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ABSTRACT

We present new evidence on the labor market effects of large minimum wage increases by examining the policy changes implemented by Minneapolis and Saint Paul. Beginning with synthetic difference-in-differences methods, we find that the increase in the minimum wage decreased substantially restaurant and retail employment, even after accounting for potential confounding effects from the pandemic and civil unrest. Next, using variation in exposure to the minimum wage across establishments and workers within zip codes and industries of the Twin Cities, we find employment effects that are about half as large as those from the time series. The cross-sectional estimates difference out contemporaneous city-industry effects across establishments and workers, but they do not include equilibrium effects induced by the minimum wage such as changes in entry. We quantify a model of establishment dynamics to reconcile the different estimates and argue that they plausibly reflect lower and upper bounds of employment losses. We use the model to show that our estimates are consistent with an establishment elasticity of labor demand of –1 and illustrate how they can inform deeper parameters characterizing product and labor market competition, factor substitution, and establishment dynamics.

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1 Introduction

Increasing the minimum wage is one of the most debated economic policies in the United States. Despite decades of academic research, both policymakers and economists are still debating the economic impacts of minimum wages on workers, firms, and regions. Estimating the effects of the minimum wage is also important for understanding which classes of models better characterize labor markets and for estimating the deeper parameters that affect product and labor market competition, factor substitution, and establishment dynamics.[1]

We present new evidence from two minimum wage policies instituted recently in Minneapolis and Saint Paul that bears on this debate. Our paper differs in several ways from previous studies on the minimum wage. First, the minimum wage increase we examine is local, large, and interacts with a recession for a subperiod of our sample. Second, we use a new administrative dataset that improves measurements relative to previous studies. Third, in terms of the research design, we use various time series and cross-sectional sources of variation to estimate the effects of the minimum wage, adjust our estimates for the impact of pandemic and civil unrest conditions, and explore economic mechanisms to reconcile differences among the various estimates. Finally, we present evidence of anticipation effects arising from the announcement of a future minimum wage change.

Our analysis proceeds in three steps. First, following a standard approach in the minimum wage literature, we use time series variation to compare outcomes in the Twin Cities with those of appropriate control cities within Minnesota or the rest of the country. We find wage gains in most low-wage industries in Minneapolis, with the magnitude of the gains overlapping with our estimate of the direct effect of the minimum wage increase on establishments' labor costs. We do not detect significant employment changes for most industries. The minimum wage increase, however, is associated with jobs declines of 28 percent for restaurants and 16 percent for retail. Our finding of job declines in restaurants and retail is robust to using various indicators to adjust jobs in the Twin Cities for the impact of the pandemic and civil unrest. Second, using

[1] Hamermesh (1993) presents early estimates of labor demand elasticities and how they relate to these deeper parameters. Flinn (2006) analyzes the effects of minimum wages in a model with search frictions that give rise to monopsony power. Sorkin (2015) and Aaronson, French, Sorkin, and To (2018) explore production technologies that generate different effects of the minimum wage in the short run and the long run. Recent quantitative work includes Berger, Herkenhoff, and Mongey (2022), who analyze the efficiency and redistribution gains induced by the minimum wage, and Hurst, Kehoe, Pastorino, and Winberry (2022), who study the distributional impacts of the minimum wage in the short and the long run.

the differential exposure of establishments and workers to the minimum wage within the Twin Cities, we find employment effects that are roughly half as large as those from the time series. Our cross-sectional estimates are consistent with an elasticity of establishment labor demand of -1. Finally, we quantify a model of establishment dynamics to reconcile estimates from the cross section with those from the time series. We argue that they plausibly reflect lower and upper bounds of employment losses. Despite our attempt to adjust estimates for pandemic and civil unrest, the employment losses from the time series may still be confounded by factors contemporaneous with the minimum wage. On the other hand, the losses from the cross section omit equilibrium effects such as changes in the entry rate of establishments.

Minneapolis began implementing its minimum wage policy in 2018 with the aim of reaching the statutory minimum of 15 dollars per hour for all workers by 2024. Saint Paul introduced a similar policy in 2018 for implementation in 2020. The changes in the minimum wage are permanent and large by historical standards, with the minimum wage increasing by 46 percent in Minneapolis and by 26 percent in Saint Paul. Our analyses use a new administrative dataset on workers and establishments from Minnesota. The dataset merges worker-level Unemployment Insurance data with establishment-level Quarterly Census of Employment and Wages data to create a quarterly dataset between 2001(1) and 2021(4) on workers' hours and wages, as well as the establishments where they work by industry, zip code, and city.

Our dataset improves measurement relative to that of previous studies in three ways. First, we provide estimates from an administrative dataset for the effects of the minimum wage increase on hours worked. Second, in contrast to recent city-level minimum wage studies using administrative data, we include in our analyses firms with multiple establishments across city borders, which account for roughly 50 percent of jobs in the Twin Cities. Finally, we leverage detailed physical location data to increase the precision of our estimates in the time series and exploit within-city variation in the cross sections of establishments and workers.

We treat Minneapolis with an increase in the minimum wage for all periods after 2018, reflecting that the entire schedule of minimum wage increases was announced at once and the minimum wage will be indexed to inflation. We also treat Saint Paul with an increase in the minimum wage after 2018, because Saint Paul credibly committed to the policy when the Minneapolis ordinance was introduced in 2017 and its own ordinance passed in 2018. This aspect of our research design allows us to bring new evidence of anticipation effects to the literature, because any effects we detect in Saint Paul before 2020 are consistent with advance notice of future minimum wage increases.

For our time series analysis, we adopt the synthetic control groups approach proposed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2015) and augment it with fixed effects, following Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021). We generally find statistically and economically significant declines in restaurants' employment before the pandemic recession. For restaurants in Saint Paul, the decline in employment does not accelerate significantly after the pandemic. That is, the pandemic does not potentially confound our estimates. However, for restaurants in Minneapolis, the employment effects accelerate significantly in 2020. We detect a significant decline in retail employment for both cities, without an acceleration of the decline during the pandemic. Finally, we document a systematic pattern of responses across all low-wage industries, where industries with lower median wages before the minimum wage change experience larger wage gains and larger jobs, hours, and earnings losses in response to the minimum wage change.

One interpretation for the decline in restaurant employment is that the pandemic recession interacted with the minimum wage to accelerate the employment losses. However, the post-pandemic results for Minneapolis could potentially reflect a differential sensitivity to aggregate shocks for the synthetic control relative to that of Minneapolis. For example, the economic impact of lockdowns may have been more significant in larger, more densely populated cities than in the smaller cities that compose our control group. Additionally, as with any research design that uses time series variation, it may still be the case that the Twin Cities experienced idiosyncratic shocks that are not being differenced out in the post-treatment period. An example of such an idiosyncratic shock is the civil unrest in the second quarter of 2020, which impacted the Twin Cities differently from other cities in Minnesota.

We overcome these challenges for the interpretation of our results in three ways. First, we use variation from other U.S. cities that are also densely populated and faced lockdowns, but did not experience increases in their minimum wage. Using the sample of other U.S. cities also allows us to control for nationwide changes in economic conditions that either were induced by the pandemic or accelerated during the pandemic. Examples include demand changes such as substitution of services prone to virus transmission with online shopping and supply changes such as the rise of gig work and labor shortages in certain industries. Using the sample of other

U.S. cities, we continue to find significant jobs declines in restaurants and retail of the Twin Cities. However, some of these declines are smaller in magnitude than the ones we estimated using variation from within Minnesota.

Our second solution is to directly adjust jobs for the impact of the pandemic and civil unrest. We estimate the impact of pandemic and civil unrest on jobs from the cross section of control cities in the post-2020 sample using cell phone mobility and protest data. We then use the estimated impact and the exposure of all cities, including the Twin Cities, to the pandemic and civil unrest in order to adjust jobs. The adjusted synthetic difference-in-differences estimates are very similar to the unadjusted ones. The logic is that, while the Twin Cities are more exposed to pandemic restrictions and civil unrest than the typical control unit, their observed jobs declines are outliers relative to the jobs declines predicted by the cross-sectional relationship between jobs and pandemic and civil unrest conditions in the sample of control cities.

The third solution is to shift our research design away from using other cities in the control group. Instead, we use variation from the cross sections of establishments and workers within a zip code of a city. The cross-sectional estimates do not suffer from the concern that other factors confound the effects of the minimum wage, as long as the Twin Cities shocks are differenced out during a quarter either across establishments that belong to the same industry and zip code or across workers who belong to the same industry.

We demonstrate that establishments with larger exposure of their labor costs to the minimum wage experienced larger increases in their wage and larger declines in their jobs, hours, and wage bill. The ratio of employment to wage changes induced by exposure to the minimum wage policy is -1. Reassuringly for our research design, which attempts to difference out common factors at the zip code and industry level, we find that the response of all variables to an exposure to the minimum wage is remarkably stable between 2019, 2020, and 2021 in Minneapolis and between 2019 and 2020 in Saint Paul. Additionally, we document that the responsiveness of all establishment variables to changes in their labor costs differs before and after the minimum wage increase. Importantly, these responses do not exhibit trends before the minimum wage increase.

A concern about the interpretation of the results that use variation across establishments within an industry and zip code is that workers may have reallocated from exposed to non-exposed establishments or found jobs outside of their zip code. For this reason, we also analyze the cross section of workers whose jobs we can track everywhere within Minnesota. We find that workers who are more exposed to the minimum wage experience significantly larger employment and earnings losses than workers who are less exposed to the minimum wage, confirming that the establishment results are not driven by worker reallocation.

To summarize our various estimates for the jobs effects, the time series analysis shows that the minimum wage increase was associated with an average jobs decline of roughly 3 percent in the Twin Cities. The jobs losses appear in the restaurant and retail industries, which account for between 10 to 15 percent of total jobs in the Twin Cities. The analysis using variation from the cross section leads to estimates of jobs losses around half as large as the estimates from the time series.

In the last part of the paper, we discuss how to reconcile the estimates from the cross section with those from the time series. Despite our efforts to difference out other shocks, the Twin Cities may have experienced idiosyncratic shocks or had a differential response to an aggregate shock that cannot be differenced out using other cities during the post-treatment period. Time series effects of the minimum wage on employment sum up employment effects at the intensive margin, effects arising from the exit of establishments, and effects arising from the lack of entry of new establishments. By design, the estimates from the cross section do not account for the effects of entry, because they use establishments and workers that exist for at least one period.

We develop a model of establishment dynamics to formalize our intuition that it is plausible to reconcile the time series estimates with those from the cross section by appealing to entry dynamics that are omitted from the analysis of the cross section. When we quantify the model to reproduce the establishment responses we observe in the cross section, we find that the industry-level employment decline is of similar magnitude to that we estimate in the time series of restaurants. The similarity of the responses is reassuring for the potential of endogenous entry to account for the difference between employment effects in the time series and the cross section, because we calibrate the entry cost independently of our time series treatment effects by appealing to the industrial organization literature for estimates of the entry cost. Further, the quantitative responses from the model are consistent with the observed declines in the number of operating establishments for full-service restaurants in the data. To be clear, our results cannot identify whether missing entry, other equilibrium effects, or confounding factors account for the difference between the cross section and the time series. However, they suggest that both types of analysis are informative and offer plausible bounds of the effects of the minimum wage increase.

Our paper contributes to various strands of literature. Similar to previous studies examining low-wage workers across industries (Cengiz, Dube, Lindner, and Zipperer, 2019; Dube and Lindner, 2021; Neumark and Shirley, 2021), for most industries we fail to detect statistically significant employment effects. By contrast, our estimated employment elasticity with respect to the minimum wage for restaurants is more negative than the typical estimate found in the literature. For example, the comprehensive analysis of estimates for low-wage workers and low-wage industries in Neumark and Shirley (2021) reveals that roughly 80 percent of estimates are negative, with the average reported elasticity across studies being around -0.15. Our own analysis of published estimates for the restaurant industry finds an average elasticity of -0.1.

Our estimated jobs elasticity with respect to the minimum wage for restaurants is -0.37 using the cross-sectional analysis and -0.78 using the time series analysis. The employment impacts we document might be larger than those in the literature because the policy change we examine is larger. The 46 percent increase in the Minneapolis minimum wage by 2020 is significantly larger than the policy changes classified as "large minimum wage increases" in Clemens and Strain (2021), which range between 20 and 25 percent. The authors estimate a -0.5 elasticity of low-skill young employment with respect to the minimum wage for large policy changes and a non-negative elasticity for small policy changes.

An interpretation of our time series results is that the jobs elasticity is more negative when a higher minimum wage interacts with a recession. For example, the Seattle study by Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething (2022) estimates an elasticity of -0.3 for restaurants. However, Seattle was booming during the implementation of their large minimum wage increase, whereas the Twin Cities were hit by a recession. The acceleration of jobs losses during the pandemic recession is consistent with earlier studies such as Addison, Blackburn, and Cotti (2013), who document the larger sensitivity during recessions, and more recent work such as Clemens and Wither (2019), who find an employment elasticity with respect to the minimum wage of -1 when analyzing the increase in federal minimum wage during the Great Recession.

Through the lens of the model, we show that our cross-sectional estimates are consistent with an elasticity of labor demand at the establishment level of -1. We interpret this elasticity as a long-run elasticity for establishments operating in competitive labor markets and can flexibly adjust all other inputs. Our estimate is comparable to some other estimates found in the literature (Hamermesh, 1993; Beaudry, Green, and Sand, 2018). A puzzle in the labor economics literature is how to reconcile small or zero employment effects of the minimum wage with larger estimated elasticities of labor demand at the establishment level. One potential answer is that minimum wage increases have taken place in regions characterized by imperfectly competitive labor markets and that these increases are small enough relative to the competitive wage. Our results are not as puzzling as those in the literature, because we find an elastic labor demand at the establishment level and large employment effects from the introduction of the minimum wage for some low-wage industries. We caution readers that our results cannot be used to directly identify how competitive labor markets were in the Twin Cities before the minimum wage increase. The model that we can reject is that there is labor market power and that the increase in the minimum wage was sufficiently small to induce an equilibrium wage below the competitive level.

2 Policy Background and Data Sources

In this section, we detail the policy background underlying the minimum wage increases in the Twin Cities and describe our sources of data.

2.1 Minimum Wage Policy

The last minimum wage change in the state of Minnesota occurred in 2014, with the minimum wage reaching 7.75 dollars for small firms and 9.50 dollars for large firms by 2017 (see Online Table A.1). In 2018, Minneapolis increased the minimum wage for establishments that operate within the city. The increase was implemented in phases, with the goal of reaching 15 dollars per hour in 2022 for large firms and in 2024 for small firms (see Online Table A.2). In 2018, Saint Paul followed Minneapolis in adopting a 15 dollar minimum wage policy. Saint Paul also enacted a phased implementation that began increasing its minimum wage in 2020, with the goal of reaching 15 dollars for all firms by 2027. Both cities followed the statewide minimum wage policy regarding gratuities, which requires employers to pay their employees a wage equal at least to the minimum wage before gratuities are applied.

The changes in the minimum wage of the Twin Cities are large relative to other minimum wage changes, even relative to the ones classified as large in the analysis of Clemens and Strain (2021). Using the average minimum wage applicable to small and large firms, we find that in 2021 the minimum wage increased by 46 percent in Minneapolis and by 26 percent in Saint Paul relative to implementing the statewide minimum wage. Further, in both cities the minimum wage will be indexed to inflation once the target level of 15 dollars per hour is reached. Thus, to the extent that firms and workers do not perceive further increases in the statewide minimum wage, the minimum wage increase for operating within the Twin Cities is permanent.

2.2 Data Sources

We use two main sources of data on workers and establishments. Both sources are administrative and non-publicly-available data that were provided to us by Minnesota's Department of Employment and Economic Development (DEED).

The first data source is individual-level data on workers from Unemployment Insurance (UI). Minnesota requires most employers to file unemployment wage detail reports quarterly for the purpose of estimating the amount of unemployment insurance tax they owe. These reports provide us with data on quarterly earnings and hours worked for each worker. We calculate the hourly wage for each worker by dividing total quarterly earnings by quarterly hours.[2] Minnesota collects these data for each employee of a firm at the level of the establishment where they work. This feature of the data is especially important in studying the minimum wage effects, as roughly 50 percent of employment is generated in multi-establishment firms.[3]

The UI data do not contain information on the physical location of establishments, which is necessary in order to identify which establishments were affected by the minimum wage increase. To overcome this problem, the UI data is merged with establishment-level data from the Quarterly Census of Employment and Wages (QCEW). The QCEW records jobs that account for roughly 97 percent of employment in Minnesota. From these data, we observe the six-digit NAICS code for the industry that the establishment operates in, the physical location

[2] We exclude roughly 1 percent of observations with jobs that reported a hourly wage below the applicable youth minimum wage for Minnesota. For calculating the wage, we exclude the roughly 5 percent of observations that reported zero hours worked. We keep these observations for calculating other outcomes.

[3] While reporting is required at the establishment level, a few firms file reports under a single account. We have to exclude from our analysis multi-establishment firms that have at least one establishment in Minneapolis or Saint Paul and at least one establishment outside of Minneapolis or Saint Paul, but report the UI wage details of all their employees under a single account. These establishments constitute roughly 3 percent of all establishments and 5 percent of all wage records.

of the workplace, and the firm to which the establishment belongs. The physical location data consist of both the city and the zip code in which the establishment operates.[4]

The merged data result in a quarterly dataset between 2001(1) and 2021(4). Our geographic unit of analysis is a zip code within a city. This allows the same zip code to be affected differently by the minimum wage policy if it belongs to two different cities. It also allows for multiple treated units within a city that faces an increase in its minimum wage. For each industry, we calculate the average wage, aggregate number of jobs (sum of full-time and part-time jobs), aggregate hours, and aggregate earnings paid within geographic units for each quarter. For the wage, we calculate the average hourly wage per worker for workers below the 90th percentile within the industry and year and treatment or control groups.[5] Finally, we aggregate all units with less than 50 full-time equivalent jobs to one unit, separately for each industry and for treatment or control groups.

To summarize, by merging the worker-level UI data with the establishment-level QCEW data, we create a dataset on workers' hours and wages, as well as their establishments of employment by industry, zip code, and city. Our dataset improves measurement relative to that of the typical minimum wage study along three dimensions. First, using administrative sources, we provide estimates for the effects of a minimum wage increase on hours worked.[6] Second, Minnesota is unique in that it records employee hours worked at the establishment level within firms. This feature allows us to include in our analyses firms with multiple establishments across city borders.[7] Finally, we leverage physical location data at the zip code level to increase the precision of our estimates and conduct additional analyses at the establishment level that

[4]The raw data do not have physical location information for roughly 4 percent of establishments. In addition, we exclude 0.1 percent of establishments for which the city name and zip codes are contradictory.

[5]We examined wage results without excluding the top of the wage distribution. This measure of the wage is too noisy because of outliers at the top of the wage distribution and, in many cases, leads to statistically insignificant results for the wage. We also examined results that trimmed workers at the 75th percentile and generally find similar wage effects as those in our baseline measures, which trim at the 90th percentile.

[6]Some studies examining effects on hours worked (Zavodny, 2000; Couch and Wittenburg, 2001; Neumark, Schweitzer, and Wascher, 2004) have largely used reported usual weekly hours from the Current Population Survey. This measure has been documented to contain significant measurement errors (Heckman, 1993; Bound, Brown, and Mathiowetz, 2001; Barrett and Hamermesh, 2019). Our measure of hours worked is more precise than these survey measures, suggesting less attenuation bias in our estimates. There are only four states in the U.S. that collect hours worked in the matched employer-employee administrative data, the other three being Oregon, Rhode Island, and Washington. Minnesota Statute (Section 268.044) requires employers to report total number of paid hours for the purpose of UI administration.

[7]For example, this crucial subsample of firms, accounting for roughly 40 percent of jobs, was missing in the evaluation of the minimum wage increase in Seattle by Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething (2022).

require within-city variation.

3 Evidence from the Time Series

We begin by laying out the econometric framework for analyzing the impact of the minimum wage increase using variation from the time series of cities. We present our baseline estimates that use other Minnesota cities in the control group. Next, we use other U.S. cities in the control group and conclude by adjusting our estimates for the effects of the pandemic and civil unrest using additional cell phone mobility and protest data.

3.1 Econometric Methodology: Time Series

We analyze the labor market effects of the increase in the minimum wage separately in Minneapolis and Saint Paul. We exclude Saint Paul from the control group in our analysis of Minneapolis. Similarly, we exclude Minneapolis from the control group in our analysis of Saint Paul. Our choice to analyze Minneapolis and Saint Paul in parallel, as opposed to merging them in one treated unit, is appropriate because the Minneapolis ordinance was implemented in 2018, whereas the Saint Paul ordinance was implemented in 2020. We choose to treat both cities with an indicator of a minimum wage increase after 2018. Our logic for treating both cities in 2018 is that Saint Paul credibly committed to a minimum wage policy immediately after Minneapolis passed its ordinance in 2017 and Saint Paul passed its ordinance in 2018 for implementation in 2020. Our logic for adopting a treatment indicator that covers the entire period after 2018 is that both cities announced the entire schedule of minimum wage increases once and not in increments. To the extent that establishments face entry costs or adjustment costs of changing their labor inputs or production techniques, we expect them to react upon the announcement of the schedule. As a result, the results we present in Saint Paul before 2020 reflect advance notice of the minimum wage increasing from 2020.

3.1.1 Synthetic Difference-in-Differences

The key to analyzing the impact of a minimum wage increase is the credible estimation of the counterfactual in the absence of the minimum wage increase. To construct the counterfactual, we use synthetic control methods developed originally by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2015) and extended recently by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021). In this section, we summarize the methodology from these papers and explain why it is appropriate to use it in our context.

We have a balanced panel with N geographic units for T periods. The outcome for unit i in period t is Y_{it}. Exposure to the treatment of a minimum wage increase is $W_{it} \in \{0, 1\}$, where $W_{it} = 0$ denotes that unit i did not experience a minimum wage increase in period t and $W_{it} = 1$ denotes that it did. We order units so that the first N_{co} units are never exposed to the treatment, while the last $N_{\text{tr}} = N - N_{\text{co}}$ units are exposed to the treatment after time T_{pre}. In our specifications that use DEED data from Minnesota, we have multiple treated units because the unit of analysis is a zip code within a city. All zip codes in Minneapolis and Saint Paul are treated with a minimum wage increase after 2018.

Let Y_{it}^1 denote the outcome for unit i in period t if the unit has been exposed to the minimum wage increase. Let Y_{it}^0 denote the counterfactual outcome that we would have observed in the absence of the minimum wage increase. The average treatment effect in period t is $\tau_t = \frac{1}{N_{\text{tr}}} \sum_{i=N_{\text{co}}+1}^{N} \left(Y_{it}^1 - Y_{it}^0\right)$ and the average treatment effect across all periods is $\tau = \frac{1}{T-T_{\text{pre}}} \sum_{t=T_{\text{pre}}+1}^{T} \tau_t$.

The fundamental problem in estimating the treatment effect is that the counterfactual outcome Y_{it}^0 is not observed, because unit i is exposed to the minimum wage increase after time $t > T_{\text{pre}}$. Since the seminal study of Card and Krueger (1994) on the minimum wage increase in New Jersey, a popular method to overcome this problem has been to find a control group of non-treated units and use its post-treatment outcomes to estimate the counterfactual Y_{it}^0 for treated units. With multiple units and time periods in the sample, this amounts to a two-way fixed effects regression

$$Y_{it} = \alpha_i + \beta_t + \tau W_{it} + u_{it}, \tag{1}$$

where α_i is a unit fixed effect, β_t is a time fixed effect, and u_{it} is the error term. The specification in equation (1) assumes that outcomes of treated and non-treated units are equal (up to a constant) in the post-treatment period in the absence of the minimum wage increase. Typically, the plausibility of parallel trends is assessed by evaluating whether trends are parallel during the pre-treatment period.

The concern with the difference-in-differences specification is that there is no control group with pre-treatment outcomes that resemble those of treated units. Synthetic control methods, such as those in Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2015), aim to overcome this problem by finding a vector of weights $\hat{\omega}$ that forces pre-treatment trends in the outcomes for the non-treated units to align with pre-treatment trends in the outcomes for the treated units. More explicitly, the goal is to find weights such that $\sum_{i=1}^{N_{\text{co}}} \hat{\omega}_i Y_{it} \approx N_{\text{tr}}^{-1} \sum_{i=N_{\text{co}+1}}^{N} Y_{it}$ for each time period before the treatment $t = 1, \ldots, T_{\text{pre}}$.

Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) propose a synthetic difference-in-differences methodology, which uses estimating equation (1) and, additionally, weights observations with ω_i so that treated and non-treated units are as close as possible in terms of pre-treatment outcomes. The weights are estimated as [8]

$$(\hat{\omega}_0, \hat{\omega}) = \arg \min_{\omega_0 \in \mathbb{R}, \omega \in \Omega} \sum_{t=1}^{T_{\text{pre}}} \left(\omega_0 + \sum_{i=1}^{N_{\text{co}}} \omega_i Y_{it} - \frac{1}{N_{\text{tr}}} \sum_{i=N_{\text{co}}+1}^{N} Y_{it} \right)^2 + \zeta^2 T_{\text{pre}} ||\omega||_2^2, \tag{2}$$

$$\Omega = \left\{ \omega \in \mathbb{R}_+^N : \sum_{i=1}^{N_{\text{co}}} \omega_i = 1, \omega_i = N_{\text{tr}}^{-1} \text{ for all } i = N_{\text{co}} + 1, \ldots, N \right\}.$$

If we use the estimated $\hat{\omega}$ from equation (2) as weights in the estimating equation (1), the synthetic difference-in-differences treatment effect $\hat{\tau}$ is

$$\left(\hat{\tau}, \hat{\alpha}, \hat{\beta} \right) = \arg \min_{\tau, \alpha, \beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \alpha_i - \beta_t - \tau W_{it} \right)^2 \hat{\omega}_i \right\}. \tag{3}$$

Removing the estimated weights $\hat{\omega}_i$ from the least-squared problem in equation (3) leads to the standard difference-in-differences specification. Removing the unit fixed effects α_i from equation (3) and ω_0 from equation (2) leads to the standard synthetic control specification.

Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) also propose choosing time weights λ_t to balance the pre-treatment and the post-treatment periods for the control group. A problem with using time weights is that the weights may change significantly as additional quarters of data become available. For our baseline, we settle on equally weighting all pre-treatment periods to keep the analysis as transparent as possible. However, as a robustness check, we also present analyses using estimated time weights.

Figure 1 illustrates the use of synthetic difference-in-differences and contrasts it to standard difference-in-differences in the context of the minimum wage increase in Minneapolis. The

[8] Following these authors, we allow for a shifter ω_0 that aligns the pre-treatment trends for the synthetic control and the treated units up to a constant, which is differenced out by the fixed effect. The regularization parameter ζ penalizes non-zero weights to ensure the minimization problem has a unique solution. We find that a small penalty of $\zeta = 10^{-6}$ works well in terms of minimizing the weight on control units with dissimilar pre-trends to treated units.

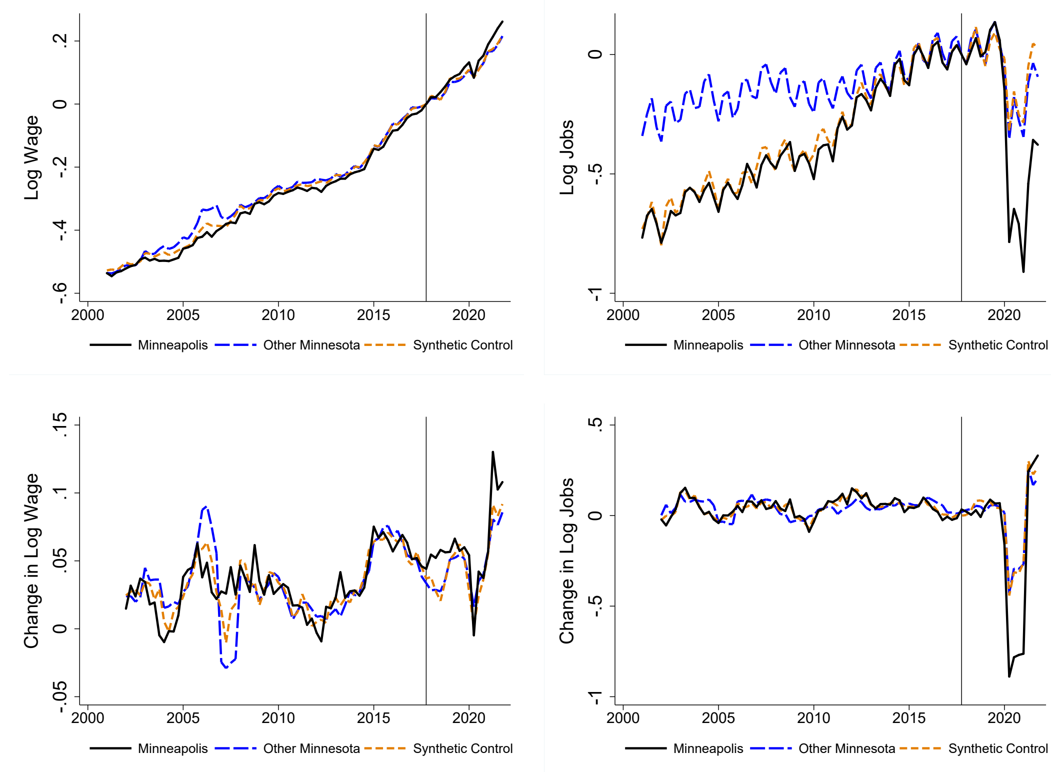


Figure 1: Time Series of Full-Service Restaurants

upper panels of the figure plot quarterly time series of the average hourly wage and the total number jobs for the full-service restaurant industry between 2001(1) and 2021(4). All series are in logs and normalized to 0 in 2017(4), which is the last quarter before the minimum wage increased in Minneapolis. The solid lines in the upper panel show that full-service restaurants in Minneapolis experienced a significant increase in the wage and jobs over time.

The long-dashed blue lines show the evolution of the wage and jobs for the average of all cities in Minnesota besides Minneapolis and Saint Paul. This average represents the control group in a difference-in-differences specification. The jobs trends before 2018 in Minneapolis and in other cities in Minnesota are significantly different. The dashed orange line shows the evolution of the wage and jobs for the synthetic control of Minneapolis, which is the weighted average of cities in Minnesota other than Minneapolis and Saint Paul using weights $\hat{\omega}_i$. As the figure shows, the time series for the synthetic control reproduce very closely the time series of the wage and jobs in Minneapolis in the pre-treatment period.

In our analyses below, we focus on outcome variables that are expressed in yearly growth rates. The lower panels of Figure 1 demonstrate that the synthetic control also fits well the growth of Minneapolis during the pre-treatment period. Using synthetic difference-in-differences, we can visualize the effect of the minimum wage increase on the growth of the wage and jobs as the difference in the post-2018 period between the dashed orange lines and the solid lines.

3.1.2 Growth Specification of Synthetic Difference-in-Differences

We express outcome variables Y_{it} in equation (3) in growth rates. We prefer a specification in growth rates to a specification in levels for two reasons.[9] If the Twin Cities implemented a minimum wage policy because they were growing at a different rate than other cities, that would invalidate the identifying assumption that the treatment effect is independent to other determinants of outcome variables.[10] The unit fixed effect α_i in a growth specification removes heterogeneity in average growth rates that may be correlated with the treatment of increasing the minimum wage. Additionally, using yearly growth rates allows us to remove quarterly seasonal variation, thus improving the efficiency of our estimates.

Accordingly, if y_{it} is a time series in levels, we take year-over-year differences in logs and define

$$Y_{it} \equiv \log y_{it} - \log y_{i,t-4}, \forall i = 1, ..., N_{\text{co}}, \quad Y_{it} \equiv (\log y_{it} - \log y_{i,t-4})\, \bar{\nu}_i, \forall i = N_{\text{co}} + 1, ..., N. \quad (4)$$

In equation (4), we weight zip codes of the treated cities with their share $\bar{\nu}_i$ of the corresponding variable in the three years before the minimum wage increase. Doing so allows us to interpret the treatment effect as pertaining to the city as a whole as opposed to the average zip code within a city.[11] Holding the zip code weights $\bar{\nu}_i$ constant over time allows us to interpret the treatment effects as counterfactual outcomes that the Twin Cities would have experienced in

[9] Another popular specification in the minimum wage literature is to add unit-specific linear time trends to equation (1). However, pre-treatment trends could be non-linear. Meer and West (2016) critique the practice of using unit-specific time trends in levels specifications and argue in favor of specifications that use growth rates of employment as the dependent variable. In our context, an example of non-linearity is retail trade in Minneapolis which exhibits a secular decline in the 2000s, stability in the first part of the 2010s, and an upward trend after 2015. See Online Figures A.1 to A.16 for the time series in the other low-wage industries that are included in our analyses.

[10] Ferman and Pinto (2021) show that the synthetic control estimator is biased if treatment assignment is correlated with the factor structure underlying the dynamics of outcome variables, even when the number of pretreatment periods goes to infinity.

[11] The exception is the wage, for which we do not use any weights. The reason is that we are interested in the effects of the minimum wage increase on the wage of the average worker. For the control units, we do not weight the growth rates of zip codes, because these weights enter multiplicatively with the synthetic control weights ω_i in equation (3).

the absence of the minimum wage increase, holding the spatial distribution of economic activity constant at the same levels observed just before the policy change.

Working with the outcome variable in equation (4) means that our treatment τ is the effect of the minimum wage increase on the average yearly growth rate of the variable over the entire post-treatment period, $T - T_{\text{pre}}$. We transform the growth effect into a cumulative effect up to final period T on the (log) variable with the formula $g_T \equiv \mathbb{E}\big(\log y^1_{i,T} - \log y^0_{i,T}\big) = \frac{(T-T_{\text{pre}})\tau}{4}$, where 4 appears in the formula because τ is a yearly, as opposed to a quarterly, growth rate. Variable g_T is the log change in outcome y in the final period T due to the minimum wage increase between periods $T_{\text{pre}+1}$ and T.

3.1.3 Discussion of Methodology

Before presenting the impacts of the minimum wage increase, we pause to discuss the performance of the synthetic control method in accounting for outcomes in Minneapolis and Saint Paul before the minimum wage increase. In Online Table A.3, we present R-squared coefficients from regressions of variables' growth in Minneapolis or Saint Paul on the growth of the synthetic control calculated using the weights $\hat{\omega}_i$. The regressions are performed only during the pre-treatment period. We find that for five out of the six low-wage industries included in our time series analyses and separately for restaurants, the synthetic control accounts for a substantial fraction of the variation of growth of Minneapolis and Saint Paul before the minimum wage increase. To give an example from a key industry that we elaborate upon below, for full-service restaurants the synthetic control accounts for 87 percent of the time series variation of jobs growth in Minneapolis and 73 percent of that in Saint Paul. Despite the overall success in accounting for a substantial variation of the pre-treatment growth, the synthetic control does not perform equally well in all industries. The most notable lack of fit is for the arts, entertainment, and recreation industry. As a result, we interpret the results for this industry with more caution.

While these R-squared statistics are informative, we do not rely solely on them to assess the appropriateness of the synthetic difference-in-differences methodology. Recent research by Ferman and Pinto (2021) has documented biases when the pre-treatment fit is less than perfect. We alleviate these concerns by using a specification in growth rates with a fixed effect instead of a levels specification.[12] Additionally, in robustness checks reported below, we add time weights to our specification, following Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021). Doing so allows us to balance the pre-treatment and the post-treatment periods for the control group. Finally, assuming that the data generating process is a linear factor model, we perform Monte Carlo simulations to assess the size of the bias in the presence of imperfect fit and generally conclude that the bias in our context is small.

3.2 Evidence from Minnesota Cities

We focus on the two-digit industries in which 30 percent or more of workers earn below 15 dollars per hour in 2017 (see Online Table A.4 for the detailed estimates). The six industries that satisfy this criterion are retail trade (44); administrative services (56); health care and social assistance (62); arts, entertainment, and recreation (71); accommodation and food services (72); and other services (81), which consists of repair and maintenance shops, personal and laundry services, and various civic, professional, and religious organizations. In addition, we separately analyze full-service (722511) and limited-service (722513) restaurants, which have a high fraction of potentially impacted workers and have been studied extensively in the literature.[13]

Table 1 presents results for these low-wage industries and separately for restaurants. Entries are multiplied by 100 and equal the log point change in outcomes in 2021(4) due to the minimum wage increase, $100 \cdot g_T$. The columns present different outcome variables. For example, the first row shows that the increase in the minimum wage in Minneapolis caused a 9.3 log points (roughly 10 percent) increase in the retail wage and a 34 log points (roughly 29 percent) decrease in the number of retail jobs. Each entry in parentheses is the p-value associated with the estimated treatment effect, which is the probability of obtaining a treatment effect as extreme as the point estimate under the null hypothesis that the treatment effect is zero. To infer the statistical significance of the estimated effects, we use the "placebo method." Continuing the example, the placebo method produces a p-value of 0 for both the wage and jobs, and thus we conclude that the effects are precisely estimated and can be statistically distinguished from

[12] Using growth rates means that we are requiring that the synthetic control fits the high-frequency movements of the wage, jobs, hours, and earnings. While the pre-treatment fit using a levels specification would have been significantly better, we prefer to match higher-frequency variations in order to alleviate concerns about over-fitting or correlation of the incidence of the treatment with underlying structural characteristics that affect outcomes in the treated units.

[13] We also analyzed other industries and did not find statistically or economically significant responses. As we explain below, we use data from these other industries in our analysis of the cross section.

Table 1: Effects of the Minimum Wage Increase from the Time Series

Minneapolis	Wage	Jobs	Hours	Earnings
Retail Trade (44)	9.3	−34.0	−23.4	−14.8
	(0.0)	(0.0)	(0.8)	(7.4)
Administration and Support (56)	11.5	9.6	11.3	17.7
	(0.0)	(46.8)	(49.6)	(18.6)
Health Care and Social Assistance (62)	−2.3	3.9	6.9	2.8
	(13.8)	(52.7)	(34.0)	(86.3)
Arts, Entertainment and Recreation (71)	−2.4	−15.7	−7.9	7.0
	(38.4)	(3.6)	(28.4)	(89.3)
Accommodation and Food Services (72)	0.7	−27.1	−45.7	−40.1
	(70.7)	(0.0)	(0.0)	(0.0)
Other Services (81)	10.3	−0.9	−15.2	−0.4
	(0.0)	(81.7)	(4.8)	(87.3)
Full-Service Restaurants (722511)	5.9	−51.9	−49.3	−50.0
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	9.5	−35.5	−26.9	−25.5
	(0.0)	(0.6)	(2.6)	(5.8)
Saint Paul	**Wage**	**Jobs**	**Hours**	**Earnings**
Retail Trade (44)	9.2	−26.5	−53.6	−28.8
	(0.0)	(0.4)	(0.0)	(0.4)
Administration and Support (56)	4.8	−5.2	21.1	−60.5
	(9.4)	(69.7)	(12.4)	(0.0)
Health Care and Social Assistance (62)	−0.9	4.1	7.1	−3.3
	(50.9)	(54.3)	(31.0)	(59.9)
Arts, Entertainment and Recreation (71)	−0.2	−25.1	−17.4	−4.1
	(83.3)	(0.0)	(2.8)	(30.6)
Accommodation and Food Services (72)	4.0	−39.2	−58.8	−30.0
	(0.0)	(0.0)	(0.0)	(0.0)
Other Services (81)	2.3	21.2	−2.1	11.1
	(32.2)	(0.2)	(85.3)	(11.6)
Full-Service Restaurants (722511)	4.2	−28.7	−26.6	−29.4
	(0.0)	(0.0)	(0.2)	(0.0)
Limited-Service Restaurants (722513)	4.2	−59.6	−76.0	−88.3
	(0.0)	(0.0)	(0.0)	(0.0)

Notes: The estimates are in log points, multiplied by 100. Entries in parentheses are p-values in percent using the placebo method.

zero at conventional levels of significance.[14]

In Minneapolis, in 2021(4) we estimate wage increases with low p-values for retail; administrative and support services; other services; and restaurants. Among industries with statistically significant increases, we document increases that range between 6 and 12 log points. In Saint Paul, we estimate wage increases with low p-values for retail; administrative and support services; and accommodation and food services (and for restaurants within that industry). The wage increases in Saint Paul range between 4 and 9 log points. The smaller wage gain in Saint Paul relative to Minneapolis is intuitive, because Saint Paul announced the minimum wage increase in 2018 but did not implement it until 2020, so there are only eight quarters of data under the new minimum wage policy.

We find the wage increases reasonable. The difference between the minimum wage in Minneapolis and the one in the control cities is 46 percent, whereas this difference is 26 percent in Saint Paul. However, many workers are not close to the minimum wage, even in low-wage industries, and thus the estimated effects of the minimum wage increase on the wage are expected to be smaller than the change in the minimum wage. Holding worker hours constant at their 2017 level, the average establishment in the Twin Cities would experience a 9 percent increase in its labor cost if all its workers were paid 15 dollars per hour. Weighted with employment, the increase in labor costs is 6 percent. The direct effect of the minimum wage on labor costs falls comfortably in the range of wage gains we estimate.

Turning to the second column, in Minneapolis we find negative jobs effects in 2021(4) for retail; arts, entertainment, and recreation; and accommodation and food services. Within accommodation and food services, we find large declines for both full-service restaurants and for limited-service restaurants. We also document large jobs declines in Saint Paul for the same

[14] The placebo method estimates placebo treatment effects in samples of subsets of non-treated units. Since we should be estimating a zero treatment effect in the absence of a treatment, the distribution of treatment effects under the placebo method gives us the distribution of noise inherent in the data. See Algorithm 4 in Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) for implementation details to construct the placebo standard errors. In our application, we have multiple treated units, as our geographic unit of analysis is a zip code within a city. Thus, we construct placebo estimates by assigning a treatment status to 999 random subsamples of zip codes, with each subsample having a size equal to the number of treated units in Minneapolis or Saint Paul. We use the formula $p = 2\min\{p_H, p_L\}$ to calculate the p-value for a point estimate, $g_{2020(4)}$, where p_H is the fraction of placebo samples with point estimates that are higher than the estimate of Minneapolis or Saint Paul in 2020(4) and p_L is the fraction of placebo samples with point estimates that are lower than the estimate of Minneapolis or Saint Paul in 2020(4). To confirm the robustness of our conclusions, we also examined bootstrapped standard errors, which are popular in difference-in-differences settings (Bertrand, Duflo, and Mullainathan, 2004). Our conclusions do not change when we use bootstrapped standard errors, and thus we omit them from our presentation.

industries as in Minneapolis.

The third column presents results for total hours. The decline in hours is generally of similar magnitude to the decline in jobs for the restaurant industries. For arts, entertainment, and recreation, the decline in hours is smaller than the decline in jobs. Retail hours decline more than jobs in Saint Paul, whereas they decline by less in Minneapolis. Finally, for other services, we detect a statistically significant decline in hours, whereas we did not detect a statistically significant decline in jobs.

The last column of the table presents results for worker earnings. Given the modest wage gains for all industries and the significant negative effects on employment for some industries, it is not surprising that in both cities we fail to detect a statistically significant increase in worker earnings in any industry. We detect statistically significant declines in worker earnings for retail and accommodation and food services in both cities and separately for restaurants.[15]

Figure 2 helps understand the sources of industry heterogeneity in wage and employment responses. The figure plots the wage, jobs, hours, and earnings responses of each industry in Table 1 against each industry's median wage before the minimum wage increase. The median wage proxies for the intensity of the minimum wage treatment, because we expect a larger share of workers to be affected by the minimum wage in industries with a lower median wage. We find that industries with a lower median wage experience more positive wage responses and more negative jobs, hours, and earnings responses. The median wage absorbs between one-third and one-half of the variation in these responses. We find this result sensible and acknowledge that the heterogeneity in responses across sectors could reflect factors other than the intensity of the treatment, such as the product and labor market structure of each industry. We discuss these factors in more detail in Section 6.

Next, we examine the time variation of the estimated effects for restaurants. Figure 3 plots the quarterly cumulative wage effects of the minimum wage increase.[16] Along with our

[15] We find a large decline in earnings for administration and support in Saint Paul that it is difficult to reconcile with its wage and hours changes. We investigated the time series of earnings and concluded that the estimated effect on earnings entirely reflects an extreme increase in reported earnings for some establishments just before 2018. This increase is not reflected in the wage because we trimmed the wage at the 90th percentile. When we also trim earnings at the 90th percentile, the decline in earnings in this industry is statistically indistinguishable from zero.

[16] We run the regression $Y_{it} = \alpha_i + \beta_t + \sum_{h=T_{\text{pre}+1}}^{T} \tau(h) W_{it}(h) + u_{it}$ using weights $\hat{\omega}_i$, where $h = T_{\text{pre}+1}, ..., T$ denotes the quarter of the treatment. For each quarter h shown in the figure, its cumulative effect equals $100 \cdot \sum_{j=T_{\text{pre}+1}}^{h} \hat{\tau}(j)/4$, where 4 appears in the formula because $\tau(h)$ is a yearly, as opposed to a quarterly, growth rate.

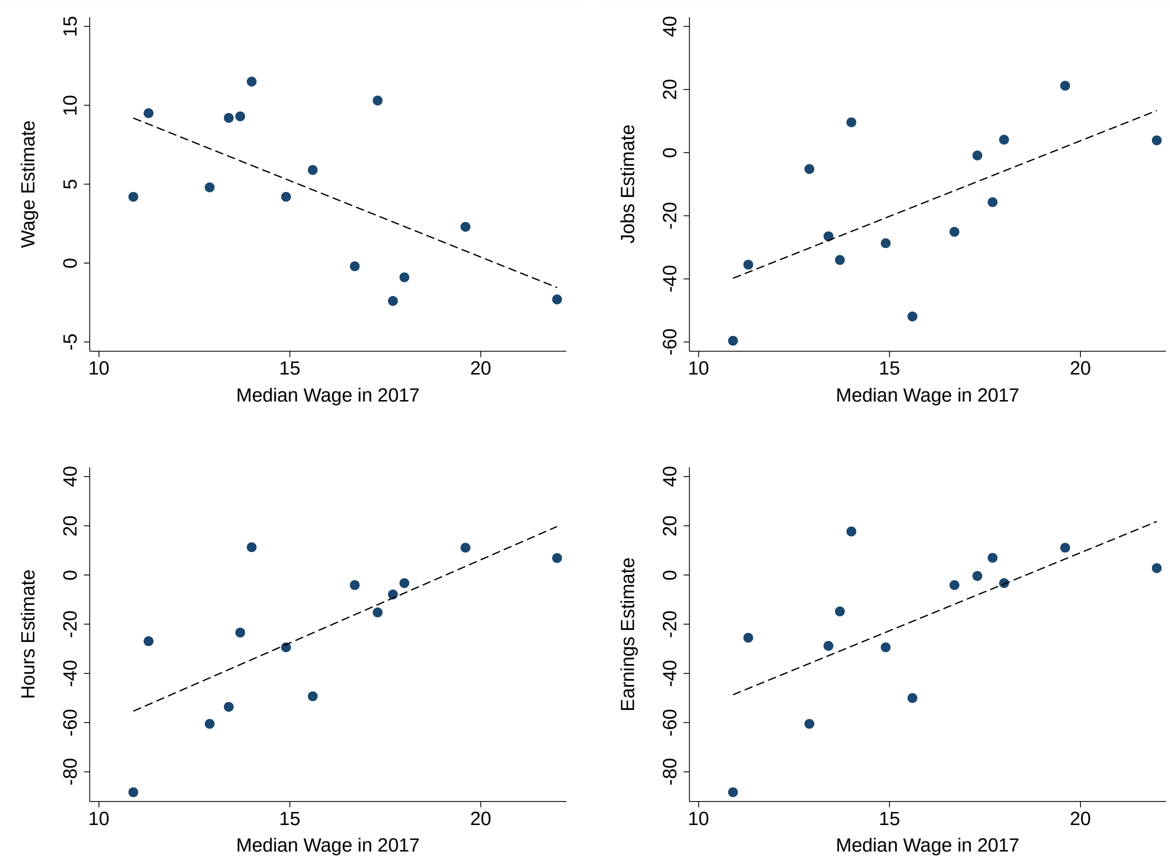
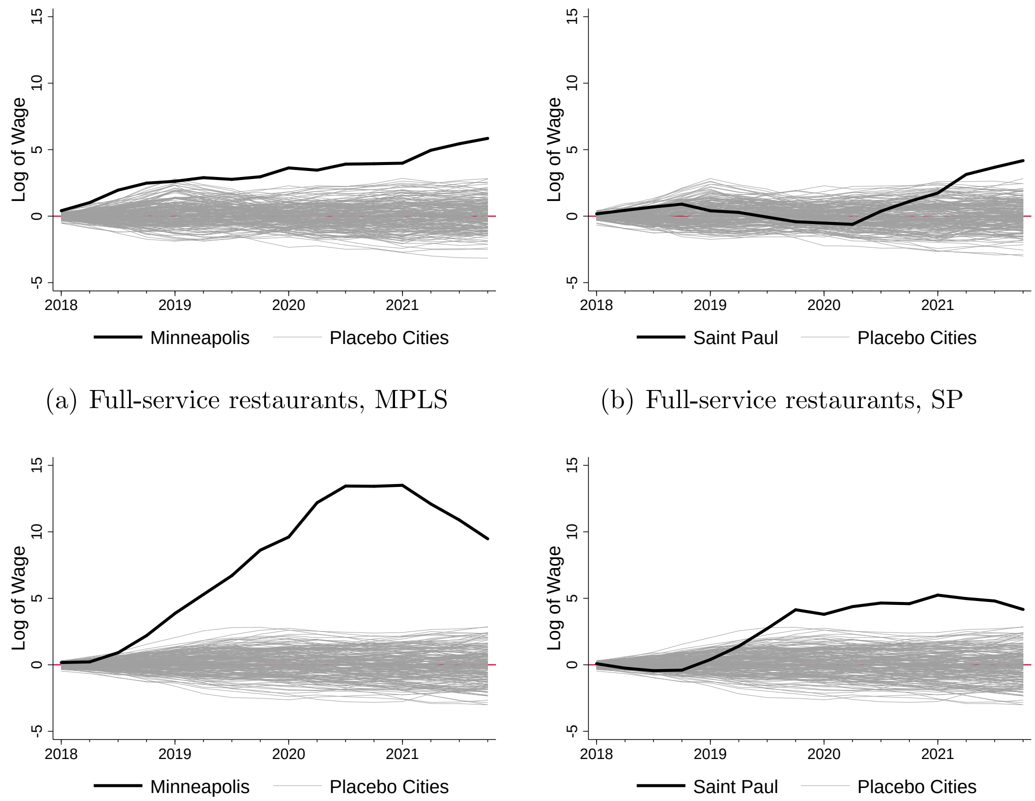


Figure 2: Wage and Employment Responses Across Industries and Cities

estimated effects, we plot placebo effects for 200 collections of units that were not subject to the minimum wage increase. Since we know that these placebo units did not experience an increase in their minimum wage, any effect we estimate for these units is due only to random noise.

The left panels of the figure show that the wage for restaurants in Minneapolis increased soon after the minimum wage ordinance went into effect. By contrast, in Saint Paul there is no statistically significant increase in the wage for full-service restaurants until 2021. For limited-service restaurants, wages increase in the second half of 2019. We find the difference in the response of the wage between Minneapolis and Saint Paul intuitive, because the Saint Paul ordinance was implemented two years after the Minneapolis ordinance.

Figure 4 plots the quarterly cumulative jobs effects of the minimum wage increase for restaurants in the Twin Cities, from which we draw three conclusions. First, in contrast to the wage, jobs in Saint Paul declined before the implementation of the minimum wage and the pandemic



(a) Full-service restaurants, MPLS (b) Full-service restaurants, SP

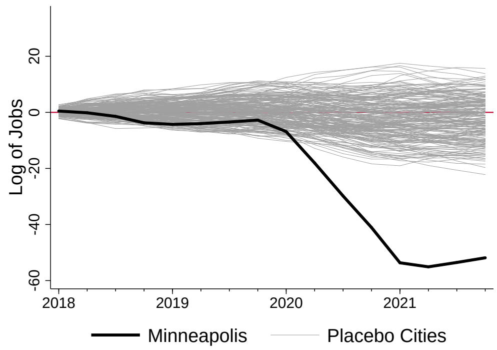
(c) Limited-service restaurants, MPLS (d) Limited-service restaurants, SP

Figure 3: Time-Varying Wage Effects in the Twin Cities Restaurants

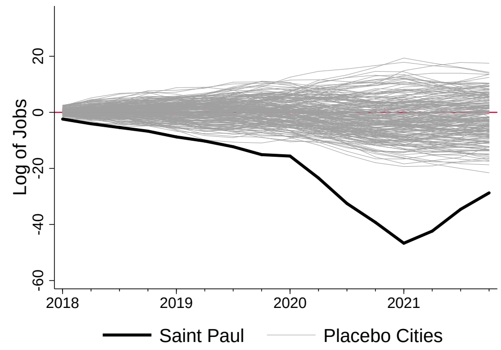
in 2020. This evidence of advance notice is consistent with our cross-sectional results below, which also show jobs declines before 2020 in Saint Paul.[17] Second, in Minneapolis the jobs declines for limited-service restaurants appear before the pandemic, but the job declines for full-service restaurants appear after the pandemic. Finally, with the exception of jobs in full-service restaurants in Saint Paul during 2021, in all other cases jobs do not tend to revert back to their level before the policy change.

Figure 5 presents wage and jobs effects of the minimum wage increase for retail in the Twin Cities. Wages in retail increase smoothly over time in both cities. Jobs begin to decline between mid 2019 and early 2020. In retail, we do not observe an acceleration of the wage and jobs effects during 2020, with wages continuing to increase and jobs continuing to decline throughout 2021.

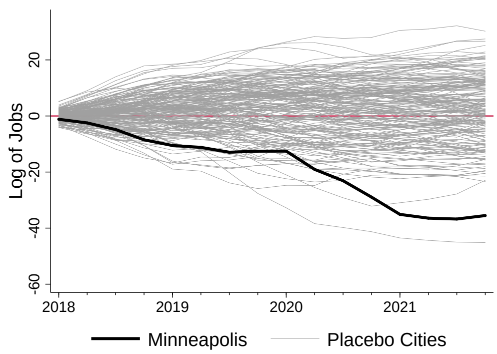
[17] We also examined advance notice in Minneapolis by backdating the treatment of the minimum wage to 2016 and 2017. We failed to detect significant effects in Minneapolis. The difference with Saint Paul is explained by the greater uncertainty during 2016 and 2017 about whether the Minneapolis minimum wage ordinance would pass, whereas in Saint Paul the ordinance was passed in 2018 but implemented in 2020.



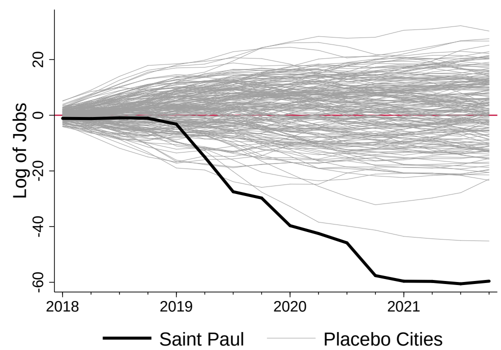
(a) Full-service restaurants, MPLS



(b) Full-service restaurants, SP



(c) Limited-service restaurants, MPLS



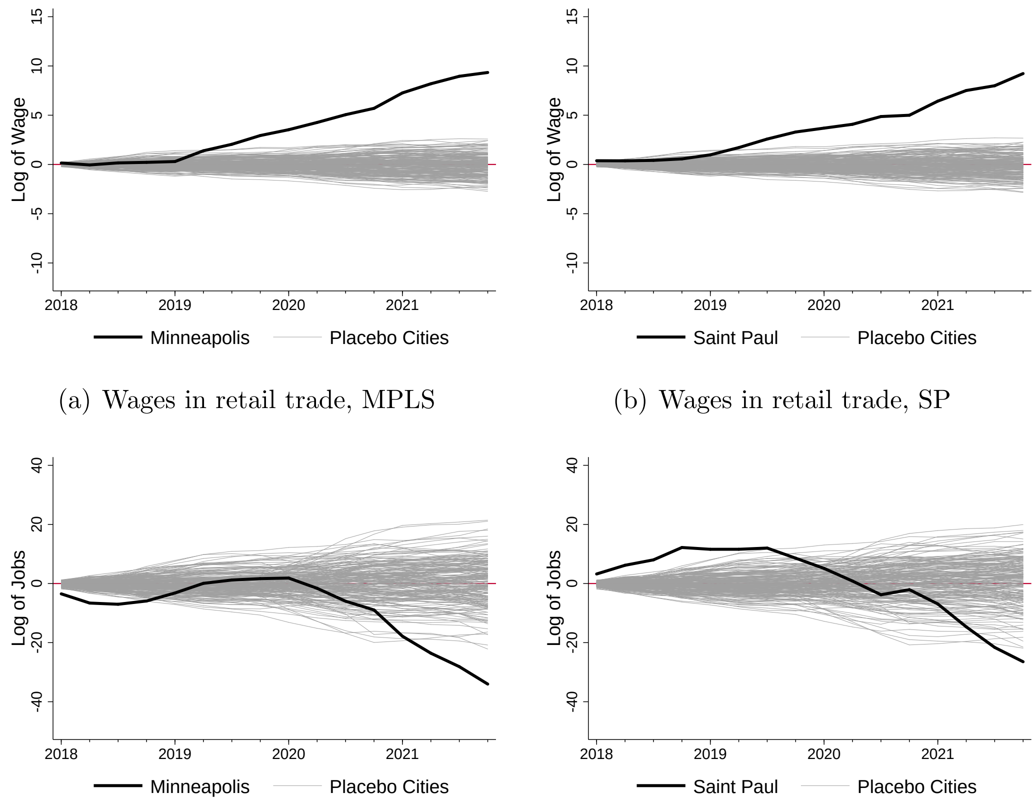
(d) Limited-service restaurants, SP

Figure 4: Time-Varying Jobs Effects in Restaurants

We conclude this section by discussing two robustness checks. In Online Table A.5, we repeat our estimates by adding time weights λ_t into the regression (3), following Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021).[18] The time weights are chosen to make the average pre-treatment growth as similar as possible to the average post-treatment growth in the control group. Thus, this exercise allows us to examine the robustness of our results when we place more weight on periods when, similar to the pandemic, the synthetic control experiences large negative growth. Even when we re-weight the data, the results are quite similar to the baseline results.

Our second robustness check repeats our estimates in a sample of cities that excludes cities bordering Minneapolis and Saint Paul. It is conceivable that the implementation of a higher minimum wage reallocated jobs from the Twin Cities to neighboring cities. From the perspective

[18]Equation (3) is replaced by $\left(\hat{\tau}, \hat{\alpha}, \hat{\beta}\right) = \arg\min_{\tau,\alpha,\beta} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \alpha_i - \beta_t - \tau W_{it}\right)^2 \hat{\omega}_i \hat{\lambda}_t \right\}$. The time weights $\hat{\lambda}_t$ are chosen so that $(\hat{\lambda}_0, \hat{\lambda}_t) = \arg\min_{\lambda} \sum_{i=1}^{N_{\mathrm{co}}} \left(\lambda_0 + \sum_{t=1}^{T_{\mathrm{pre}}} \lambda_t Y_{it} - T_{\mathrm{post}}^{-1} \sum_{t=T_{\mathrm{pre}}+1}^{T} Y_{it}\right)^2$.



(a) Wages in retail trade, MPLS

(b) Wages in retail trade, SP

(c) Jobs in retail trade, MPLS

(d) Jobs in retail trade, SP

Figure 5: Time-Varying Wage and Jobs Effects in Retail

of a city that implements a minimum wage increase, the policy-relevant statistic is its change in jobs, irrespective of whether these jobs disappeared or were reallocated to neighboring cities. Therefore, we do not merge neighboring cities with the Twin Cities in estimating the effects of the minimum wage change. However, to the extent that jobs were reallocated to neighboring cities and these cities are part of the synthetic control, we could be double-counting the effects of the minimum wage because cities in the control group experience jobs growth. Online Table A.6 shows that this is not the case, because our estimates do not change significantly when we exclude bordering cities from the sample of cities that form the synthetic control.

3.3 Evidence from Other U.S. Cities

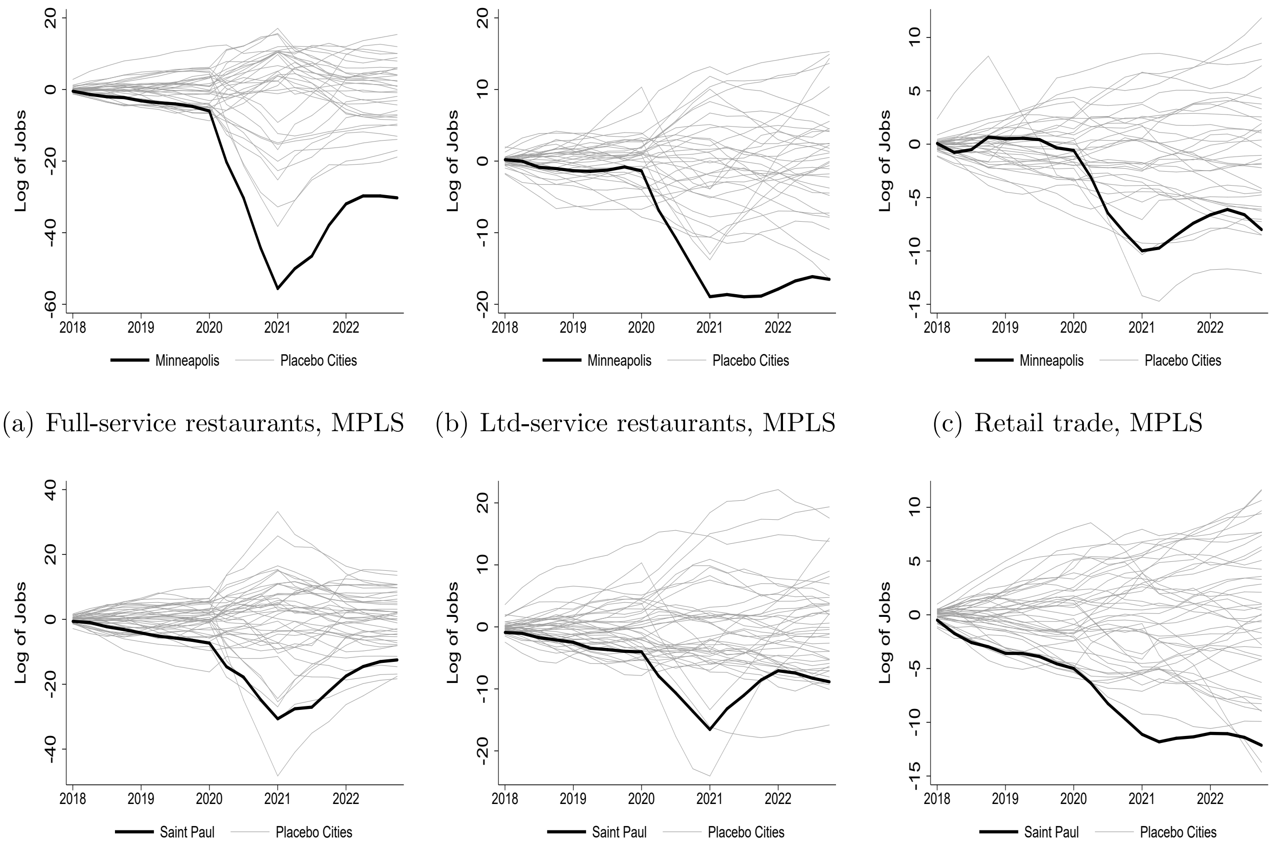
While some of our estimated negative jobs effects following the minimum wage increase in the Twin Cities become apparent by the end of 2019, the largest yearly decline in jobs for full-service restaurants is observed during 2020, the year when the pandemic recession began. By design, the synthetic control aims to fit pre-treatment series of Minneapolis and Saint Paul in both expansions and downturns. However, we acknowledge that the pandemic recession is quite atypical relative to other downturns observed in our sample. A potential threat to identification would arise if in 2020 the sensitivity to aggregate shocks changed for the control group relative to that of Minneapolis and Saint Paul. For example, it may be that the enforcement and economic impact of lockdowns was larger in more densely populated cities than in smaller cities. Additionally, the Twin Cities may have experienced idiosyncratic shocks, such as civil unrest, that is not being differenced out in the post-treatment period.

To address these concerns, we now use variation from other U.S. cities of similar size to Minneapolis and Saint Paul. Using these cities allows us to control for nationwide changes in economic conditions that were also prevalent in larger cities. Examples of such changes are the substitution of services prone to virus transmission with online shopping, the rise of gig work, and labor shortages in low-wage industries. Additionally, using data on the intensity of pandemic lockdowns and civil unrest, we directly adjust our estimates for pandemic and civil unrest conditions that may have affected the Twin Cities differentially from other cities.

For our analyses using other U.S. cities of similar size, we use publicly available data from the Quarterly Census of Employment and Wages (QCEW) produced by the U.S. Bureau of Labor Statistics.[19] The measure of employment refers to the number of workers who worked during or received pay for a pay period that includes the 12th of the month, as reported by establishments covered under the unemployment insurance program.

We note three differences between the research design using the QCEW data and that of our previous analyses using the DEED data. First, the QCEW does not have a measure of hours, and the wage measure differs from that in the DEED. Thus, we analyze only jobs and

[19]Before the minimum wage increases, Minneapolis employment is roughly 280,000 and Saint Paul employment is roughly 150,000. We include in the control group only cities without a minimum wage change between 2017 and 2019 and employment between half and double of that of either Minneapolis or Saint Paul. This restriction results in a sample of 33 cities for the Minneapolis control group and of 42 cities for the Saint Paul control group. Online Table A.7 shows the U.S. cities included in the control groups. We have also examined results without the size restriction and find similar results when all U.S. cities are allowed to be included in the synthetic control. The data collection process we followed to construct our control group, before the size restriction is applied, is to include municipalities or local government units for which data could be compiled from the publicly available files. This was possible in the following circumstances: 1) the city consists of two or more counties; 2) the city is coterminous with a county or is governed by a consolidated city-county government; 3) the city is independent; 4) the local minimum wage policy is enacted or harmonized at the county level. To further expand our control group, we also include cities that are the county seat and whose population accounted for more than 75 percent of their county's population. In these circumstances, we use the county as a reliable proxy for the corresponding city.



(a) Full-service restaurants, MPLS (b) Ltd-service restaurants, MPLS (c) Retail trade, MPLS

(d) Full-service restaurants, SP (e) Ltd-service restaurants, SP (f) Retail trade, SP

Figure 6: Time-Varying Jobs Effects, Cities with Comparable Employment

not hours or wages. Second, for the QCEW data, we extend the sample by one more year to 2022(4). Finally, the unit of analysis in the QCEW data is other U.S. cities of similar size to Minneapolis and Saint Paul, whereas in the DEED data we used zip code within a city as our unit of analysis.

Figure 6 presents our estimates from the QCEW until 2022(4) for the restaurants and retail industries, using only the unit weights ω_i to construct the synthetic control.[20] The estimates from the QCEW tend to be less precise than those from the DEED, which is not surprising given that the QCEW sample includes a smaller number of control cities and we have only one treated unit. Despite the differences in the size of the sample and the research design,

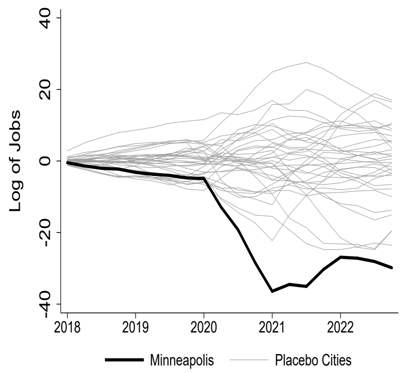
[20]In Figure 6, we present results for the three industries for which we previously documented jobs declines in the DEED data. For all other low-wage industries we find statistically insignificant jobs effects, with the exception of health care and social assistance in Minneapolis in which jobs begin to decline in in 2022. Figure A.17 shows the stability of our results when adding time weights λ_t to the specification underlying the analysis of Figure 6.

we continue to estimate negative jobs effects. The jobs declines in the sample of other U.S. cities using the QCEW are generally smaller than the declines we previously documented in the sample of Minnesota cities using the DEED. For example, we estimate jobs declines of roughly 30 log points for full-service restaurants in the QCEW as opposed to 40 log points in the DEED. For limited-service restaurants, the declines are roughly 15 log points in the QCEW as opposed to 45 log points in the DEED. For retail, the declines are roughly 10 log points in the QCEW as opposed to 30 log points in the DEED. We find these differences intuitive, because we expect that using variation from other U.S. cities of similar size to Minneapolis and Saint Paul allows to difference out other factors affecting jobs that are contemporaneous to the minimum wage change.

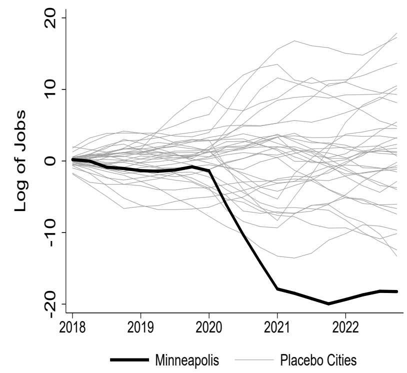
To examine more formally by how much these other factors affect our estimates, we now extend our methodology to directly adjust our estimates for pandemic and civil unrest conditions in the sample of other U.S. cities. We use four indicators of pandemic and civil unrest conditions. The first two come from Chetty, Friedman, Stepner, and The Opportunity Insights Team (2023) who develop a database tracking economic activity in the United States at a granular level. From this database, we use Google's COVID-19 Community Mobility Reports to measure mobility in retail and recreation and in workplaces. These two mobility indicators likely capture both pandemic and civil unrest conditions. The next two indicators capture only civil unrest conditions and come from the Armed Conflict Location & Event Data Project that collects information on the dates, actors, locations, and types of all reported protest events across U.S. cities. We use violent protests and total protests where Black Lives Matter was listed an affiliated actor because we wish to adjust for civil unrest conditions similar to those in the Twin Cities during 2020.

Our methodology of adjusting for pandemic and civil unrest conditions proceeds in three steps. We denote by Z_{it} the variables we wish to adjust for, where depending on the application Z_{it} denotes either changes in retail and recreation mobility, or changes in workplace mobility, or violent protests, or total protests. The first step is to project outcomes Y_{it} on Z_{it} in the sample of non-treated units, $i = 1, ..., N_{\text{co}}$, during the post-pandemic period, $t = 2020, ...$ From this step, we obtain the effect of Z_{it} on Y_{it} which we denote by $\hat{\beta}_Z$.[21] The second step is to residualize

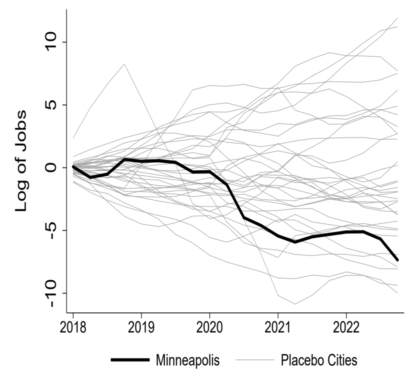
[21] We perform these projections separately for each industry and separately for the control cities of Minneapolis and of Saint Paul. For violent and total protests, we use only 2020 data since most civil unrest took place during this year. For changes in retail and recreation mobility and workplace mobility we pool 2020, 2021, and 2022



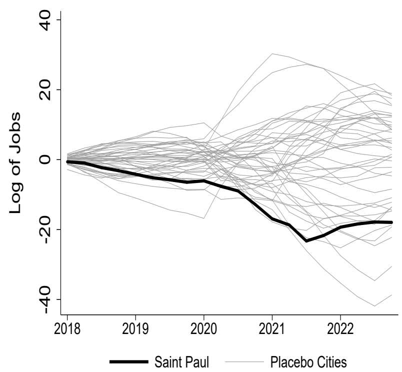
(a) Full-service restaurants, MPLS



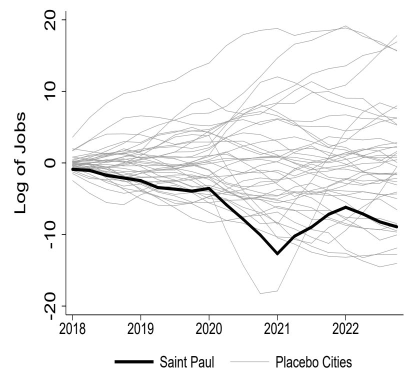
(b) Ltd-service restaurants, MPLS



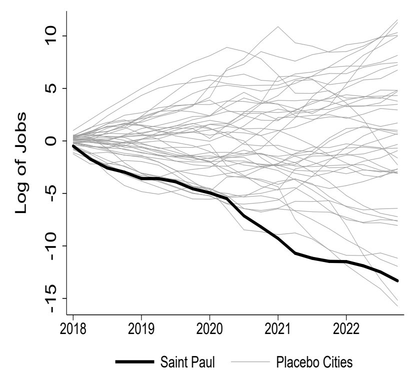
(c) Retail trade, MPLS



(d) Full-service restaurants, SP



(e) Ltd-service restaurants, SP



(f) Retail trade, SP

Figure 7: Time-Varying Jobs Effects, Adjusted for Retail and Recreation Mobility

outcome variables for both treated and non-treated units using the cross-sectional estimate of the effect of Z_{it} on Y_{it}. From this step, we obtain the residualized outcome $\hat{Y}_{it} = Y_{it} - \hat{\beta}_Z Z_{it}$. The final step is to repeat our synthetic difference-in-differences methodology using the residualized outcomes $\hat{Y}_{it}$. Thus, our methodology allows us to examine the sensitivity of our estimates in the case that jobs for all cities, including the Twin Cities, are adjusted for pandemic and civil unrest conditions as predicted by the cross-sectional relationship between jobs and these conditions in the sample of non-treated units during the period with pandemic and civil unrest.

Figure 7 presents our estimates in the sample of other U.S. cities when adjusting jobs for the effects of changes in retail and recreation mobility. The only noticeable difference between the unadjusted estimates shown previously in Figure 6 and the adjusted estimates in Figure 7 is that the jobs decline for full-service restaurants in Minneapolis in 2020 is roughly one-third smaller in the adjusted than in the unadjusted estimates. However, the adjusted estimates by the end

data, but using only 2020 data to estimate $\hat{\beta}_Z$ does not alter significantly our results.

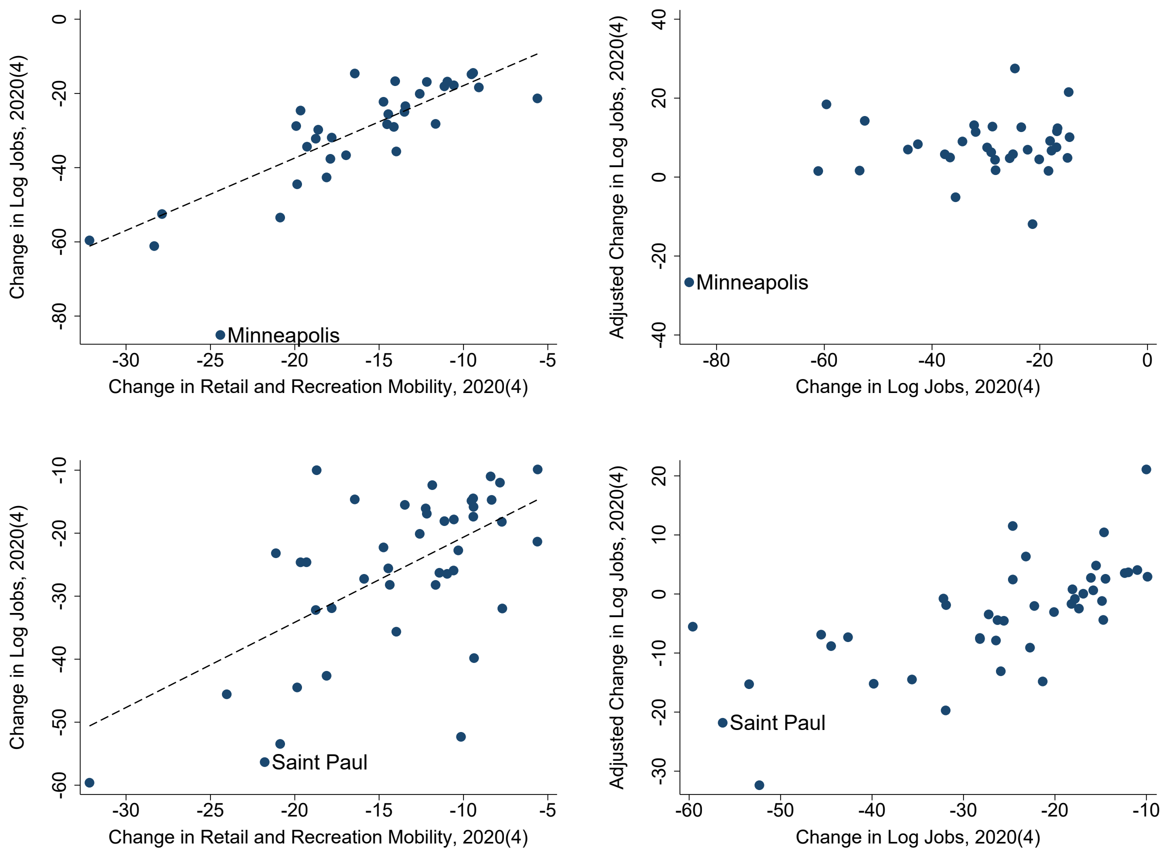


Figure 8: Adjustments for Retail and Recreation Mobility

of 2021 and throughout 2022 are very similar to the unadjusted estimates, for all industries and cities including full-service restaurants. Appendix Figures A.18, A.19, and A.20 also show the stability of our results when adjusting our estimates for workplace mobility, violent protests, and total protests.

Why does adjusting for pandemic and civil unrest conditions makes little difference for the estimated effects of the minimum wage on jobs? While the Twin Cities are more exposed to pandemic restrictions and civil unrest than the synthetic control, their observed jobs declines are outliers relative to the jobs declines predicted by the cross-sectional relationship between jobs and pandemic restrictions and civil unrest in the sample of control cities. We illustrate this in Figure 8 that uses as an example the case of full-service restaurants in 2020. The left panels display the relationship between changes in log jobs and changes in retail and recreational mobility in the cross-section of U.S. cities. As the figures show, both Minneapolis and Saint Paul are outliers relative to the predicted relationship between these two variables. That is, the

Twin Cities experience larger declines in jobs relative to the declines we would expect based on their reduced mobility during the pandemic and civil unrest. The right panels formalize this by plotting that residualized jobs declines $\hat{Y}_{it}$ against the observed jobs declines Y_{it}. Even after adjusting for pandemic and civil unrest conditions, the jobs declines are among the largest in the cross-section of U.S. cities. Appendix Figures A.21, A.22, and A.23 illustrate that the same result applies when using the other three indicators to adjust for pandemic and civil unrest conditions.

4 Evidence from the Cross Section

The adjustments in the time series methodology aim to difference out pandemic and civil unrest conditions that may have impacted Twin Cities differently from other cities. However, one might still be concerned that Twin Cities are somehow special and either experienced other idiosyncratic shocks or experienced a larger loading of pandemic and civil unrest conditions on their jobs than other cities. Estimating the labor market effects of the minimum wage increase using variation from the cross sections of establishments and workers does not suffer from these concerns, to the extent that the Twin Cities shocks are differenced out across establishments and workers in the same industry and zip code and these establishments and workers have similar responses to the pandemic or civil unrest. We present evidence of the stability of these responses before and after 2020, which is consistent with this premise.

4.1 Econometric Methodology: Cross Section

Our starting point is the statistical model

$$Y_{jszt} = \gamma_{szt} + \sum_{t=2018}^{2020} \tau_t \left(\text{GAP}_{jszt-3} \cdot d_t\right) + u_{jszt}, \tag{5}$$

where Y_{jszt} denotes an outcome for establishment j in industry s, zip code z, and period t. The outcome variables are the arc percent change of y_{jszt} over three years

$$Y_{jszt} = \frac{y_{jszt} - y_{jszt-3}}{(1/2)(y_{jszt} + y_{jszt-3})},$$

where y_{jszt} is the level of the wage, jobs, hours, and worker earnings for an establishment. We adopt the arc percent change transformation of growth rates to capture potential changes in the propensity of establishments to exit in response to the minimum wage increase. The lowest value of Y_{jszt} is -2, which we obtain for jobs, hours, and earnings when an establishment exists in period $t-3$ and exits in period t. The establishments we include in this regression are located only within Minneapolis or Saint Paul and have to exist in the sample in period $t-3$.[22]

In regression (5), the fixed effect γ_{szt} absorbs the common growth in period t of all establishments that belong to the same industry s and zip code z of the Twin Cities. For example, among other things, the fixed effect could capture the common effect for each industry within zip code arising from the pandemic recession or civil unrest in 2020.

The key variable of interest in regression (5) is the gap in labor costs over three years

$$\text{GAP}_{jszt} = \frac{\sum_{i \in j} \max(15/(1+\pi_{t,2017}) - w_{ijszt}, 0) h_{ijszt}}{\sum_{i \in j} w_{ijszt} h_{ijszt}}. \tag{6}$$

The numerator of the GAP variable is the additional costs incurred by establishment j when its workers i earn wages that are below the projected level of the minimum wage. The denominator of the GAP variable denotes the wage bill of the establishment. Therefore, the GAP variable captures the exposure of an establishment to the minimum wage increase, where exposure is expressed as the fraction of the wage bill accruing to additional labor costs.[23] In equation (6), we adjust the projected level of the minimum wage in each period with the metro-level CPI deflator $\pi_{t,2017}$, where $\pi_{2017,2017} = 1$. As an example, if an establishment pays all of its workers above 15 dollars per hour in 2017, its GAP measure equals zero.

One might be tempted to interpret the coefficients τ_{2018}, τ_{2019}, and τ_{2020} as the difference in establishment outcomes arising from differences in their exposure to the minimum wage increase in 2018, 2019, and 2020, after differencing out any common time effect that these establishments share with other establishments in the same zip code and industry. These coefficients, however, do not only capture differences in exposure to the minimum wage increase because typical establishment dynamics unrelated to exposure introduce a spurious correlation between exposure and various outcomes. Smaller establishments pay lower wages and thus have larger gaps. At the same time, smaller establishments tend to exit at faster rate, which may generate a negative τ_t for jobs, hours, and earnings. The wage regressions include only

[22]Different from our analysis from the time series that focuses on industries with a high share of affected workers, here we include all industries in our sample. This is appropriate because, even within industries that are relatively less exposed to the minimum wage, there exist establishments and workers with high exposure to the minimum wage.

[23]Previous studies that also used the GAP measure of exposure to the minimum wage include Card and Krueger (1994), Draca, Machin, and Van Reenen (2011), Harasztosi and Lindner (2019), and Dustmann, Lindner, Schonberg, Umkehrer, and vom Berge (2022).

establishments that exist in both period t and period $t-3$. We expect smaller establishments that survived to experience higher wage growth, which may generate a positive τ_t for the wage.

To address this concern, we augment our regression to include three more years before the minimum wage increase. The final specification is

$$Y_{jszt} = \gamma_{szt} + \sum_{t=2018}^{2020} \tau_t \left(\text{GAP}_{jszt-3} \cdot d_t\right) + \tau_0 \text{GAP}_{jszt-3} + u_{jszt}, \tag{7}$$

where τ_0 controls for any correlation between GAP and outcomes due to typical establishment dynamics unrelated to the minimum wage increase. The identifying assumption is that, conditional on any typical establishment dynamics and any determinant of outcomes that is common within industry, zip code, and quarter, other determinants of outcomes u_{jszt} are orthogonal to the GAP measure of exposure from three years before. Using this specification, we now interpret the coefficients τ_{2018}, τ_{2019}, and τ_{2020} as the difference in establishment outcomes due to differential exposure to the minimum wage increase.[24]

4.2 Evidence from the Cross Section of Establishments

The upper panel of Table 2 presents estimates of the coefficients τ_t from specification (7) applied to the wage, jobs, hours, and earnings, separately by each city. The entries are multiplied by 100 and are interpreted as the percent change in establishments' outcomes when the GAP changes from 0, which is the value for an establishment that is not exposed to the minimum wage, to 1, which is the value for an establishment that experiences 100 percent increase in its wage bill due to the minimum wage.[25] The maximum GAP is around 100 percent and the average GAP is around 6 percent. We will later use these moments of the GAP to translate the coefficient estimates from the cross section into most extreme and average labor market effects arising from the minimum wage increase. Entries in parentheses are p-values in percent associated with each coefficient. We cluster standard errors at the establishment level.

Beginning with the wage effects, we estimate wage growth between 12 and 15 percent in Minneapolis establishments. We fail to detect statistically significant wage increases in Saint

[24] We run regression (7) with quarterly data but estimate one coefficient common to all quarters within a year. To improve the readability, we have suppressed the notation of the quarters from regression (7).

[25] Our sample includes many establishments with a zero GAP. The average outcome of these establishments estimates the constant γ_{szt}. We believe it is appropriate to include non-exposed establishments in the regression, because they are a valid control group for exposed establishments within a zip code and industry. To examine how sensitive our results are to the linear specification adopted in regression (7), we have repeated our regressions by excluding establishments with a zero GAP. We find no significant differences in our results.

Table 2: Labor Market Effects of Minimum Wage Increases: Cross Section of Establishments

	Minneapolis				Saint Paul			
Baseline	Wage	Jobs	Hours	Earnings	Wage	Jobs	Hours	Earnings
2018	11.5	−11.1	−12.8	−8.0	4.9	−11.8	−12.9	−13.4
	(0.0)	(1.2)	(0.4)	(12.5)	(15.1)	(6.0)	(4.3)	(6.0)
2019	13.5	−15.8	−16.3	−11.6	6.0	−24.1	−22.4	−24.9
	(0.0)	(0.4)	(0.4)	(7.5)	(18.7)	(0.1)	(0.4)	(0.4)
2020	15.1	−14.6	−13.3	−13.1	4.9	−24.7	−24.1	−23.6
	(0.0)	(1.3)	(2.4)	(5.7)	(30.3)	(0.1)	(0.2)	(0.7)
2021	14.7	−14.6	−15.8	−16.1	10.5	−13.2	−14.8	−14.8
	(0.0)	(1.9)	(1.2)	(2.7)	(3.1)	(9.5)	(7.1)	(11.5)
Add Lagged Growth	Wage	Jobs	Hours	Earnings	Wage	Jobs	Hours	Earnings
2018	7.5	−10.8	−12.7	−8.1	5.6	−10.9	−12.4	−12.8
	(1.1)	(1.4)	(0.5)	(11.3)	(13.4)	(8.3)	(5.2)	(7.1)
2019	8.4	−15.3	−16.1	−11.5	3.4	−23.5	−22.0	−23.3
	(2.0)	(0.6)	(0.4)	(7.1)	(52.3)	(0.2)	(0.4)	(0.7)
2020	6.0	−14.4	−13.3	−13.6	2.5	−24.9	−24.1	−23.1
	(11.7)	(1.5)	(2.5)	(4.6)	(65.5)	(0.1)	(0.2)	(0.9)
2021	8.5	−14.7	−15.8	−15.6	10.8	−13.2	−14.5	−15.7
	(4.8)	(1.8)	(1.2)	(3.1)	(7.0)	(9.4)	(7.7)	(9.1)
Pre-sample to Six Years	Wage	Jobs	Hours	Earnings	Wage	Jobs	Hours	Earnings
2018	9.9	−9.1	−10.9	−8.2	4.5	−11.7	−13.2	−12.3
	(0.1)	(4.9)	(2.1)	(13.0)	(22.9)	(6.4)	(4.1)	(9.3)
2019	11.9	−13.8	−14.4	−11.8	5.5	−24.1	−22.7	−23.8
	(0.0)	(1.2)	(1.0)	(6.4)	(23.2)	(0.1)	(0.2)	(0.4)
2020	13.6	−12.6	−11.4	−13.3	4.4	−24.6	−24.5	−22.4
	(0.0)	(2.7)	(4.4)	(4.4)	(34.5)	(0.1)	(0.1)	(0.7)
2021	13.1	−12.7	−14.0	−16.3	10.0	−13.1	−15.1	−13.6
	(0.0)	(3.5)	(2.3)	(2.0)	(3.8)	(8.2)	(5.5)	(12.9)

Notes: The estimates are in percent, multiplied by 100. Entries in parentheses are p-values in percent using standard errors clustered at the establishment level.

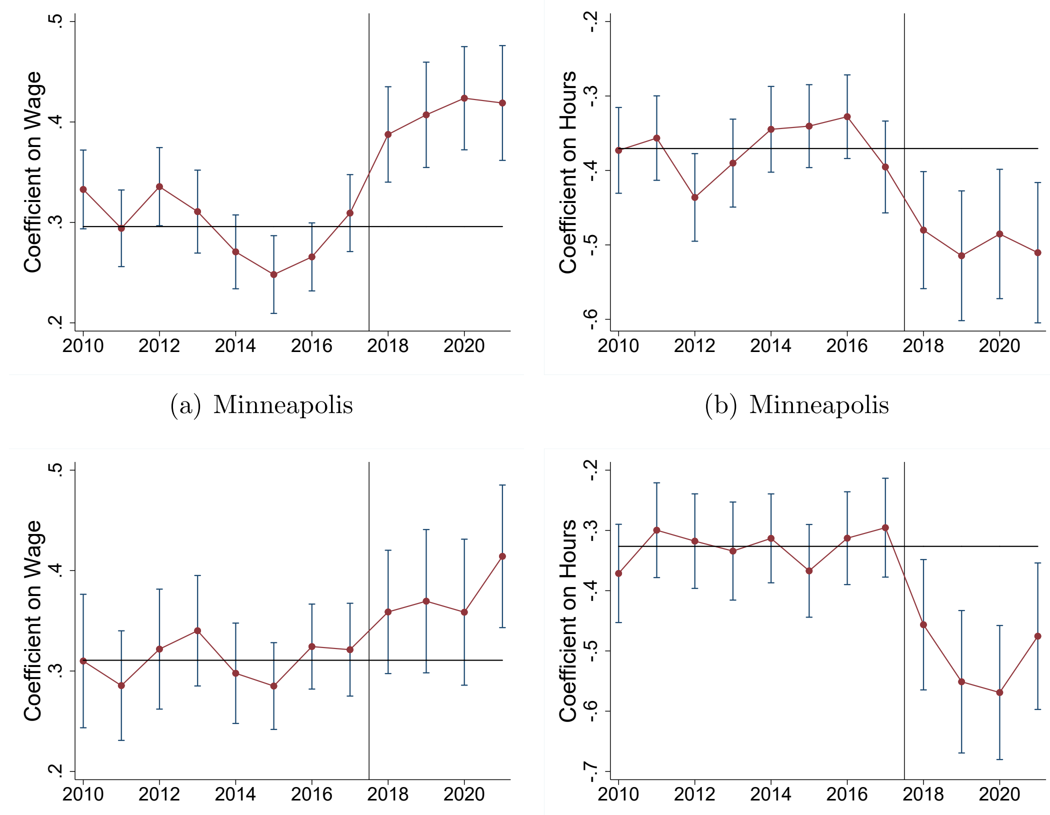
Paul establishments until 2021. This result echoes the result using time series variation, which showed stronger wage responses in Minneapolis implementing a minimum wage ordinance in 2018 than in Saint Paul passing a minimum wage ordinance in 2018 and implementing it in 2020.

Turning to the employment responses, we estimate declines of jobs and hours that range between 11 and 16 percent in Minneapolis establishments. We estimate even larger employment declines in Saint Paul establishments in 2019 and 2020. The employment declines in Saint Paul establishments, without a corresponding increase in the wage, are consistent with advance notice from the increase in the minimum wage, which shows up in quantities rather than prices. Saint Paul responses in 2021, the first year after the implementation of the minimum wage ordinance, resemble the Minneapolis responses in 2019. Finally, in both cities, we estimate negative relationships between exposure to the minimum wage and earnings of workers at the establishment level.

A reasonable concern about our cross-sectional results in 2020 is whether our strategy identifies establishments' sensitivity to the minimum wage or whether it identifies the sensitivity of smaller establishments with larger GAP exposure to the pandemic recession or civil unrest. However, the estimated coefficients on all variables for Minneapolis are quite stable over time. We find the stability of the estimated coefficients reassuring and conclude that our identification strategy from the cross section of establishments isolates their differential exposure to the minimum wage rather than a heterogeneous effect of the pandemic or civil unrest on establishments.

The middle panel of Table 2 presents estimated coefficients τ_t in a specification in which we add lags of the dependent variable into the regression. Our estimated coefficients do not change much, with the exception of the wage effects in Minneapolis establishments, which decrease between 4 and 9 percentage points. In the lower panel, we include six years of data before the minimum wage increase, as opposed to the three year period in the baseline specification. Our logic for including three years of data in the baseline specification is that the outcome variables are defined as changes over three years and that we wish to control for typical establishment dynamics during a period close to the minimum wage increase. However, our results are not sensitive to expanding the sample to include the last six years before the minimum wage increase.

The responses of the wage, jobs, hours, and earnings are above and beyond those generated by typical establishment dynamics because regression (7) includes the GAP measure in the period before the minimum wage increase. However, it could still be the case that there is a trend in establishment dynamics that increases over time these coefficients in absolute value, irrespective of the minimum wage policy change. To examine this possibility, we allow the coefficients in regression (7) to vary over time for all periods. Figure 9 shows that the largest absolute values of the coefficients for wages and hours in both cities are estimated during the



(a) Minneapolis

(b) Minneapolis

(c) Saint Paul

(d) Saint Paul

Figure 9: Cross-Sectional Responses Over Time

Notes: The figure shows estimates for τ_t from regressions $Y_{jszt} = \gamma_{szt} + \sum_{t=2012}^{2020} \tau_t \left(\mathrm{GAP}_{jszt-3} \cdot d_t\right) + u_{jszt}$, together with 95 percent confident intervals. The horizontal line represents the average of the estimated τ_t between 2012 and 2017.

minimum wage increase and, with the exception of wages in Saint Paul, that these coefficients are statistically different from the average coefficient before the minimum wage increase in 2018, indicated by the horizontal line. It also shows that there is no noticeable trend in these estimated coefficients before the minimum wage increase.

4.3 Evidence from the Cross Section of Workers

A challenge in interpreting the results that use variation from the cross section of establishments is that there may be spillovers from high to low GAP establishments. These spillovers may be important, given that we use within-zip-code and within-industry variation in establishments' outcomes. As an example, if workers reallocated from high to low GAP establishments, then we would be double-counting the effects of the minimum wage increase on establishments' employment.[26] Another challenge arises from reallocation outside of the Twin Cities, because our estimates could reveal negative employment effects from the minimum wage even if all affected workers find jobs outside of the Twin Cities. Thus, while our estimates directly speak to the outcomes of establishments that were located in the Twin Cities before the minimum wage increase, they may not be informative about workers' labor market outcomes. To address these challenges, we now turn to specifications from the cross section of workers.

In this section, we use variation in wage gaps across workers and track workers' outcome directly over time, irrespective of whether workers reallocated to other establishments in or outside of the Twin Cities. Our specification is

$$Y_{it} = \sum_{s} \gamma_{st} X_{ist} + \sum_{t=2018}^{2020} \tau_t \left(\overline{\text{GAP}}_{it-3} \cdot d_t \right) + \tau_0 \overline{\text{GAP}}_{it-3} + \rho Y_{it-1} + u_{it}, \tag{8}$$

where the wage gap over three years is

$$\overline{\text{GAP}}_{it} = \frac{1}{\# J_t(i)} \sum_{j \in J_t(i)} \text{GAP}_{jt}, \tag{9}$$

$\# J_t(i)$ denotes the number of establishments that worker i worked in during period t, and GAP_{jt} is establishment's j gap in labor costs defined in equation (6). Thus, we treat workers with their establishments' gaps to capture their exposure to the minimum wage. This specification allows us to track workers' outcomes over time, as workers reallocate to other establishments both in and outside of the Twin Cities. However, the treatment is defined at the establishment level, thus alleviating the concern that low-wage workers' difficulty finding jobs in 2020 is because of the pandemic or civil unrest.

Our specification is again agnostic about the intercept γ_{st}, which absorbs all time effects common to workers belonging to industry s. Workers may work in more than one industry in a year, so the variable X_{ist} denotes the share of worker i's employment in industry s. We interpret the τ coefficients as the percent change in worker outcomes resulting from a higher exposure to the minimum wage for workers with the same growth rate Y_{it-1} in the period preceding the policy change and after differencing out the common effect that workers in the same industry experience, γ_{st}, and any effects we would detect due to typical worker dynamics, captured by the coefficient τ_0.

[26] Dustmann, Lindner, Schonberg, Umkehrer, and vom Berge (2022) document the reallocation of low-wage workers from smaller, lower-paying, and less productive establishments to larger, higher-paying, and more productive establishments following the introduction of a national minimum wage in Germany in 2015.

Table 3: Labor Market Effects of Minimum Wage Increases: Cross Section of Workers

	Minneapolis			Saint Paul		
	Wage	Hours	Earnings	Wage	Hours	Earnings
2018	−4.0	−1.2	−6.1	−3.4	−10.4	−10.2
	(0.1)	(67.1)	(3.3)	(0.9)	(0.3)	(0.5)
2019	5.7	−14.7	−12.1	−2.8	−15.7	−14.0
	(0.0)	(0.0)	(0.0)	(7.5)	(0.0)	(0.0)
2020	4.9	−12.7	−9.5	1.4	−11.9	−5.8
	(0.1)	(0.0)	(0.4)	(39.4)	(0.2)	(14.2)
2021	13.6	−14.1	−8.1	6.5	−13.1	−9.9
	(0.0)	(0.0)	(1.8)	(0.0)	(0.1)	(1.4)

Notes: The estimates are in percent, multiplied by 100. Entries in parentheses are p-values in percent using standard errors clustered at the worker level.

Table 3 presents the estimated coefficients from specification (8). The estimated coefficients on hours are generally similar to those from the cross section of establishments and, with the exception of Minneapolis in 2018, are stable over time. We detect statistically significant declines in worker earnings in all years, except for Saint Paul in 2020. A difference relative to the establishment results is that here we estimate smaller wage gains until 2020 for Minneapolis. However, the wage gains become more similar to those from the cross section of establishment in 2021.

5 Summary of Estimates

In this section, we compare our estimates from the time series with those from the cross section. We then compare our estimates of employment losses with previous estimates found in the minimum wage literature. We conclude by comparing our estimates of the elasticity of labor demand at the establishment level with other estimates in the literature.

5.1 Comparison of the Time Series with the Cross Section

Table 4 summarizes our estimates using variation from the time series of cities and the cross sections of establishments and workers. In the first row, we present the average jobs losses in Minneapolis in 2021. The time series estimate of the jobs losses is 2.8 percent. We calculate this number as the average jobs losses across all two-digit industries, where losses are weighted

Table 4: Jobs Effects of Minimum Wage Increases: Summary of Estimates

Jobs (2021, percent)	Time Series	Cross Section	Ratio
Minneapolis Average	−2.8	−0.7	0.25
Minneapolis Most Negative	−31.8	−14.3	0.45
Saint Paul Average	−3.5	−1.3	0.37
Saint Paul Most Negative	−22.4	−18.7	0.83
Average			**0.48**

Notes: Average from the time series includes only two-digit industries with statistically significant changes in jobs. Most Negative from the time series uses the estimates for the restaurant industries. The estimates for the cross section multiplies each year's jobs coefficient from the establishments' regressions with the weighted average and maximum GAP and then averages across years. The ratio of 0.48 is the average across the four ratios.

with the employment of the corresponding industry in total Minneapolis employment before the minimum wage increase. We include only industries with statistically significant changes in jobs and average our estimates between the DEED and the QCEW data sources. The estimate of jobs losses using variation from the cross section is 0.7 percent. We calculate this number by multiplying each year's jobs coefficient from the establishments' regressions with the weighted average GAP and then averaging across years. Similar calculations in Saint Paul lead to estimated jobs losses of 3.5 percent from the time series and 1.3 percent from the cross section.

The second and fourth rows summarize our most negative jobs estimates. For the time series, we use the estimates for restaurants and conclude that the most negative jobs effects are 32 percent in Minneapolis and 22 percent in Saint Paul. For the cross section, we multiply each year's jobs coefficient from the establishments' regressions with the maximum GAP and then average across years. We use the maximum GAP so that we can get a comparable estimate of the most negative jobs effects. This yields estimated jobs losses of 14 percent in Minneapolis and 19 percent in Saint Paul.

The last column of Table 4 shows the ratio of estimates from the cross section and the time series. This ratio ranges between 0.3 and 0.8, with an average value of 0.48. In Section 6, we discuss reasons why the time series estimates of jobs losses differ from estimates that use the cross section and argue that they plausibly reflect upper and lower bounds.

5.2 Comparison of Minimum Wage Estimates with Other Studies

In this section, we compare our estimates to estimates of other papers that use data from the U.S. restaurant industry. To compare our findings with those of the literature, we find it useful to transform the estimated employment effects from the time series into elasticities. The elasticity of an outcome in period T with respect to the change in the minimum wage is

$$\varepsilon_T = \frac{\exp\left((T - T_{\text{pre}})\hat{\tau}/4\right) - 1}{w^1_{\min,T}/w^0_{\min,T} - 1}, \tag{10}$$

where $w^1_{\min,T}$ is the minimum wage in effect at time T for the treated units and $w^0_{\min,T}$ is the minimum wage in effect at time T for the untreated units. Our time series elasticity of employment with respect to the minimum wage is -0.78. This estimate averages the elasticities for Minneapolis and Saint Paul, where both elasticities in turn average the 2021(4) estimates from the DEED and the QCEW. For the elasticity using variation from the cross section, we multiply the elasticity from the time series with 0.48, which Table 4 shows to be the average ratio of estimates from the cross section to those from the time series. This yields an elasticity of employment with respect to the minimum wage of -0.37.

Figure 10 presents our estimated elasticity of restaurant employment with respect to the minimum wage and elasticities found in other studies.[27] As seen in the figure, our employment elasticities are more negative than those found in the literature, which average around -0.1. Our analysis for the estimates in the restaurant industry from other studies corroborates the more comprehensive analysis of estimates for all low-wage workers and low-wage industries in Neumark and Shirley (2021), which reveals that while almost 80 percent of estimates are negative, reported elasticities average -0.15 across studies.

There are several reasons why our employment elasticities with respect to the minimum wage might be larger than those found in the literature. First, our policy change is at the local level. Product substitution elasticities are larger at the local level than the state or national level. For example, consumers might be substituting more expensive Twin Cities restaurants or shops with cheaper establishments in surrounding suburbs. But we would not expect consumers

[27] We include papers published after the study of Card and Krueger (1994) and for which we could obtain an estimated elasticity with respect to the minimum wage for the U.S. restaurant industry. Our estimate is the jobs elasticity, because the analysis using other U.S. cities from the QCEW has only jobs and not hours. However, we showed that in most cases the results using hours are similar to those using jobs. Other papers mainly report jobs elasticities, but we also include few papers with hours elasticities such as Michl (2000) and Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething (2022).

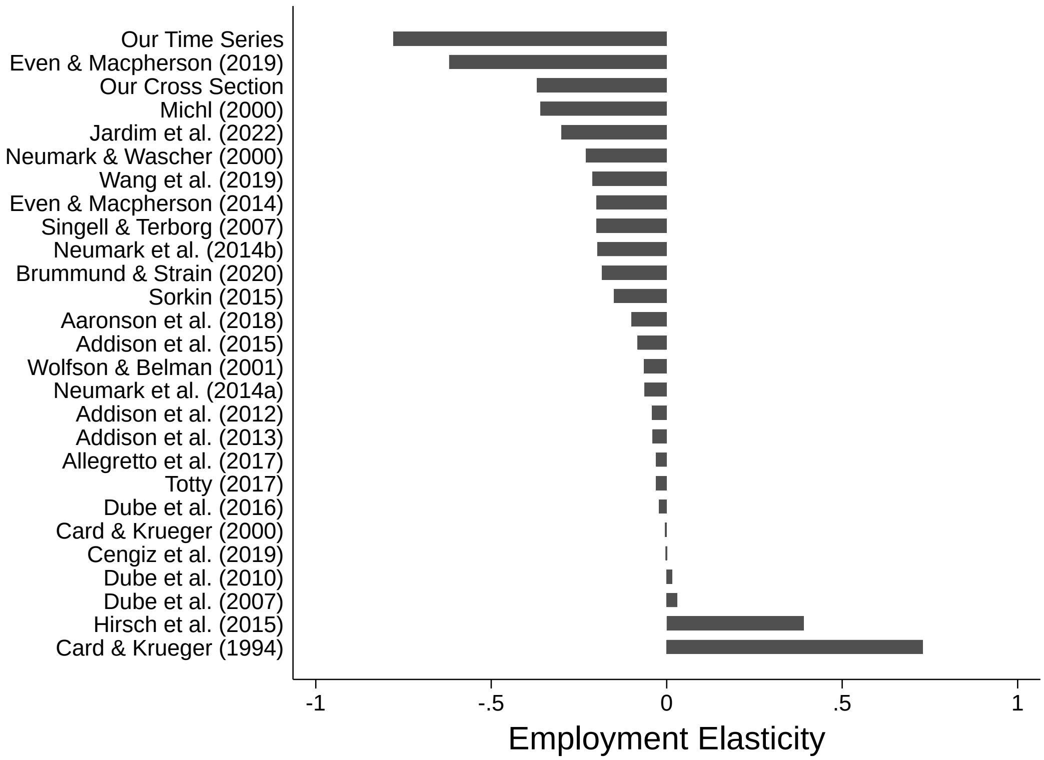


Figure 10: Comparison of Restaurant Employment Elasticity to the Literature

to substitute that much between Minnesota establishments and establishments in other states, had the minimum wage policy been enacted at the state level.

Additionally, the policy variation we examine is larger than the typical variation found in the literature. Clemens and Strain (2021) estimate an elasticity of -0.5 for low-skilled employment during "large minimum wage increases," and the 46 percent increase in the Minneapolis minimum wage by 2021 is larger than the large policy changes examined by these authors.[28] The Seattle study by Jardim, Long, Plotnick, Van Inwegen, Vigdor, and Wething (2022) examines a policy change as large as the one in the Twin Cities and estimates a -0.3 elasticity of employment with respect to the minimum wage for restaurants. The difference in their elasticity with the one we estimate for the Twin Cities could reflect that Seattle was booming during the implementation of its large minimum wage increase. It is conceivable that the sensitivity of employment with respect to the minimum wage becomes larger during recessions, like the one

[28]Appendix Figure A1 of Clemens and Strain (2021) shows that the 95 percent confidence interval of large minimum wage increases ranges between 20 and 25 percent after three years. The five-year range is 30 to 40 percent, whereas the Minneapolis minimum wage increases by more than 50 percent in 2023.

induced by the pandemic. The more negative elasticity during the pandemic that we estimate is consistent with previous studies documenting larger elasticities during recessions (Addison, Blackburn, and Cotti, 2013; Clemens and Wither, 2019).

5.3 Comparison of Labor Demand Elasticity with Other Studies

Dividing the cross-sectional coefficients for jobs or hours, τ_ℓ, in the upper panel of Table 2 with the cross-sectional coefficients for the wage, τ_w, we find a cross-sectional own-wage employment elasticity of around -1. This elasticity is estimated using employment and wage variation induced by heterogeneous exposure to the minimum wage increase. As shown in the table, this ratio is stable across time. In Section 6, we show more formally that this ratio is informative about the own-wage elasticity of labor demand at the establishment level, which we interpret as a long-run elasticity, as we allow establishments to substitute between labor and other inputs.

How does our estimate of a -1 labor demand elasticity at the establishment level compares with the estimates in the literature? Hamermesh (1993) summarizes various early estimates of labor demand elasticities. While many studies estimate elasticities below one in absolute value, some studies from British plants find an elasticity of around -1. Lichter, Peichl, and Siegloch (2015) update the analysis of Hamermesh (1993) with newer studies and find a mean own-wage elasticity of around -0.6, with roughly 80 percent of the estimates ranging between 0 and -1. However, as discussed by Hamermesh (1993) and Lichter, Peichl, and Siegloch (2015) several of the studies in the literature hold output constant and thus are more appropriately interpreted as estimating factor substitution rather than the total elasticity of labor demand. Beaudry, Green, and Sand (2018) estimate a labor demand elasticity of -1 at the city-industry level. Using a model with search frictions and scarcity in new firm creation, they estimate that the increase in the minimum wage observed in Seattle is associated with a 2.1 percent decline in employment. This estimate matches exactly our estimates in Table 4 for the Twin Cities minimum wage increase, which average 2.1 percent between cities and estimation methods.

6 Reconciling the Time Series with the Cross Section

There are three reasons why time series estimates of employment losses differ from those that use variation from the cross section. First, the Twin Cities may have experienced idiosyncratic shocks or had a differential response to an aggregate shock that cannot be differenced out using other cities during the post-treatment period. We attempted to address this concern by using additional variation from other U.S. cities and by adjusting our estimates to difference out pandemic and civil unrest conditions across cities. However, one might still be concerned that pandemic and civil unrest conditions affected Twin Cities jobs more than inferred using the cross-sectional coefficient of jobs with respect to pandemic and civil unrest conditions for non-treated cities. The cross-sectional estimates do not suffer from this concern, because the coefficients we estimate are stable before and after 2020.

Second, the time series effects of the minimum wage on employment are at the industry level and sum up employment effects at the intensive margin, effects arising from the exit of establishments, and effects arising from the lack of entry of new establishments. By design, the estimates from the cross section do not account for the effects of entry, because they use establishments and workers that exist for at least one period. Finally, any other equilibrium adjustment at the industry level affecting the average establishment or worker is included in the time series estimates but not in those from the cross section.[29]

We expect that the first two differences in research design generate more negative employment effects in the time series than in the cross section. We formalize this claim by developing a model of establishment dynamics. We use the model to answer two questions. First, is it plausible to reconcile the time series estimates with those from the cross section by appealing to entry dynamics that are omitted from the analysis of the cross section? Second, what do our estimates imply about the deeper determinants of labor demand, such as parameters that characterize the degree of product and labor market competition, factor substitution within establishments, and establishment dynamics?

6.1 Model Environment

Our model economy consists of establishments that are heterogeneous along four dimensions: productivity z, technology of production ϕ, labor supply to the establishment $\bar{w}$, and fixed cost of entry κ. Establishments choose prices, factors of production, entry, and exit. We consider two periods and denote with a subscript 0 the period before the minimum wage change and with a

[29]Examples of such equilibrium effects are wage spillovers to establishments not directly exposed to the minimum wage, a shift of product demand away from an industry, or a shift of labor supply toward an industry. We addressed the concern that non-exposed establishments changed their employment because of worker reallocation by using the cross section of workers to infer the effects of the minimum wage on employment.

subscript 1 the period after the minimum wage change. The model is partial equilibrium and the only market clearing condition is that an establishment's labor demand equals workers' labor supply at the establishment level, which, in turn, pins down the wage that each establishment faces.

Period before the minimum wage increase. Establishment $(z, \phi, \bar{w}, \kappa)$ solves the following problem.[30] Conditional on having entered, the establishment chooses price p_0, labor demand ℓ_0, and all other inputs m_0 (for example, materials and capital) to maximize flow profits

$$\max_{p_0,\ell_0,m_0} \pi_0 = \max\{p(y_0) \cdot y_0(m_0, \ell_0) - w(\ell_0)\ell_0 - m_0 - f, 0\}, \tag{11}$$

where the price of all other inputs is normalized to one. Establishments face operating fixed cost f that is constant over time and establishments. In equation (11), profits cannot be negative, because establishments choose to exit when total costs exceed revenues.

We allow establishments to potentially have market power in both product and labor markets. In the product market, establishments internalize that their product demand is downward sloping

$$p_0 = y_0^{-\frac{1}{\varepsilon}}, \tag{12}$$

where the elasticity of product demand $\varepsilon > 1$ is constant over time and across establishments. When $\varepsilon \to \infty$, we obtain the limiting case of perfect competition in the product market.[31] In the labor market, establishments internalize that their labor supply is upward sloping

$$w_0 = \bar{w}\ell_0^{\frac{1}{\theta}}, \tag{13}$$

where the labor supply elasticity $\theta > 0$ is also constant over time and across establishments. When $\theta \to \infty$, we obtain the limiting case of perfect competition in the labor market.[32]

[30] Establishment type $(z, \phi, \bar{w}, \kappa)$ is fixed over time, with the only exception of an aggregate shock that we consider later. The model can be extended to accommodate changes in establishment productivity and labor supply over time. If changes in establishment heterogeneity occur with a perceived probability of zero, then we can still focus on a sequence of static profit maximization problems conditional on entry, making entry the only dynamic choice.

[31] We have normalized the shifter of product demand to one for all establishments. Given this normalization, productivity z is better understood as a convolution of physical productivity and demand for an establishment's product. For brevity, we do not present the household side of the model, which gives rise to establishments' demand functions. Establishments face a downward sloping demand for their product when households view their products as imperfect substitutes, with ε governing the substitutability of product demand across different establishments.

[32] As we did for product demand, for brevity we do not present the worker side of the model. Establishments

Establishments operate a CES technology

$$y_0 = z \left(\phi m_0^{\frac{\sigma-1}{\sigma}} + (1-\phi) \ell_0^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}, \tag{14}$$

where $\phi \in [0,1]$ is a distribution parameter characterizing the technology of production and $\sigma \geq 0$ is the elasticity of substitution between all other inputs m and labor ℓ, which is constant over time and across establishments.

Establishments do not know part of their type, $(z, \phi, \bar{w})$, before they enter. As a result, the decision to enter compares expected profits to the fixed cost of entry. Establishments enter if

$$\mathbb{E}\pi_0 \geq \kappa. \tag{15}$$

Establishments have different entry outcomes because they differ in their cost of entry κ.

Period after the minimum wage increase. Fraction $\delta \in [0,1]$ of establishments are exogenously destroyed in the end of the first period after exit, pricing, and production decisions are made. A fraction $1-\delta$ of establishments continues in the next period. For a continuing establishment, the maximization problem is similar to that of the first period, with two differences. First, continuing establishments do not pay the entry cost κ, because they know their type. Second, establishments potentially face a binding minimum wage. The minimum wage increase is unexpected as of the first period, which justifies focusing on a sequence of static problems for establishments. For continuing establishments, profits equal

$$\max_{p_1, \ell_1, m_1} \pi_1^c = \max\{p(y_1) \cdot y_1(m_1, \ell_1) - \max\{w(\ell_1), w_{\min}\}\ell_1 - m_1 - f, 0\}, \tag{16}$$

where the max operator inside the brackets denotes that establishments cannot pay a wage that falls below the minimum wage. The establishments face the same functional forms for product demand in equation (12), labor supply in equation (13) if the minimum wage does not bind, and production function in equation (14).

There is a mass δM of potential entrants in the second period, where M is the mass of potential entrants in the first period. Potential entrants in the second period behave similarly to potential entrants in the first period. Conditional on having entered, they maximize

$$\max_{p_1, \ell_1, m_1} \pi_1^n = \max\{p(y_1) \cdot y_1(m_1, \ell_1) - \max\{w(\ell_1), w_{\min}\}\ell_1 - m_1 - f, 0\}, \tag{17}$$

face an upward sloping labor supply when workers view them as imperfect substitutes, with θ governing the substitutability of labor supply across different establishments and $\bar{w}$ capturing parameters related to the disutility of work across establishments.

where product demand is given by equation (12), labor supply by equation (13) if the minimum wage does not bind, and the production function by equation (14). Potential entrants in the second period enter if

$$\mathbb{E}\pi_1^n \geq \kappa, \tag{18}$$

where the expectation of profits uses the distribution of establishment characteristics of potential entrants, as opposed to the characteristics of continuing establishments.

6.2 Effects of the Minimum Wage on Establishment Labor Demand

Labor demand of operating establishments in the period before the minimum wage increase is

$$\ell_0 = (1-\phi)^{\varepsilon} z^{\varepsilon-1} \left(\phi \left(\frac{\phi \mu_w w_0}{1-\phi} \right)^{\sigma-1} + (1-\phi) \right)^{\frac{\varepsilon-\sigma}{\sigma-1}} (\mu_p \mu_w w_0)^{-\varepsilon}, \tag{19}$$

where $\mu_p = \frac{\varepsilon}{\varepsilon-1} \geq 1$ denotes the product market markup and $\mu_w = \frac{\theta+1}{\theta} \geq 1$ denotes the labor market markup. Both markups reduce labor demand for a given level of the wage.

As in Hamermesh (1993), we calculate the total elasticity of labor demand (in absolute value)

$$\eta_0 = (1-\alpha_0)\varepsilon + \alpha_0 \sigma, \tag{20}$$

which is a weighted average of the two elasticities ε and σ. The elasticity of product demand ε appears in the elasticity of labor demand because a higher wage increases the marginal cost and prices and thus lowers demand for an establishment's product. This is the scale effect that allows output y to adjust. The elasticity of factor substitution σ appears in the elasticity of labor demand because a higher wage induces establishments to lower labor and increase all other inputs along a stable isoquant. This is the substitution effect that holds output y constant.[33] Weights $\alpha_0 = \frac{\left(\frac{\phi}{1-\phi}\right)^{\sigma}(\mu_w w_0)^{\sigma-1}}{1+\left(\frac{\phi}{1-\phi}\right)^{\sigma}(\mu_w w_0)^{\sigma-1}}$ equal ϕ in the limiting case of a Cobb-Douglas production function with $\sigma = 1$. With an elasticity of substitution different than one, weights α_0 vary as a function of the wage. Weights α_0 do not equal the input share of variable costs unless there is a perfectly competitive labor market, $\mu_w = 1$.

[33] Our elasticity of labor demand is a long-run elasticity because we allow other inputs to adjust. If we keep other inputs m fixed at the level before the minimum wage increase, the absolute value of the labor demand elasticity equals $\eta^m = \frac{\varepsilon\sigma}{\sigma+\tilde{\alpha}(\varepsilon-\sigma)}$, where $\tilde{\alpha} = \phi m^{\frac{\sigma-1}{\sigma}} / \left(\phi m^{\frac{\sigma-1}{\sigma}} + (1-\phi)\ell^{\frac{\sigma-1}{\sigma}}\right)$. We show that $\eta > \eta^m$ in the limiting case $\tilde{\alpha} \to \alpha$, which one obtains when $\sigma = 1$ for example.

Table 5: Parameters and Data Moments

Parameter	Value	Data Moments	Value
Elasticity of Factor Substitution, σ	0.19	Cross-Sectional Elasticity, τ_ℓ/τ_w	-1.01
Mean Intensity of Other Inputs, μ_ϕ	0.52	Labor Share of Costs, Mean	0.30
Share of Affected Establishments, q	0.23	GAP, Employment-Weighted	0.14
Mean Entry Cost, μ_κ	0.06	Entry Cost to Profit, Median	0.36
Fixed Operating Cost, f	0.08	Operating Cost to Profit, Median	0.59

Notes: The first two columns present the value of calibrated parameters. The last two columns present the moments in the data most closely associated with the chosen value of each parameter.

Labor demand of operating establishments in the period after the introduction of the minimum wage is

$$\ell_1 = \begin{cases} (1-\phi)^\varepsilon z^{\varepsilon-1}\left(\phi\left(\frac{\phi\mu_w w_1}{1-\phi}\right)^{\sigma-1} + (1-\phi)\right)^{\frac{\varepsilon-\sigma}{\sigma-1}} (\mu_p\mu_w w_1)^{-\varepsilon}, & \text{if } w_1 > w_{\min}, \\ (1-\phi)^\varepsilon z^{\varepsilon-1}\left(\phi\left(\frac{\phi w_{\min}}{1-\phi}\right)^{\sigma-1} + (1-\phi)\right)^{\frac{\varepsilon-\sigma}{\sigma-1}} (\mu_p w_{\min})^{-\varepsilon}, & \text{if } w_1 \leq w_{\min}. \end{cases} \tag{21}$$

The effects of the minimum wage on establishments' labor demand can be understood by comparing labor demand before the minimum wage in equation (19) to labor demand with a binding minimum wage in the second line of equation (21). In the absence of market power in the labor market, $\mu_w = 1$, labor demand unambiguously falls for establishments for which the minimum wage $w_{\min}$ exceeds their competitive wage w_0. This is a movement along the labor demand curve, with the elasticity of labor demand η governing the magnitude of reduction in labor. With labor market power, $\mu_w > 1$, the employment effect of the minimum wage is theoretically ambiguous. A binding minimum wage removes labor market power because establishments perceive an infinite elasticity of labor supply for all labor units below the minimum wage. This effect represents an upward shift of labor demand. Depending on the values of $\mu_w w_0$ and $w_{\min}$, the minimum wage may result in an increase or decrease in equilibrium labor.

6.3 Effects of Minimum Wage on Aggregate Labor Demand

We now parameterize the model economy and evaluate how aggregate outcomes at the industry level change when the minimum wage is introduced.

Parameterization. We set the exogenous destruction rate to $\delta = 0.28$ to match the average exit rate of the Twin Cities restaurants in the DEED data during the years before the minimum wage increase. We assume that $(\log z, \phi, \log w, \kappa)$ are normally distributed and, as a baseline, uncorrelated with each other.[34] From the DEED data, we calculate that the standard deviation of the log wage across restaurants equals 0.46 and thus we set $\sigma_w = \sigma_z = 0.46$.[35] For the parameters characterizing market power, we begin our analysis with $\varepsilon = 2$ and $\theta \to \infty$. Our choice reflects two observations. First, under a higher product market elasticity ε, the model produces higher labor demand elasticities than those implied from the cross-sectional relationship between employment and the wage that we estimate in Section 4. Second, we can rationalize the negative employment effects from both the time series and the cross section as outcomes from a model with perfectly competitive labor markets. Given the data that we have access to, we are not able to identify θ from other parameters and thus we will examine the sensitivity of our results to this parameter.

Table 5 presents the remaining five parameters, which we calibrate so that our model perfectly matches five moments from the data. An elasticity of factor substitution $\sigma = 0.19$ allows the model to replicate the cross-sectional elasticity of employment with respect to the wage, $\tau_\ell/\tau_w = -1.01$, found by dividing the jobs column with the wage column of the upper panel of Table 2. The mean technology parameter is $\mu_\phi = 0.52$, so that the model generates a labor share of operating costs for restaurants equal to 0.30, as calculated by Aaronson and French (2007). Next, we calibrate the share of the establishments q for which the minimum wage exceeds their wage in the absence of a minimum wage. We find that the employment-weighted average GAP in our model equals its analog from the data for $q = 0.23$.[36] Finally, we calibrate the mean fixed cost of entry μ_κ and the fixed cost of operation f to match estimates of these costs for restaurants by Aguirregabiria and Mira (2007). The entry cost represents 36 percent of profits and 19 percent of revenues. The operating cost is larger than the entry cost, representing 59

[34]To simplify the exposition, we invert the maximization problem and represent establishments as directly drawing a wage w. We then find ℓ from the labor demand functions, equations (19) and (21), and use the labor supply function to solve for the primitive source of heterogeneity characterizing each establishment, $\bar{w} = w\ell^{-1/\theta}$. The only subtlety is how we treat aggregate productivity shocks that directly change the equilibrium wage. For that case, we have opted for adjusting labor supply parameters $\bar{w}$ so that the equilibrium wage changes only in response to the increase in the minimum wage, because our time series and cross-sectional evidence attempt to difference out aggregate shocks and therefore show an increase in the wage.

[35]We do not have information that allows us to estimate the standard deviation of technology, σ_ϕ, and entry cost, σ_κ. To be conservative, we have opted for a small dispersion of these sources of heterogeneity and set the two standard deviations to 10 percent of their means.

[36]We cannot directly target the increase in the minimum wage, because our model does not have a binding minimum wage in the first period. The minimum wage is 40 percent higher than the 5th percentile of the wage without the minimum wage, an increase which is consistent with the Minneapolis minimum wage implemented in 2020.

Table 6: Aggregate Effects of the Minimum Wage Increase in the Model

	τ_ℓ/τ_w	η	W_1/W_0-1	L_1/L_0-1	Ratio of Entry
Baseline	-1.01	-1.07	0.09	-0.25	2.28
$\mu_\kappa = 0$	-1.01	-1.07	0.09	-0.08	1.00
$\delta = 0$	-1.01	-1.07	0.09	-0.08	∞
$f = 0.07$	-0.96	-1.07	0.09	-0.15	1.26
$f = \bar{f}w^{1/2}$	-0.95	-1.04	0.08	-0.29	3.50
$\mu_\phi = 0.80$	-0.88	-0.96	0.08	-0.16	1.40
$\sigma = 1$	-1.57	-1.48	0.14	-0.36	2.37
$\text{corr}(\log z, \log w) = 0.50$	-1.46	-1.10	0.07	-0.29	3.43
$\text{corr}(\phi, \log w) = 0.50$	-1.03	-1.07	0.09	-0.26	2.35
$\theta = 5$	-0.77	-1.12	0.07	-0.20	2.33
$\theta = 2$	-0.45	-1.19	0.06	-0.12	2.46
$\Delta \log Z = -0.11, \mu_\kappa = 0$	-1.01	-1.26	0.09	-0.25	1.00

Notes: The table presents values of the cross-sectional elasticity of employment with respect to the wage, τ_ℓ/τ_w, the absolute value of the elasticity of labor demand at the establishment level, η, the percent change in the aggregate wage between the period with the minimum wage and the period without the minimum wage, W_1/W_0-1, the percent change in aggregate employment between the two periods, L_1/L_0-1, and the ratio of entry in the period with the minimum wage to entry in the period without the minimum wage. The first row presents these values for the baseline parameters and the other rows present these values for alternative model parameterizations.

percent of profits and 30 percent of revenues.

Baseline results. The first row of Table 6 presents outcomes from our baseline calibrated economy. By design, the model reproduces the elasticity of employment with respect to the wage, $\tau_\ell/\tau_w = -1.01$. Following the introduction of the minimum wage, the average wage in the economy grows by 9 percent. Had we used τ_ℓ/τ_w to infer the employment effects of the minimum wage, we would also have concluded that employment declines by 9 percent. However, aggregate employment in the calibrated economy declines by 25 percent. The logic is that the increase in the minimum wage puts a downward pressure on expected profits and thus limits the entry of establishments. The effect of reduced entry on aggregate employment is not reflected in the cross-sectional elasticity τ_ℓ/τ_w, because τ_ℓ/τ_w uses information only from establishments which have entered.

The wage increase of 9 percent in the model matches closely the 8 percent wage increase for restaurants that we estimated from the time series. The employment decline of 25 percent in the model is close to the roughly 30 percent decline that we estimated for Minneapolis restaurants. The similarity of the employment and wage responses between the model and the data is reassuring for the potential of endogenous entry to account for the difference between employment effects in the time series and in the cross section. We calibrated parameters to target cross-sectional moments and calibrated the entry cost independently of our time series treatment effects by appealing to the industrial organization literature for estimates of this cost.

Our model generates 18 log points decline in the number of establishments operating after the minimum wage increase. We find some evidence that declines in the number of operating establishments are observed for full-service restaurants in the data.[37] Partly because of the decline in the number of establishments and partly because we find negative employment effects using variation from both the time series and the cross section, our model of establishment dynamics is closer in spirit to the model of Hopenhayn (1992), which allows for flexible adjustment of labor, than putty-clay models such as those in Sorkin (2015) and Aaronson, French, Sorkin, and To (2018), which highlight the importance of higher entry of capital-intensive establishments. Consistent with our mechanism, Draca, Machin, and Van Reenen (2011) document a decline in firm profitability induced by the introduction of a UK national minimum wage in 1999. Harasztosi and Lindner (2019) also document a decline in firm profitability for the case of Hungary, but find that consumers bear the largest share of the increased cost from the minimum wage partly by paying higher product prices.

The column labeled η shows the absolute value of the elasticity of labor demand, averaging formula (20) across periods. The model-generated elasticity of labor demand is -1.07, which is close to the cross-sectional elasticity of employment with respect to the wage, $\tau_\ell/\tau_w = -1.01$, induced by the increase in the minimum wage in the data. Thus, our results from the cross section are informative about the elasticity of labor demand at the establishment level.[38]

Understanding model mechanisms. The second row shows the importance of endogenous entry

[37] We define an establishment as active in a quarter if it reports positive employment in that quarter. Using DEED data and the same synthetic difference-in-differences approach as in our other time series estimates, in 2020 we find 17 log points decline in Minneapolis and 6.3 log points decline in Saint Paul in the number of establishments.

[38] The two elasticities need not be equal, partly because there is an omitted variable bias when projecting employment on the wage in the cross section in order to infer the elasticity of labor demand. The bias arises because the weight α and productivity z are correlated with the wage owing to selection into entry and exit and because α varies endogenously with the wage whenever the elasticity of factor substitution $\sigma \neq 1$. In practice, the gap between the two elasticities is small under the baseline parameterization, because using the arc growth of employment removes persistent sources of heterogeneity correlated with the wage and allows us to include exiting establishments.

by setting the entry cost to zero for all establishments. In the absence of endogenous entry, the decline in aggregate employment is only 8 percent and can be inferred from the cross-sectional elasticity of employment with respect to the wage. Similarly, when the exogenous rate of destruction δ is zero, the decline in aggregate employment is only 8 percent because there is no entry in the period with the minimum wage.

The next row shows that lowering the fixed operating cost f leads to a smaller sensitivity of entry to the minimum wage and thus smaller employment losses at the aggregate. Denominating the fixed cost as a geometric mean of input prices, $f = \bar{f}w^{1/2}$, increases the sensitivity of entry and the employment losses in response to the increase in the minimum wage, because establishments with a binding minimum wage face an increasing operating cost over time. Lowering the labor share of operating costs by setting $\mu_\phi = 0.80$ weakens the cross-sectional elasticity of employment with respect to the wage, τ_ℓ/τ_w, and also makes entry less sensitive to the minimum wage increase. By contrast, raising the elasticity of factor substitution to $\sigma = 1$ increases the labor demand elasticity η and leads to larger employment losses from the introduction of the minimum wage.

Next, we consider how of our results change if we allow the sources of heterogeneity to be correlated within establishments. Introducing a correlation of 0.5 between productivity and the wage strengthens the cross-sectional elasticity of employment with respect to the wage, τ_ℓ/τ_w, and also makes entry more sensitive to the minimum wage increase. By contrast, introducing a correlation between the intensity of all other inputs in production and the wage does not have a discernible effect on the outcomes of the model economy.

The next experiment is to introduce labor market power by allowing parameter θ to be lower than infinity. In an economy with labor market power, the cross-sectional elasticity of employment with respect to the wage, τ_ℓ/τ_w, is less informative about establishments' elasticity of labor demand η. The logic is that employment and the wage in the cross section of establishments reflect both movements along a stable labor demand and shifts of the labor demand, because some establishments with a binding minimum wage increase their employment. The employment growth for some establishments also makes aggregate employment decline by less than in an economy without labor market power.[39]

[39] The cross-sectional elasticity of employment with respect to the wage is more informative about η if establishments with a binding minimum wage have a low labor market power. Berger, Herkenhoff, and Mongey (2022) develop a general equilibrium model with labor market power that varies with establishment size to

It is worth highlighting that our results do not allow us to directly identify how competitive labor markets were in the Twin Cities before the introduction of the minimum wage. However, the model we reject is that labor markets are imperfectly competitive and that the increase in the minimum wage is sufficiently small to induce an equilibrium wage below the competitive level. This model generates positive employment effects in both the cross section and the time series, which are inconsistent with our estimates. We conclude that our results are compatible either with perfectly competitive labor markets or the introduction of a minimum wage above the competitive level for most establishments that may operate in labor markets with some degree of monopsony power.

Aggregate shock contemporaneous with the minimum wage increase. The final row of the table shuts off endogenous entry and introduces an aggregate shock to productivity, $\Delta \log Z = -0.11$.[40] The outcomes in this economy are identical to the outcomes in the baseline economy with entry but no aggregate shocks. We conclude that the divergence between the employment effects in the time series and the employment effects in the cross section can reflect either endogenous entry that is missing from the cross section or an aggregate shock that is contemporaneous with the minimum wage change and differenced out in the cross section. Thus, both estimation strategies are informative and provide plausible bounds for the employment effects of the minimum wage increase.

7 Conclusion

We use high-quality administrative data from the state of Minnesota to analyze the labor market effects of two large increases in the minimum wage from Minneapolis and Saint Paul. Our analysis proceeds in three steps. Leveraging recent advances in synthetic difference-in-differences approaches, we estimate counterfactual outcomes in the absence of the minimum wage using variation at the zip code within Minnesota or at the city level from the rest of the country. Using variation from the cross sections of establishments and workers within the

evaluate the efficiency and redistributive effects of a minimum wage. They find small total factor productivity and employment gains from introducing a minimum wage because establishments with a binding minimum wage tend to be unproductive and have a low labor market power.

[40] The economy is recalibrated to match the targets in Table 5. This leads to $\sigma = 0.57$ and $q = 0.20$, whereas all other parameters are held constant at their previous values. In this economy, we adjust the labor supply shifters $\bar{w}$ so that the wage is constant when aggregate Z declines. Thus, the 9 percent growth of the wage reflects only the introduction of the minimum wage.

Twin Cities, we estimate the labor market effects of differential exposure to the minimum wage increase. Finally, using a model of establishment dynamics, we attempt to reconcile the results from the time series with those from the cross section and discuss how our estimates are informative for deeper determinants of establishments' labor demand.

We reach several substantial conclusions. The minimum wage increase is associated with wage gains in most low-wage industries that accord well with estimates of the direct effect of the minimum wage on establishments' labor costs. The time series analysis shows that in the Twin Cities, the minimum wage increase is associated with an average jobs decline of roughly 3 percent. The jobs losses are concentrated in the restaurant and retail industries. The analysis using variation from the cross section leads to estimates of jobs losses that are half as large as the estimates from the time series. Our cross-sectional estimates are consistent with an elasticity of labor demand at the establishment level equal to -1, for establishments that operate in competitive labor markets and can adjust flexibly their inputs.

We offer two ways to reconcile the results from the time series with those from the cross section. One possibility is that the Twin Cities may have experienced idiosyncratic shocks or had a differential response to an aggregate shock that cannot be differenced out using other cities during the post-treatment period, even when adjusting for pandemic and civil unrest conditions because Twin Cities have a larger sensitivity to these conditions. Another possibility is that estimates from the cross section do not account for the effects of entry or other equilibrium effects. Using our model, we illustrate that either endogenous entry or confounding factors contemporaneous with the minimum wage can account for the different results between the two research designs. We argue that both research designs are informative and plausibly bound the effects of the minimum wage increase.

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Minimum Wages and Labor Markets in the Twin Cities

Online Appendix

Loukas Karabarbounis Jeremy Lise Anusha Nath

This Appendix reports additional results and analyses.

- Table A.1 presents the statewide minimum wage for Minnesota during the period of our study.

- Table A.2 details the minimum wage policy changes introduced by the cities of Minneapolis and Saint Paul.

- Table A.3 presents R-squared coefficients from regressions of outcome variables in Minneapolis or Saint Paul on the outcome variables of the synthetic control calculated using the weights $\hat{\omega}_i$. To set a baseline of comparison, we also present the R-squared coefficients when using the outcome variables of the unweighted average of all other zip codes within cities in Minnesota. The regressions are performed only during the pre-treatment period.

- Figures A.1 and A.2 show the time series of the wage and jobs in Minneapolis and Saint Paul, as well as for the Minnesota average of other cities and for the synthetic control, for retail; Figures A.3 and A.4 for administration and support; Figures A.5 and A.6 for health care and social assistance; Figures A.7 and A.8 for arts, entertainment, and recreation; Figures A.9 and A.10 for accommodation and food services; Figures A.11 and A.12 for other services; Figures A.13 and A.14 for full-service restaurants; and Figures A.15 and A.16 for limited-service restaurants.

- Table A.4 reports the industry distribution of employment shares and the fraction of workers earning below 15 dollars in 2017 by industry. The shares of employment do not add up to 100 percent, as some industries have been excluded because of confidentiality concerns based on the presence of few establishments. The excluded industries are Agriculture, Forestry, Fishing, and Hunting (11); Mining, Quarrying, and Oil and Gas Extraction (21); Construction (23); Information (51); Real Estate and Rental and Leasing (53); and Public Administration (92). The fraction of workers earning below 15 dollars reported in

Table A.4 for the restaurant industries is a lower bound for the fraction of workers who are affected by the minimum wage increase. This is because earnings reported to DEED include tips and the minimum wage ordinance excludes tips.

- Table A.5 repeats our estimates by adding time weights λ_t, following Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021).

- Table A.6 repeats our estimates from the DEED data when we exclude bordering cities from the sample of cities that form the synthetic control.

- Table A.7 presents the list of cities included in the control group of Minneapolis and Saint Paul for the analyses using QCEW data from other U.S. cities of similar size.

- Figure A.17 presents estimates from the QCEW for the restaurants and retail industries, adding time weights λ_t to the specification.

- Table A.8 presents Monte Carlo simulations to assess the size and sources of bias of the synthetic difference-in-differences method when the true data generating process is a factor model.

- Figures A.18, A.19, and A.20 repeat the analysis of Figure 7 in the main text for the jobs effects when we adjust jobs for workplace mobility, violent protests, and total protests.

- Figures A.21, A.22, and A.23 repeat the analysis of Figure 8 in the main text for the jobs effects when we adjust jobs for workplace mobility, violent protests, and total protests.

Table A.1: Minimum Wage Policy in the State of Minnesota

(Annual Revenue in Dollars)	Youth	Small Firms ($< 500,000$)	Large Firms ($\geq 500,000$)
2000-2005	4.25	4.90	5.15
2006-2013	4.90	5.25	6.15
2014	6.50	6.50	8.00
2015	7.25	7.25	9.00
2016	7.75	7.75	9.50
2017	7.75	7.75	9.50
2018	7.87	7.87	9.65
2019	8.04	8.04	9.86
2020	8.15	8.15	10.00
2021	8.21	8.21	10.08
2022	8.42	8.42	10.33
2023	8.63*	8.63*	10.59*

Notes: Symbol * denotes that the minimum wage is scheduled to increase every year according to the price deflator for personal consumption expenditures produced by the Bureau of Economic Analysis. The threshold of $500,000$ represents revenue reported to the state of Minnesota.

Table A.2: Minimum Wage Policy in the Twin Cities

	Minneapolis		Saint Paul			
Firms (Employees)	Small (< 100)	Large (≥ 100)	Micro (≤ 5)	Small (6-100)	Large (101-10,000)	Macro ($> 10,000$)
2018 (Jan)		10.00				
2018 (Jul)	10.25	11.25				
2019 (Jul)	11.00	12.25				
2020 (Jan)						12.50
2020 (Jul)	11.75	13.25	9.25	10.00	11.50	
2021 (Jul)	12.50	14.25	10.00	11.00	12.50	
2022 (Jul)	13.50	15.00*	10.75	12.00	13.50	15.00*
2023 (Jul)	14.50		11.50	13.00	15.00	
2024 (Jul)	equal to large		12.25	14.00	equal to macro	
2025 (Jul)			13.25	15.00		
2026 (Jul)			14.25	equal to macro		
2027 (Jul)			15.00			
2028 (Jul)			equal to macro			

Notes: Symbol * denotes that the minimum wage is scheduled to increase every year according to the price deflator for personal consumption expenditures produced by the Bureau of Economic Analysis. The size thresholds represent total firm employment across all establishments. Franchises are considered large firms if they have more than 10 franchises nationally. For full-service restaurants, if there are fewer than 10 locations nationally, each restaurant counts as a separate business for the purpose of determining size.

Table A.3: Pre-treatment Fit: Synthetic Control versus Difference-in-Differences

	Wage		Jobs		Hours		Earnings	
(R-squared, percent)	SC	DD	SC	DD	SC	DD	SC	DD
				Minneapolis				
Retail Trade (44)	85	32	84	0	77	4	72	3
Administration and Support (56)	57	5	87	12	71	13	80	18
Health Care and Social Assistance (62)	94	32	92	7	79	15	92	7
Arts, Entertainment and Recreation (71)	29	9	46	5	45	14	21	5
Accommodation and Food Services (72)	85	57	94	46	92	36	94	58
Other Services (81)	64	0	79	4	82	2	87	13
Full-Service Restaurants (722511)	65	33	87	25	84	38	84	25
Limited-Service Restaurants (722513)	64	29	58	10	56	3	50	4
Saint Paul								
Retail Trade (44)	69	0	65	1	60	0	68	3
Administration and Support (56)	61	4	65	3	74	4	73	3
Health Care and Social Assistance (62)	95	17	92	1	95	15	96	34
Arts, Entertainment and Recreation (71)	39	16	33	2	53	3	31	0
Accommodation and Food Services (72)	77	39	70	6	57	0	68	13
Other Services (81)	80	30	84	28	87	15	92	19
Full-Service Restaurants (722511)	79	51	73	2	65	3	67	5
Limited-Service Restaurants (722513)	66	48	53	4	46	0	58	4

Notes: SC: synthetic control. DD: difference-in-differences.

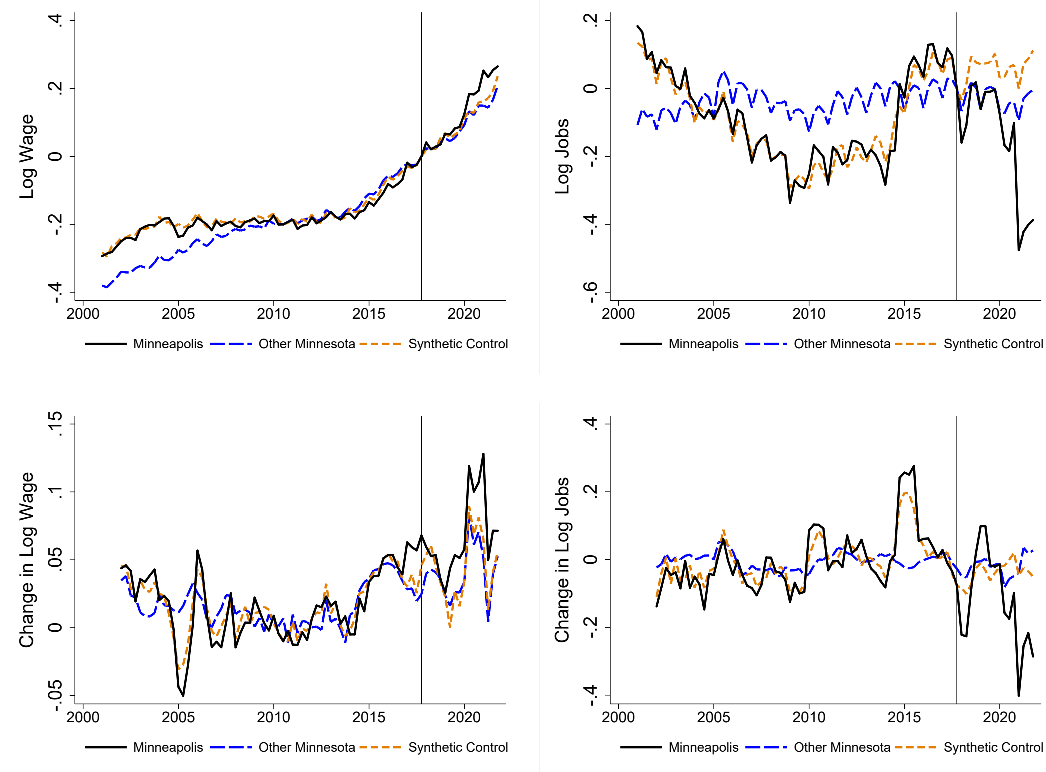


Figure A.1: Time Series of Retail Trade in Minneapolis

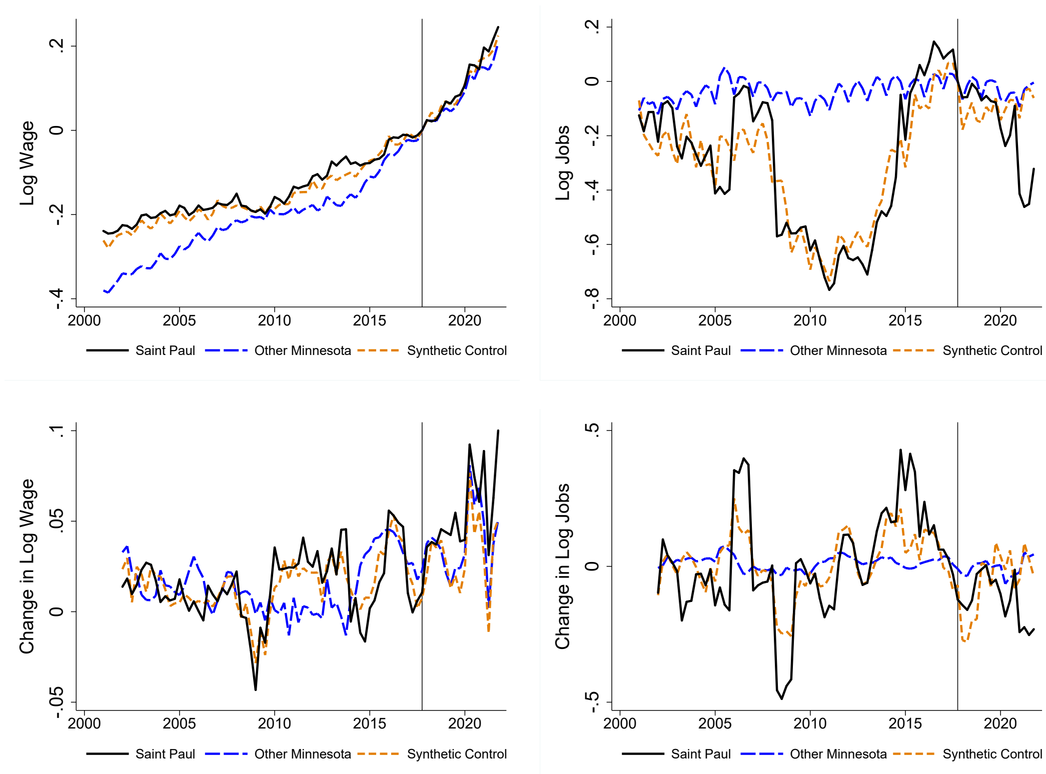


Figure A.2: Time Series of Retail Trade in Saint Paul

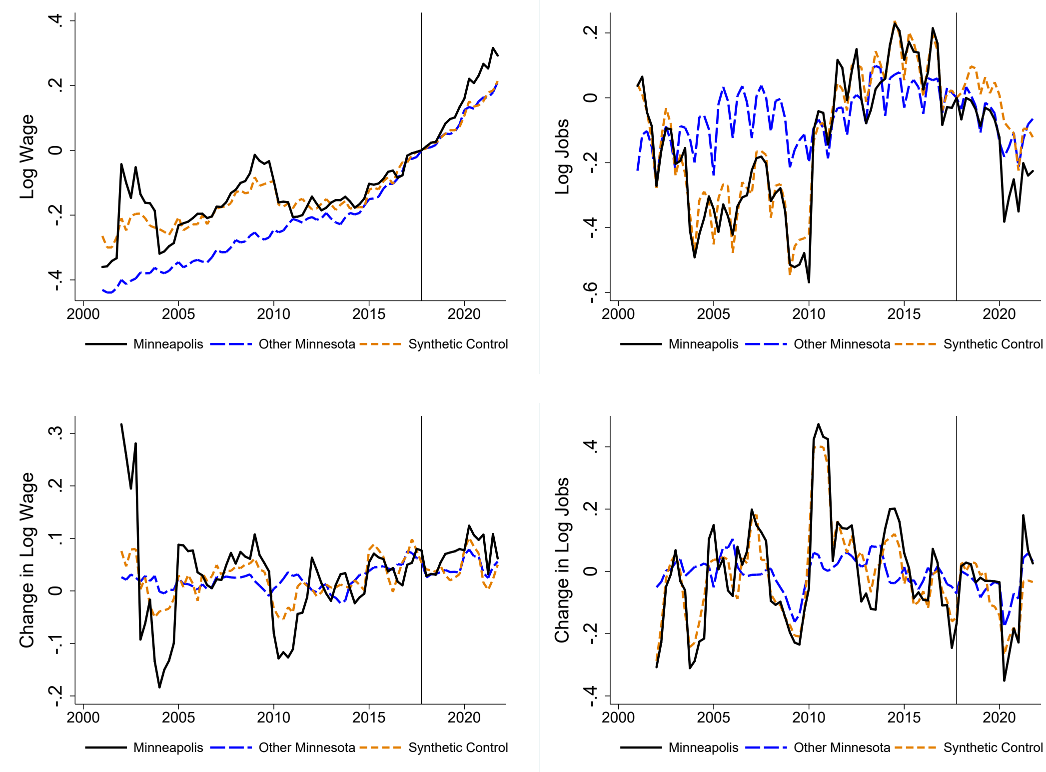


Figure A.3: Time Series of Administration and Support in Minneapolis

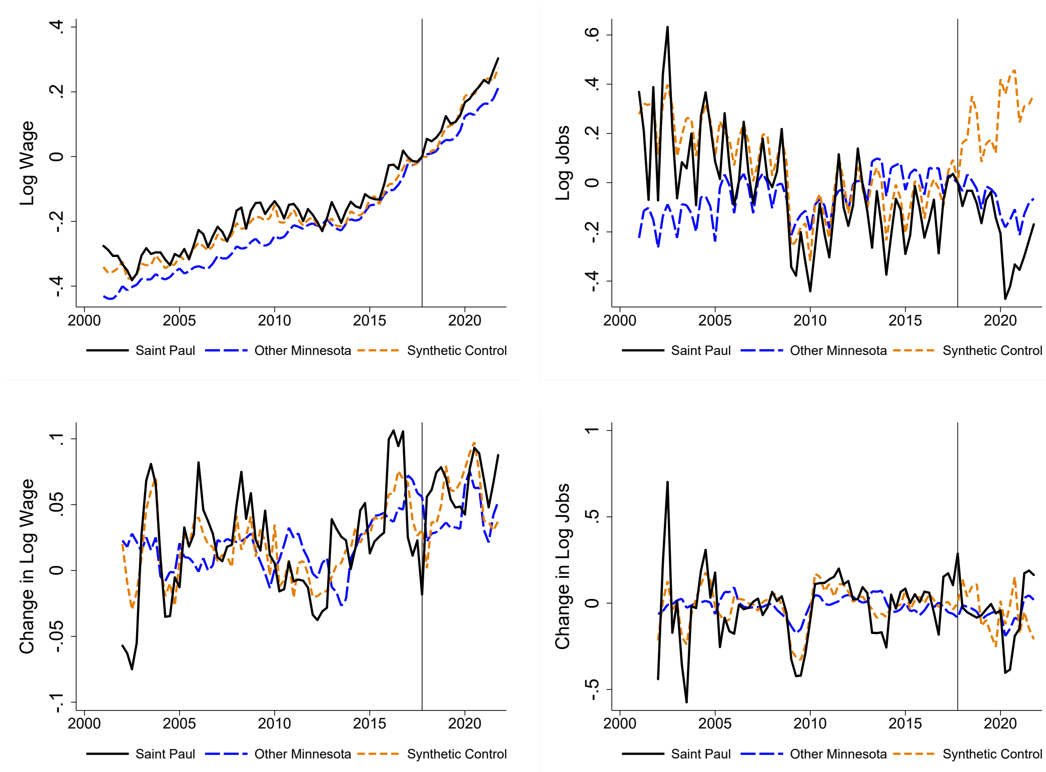


Figure A.4: Time Series of Administration and Support in Saint Paul

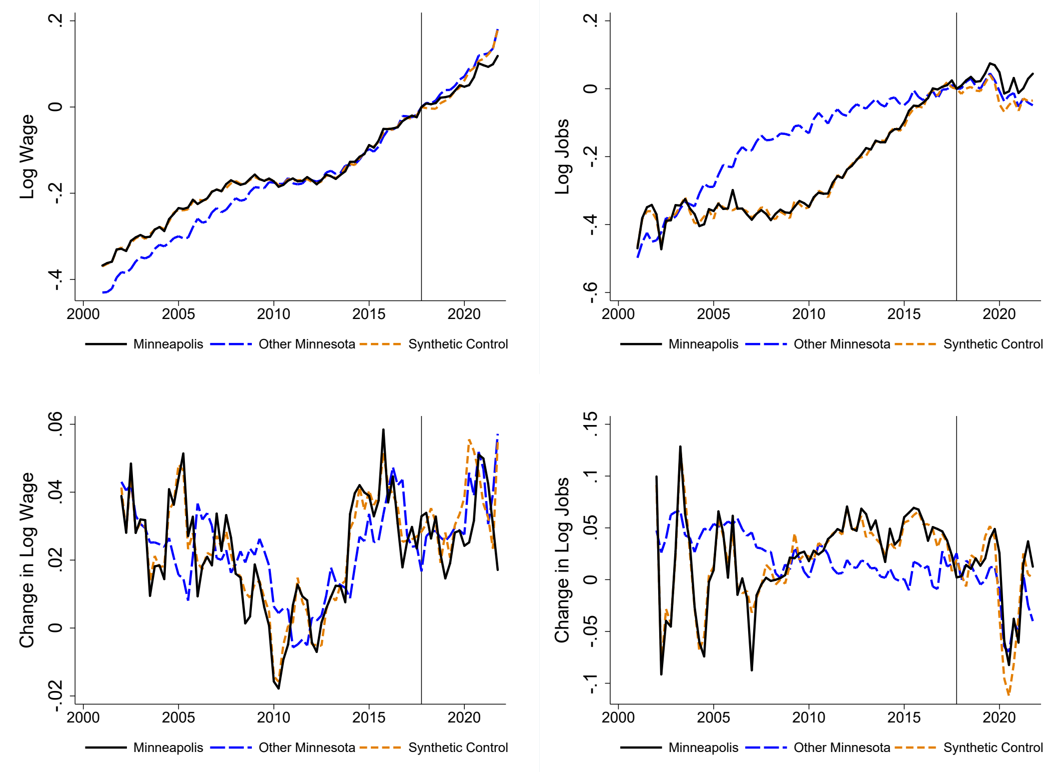


Figure A.5: Time Series of Health Care and Social Assistance in Minneapolis

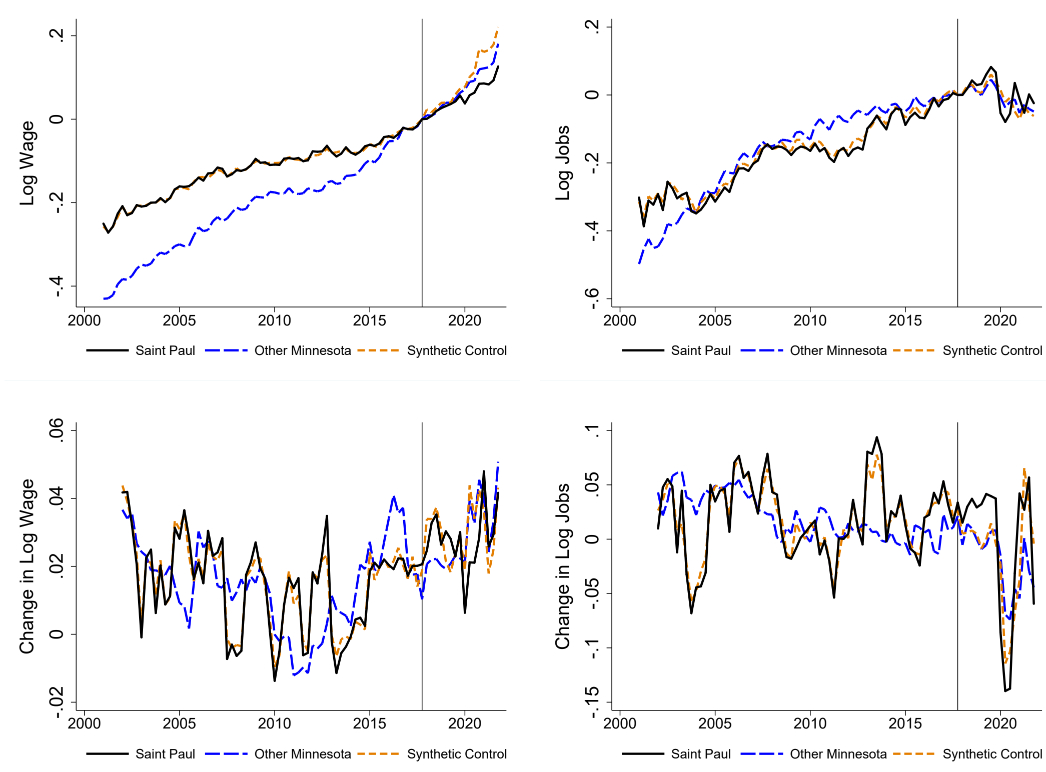


Figure A.6: Time Series of Health Care and Social Assistance in Saint Paul

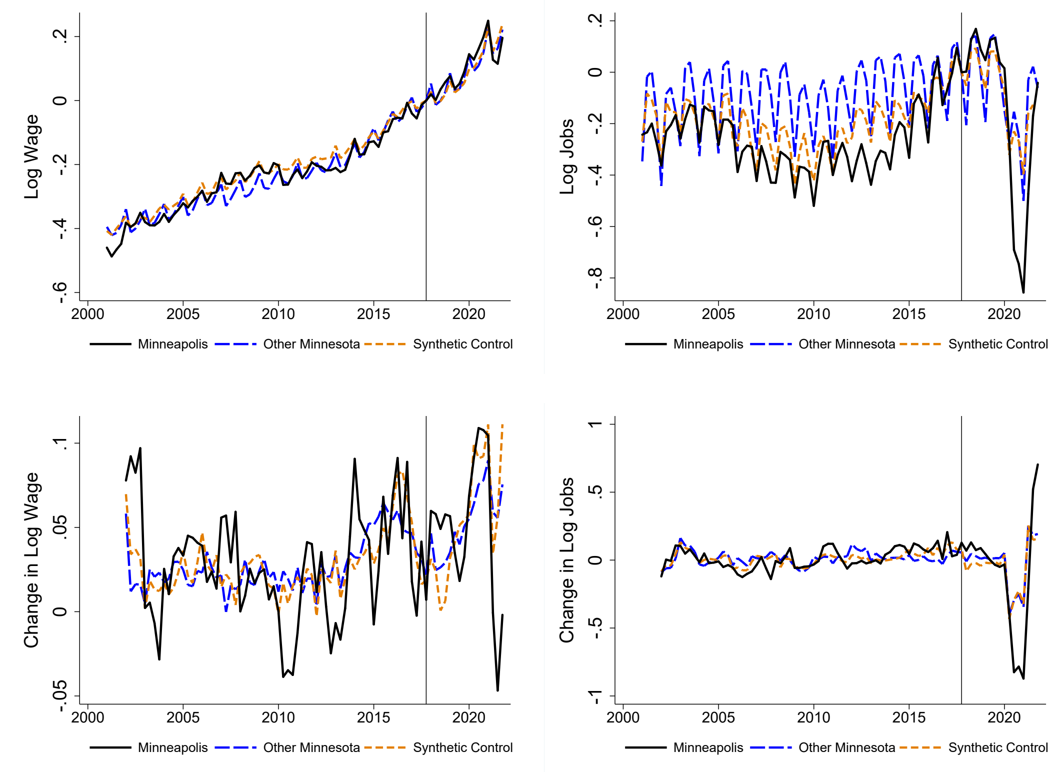


Figure A.7: Time Series of Arts, Entertainment, and Recreation in Minneapolis

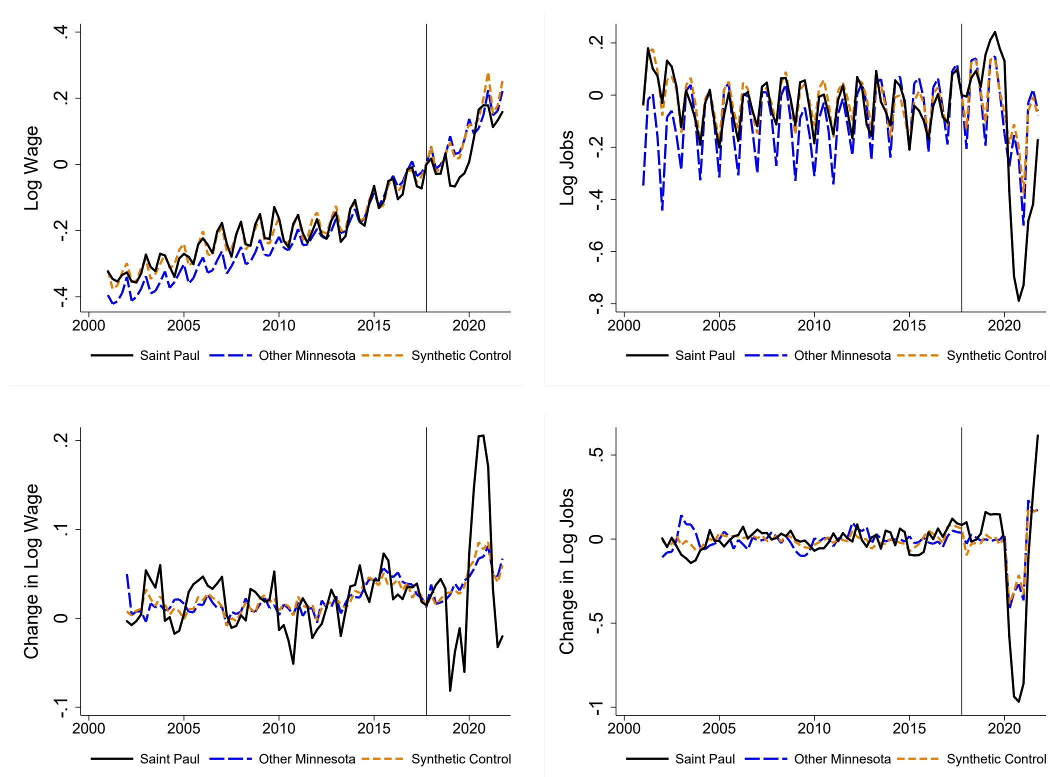


Figure A.8: Time Series of Arts, Entertainment, and Recreation in Saint Paul

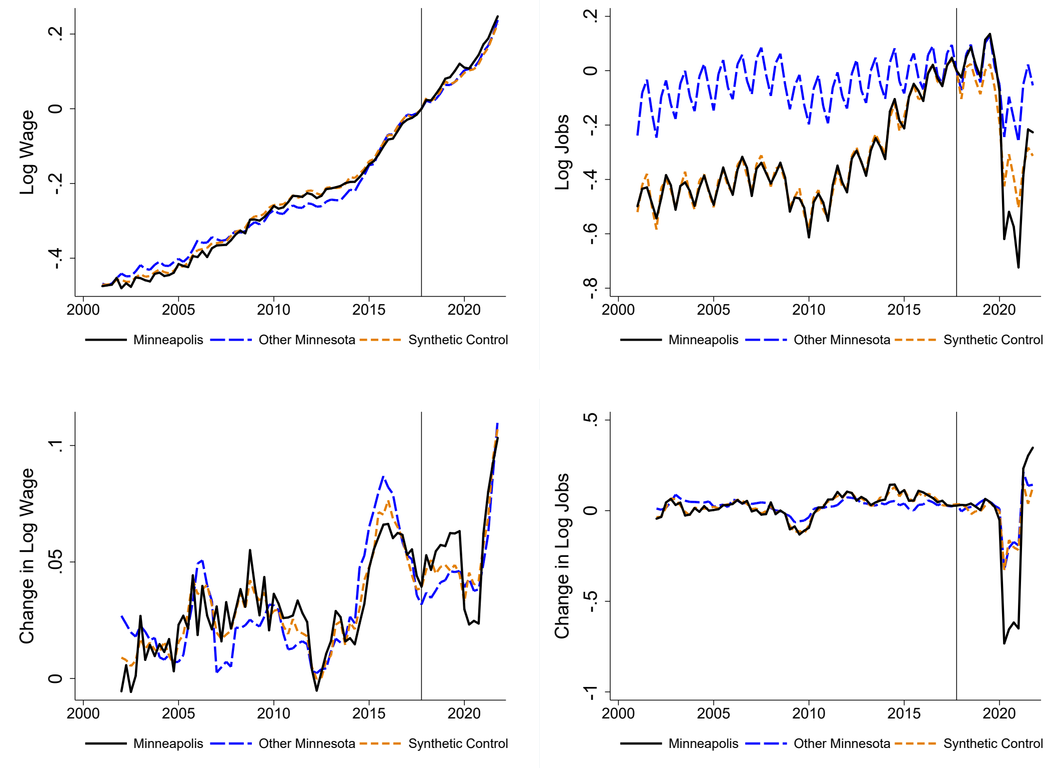


Figure A.9: Time Series of Accommodation and Food Services in Minneapolis

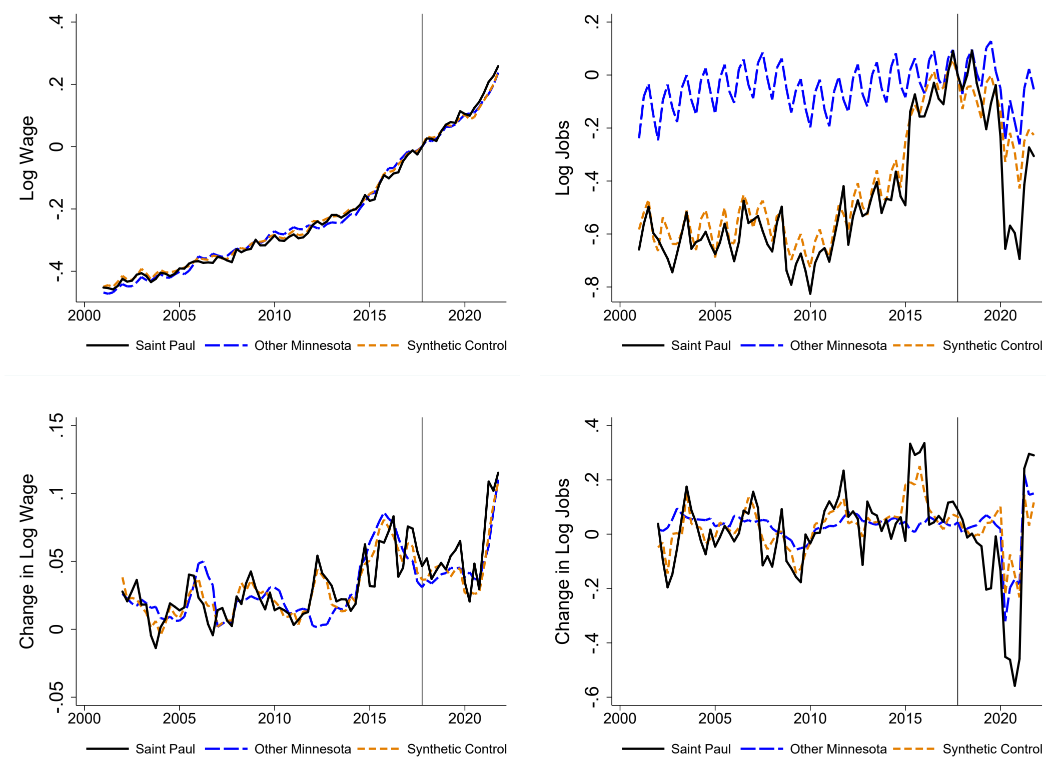


Figure A.10: Time Series of Accommodation and Food Services in Saint Paul

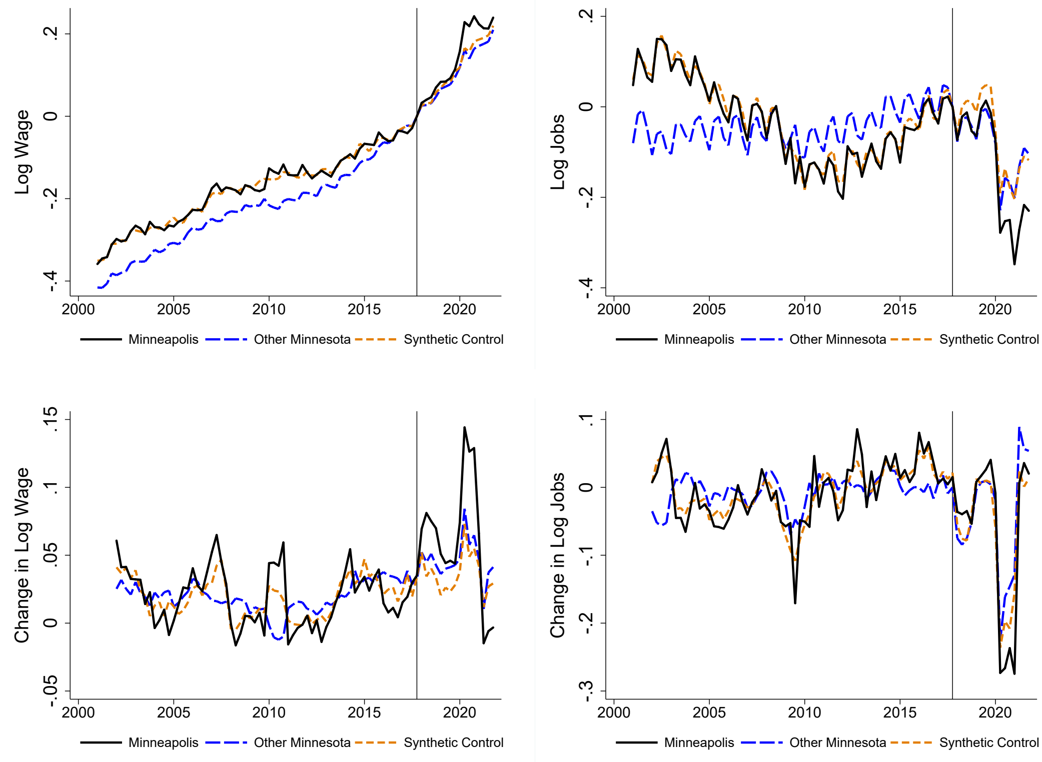


Figure A.11: Time Series of Other Services in Minneapolis

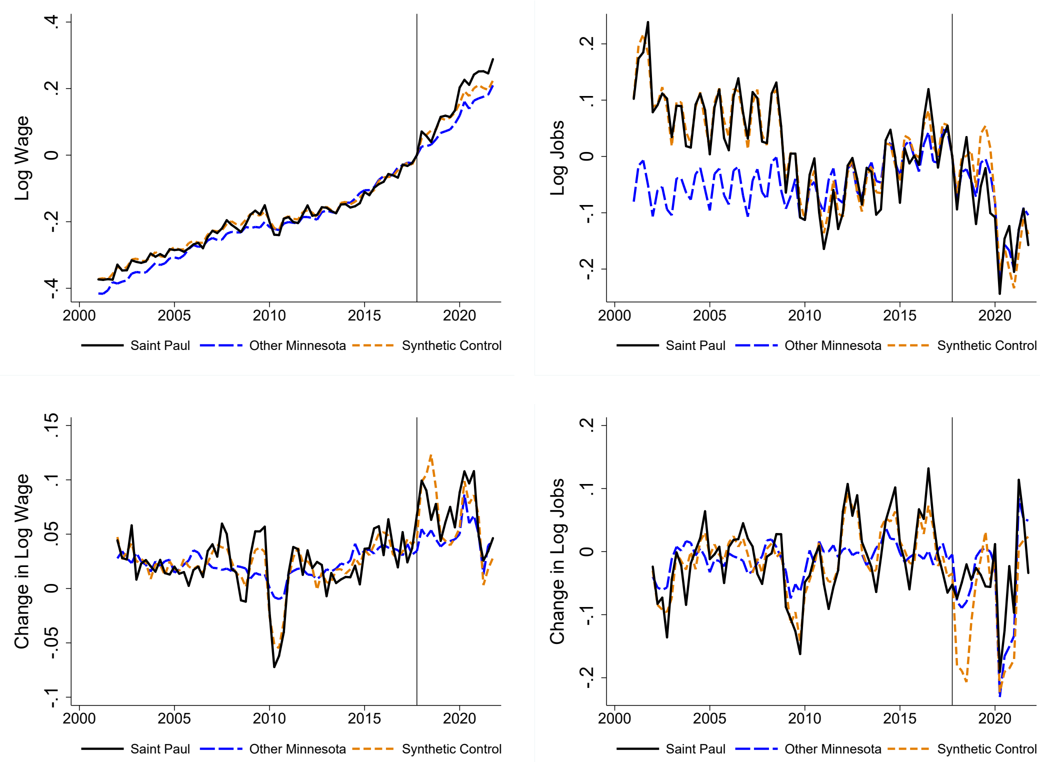


Figure A.12: Time Series of Other Services in Saint Paul

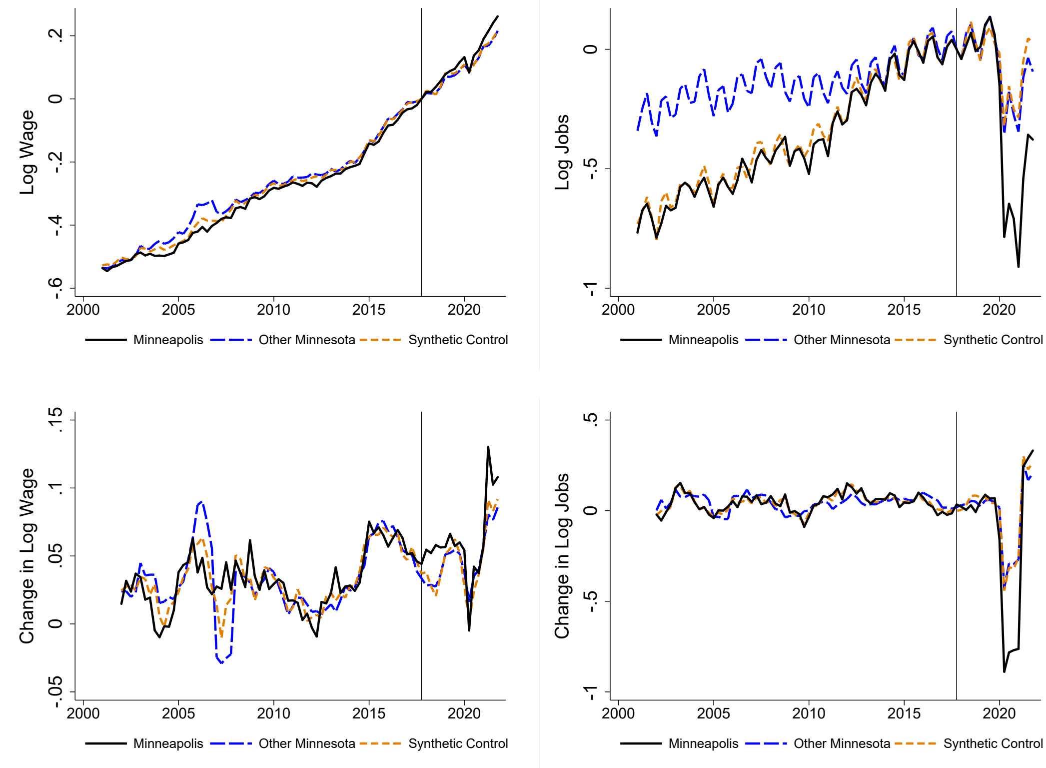


Figure A.13: Time Series of Full-Service Restaurants in Minneapolis

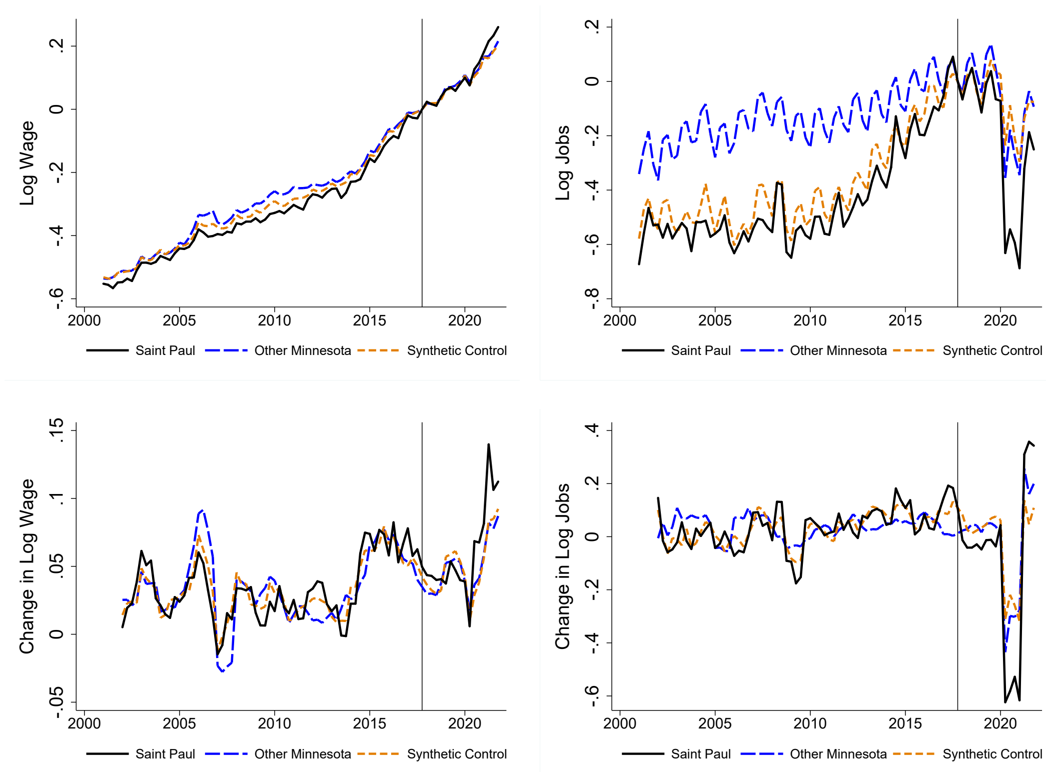


Figure A.14: Time Series of Full-Service Restaurants in Saint Paul

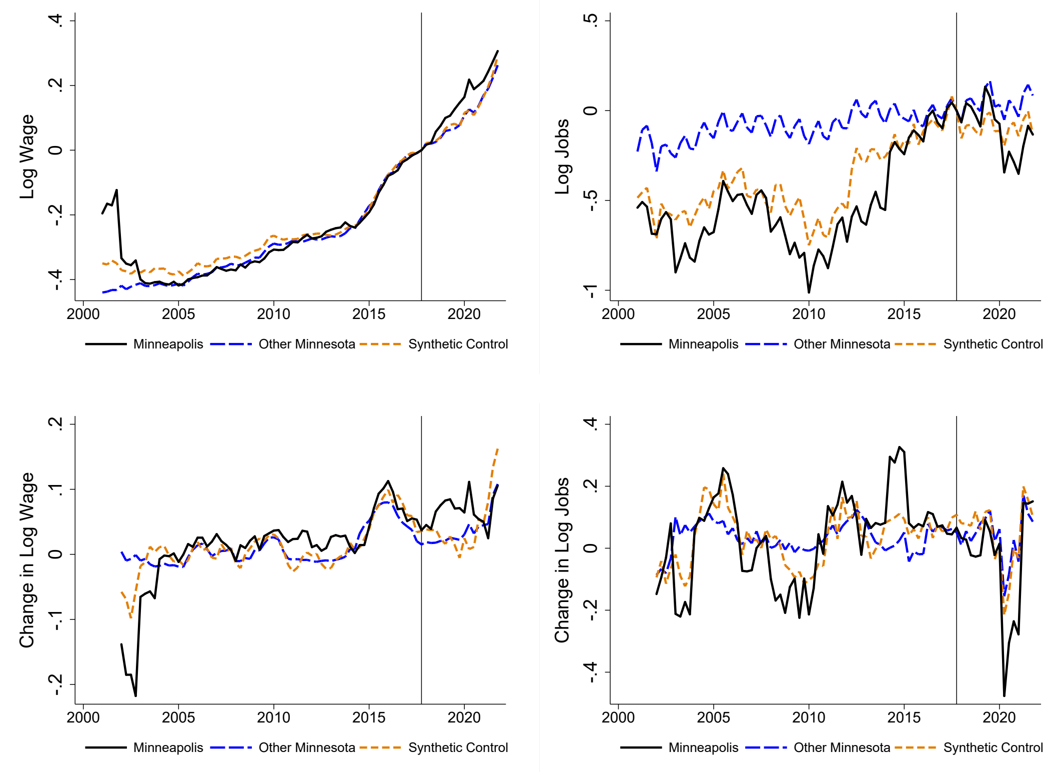


Figure A.15: Time Series of Limited-Service Restaurants in Minneapolis

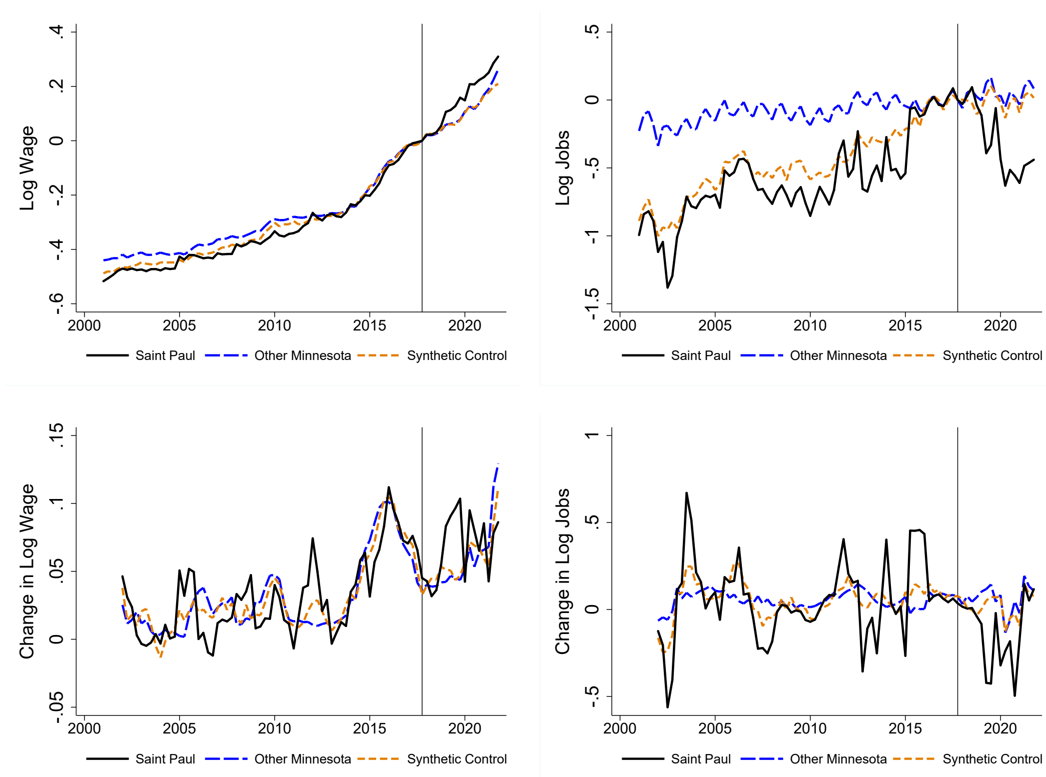


Figure A.16: Time Series of Limited-Service Restaurants in Saint Paul

Table A.4: Employment Shares and Fraction of Workers Earning below 15 Dollars

(2017)	Share of Employment (percent)			Fraction of Workers Earning Below 15 Dollars		
	MPLS	SP	Other MN	MPLS	SP	Other MN
Manufacturing (31)	4	4	12	14	18	17
Wholesale Trade (42)	3	3	4	11	16	15
Retail Trade (44)	5	7	12	59	63	65
Transportation (48)	2	2	3	20	21	23
Finance and Insurance (52)	11	5	4	5	6	13
Professional Services (54)	11	4	4	5	12	12
Management of Companies (55)	5	4	3	15	29	12
Administration and Support (56)	6	6	5	58	66	48
Educational Services (61)	13	13	8	22	23	23
Health Care and Social Assistance (62)	17	18	17	30	42	34
Arts, Entertainment, and Recreation (71)	2	2	2	42	45	61
Accommodation and Food Services (72)	8	10	9	54	63	71
Other Services (81)	3	4	3	40	34	49
Restaurant Industries						
Full-Service Restaurants (722511)	4	4	3	46	51	56
Limited-Service Restaurants (722513)	2	4	3	80	82	90

Note: MPLS denotes Minneapolis, SP denotes Saint Paul and Other MN denotes the sum of all other cities in Minnesota.

Table A.5: Adding Time Weights in the Synthetic Difference-in-Differences Estimation

Minneapolis	Wage	Jobs	Hours	Earnings
Retail Trade (44)	9.6	−39.5	−34.9	−19.9
	(0.0)	(0.0)	(0.0)	(1.2)
Administration and Support (56)	11.4	13.1	16.4	23.4
	(0.0)	(32.4)	(26.0)	(7.0)
Health Care and Social Assistance (62)	−2.3	9.5	4.0	6.8
	(7.6)	(11.0)	(58.9)	(53.3)
Arts, Entertainment and Recreation (71)	−0.4	−12.9	−10.5	7.9
	(84.7)	(6.0)	(25.4)	(99.5)
Accommodation and Food Services (72)	2.1	−29.2	−38.0	−43.8
	(4.8)	(0.0)	(0.0)	(0.0)
Other Services (81)	7.9	−5.2	−10.2	−7.7
	(0.0)	(58.3)	(16.2)	(40.2)
Full-Service Restaurants (722511)	4.7	−54.2	−50.8	−46.1
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	7.8	−26.3	−18.7	−17.1
	(0.0)	(3.2)	(13.0)	(22.4)
Saint Paul	**Wage**	**Jobs**	**Hours**	**Earnings**
Retail Trade (44)	9.0	−21.7	−56.1	−27.0
	(0.0)	(0.0)	(0.0)	(0.0)
Administration and Support (56)	4.7	−7.4	6.3	−74.5
	(9.0)	(59.1)	(79.3)	(0.0)
Health Care and Social Assistance (62)	−3.0	13.1	5.7	−3.5
	(2.8)	(3.8)	(40.8)	(45.0)
Arts, Entertainment and Recreation (71)	−3.4	−20.4	−14.8	−8.4
	(36.8)	(0.0)	(14.6)	(21.4)
Accommodation and Food Services (72)	6.0	−41.6	−58.9	−37.0
	(0.0)	(0.0)	(0.0)	(0.0)
Other Services (81)	1.8	14.3	0.5	8.2
	(40.4)	(2.6)	(85.9)	(18.2)
Full-Service Restaurants (722511)	4.2	−34.1	−32.2	−28.2
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	3.6	−34.1	−32.0	−55.7
	(0.2)	(1.0)	(1.8)	(0.0)

Notes: The estimates are in log points, multiplied by 100. Entries in parentheses are p-values in percent using the placebo method.

Table A.6: Excluding Neighboring Cities from the Control Group

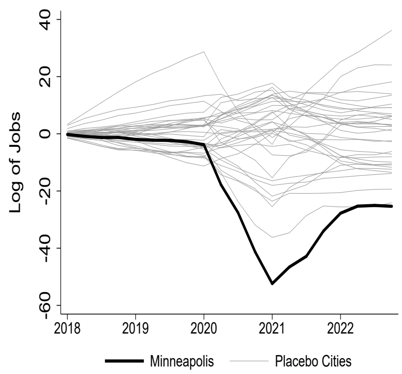
Minneapolis	Wage	Jobs	Hours	Earnings
Retail Trade (44)	9.9	−34.7	−21.5	−13.8
	(0.0)	(0.0)	(0.2)	(5.2)
Administration and Support (56)	11.6	18.1	15.0	15.8
	(0.0)	(15.8)	(34.6)	(27.2)
Health Care and Social Assistance (62)	−3.0	2.0	5.4	2.7
	(7.2)	(85.7)	(53.9)	(96.7)
Arts, Entertainment and Recreation (71)	−2.4	−16.4	−7.1	14.4
	(32.2)	(5.0)	(49.8)	(38.4)
Accommodation and Food Services (72)	0.7	−27.2	−33.3	−42.3
	(73.7)	(0.0)	(0.0)	(0.0)
Other Services (81)	10.3	4.1	−11.7	−0.8
	(0.0)	(43.6)	(9.0)	(92.3)
Full-Service Restaurants (722511)	5.8	−50.0	−47.5	−50.4
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	9.5	−35.5	−26.8	−25.7
	(0.0)	(0.6)	(4.0)	(5.4)

Saint Paul	Wage	Jobs	Hours	Earnings
Retail Trade (44)	9.2	−26.2	−53.6	−28.6
	(0.0)	(0.2)	(0.0)	(0.0)
Administration and Support (56)	4.8	−4.6	20.5	−62.1
	(10.4)	(72.7)	(15.2)	(0.2)
Health Care and Social Assistance (62)	−1.0	4.8	6.2	−2.5
	(44.0)	(53.7)	(48.6)	(55.3)
Arts, Entertainment and Recreation (71)	−0.2	−25.1	−17.4	−4.1
	(89.7)	(0.0)	(3.6)	(34.6)
Accommodation and Food Services (72)	4.2	−38.2	−59.2	−30.0
	(0.0)	(0.0)	(0.0)	(0.0)
Other Services (81)	2.3	21.2	−0.9	11.0
	(25.0)	(0.2)	(99.9)	(9.8)
Full-Service Restaurants (722511)	4.2	−31.9	−31.0	−35.8
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	2.0	−59.6	−76.0	−88.5
	(11.4)	(0.0)	(0.0)	(0.0)

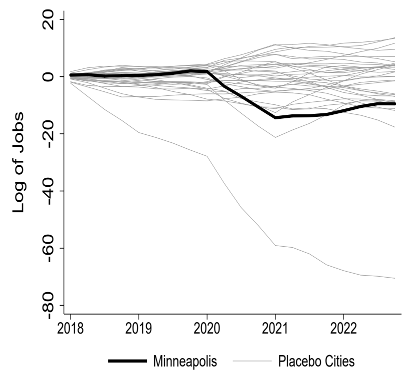
Notes: The estimates are in log points, multiplied by 100. Entries in parentheses are p-values in percent using the placebo method.

Table A.7: Cities of Similar Size to Minneapolis and Saint Paul

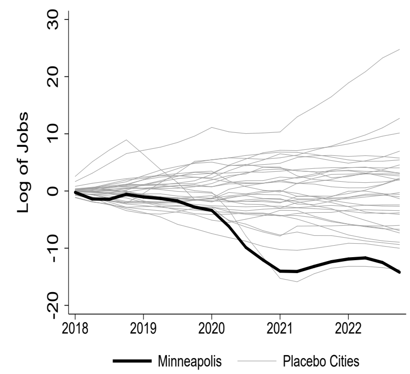
City	Jobs (000's)	City	Jobs (000's)
Indianapolis, IN	527	Albuquerque, NM	264
Jacksonville, FL	461	Greensboro, NC	251
Denver, CO	444	El Paso, TX	236
Nashville, TN	440	Prince George's County, MD	232
Memphis, TN	438	Colorado Springs, CO	225
Milwaukee, WI	434	Baton Rouge, LA	222
Louisville, KY	425	Wichita, KS	220
Honolulu, HI	380	Little Rock, AR	201
Oklahoma City, OK	374	St. Louis, MO	197
Tulsa, OK	322	Reno, NV	193
Kansas City, MO	314	New Orleans, LA	170
Fresno, CA	310	Fort Wayne, IN	169
Omaha, NE	301	Winston-Salem, NC	167
Tucson, AZ	299	Lexington, KY	159
Aurora, CO	295	Huntsville, AL	155
Minneapolis, MN	280	Virginia Beach, VA	149
Baltimore, MD	276	Springfield, MO	147
Aurora, CO	295	Corpus Christi, TX	135
Baltimore, MD	276	Salem, OR	132
Albuquerque, NM	264	Anchorage, AK	120
Greensboro, NC	251	Sioux Falls, SD	115
El Paso, TX	236	Rockford, IL	114
Prince George's County, MD	232	Richmond, VA	114
Colorado Springs, CO	225	Lubbock, TX	111
Baton Rouge, LA	222	Norfolk, VA	104
Wichita, KS	220	Tallahassee, FL	102
Little Rock, AR	201	Montgomery, AL	97
St. Louis, MO	197	Shreveport, LA	95
Reno, NV	193	Amarillo, TX	90
New Orleans, LA	170	Jackson, MS	86
Fort Wayne, IN	169	Chesapeake, VA	85
Winston-Salem, NC	167	Midland, TX	85
Lexington, KY	159	Fayetteville, NC	83
Huntsville, AL	155	Newport News, VA	83
Virginia Beach, VA	149	Augusta, GA	81
Saint Paul, MN	149	Laredo, TX	79
Springfield, MO	147	Kansas City, KS	78
Lincoln, NE	137	Birmingham, AL	77
Savannah, GA	136		



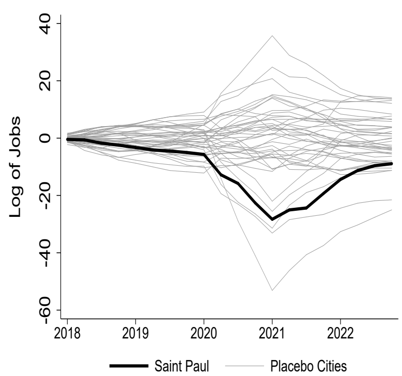
(a) Full-service restaurants, MPLS



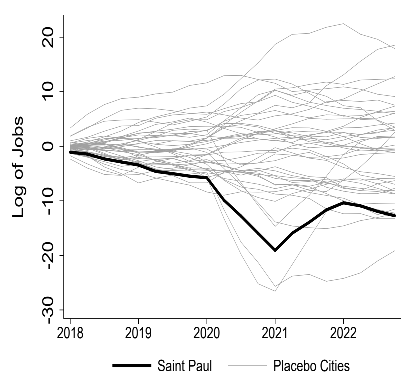
(b) Ltd-service restaurants, MPLS



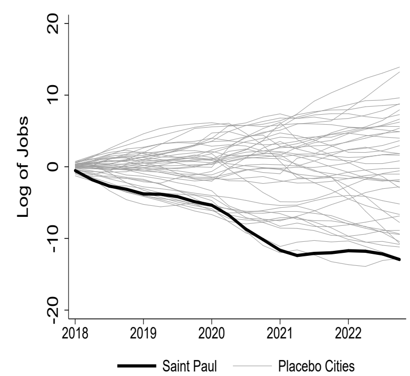
(c) Retail trade, MPLS



(d) Full-service restaurants, SP



(e) Ltd-service restaurants, SP



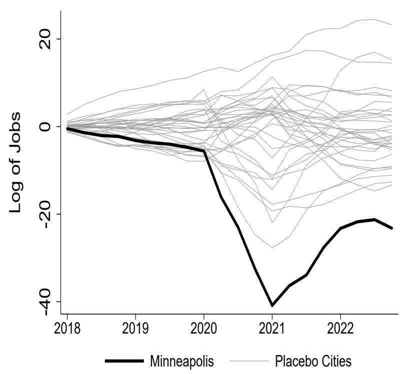
(f) Retail trade, SP

Figure A.17: Time-Varying Jobs Effects, Cities with Comparable Employment

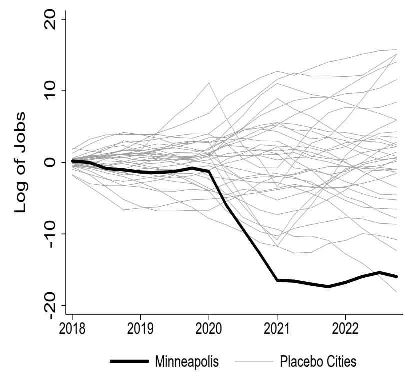
Table A.8: Assessing the Size and Sources of the Bias

Minneapolis	Jobs Effect	Bias μ	Bias u	Bias Total
Retail Trade (44)	−8	1	−2	−1
Administration and Support (56)	−10	−1	−2	−3
Health Care and Social Assistance (62)	−9	−1	−2	−3
Arts, Entertainment and Recreation (71)	−12	3	−4	−1
Accommodation and Food Services (72)	−19	0	−4	−4
Other Services (81)	−7	−4	0	−4
Full-Service Restaurants (722511)	−30	2	−7	−5
Limited-Service Restaurants (722513)	−17	−3	−3	−6
Saint Paul	**Jobs Effect**	**Bias μ**	**Bias u**	**Bias Total**
Retail Trade (44)	−12	0	−3	−3
Administration and Support (56)	−20	4	−6	−2
Health Care and Social Assistance (62)	−6	−1	−1	−2
Arts, Entertainment and Recreation (71)	8	−3	2	−1
Accommodation and Food Services(72)	−7	−7	0	−7
Other Services (81)	1	4	−1	3
Full-Service Restaurants (722511)	−13	1	−3	−2
Limited-Service Restaurants (722513)	−9	−5	−1	−6

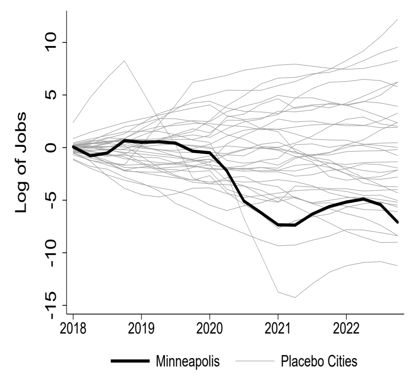
Notes: Estimates are in log points, multiplied by 100. The first column repeats our jobs estimates using synthetic difference-in-differences from the QCEW dataset. The data generating process is a factor model $Y_{it} = \alpha_i + \beta_t + \sum_{k=1}^{4} \mu_i^k \gamma_t^k + u_{it} + \sum_{s=T_{\text{pre}}+1}^{T} \tau_s W_{is}$. After generating data from this model, we perform our synthetic difference-in-differences model to assess the bias of the estimates relative to those in the factor model. The bias μ column assumes $u_{Nt} = \sum_{i=1}^{N_{\text{co}}} \omega_i u_{it}, \forall t = 1, ..., T$ and gives the bias due to not fitting the underlying factor structure. The bias u column assumes $\sum_{i=1}^{N_{\text{co}}} \omega_i \mu_i^k = \mu_N^k$ and gives the bias due to the scale of errors relative to the length of the pre-treatment period. The total bias column is the sum of the two columns.



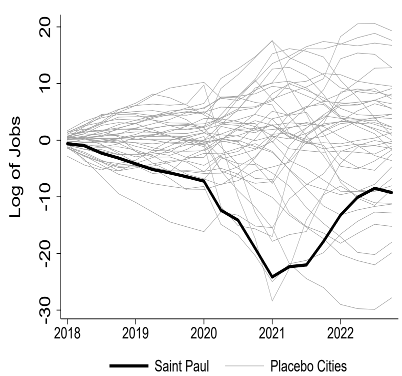
(a) Full-service restaurants, MPLS



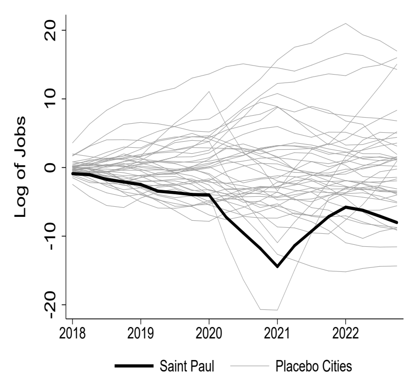
(b) Ltd-service restaurants, MPLS



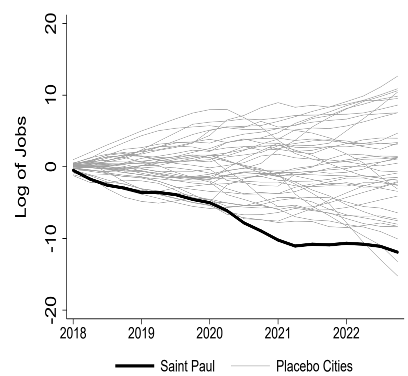
(c) Retail trade, MPLS



(d) Full-service restaurants, SP

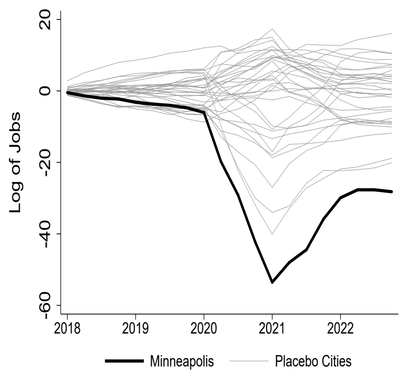


(e) Ltd-service restaurants, SP

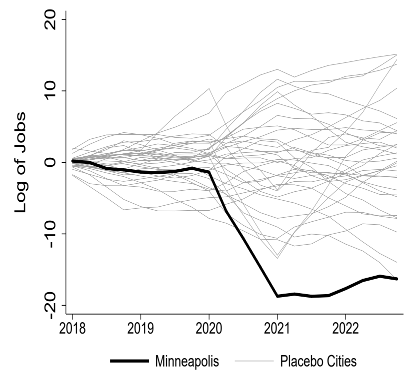


(f) Retail trade, SP

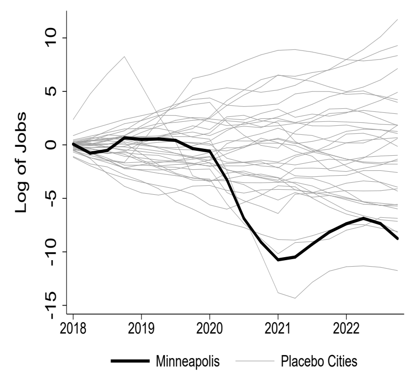
Figure A.18: Time-Varying Jobs Effects, Adjusted for Workplace Mobility



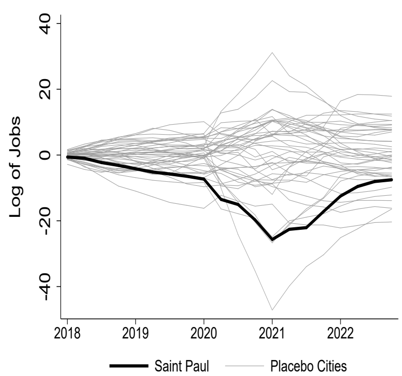
(a) Full-service restaurants, MPLS



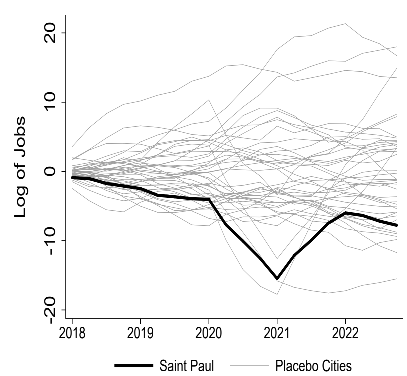
(b) Ltd-service restaurants, MPLS



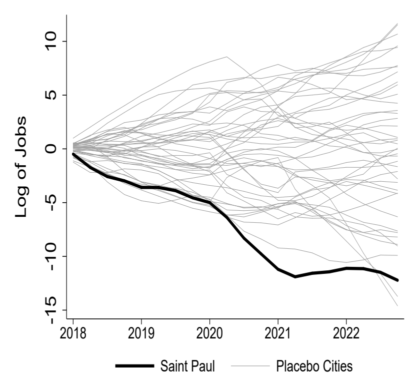
(c) Retail trade, MPLS



(d) Full-service restaurants, SP

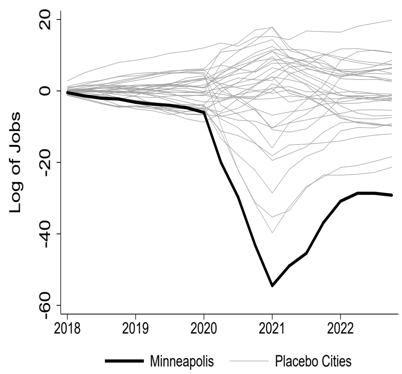


(e) Ltd-service restaurants, SP

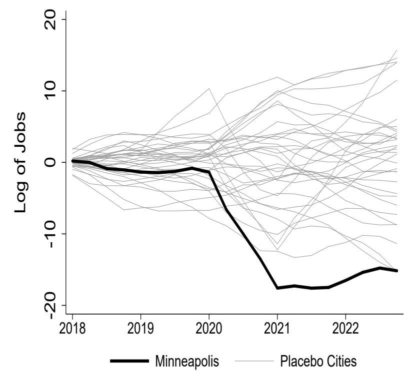


(f) Retail trade, SP

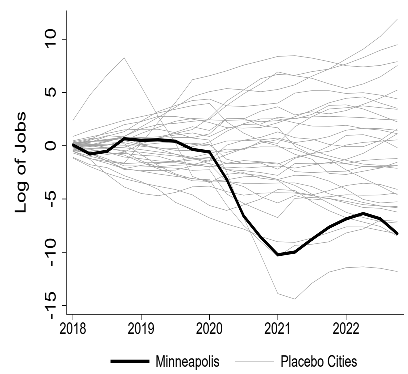
Figure A.19: Time-Varying Jobs Effects, Adjusted for Violent Protests



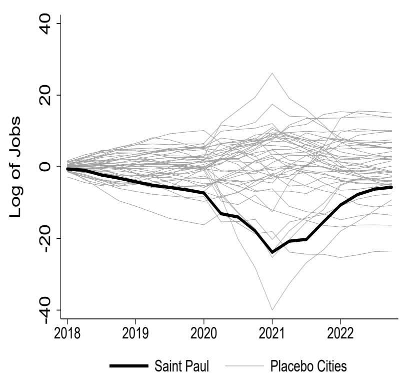
(a) Full-service restaurants, MPLS



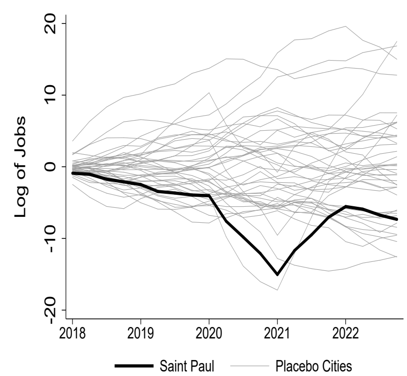
(b) Ltd-service restaurants, MPLS



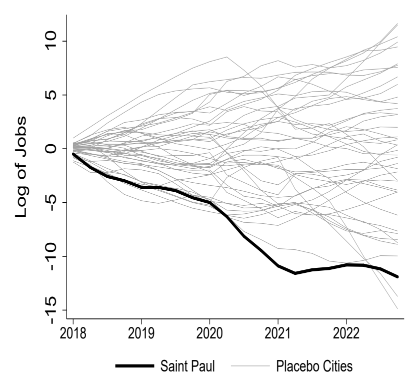
(c) Retail trade, MPLS



(d) Full-service restaurants, SP



(e) Ltd-service restaurants, SP



(f) Retail trade, SP

Figure A.20: Time-Varying Jobs Effects, Adjusted for Total Protests

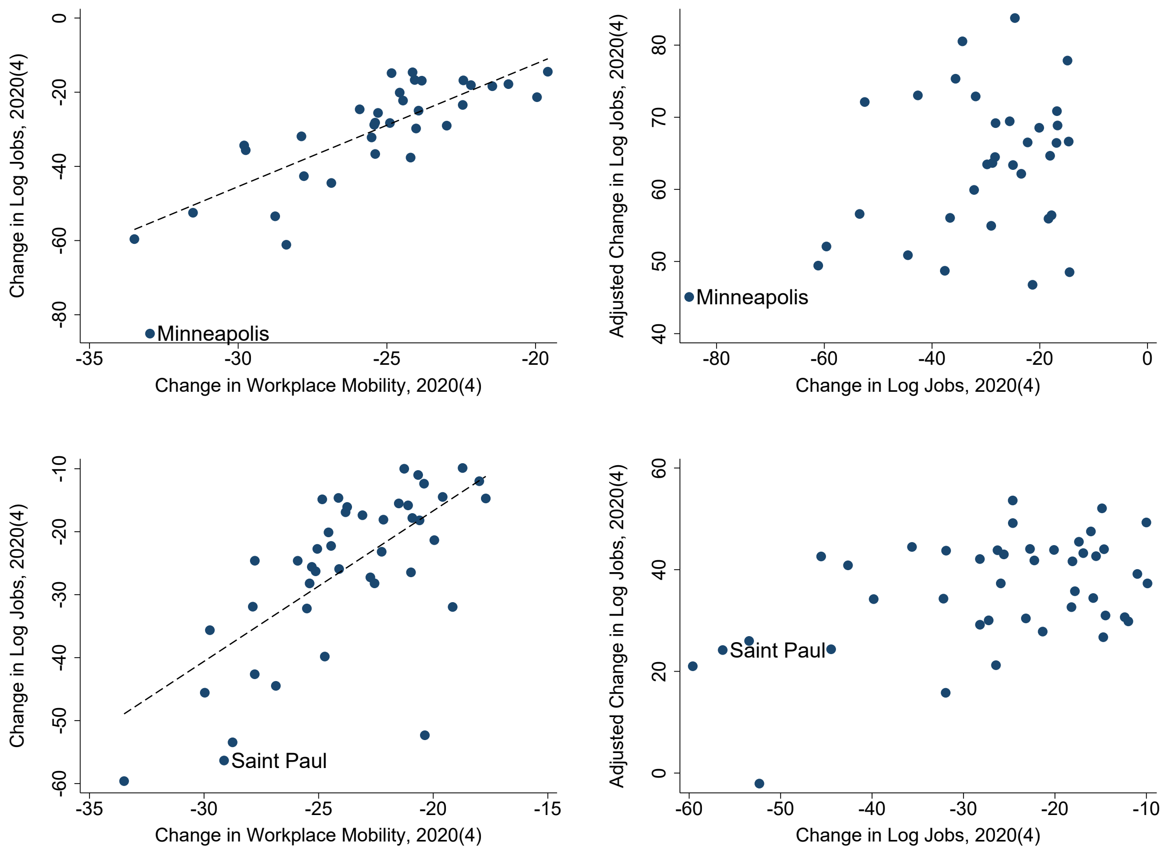


Figure A.21: Adjustments for Workplace Mobility

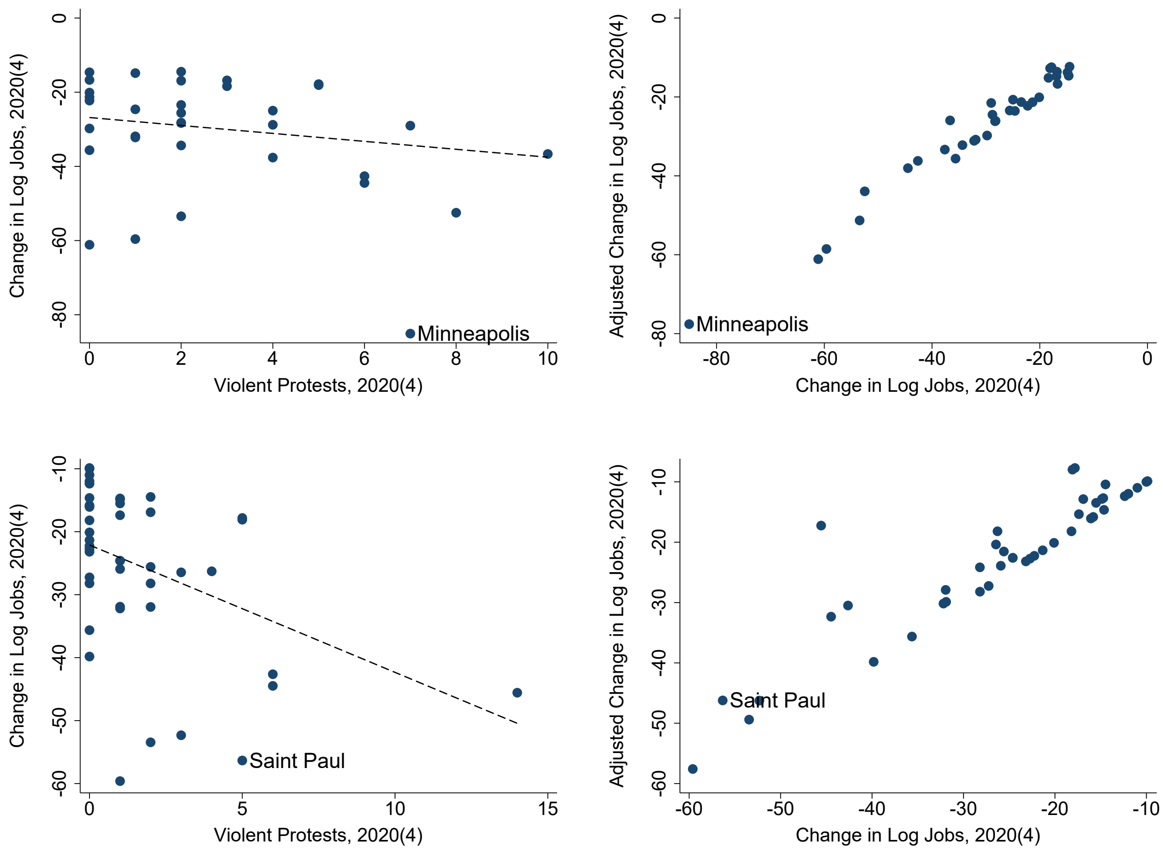


Figure A.22: Adjustments for Violent Protests

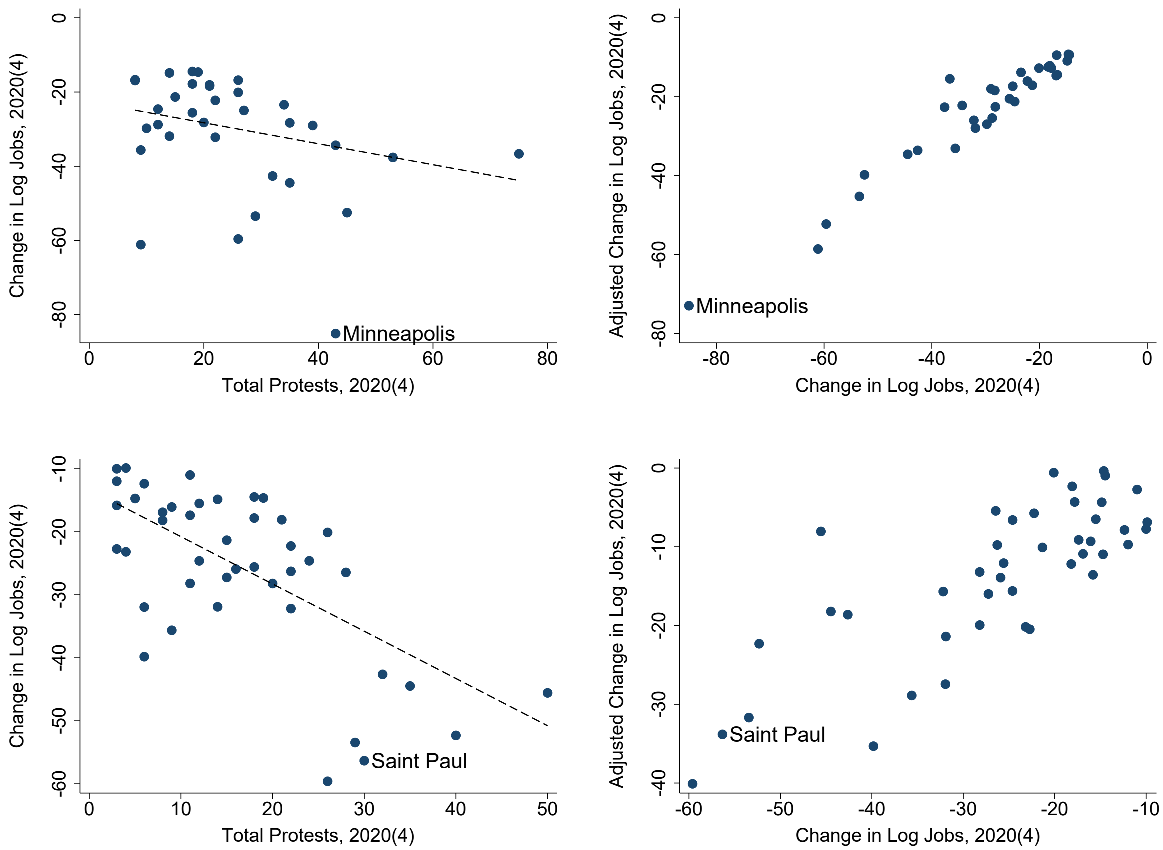


Figure A.23: Adjustments for Total Protests