being more prone to having to lower elasticities. Most recently, Nian, Pan, Huang, and (Jian) Sun (2024) studied labor elasticities for taxi drivers in China before and during the COVID-

19 pandemic. They found that the elasticity decreased from 0.35 prior to the pandemic to -0.79 during the pandemic.

Moving from taxi drivers to TNC drivers, Sheldon (2016) analyzed independent contractor data on hours and earnings from Uber, finding significantly positive elasticities and elasticities that increase with experience. Similarly, Sun, Wang, and Wan (2019) used data from Didi Chuxing, finding positive elasticities for ride-share drivers for both the number of hours worked per day and the decision to drive at all during a given day. They also found that while the elasticities remain positive, both of these elasticities decrease among drivers who drive more hours per day.

Notably, all of the papers discussed up to this point only focus on labor responses to short-term (daily or hourly) increases in the wage rate. Ashenfelter, Doran, and Schaller (2010) instead studied the effects two permanent fare increases by the body which regulates taxi fares in New York City. They found that these were associated with a negative supply elasticity of -0.2, suggesting the dominance of an income effect. Doran (2014) compared the effects of both short- and long-term shocks, with the long-term shock used being the latter of the two used in Ashenfelter et al. (2010). He estimated that the long-term shock yielded a statistically insignificant positive supply elasticity of 0.05, while the short-term shocks yielded a statistically significant negative supply elasticity of -0.25. While at first glance the two findings about long-term elasticities may appear to have different signs for partially the same data, it is worth noting that the elasticities have different estimation strategies and are over different outcomes: Ashenfelter et al. (2010) looked at miles driven, whereas Doran (2014) looked at hours. Further, only Ashenfelter et al. (2010)'s long-term elasticity estimate was statistically significant at the 5% level.

HB 2076 introduced long-term, permanent changes in minimum pay rates, not short term ones. My data are also only available at the (aggregated) annual level, not hourly. Therefore, this paper is closest in spirit to Ashenfelter et al. (2010), although it makes the contribution of studying a policy directed at TNC drivers rather than taxi drivers, with TNC drivers generally being able to enter the market more easily due to the more limited nature of taxi medallions relative to county permits issued to TNC drivers (see, for example, King County (2024)).

Lastly, this paper is also related to the vast empirical literature studying the labor market responses of worker benefit regulation in traditional labor markets. Of particular similarity to the minimum pay rates is the minimum wage, with the rates used in Seattle specifically designed to imitate the city's minimum wages for large employers (Seattle Office of Labor Standards, 2024). For a recent review of studies on the minimum wage, see Dube and Lindner (2024).

4 Data

4.1 Data Source Description

My primary data are taken from the Census Bureau's public state- and county-level Nonemployer Statistics (NES) datasets. These datasets provide annual totals of the receipts (business income) received by all business establishments with no employees that earn at least $1,000 per year and are subject to federal income tax, using their income tax returns (US Census Bureau, 2025a).[7] They also report the number of these establishments. As noted by the Census Bureau, these represent the majority of all American businesses, and are mostly comprised of self-employed individuals (US Census Bureau, 2025a).

[7]The single exception to the $1,000 minimum for inclusion is businesses in the construction sector, which need only to earn a minimum of $1 per year to be included in the data (US Census Bureau, 2025a).

Notably, the data allow for identification of four-digit NAICS industry at the national, state, and county levels. Additionally, they allow for identification of the legal form of organization at the national and state (but not county) levels. My main analysis in section 6 looks at the effect of Washington's policy by examining the changes in the earnings and number of establishments in NAICS 4853, the taxi and limousine services industry.[8] This is the classification used by taxi drivers and TNC drivers (Abraham, Haltiwanger, Hou, Sandusky, & Spletzer, 2024). While I cannot differentiate between ride-share drivers and taxi drivers or chauffeurs (who are not directly impacted by the policy), I expect non-TNC drivers to make up a small portion of the data. For example, according to a King County report, the county's Department of Records and Licensing Services Division approved 21,011 drivers for a TNC for-hire driver permit in 2023. By contrast, in the same year, the county issued only 885 for-hire driver licenses (used for both taxis and flat-rate for-hire vehicles) and licensed only 495 taxicabs (King County, 2024, 2025). That is, TNC drivers made up 97% of all approved drivers in Washington's most populous county.[9]

While it would be preferable to examine the effects directly on the sole proprietorships most directly impacted by the law, it is not possible to identify outcomes separately in the county-level data I use in my main analysis.[10] Instead, the data used include all types of nonemployer businesses, namely sole proprietorships, partnerships, and C- and S-corporations. However, in practice, I do not expect these other types of businesses to have much effect on the analysis

[8] The technical NES documentation is careful to note that, starting in 2016, many records previously identified as NAICS 4851 were corrected to NAICS 4853 (US Census Bureau, 2025b). I discuss this concern as part of section 7.1. Overall, while I find that the earnings point estimate is somewhat smaller when excluding years 2010-2015 in the construction of my synthetic control, it does not the direction of either estimate.

[9] It should be noted that this is for illustrative purposes only, as King County is specifically excluded from my main analysis. However, I expect King County to have a large presence of taxis relative to other areas given the relatively high density of Seattle and its surrounding cities, meaning that TNC drivers are likely even more dominant in the counties used in my analysis.

[10] For why I choose to rely on county-level data, see section 4.2.

results because they make up so little of the industry. For example, according to the NES state-level data, the state of Washington in 2023 had a total of 22,800 establishments in the industry, earning $942,483 for the year, across all types. 22,739 of these identified as sole proprietors, collectively earning $936,153 for the year. That is, sole proprietorships accounted for approximately 99.7% of the industry's establishments and 99.3% of the industry's earnings in Washington during the treatment period.

Additionally, I use two other supplementary data sources. First, I use annual county and state population totals from the Census Bureau so that I can study the change in the number of establishments per 100,000 residents, rather than just in absolute terms. This helps address the concern that the geographic units used in the treated group and donor pool may differ in trends in the number of establishments simply due to differences in population size and density. Next, I use data on hours worked and self-emploment income by American Community Survey respondents working in the NAICS 4853 industry to examine possible effects on full-time drivers.[11]

4.2 Treatment Unit and Donor Pool Geographic Inclusion

Although I have state-level data for Washington as a whole and county-level data for King County for all treated years, I do not use either in my main analysis to avoid including King County in the treatment group. King County includes Seattle, which was mandated to have uniquely high minimum pay rates on HB 2076 that reflected existing local pay minimums already in effect prior to the bill's effective date (Seattle Office of Labor Standards, 2024; Washington State Legislature, 2022b). Therefore, I use state-level data for units in the donor pool, but not for the treated group. Instead, I aggregate NES county-level data for all available counties in Washington besides King County. Unlike with state-level

[11]Specifically, I use the version of the data available at IPUMS USA (Ruggles et al., 2025).

data, this also necessitates excluding certain counties that withhold their data from the public dataset in some years, often due to privacy concerns because of their smaller populations.

The final treated group includes 13 of Washington's counties, namely Benton, Clark, Cowlitz, Kitsap, Kittitas, Pierce, San Juan, Skagit, Snohomish, Spokane, Thurston, Whatcom, and Yakima counties. These counties jointly captured 4,434,859 of Washington's 7,812,880 residents in 2023. Notably, King County in 2023 had a population of 2,271,380 residents, so the treatment group counties cover roughly 80% of all state residents outside of King County.

Additionally, the donor pool includes the District of Columbia and almost every other state besides Washington. The only excluded states are California, Montana, and New York. California was excluded for receiving treatment (via Proposition 22). Montana was excluded because it was missing data for 2010 due to privacy restrictions. New York was excluded because New York City received treatment through a 2018 law requiring minimum pay rates for Uber and Lyft drivers (O'Brien, 2018).

4.3 Summary Statistics

Summary statistics for the outcome variables of interest are given in table 1 below. The columns list the variable averages separately for Washington and donor pool states, as well as separately for the pre- and post-treatment periods.

Variable	Pre-period		Post-period	
	WA Counties	Donor Pool	WA Counties	Donor Pool
Panel A: Averaged Totals				
Annual Receipts	112,015,538	209,805,290	284,540,992	540,531,867
	(93,814,764)	(384,570,613)		(774,256,455)
Number of Establishments	5,245	10,152	9,060	20,761
	(4,249)	(18,392)		(31,083)
Population	4,109,235	5,436,784	4,434,859	5,690,059
	(217,889)	(5,245,455)		(5,704,909)
Panel B: Population-invariant Averages				
Annual Receipts per Establishment	24,062	21,181	31,406	24,110
	(6,680)	(7,484)		(5,430)
Establishments per 100k Residents	123	153	204	290
	(98)	(166)		(202)

Table 1: Summary Statistics. Standard deviations in parentheses. The donor pool includes all states (and DC) besides California, Montana, New York, and Washington. The pre-period is 2010-2022, while the post-period is 2023.

In addition to these period averages, figures 1 and 2 below compare how the two main outcome variables compare in the treated group and donor pool over time.

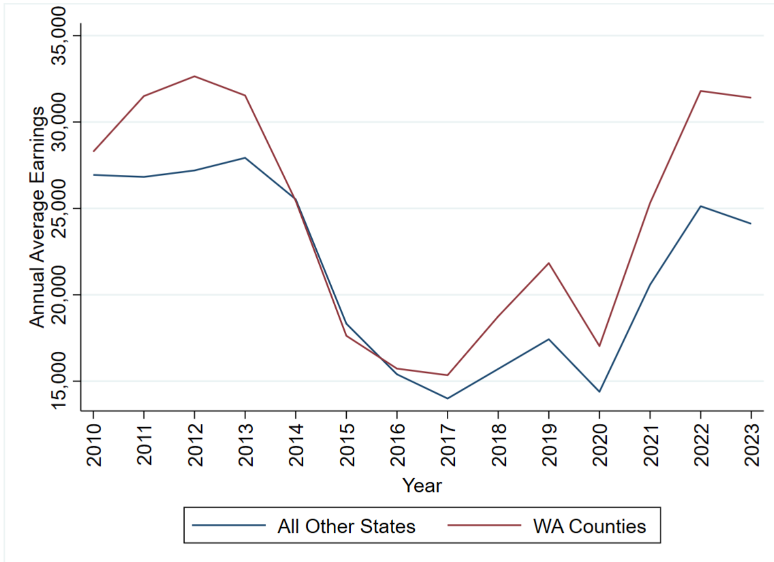

Figure 1: Average Yearly Earnings over Time

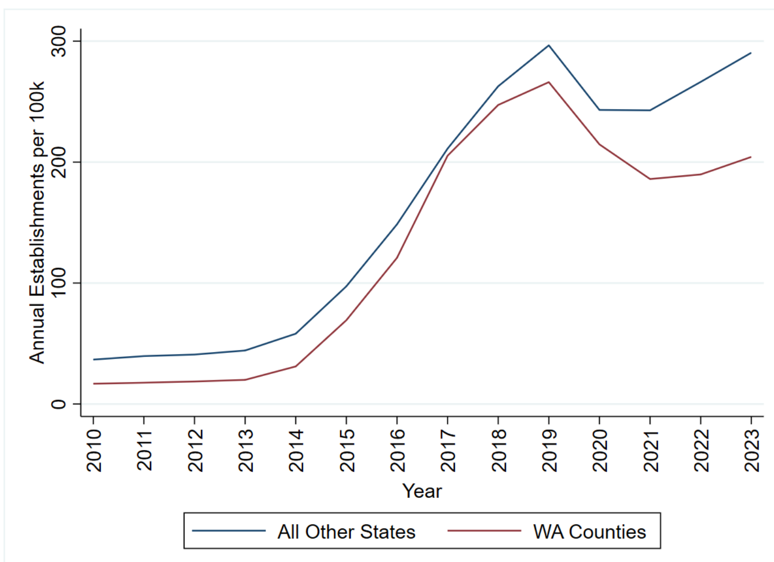

Figure 2: Establishments per 100k Residents over Time

As shown, average establishment earnings in the aggregated Washington counties are generally higher with fewer establishments per capita than in the donor pool states. In fact, there were only five states with higher earnings per establishment than the treated group when looking at either 2022 or 2023.[12] Therefore, one reason I avoid using the synthetic control method popularized by Abadie, Diamond, and Hainmueller (2010) is that it would require putting positive weight on states from this smaller group in order to produce a good pre-period fit in the years immediately before the policy was enacted, even if doing so could lead to a worse fit in other periods. Increased reliance on a small group of states could also lead the results to be more driven more by specific factors affecting the chosen states (see section 7.2).

5 Empirical Strategy

5.1 Synthetic Control Method Description

I make use of a synthetic control design to assess the policy impact of HB 2076 on the taxi and ride-share industry in Washington.[13] The synthetic control method was popularized in economics by Abadie et al. (2010). The method seeks to estimate a counterfactual for a treated unit (in this case, the Washington counties) using a weighted average of the same outcome in untreated "donor pool" states. The basic argument is that as long as the outcome in the synthetic control matches the pre-treatment outcomes well in the treated unit, then it can be compared to the treated unit as a counterfactual outcome post-treatment.

There have been several variations on the method proposed in Abadie et al. (2010), with the main question being how to best choose the donor pool weights used in forming the synthetic control. The version of synthetic control I use in this paper, referred to as *synthetic control with LASSO* (SCUL) was introduced

[12] For 2022, these states are Colorado, Illinois, Massachusetts, Minnesota, and Nevada. For 2023, these states are Colorado, Hawaii, Maryland, Massachusetts, and Nevada.

[13] All implementations of the method in my paper use a lightly edited version of the *scul* command, written for Stata by Jared Greathouse (Greathouse, 2022).

by Hollingsworth and Wing (2020). I briefly describe both methods and their differences below.

First, Abadie et al. (2010) chooses weights to minimize the quantity

$$\hat{\mathbf{w}}_{synth} = argmin_{\mathbf{w}}(\sqrt{(\mathbf{x}_1 - \mathbf{X}_0\mathbf{w})'\mathbf{V}(\mathbf{x}_1 - \mathbf{X}_0\mathbf{w})})$$

where $\mathbf{x}_1$ is a vector of predictors for the treated unit, $\mathbf{X}_0$ is a matrix of the same predictors for the donor pool units, and $\mathbf{w}$ is a vector of donor pool unit weights. $\mathbf{V}$ is a positive semi-definite, diagonal matrix which assigns additional weights to each predictor. Further, in accordance with the guidelines outlined in Abadie et al. (2010), existing implementations of this method in statistical software packages constrain the weights in $\mathbf{w}$ to be positive and sum to one to ensure interpolation over extrapolation. To use the Abadie et al. (2010) version of synthetic control, the researcher must make important decisions on which predictors to include in the $\mathbf{x}_1$ and $\mathbf{X}_0$ matrices, and which matrix $\mathbf{V}$ to use. Typically, however, lagged outcomes of the main outcome variable are the most heavily-weighted predictor (Hollingsworth & Wing, 2020). Abadie et al. (2010) also suggested setting the $\mathbf{V}$ matrix such that it minimizes the mean square prediction error between the main outcome variable and synthetic control during the pre-treatment periods.

On the other hand, the SCUL method introduced by Hollingsworth and Wing (2020) seeks to minimize

$$\hat{\mathbf{w}}_{lasso} = argmin_{\mathbf{w}}(\sum_{t=1}^{T_0}(y_{0t} - \mathbf{x}_t\mathbf{w})^2 + \lambda|\mathbf{w}|_1)$$

where y_t is the treated unit's main outcome in period t and $\mathbf{x}_t$ is a vector of main outcomes for the treated states. λ is a lasso penalty parameter which penalizes the objective function as more donor pool units receive non-zero weights in the vector of weights **w**. In theory, this methodology could be expanded to include more predictors than only lags of the outcome variable. However, in practice, I did not find that the implementation of the option to include additional predictors to work well in the method's only existing Stata implementation. Additionally, the author of the Stata implementation has shown that the method is already able to produce a well-fitting synthetic control and similar treatment effects without additional predictors using the datasets of other synthetic control papers, and has argued that covariates should rarely be needed when using it (Greathouse, 2022).

Besides the differences in the method for choosing predictors, one major difference between the methods is that the SCUL method relaxes the restrictions on the weights so that they do not need to be positive or sum to one. Hollingsworth and Wing (2020) argues that the method's identifying assumptions do not inherently require these properties and that extreme interpolation can be just as problematic as extreme extrapolation. Additionally, they give the example of applications using states of different population sizes, where raw counts of the outcome variable can lie outside the convex hull of other states' outcomes if the state is very large or small. They also note that even rescaling an outcome by population (as I do for the number of establishments) to enable use the Abadie et al. (2010) is a form of extrapolation relative to the original units of the outcome variable. As previously mentioned in section 4.3, while my treatment group is within the convex hull, it is still near the edge, with only five states with higher earnings per establishment in the year before treatment.[14] The SCUL method also includes the advantage of incorporating a cross-validation procedure to prevent overfitting and reduce threats to identification that otherwise come when the number of donor pool units outnumbers the number of observations,

[14] If using state-level data, which includes Seattle, Washington would be fully outside the convex hull, since it had higher earnings than every other state except New York in most years (and New York was excluded from the donor pool).

as would apply to my data (Hollingsworth & Wing, 2020).

5.2 Identifying Assumptions

As outlined in Hollingsworth and Wing (2020), synthetic control relies on an interactive fixed effects model and two main assumptions.

The interactive fixed effects model used is

$$y(0)_{st} = \delta_t \alpha_s + \varepsilon_{st}$$

where $y(0)_{st}$ is the untreated potential outcome of unit s at time t, δ_t is a vector of time-varying unit-specific variables, α_s is a vector of time-invariant unit-specific variables, and ε_{st} is the idiosyncratic error term. The idea behind the model is that units with similar $y(0)_{st}$ over time must have similar time-invariant unit-specific factors α_s.

The first identifying assumption is the conditional independence assumption. It states that, after conditioning on the α_s, the treatment is independent of $y(0)_{st}$. Then since the α_s should be similar according to the model if time trends are similar, the synthetic control is a plausible counterfactual for the treated unit if it is a good pre-treatment match in the outcome variable.

The second identifying assumption outlined by (Hollingsworth & Wing, 2020) is the "no dormant factors assumption." The idea behind this assumption is that there are not additional factors contained in δ_t that only become active during the treatment period. That is, if there are time-varying factors affecting the outcome variable, they must be influential during the pre-treatment period. If they begin to vary only following treatment, then there is no way to incorporate their effects into the matching. Hence, there would be no basis to argue that the constructed synthetic control's outcomes are similar to the treated unit's untreated potential outcomes post-treatment simply because it was prior to the newly-influential factors activating. Since I already exclude California for having a unique situation post-treatment (and for having already been treated prior to Washington), I do not expect this assumption to be substantially violated in my setting.

5.3 Inference

Inference in synthetic control studies is typically performed using in-place placebos. That is, the synthetic control specification is reestimated over each untreated unit in the donor pool. If the treated unit's estimated effect is substantially larger than all the others, it is less likely that the observed effect is due to chance. However, as noted by Abadie et al. (2010), a large effect difference between the treated unit and the synthetic control following treatment is likely to be less meaningful if the pre-treatment fit is poor. To account for this, Abadie et al. (2010) conducts inference by dividing the post-treatment mean square error by the pre-treatment mean square error for each placebo and then ranking these ratios from greatest to least.[15] A p-value is then obtained by dividing the rank of the treated unit's ratio by the total number of placebos (including the treated unit). This gives a measure of how unusual the effect size is relative to the placebos while accounting for differences in pre-treatment fit.

6 Results

6.1 Synthetic Control Composition

The weights for each synthetic control are given in tables 2 and 3 below.

[15] The program I use instead uses the root mean square errors, but either method produces the same ranking.

Donor Pool State	Assigned Weight
Arizona	0.146
Colorado	0.011
District of Columbia	0.045
Georgia	0.054
Idaho	0.051
Indiana	0.401
Iowa	0.003
Maryland	0.316
New Mexico	0.070
North Dakota	-0.187
Oklahoma	0.009
Pennsylvania	0.022
Rhode Island	0.289
Wyoming	0.030

Table 2: Synthetic Control Composition, Average Earnings.

Donor Pool State	Assigned Weight
Arizona	0.133
Delaware	0.015
Hawaii	-0.066
Idaho	0.362
Mississippi	-0.001
North Dakota	0.504
Oregon	0.624
Rhode Island	-0.084
South Dakota	-0.351
Tennessee	-0.052
Utah	0.093
Vermont	-0.012
Wyoming	0.353

Table 3: Synthetic Control Composition, Establishments per 100k.

Both controls gave fairly large coefficients (in absolute value terms) to Arizona and North Dakota. However, the weights differ for the other states included. In particular, the earnings synthetic control assigned large weights to Indiana, Maryland, and Rhode Island. The establishments synthetic control, on the other hand, assigned large weights to Idaho, Oregon, South Dakota, and Wyoming.

6.2 Estimated Effect

Figure 3 below graphs the trends for the treated unit and synthetic control, while 4 graphs the difference between them. Table 4 then gives this information numerically.

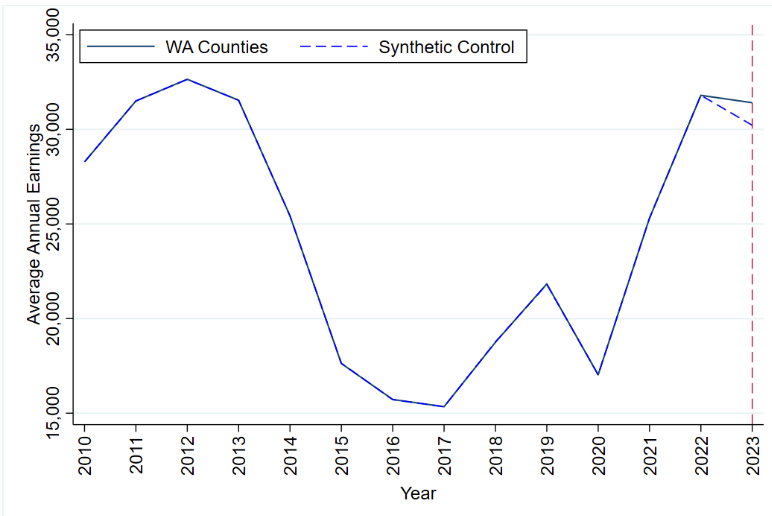

Figure 3: Synthetic Control Trends: Average Annual Earnings

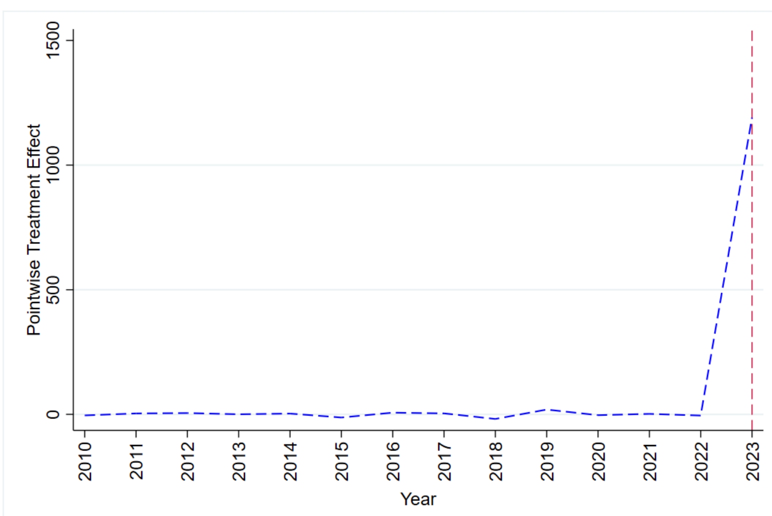

Figure 4: Synthetic Control Gap: Average Annual Earnings

Year	WA Counties	Synthetic Control	Gap
2010	28,280	28,284	-4
2011	31,501	31,497	4
2012	32,646	32,641	5
2013	31,538	31,538	0
2014	25,431	25,428	3
2015	17,626	17,639	-13
2016	15,725	15,718	7
2017	15,346	15,342	4
2018	18,749	18,767	-19
2019	21,831	21,812	19
2020	17,030	17,033	-3
2021	25,311	25,309	2
2022	31,797	31,802	-5
2023	31,406	30,216	1,191

Table 4: Average Annual Earnings Gap. All values rounded to the nearest dollar.

We see that the pre-treatment fit is very small, with a root mean square error of only 8.94. Further, the gap between the treated unit and the synthetic control is roughly centered around 0. Both of these facts suggest that the synthetic control is plausibly a good counterfactual for the aggregated Washington counties. The synthetic control estimates an increase in annual earnings in the treated period of \$1,191 dollars, or about \$99 per month. The treated group had \$31,406 in annual average receipts in 2023, meaning that the method suggests that the law increased Washington driver earnings by roughly 3.7%.

Figures 5 and 6, along with table 5 below present the analogous results for the change in the number of establishments.

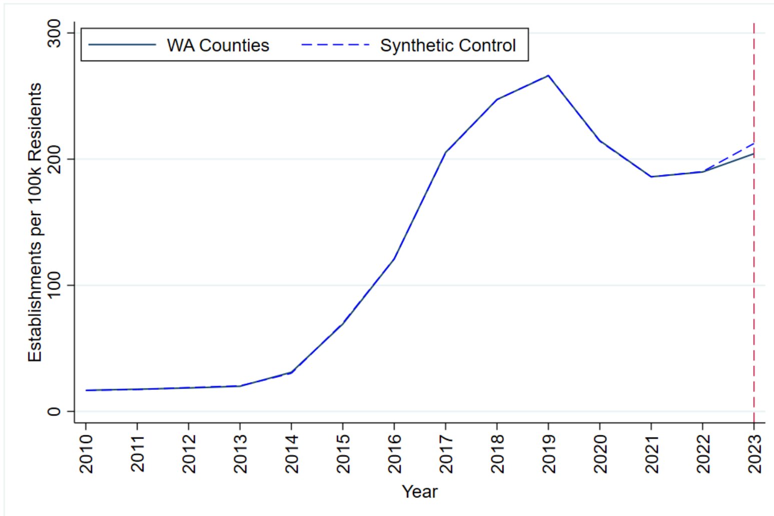

Figure 5: Synthetic Control Trends: Establishments per 100k

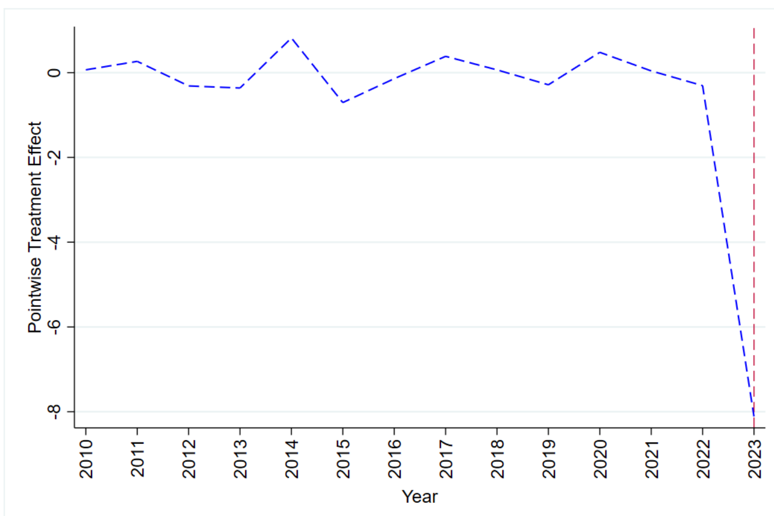

Figure 6: Synthetic Control Gap: Establishments per 100k

Year	WA Counties	Synthetic Control	Gap
2010	16.79	16.72	0.07
2011	17.62	17.36	0.27
2012	18.62	18.94	-0.31
2013	19.96	20.32	-0.36
2014	31.08	30.26	0.82
2015	69.28	69.99	-0.71
2016	120.84	120.99	-0.14
2017	205.30	204.92	0.38
2018	247.23	247.17	0.06
2019	266.11	266.40	-0.29
2020	214.67	214.19	0.48
2021	186.00	185.95	0.04
2022	189.79	190.10	-0.31
2023	204.29	212.40	-8.11

Table 5: Establishments per 100k Gap. All values rounded to two decimal places.

Again, the root mean square error, 0.4, appears small relative to the scale of the estimated effect, -8.11. The number of positive and negative gaps prior to treatment is also as equal as possible, suggesting no systematic difference in pre-trends. The estimated effect of -8.11 per 100,000 of the treated group counties' 4,109,235 residents in 2023 is a loss of about 333 establishments across all included counties. The treated group had 9,060 establishments in 2023, meaning that the method suggests that the law reduced the counterfactual by roughly 3.7%, interestingly the same magnitude in percentage as the effect on average earnings increased.

6.3 Placebo Tests and Inference

Figure 7 below shows the results of the placebo tests for the average earnings outcome, while table 6 presents the numerical ratios used to derive the p-value.

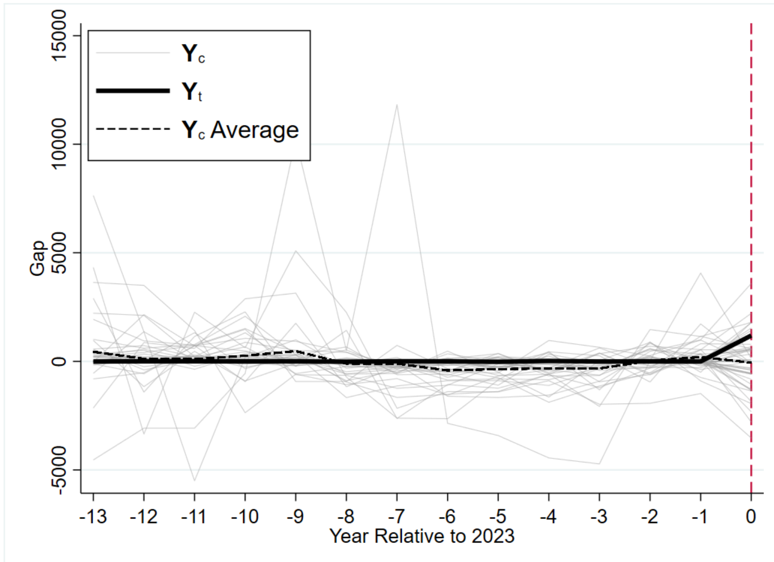

Figure 7: Placebo Testing: Average Annual Earnings

State	Pre-period RMSE	Post-period RMSE	Ratio
New Hampshire	9.63	1,778.48	184.71
South Dakota	8.97	1,421.39	158.43
District of Columbia	10.40	1,539.13	147.95
WA Counties	8.94	1190.59	133.17
Kentucky	9.93	1308.62	131.85
Minnesota	7.49	599.87	80.09
Oklahoma	7.46	336.33	45.06
South Carolina	9.11	397.38	43.63
Alaska	32.39	821.01	25.34
Georgia	14.93	300.15	20.11
Texas	42.54	543.11	12.77
Massachusetts	112.40	1282.68	11.41
Utah	69.56	550.00	7.91
Virginia	69.24	542.47	7.83
Mississippi	368.13	2797.25	7.60
North Carolina	1.88	9.93	5.28
Illinois	461.01	2319.25	5.03
Indiana	296.05	1348.09	4.55
Arizona	162.37	580.65	3.58
Florida	173.18	590.58	3.41
New Jersey	587.32	1943.56	3.31
Nevada	613.58	1848.02	3.01
Kansas	226.38	674.11	2.98
Louisiana	13.93	32.11	2.30
Delaware	788.73	1809.77	2.29
Rhode Island	982.90	2213.19	2.25
Pennsylvania	150.99	330.69	2.19
Wisconsin	489.70	1055.42	2.16
Connecticut	284.33	552.22	1.94
Michigan	293.91	555.49	1.89
Ohio	292.93	517.89	1.77
Maryland	337.18	465.15	1.38
West Virginia	2,567.68	3,528.63	1.37
Colorado	1,348.78	1,797.34	1.33
North Dakota	2,792.94	3,590.62	1.29
Alabama	1,381.10	1,283.14	0.93
Missouri	631.93	577.56	0.91
Oregon	737.83	589.96	0.80
Hawaii	8.90	6.98	0.78
Nebraska	1,737.84	1,360.77	0.78
Idaho	587.02	395.83	0.67
Vermont	1,404.38	786.08	0.56
Iowa	439.75	82.72	0.19
Maine	348.07	63.59	0.18
Tennessee	1,053.23	82.69	0.08
Arkansas	1,820.92	94.52	0.05
Wyoming	5,285.93	135.33	0.03
New Mexico	531.68	4.40	0.01

Table 6: Placebo RMSE Ranking: Average Annual Earnings

The graph shows that the post-treatment gap between the treated unit and synthetic control (bold line) is generally more positive than the average of the analogous gaps between the placebos and their synthetic controls. This produces a p-value of $p = 4/48 \approx .08$ when using the ratio ranking displayed in the table.

Analogous results are shown below in figure 8 and table 7 for establishments per 100,000 residents.

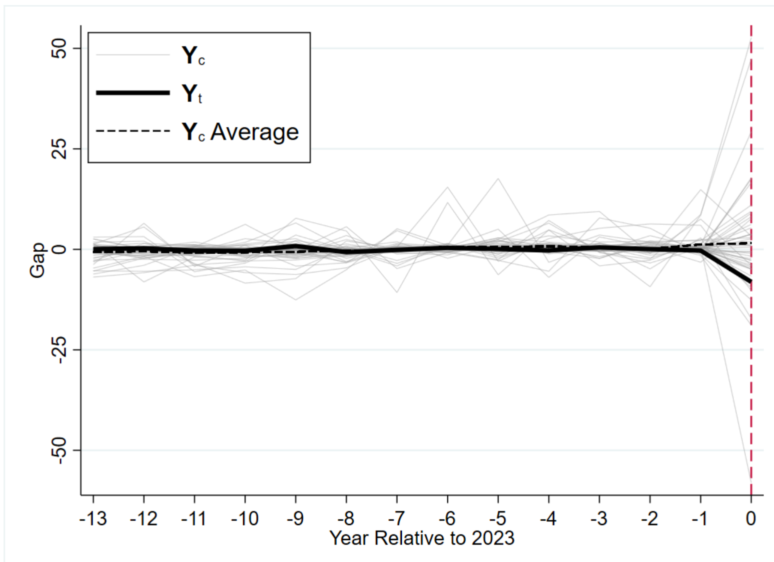

Figure 8: Placebo Testing: Establishments per 100k

State	Pre-period RMSE	Post-period RMSE	Ratio
Utah	0.21	16.59	77.81
Ohio	0.06	3.96	67.63
Wisconsin	0.16	9.01	54.91
Mississippi	0.11	4.79	45.19
Arkansas	0.10	3.82	38.28
District of Columbia	2.44	58.07	23.81
New Mexico	0.31	6.65	21.64
Colorado	0.85	17.53	20.63
Delaware	0.41	8.45	20.46
WA Counties	0.40	8.11	20.42
Georgia	1.24	18.89	15.28
Minnesota	1.18	17.79	15.05
Texas	0.49	6.22	12.78
Pennsylvania	0.34	4.36	12.77
Nebraska	0.66	8.11	12.32
Rhode Island	4.61	47.38	10.28
Kentucky	1.03	9.46	9.20
Oklahoma	0.26	2.34	9.04
Louisiana	1.94	17.21	8.88
Virginia	3.69	29.30	7.94
Massachusetts	6.98	52.65	7.54
Illinois	1.29	9.50	7.37
Hawaii	1.36	8.21	6.05
North Carolina	2.00	11.03	5.52
Maine	0.70	3.73	5.36
New Hampshire	1.19	5.01	4.23
Maryland	0.91	3.73	4.08
Kansas	0.38	1.35	3.58
South Carolina	1.74	5.36	3.08
Oregon	2.48	7.61	3.07
Idaho	0.98	2.70	2.75
Florida	4.61	12.61	2.74
Connecticut	0.67	1.72	2.55
North Dakota	1.11	2.76	2.49
West Virginia	1.17	2.63	2.25
South Dakota	0.31	0.59	1.92
Nevada	2.67	4.91	1.84
Iowa	1.08	1.92	1.78
Missouri	1.75	2.87	1.64
Arizona	6.50	10.28	1.58
Vermont	2.65	3.35	1.27
Indiana	0.82	0.95	1.17
Alabama	1.28	1.04	0.81
Michigan	5.24	3.98	0.76
Tennessee	1.33	0.55	0.42
New Jersey	8.11	3.29	0.41
Wyoming	1.73	0.63	0.37
Alaska	3.32	0.91	0.27

Table 7: Placebo RMSE Ranking: Establishments per 100k

In this case, the treated unit has a more negative estimated effect that the average of the placebos. However, the associated p-value is $p = 10/48 \approx .21$, which is noticeably higher than the p-value for average earnings.

7 Robustness Checks

7.1 2016-2023 Analysis

The NES documentation notes that, starting in 2016, many records previously identified as NAICS 4851 were corrected to NAICS 4853 (US Census Bureau, 2025b). As long as the factors affecting these misclassifications in the early periods did not differ by state, I do not anticipate that this would weaken the synthetic control. Further, the synthetic control appears to fit the treated unit outcomes well both before and after this change. However, here I reestimate the synthetic control using only data from 2016 and later as a robustness check on the inclusion of the pre-2016 data. The synthetic control results are displayed in tables 8 and 9 below.

Year	WA Counties	Synthetic Control	Gap
2016	15,725	15,725	0
2017	15,346	15,347	-1
2018	18,749	18,749	0
2019	21,831	21,831	1
2020	17,030	17,030	0
2021	25,311	25,311	0
2022	31,797	31,796	1
2023	31,406	30,425	981

Table 8: Average Annual Earnings Gap. All values rounded to the nearest dollar.

Year	WA Counties	Synthetic Control	Gap
2016	120.84	120.89	-0.05
2017	205.30	205.25	0.05
2018	247.23	247.19	0.04
2019	266.11	266.16	-0.05
2020	214.67	214.66	0.01
2021	186.00	185.96	0.03
2022	189.79	189.82	-0.03
2023	204.29	214.79	-10.50

Table 9: Establishments per 100k Gap. All values rounded to the nearest dollar.

Both point estimates decrease slightly, although the decrease is not enough to change the sign of the average earnings effect estimate. The pre-treatment fitting error also remains very small.

7.2 Leave-One-Out Analysis

Another common robustness check is leave-one-out analysis, where the synthetic control analysis is reestimated while iteratively excluding each of the units contributing to the synthetic control from the donor pool (Abadie, Diamond, & Hainmueller, 2015). The method aims to determine whether the results are dependent on the inclusion of any one donor pool unit. If so, the observed effect might be caused by, for example, large idiosyncratic shocks in that unit (Abadie, 2021). Tables 10 and 11 below show the results of this analysis on the main outcome point estimates.

Excluded State	ATT
Arizona	841
Colorado	895
District of Columbia	895
Georgia	1,210
Idaho	1,179
Iowa	1,256
Maryland	1,190
New Mexico	965
North Dakota	395
Oklahoma	1,209
Pennsylvania	1,236
Rhode Island	1,935
Wyoming	1,159

Table 10: Leave-one-out Analysis: Annual Average Earnings

Excluded State	ATT
Arizona	-9.94
Delaware	-8.89
Hawaii	-7.73
Idaho	-9.91
Mississippi	-8.44
North Dakota	-15.84
Oregon	-21.35
Rhode Island	-9.43
South Dakota	-14.28
Tennessee	-8.02
Utah	-6.78
Vermont	-9.04
Wyoming	-11.10

Table 11: Leave-one-out Analysis: Establishments per 100k

As shown, the point estimates do not change sign with the exclusion of any state and are mostly similar in magnitude across most excluded states. The most influential excluded states are North Dakota (earnings and establishments), Rhode Island (earnings), and Oregon (establishments). The exclusion of North Dakota and Rhode Island push the average earnings estimate in different directions, and the excluded states do not seem to bias greatly either higher or lower than the main estimate when taken together. On the other hand, only two of the thirteen donor pool states have more positive estimates than the main estimate for the change in establishments per 100,000 residents. This might suggest that the unique combination used in the main synthetic control is underestimating the negative impact of the new policies on the number of establishments.

8 Survey Data Analysis

In this section, I use American Community Survey data compiled by IPUMS to examine additional trends. The main benefit offered by these data over the main analysis is that the survey data records individual hours worked. Further, as noted by Wilson (2023), individuals have no financial incentive to hide or distort personal income on the survey as they do on the administrative tax data.

However, the data also come with several limitations. First, these data have been found to underreport self-employment data, particularly as individuals are only thus identified if their self-employed labor is their primary work (Abraham et al., 2024). Further, the data are already small even at the state level. For example, on the 2023 survey, there were only 42 self-employed Washington respondents in the industry reporting non-zero hours and 36 reporting non-zero earnings. Additionally, the reference period for each year is the 12 months prior to when the individual was surveyed, not the previous calendar year. Since the data are currently only available through 2023, no full year of post-treatment data is yet available from this dataset as it was for the NES data. Therefore, while I examine the change in hours of work between years, the included data for the 2023 year is partially based on hours worked and earnings received in 2022. Hence the observations marked "2023" in the graphs are only intended to be suggestive of the most recent patterns in the combined 2022 and 2023 years rather than taken as representing the change in 2023.

Figures 9 and 10 show trends in average weekly hours and annual self-employment income reported by self-employed survey respondents from the state of Washington who listed the taxi and limousine services as their primary industry.

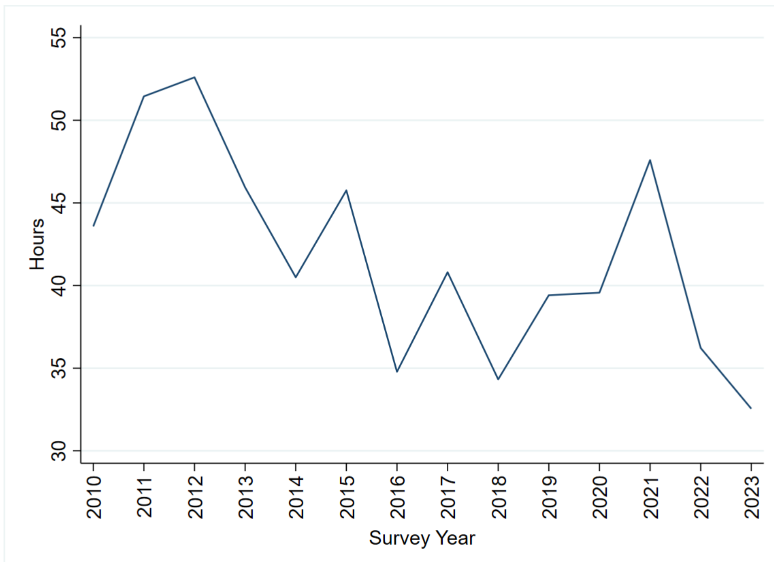

Figure 9: ACS Population Estimates: Usual Weekly Hours Worked During the Previous Year Estimates. Displayed result is probability-weighted average of respondents' reported usual hours worked per week over the 12 months prior to the survey date. Respondents were limited to self-employed individuals in the state of Washington who listed the taxi and limousine services as their primary industry.

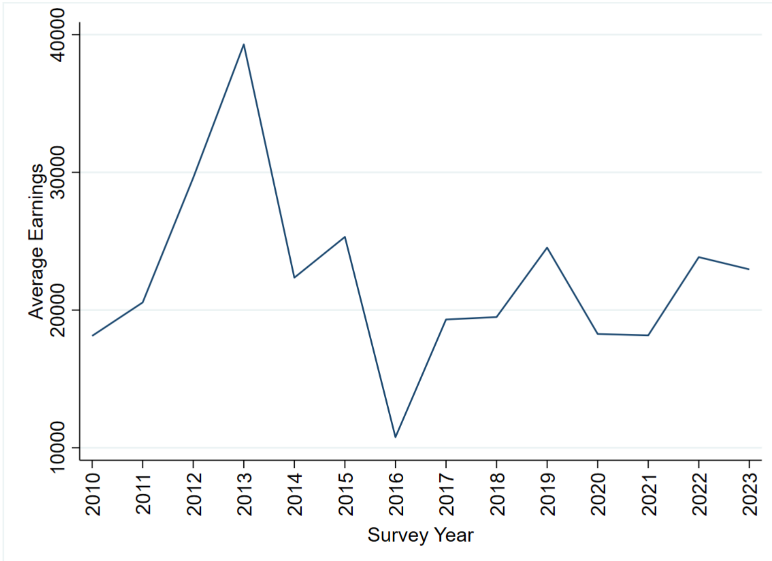

Figure 10: ACS Population Estimates: Earned Income During the Previous Year. Displayed result is probability-weighted average of respondents' reported pre-income-tax self-employment income over the 12 months prior to the survey date. These averages exclude individuals who claimed no positive or negative income, but include losses. Respondents were limited to self-employed individuals in the state of Washington who listed the taxi and limousine services as their primary industry.

Among individuals surveyed in 2023, the number of hours worked dropped to their lowest historical average, continuing the decrease from the year before. At the same time, when compared to decreased from prior years, the decrease in earnings on the 2023 surveys was fairly modest compared to historical variation. Figure 11 below combines these two measures with the number of weeks each survey respondent reported working in the previous 12 months to produce estimates of drivers' effective pre-tax hourly wages.

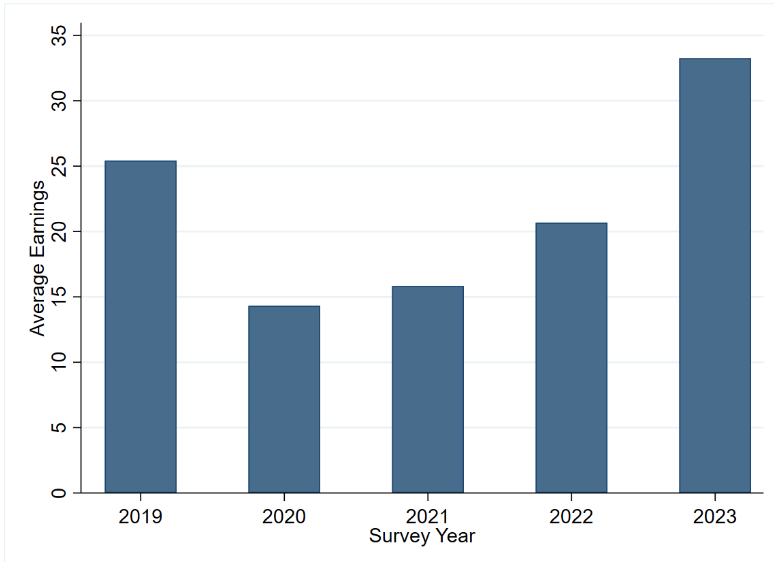

Figure 11: ACS Population Estimates: Average Hourly Earned Income During the Previous Year Displayed result comes from dividing net self-employment earnings by the product of usual hours worked and number of weeks worked in the past 12 months. Respondents were limited to self-employed individuals in the state of Washington who listed the taxi and limousine services as their primary industry.

The figure only shows these results starting with the 2019 survey since the ACS did not report exact numbers for the weeks worked on its 2010-2018 surveys, only intervals.

When comparing the 2022 and 2023 surveys across all three graphed outcomes, we see that usual weekly hours decreased from 36.22 to 32.55, total net earnings decreased from $23,844.89 to $22,957.83, and average earnings per hour increased from $20.68 to $33.26.[16] Additionally, the average weeks worked per year stayed largely the same, decreasing only slightly from 35.75 weeks to 35.37. Therefore, while the surveyed individuals made somewhat less overall, they also seem to have earned much higher effective hourly rates for the hours they did

[16] The 12-month earnings numbers here are noticeably lower than in the NES data. However, this is to be expected, since these numbers include total receipts minus business expenses, whereas the numbers from the tax data only include total receipts. Additionally, the ACS does not have a requirement to make at least $1,000 and file an income tax return to be included as NES does, although this should generally be offset by the implication that only people with passenger driving as their primary occupation should be included.

work.

9 Discussion

While my findings are not significant at the 5% level, the direction of the estimates is generally consistent with the work by Ashenfelter et al. (2010) which finds a dominant income effect when a similar permanent policy was passed for New York City taxicab drivers. Overall, the findings are not consistent with new workers working less hours crowding out workers working more hours, since that would lead to a net increase in drivers overall. Further, the limited survey evidence suggests that while high-hour workers reduced hours following the policy, they earned much higher effective hourly wages. Therefore, the policy is more likely to have raised the welfare of the majority of drivers as intended than lowered it.

Additionally, my findings could be explained with a reference-dependent model of labor supply, although deviation from neoclassical theory is not necessary given the policy's lasting effect. Even so, Nian et al. (2024)'s finding that labor elasticities became negative for taxi drivers only beginning in the pandemic, along with the authors' speculation that driver behavior might have changed, indicates that further investigations into the degree of reference-dependence now present in the TNC driver market during the post-pandemic era make for an interesting avenue for future research.

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