Economic Impact Evaluation of the City of Minneapolis's Minimum Wage Ordinance

December 2, 2024

FEDERAL RESERVE BANK
OF MINNEAPOLIS

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1 Executive Summary

Key Findings

The minimum wage in Minneapolis increased by 60 percent from 2017 to 2022. The increase in the minimum wages between 2018Q1 and 2022Q4 was associated with an average increase in hourly wages of 0.3 percent, an average decline in jobs of 1.4 percent, an average decline in hours worked of 0.8 percent, and an average decline in wage earnings of 0.6 percent. The largest effects are concentrated in the restaurant and the retail industries, in low-paying establishments, and among low-paid workers.

Overview of Analysis

Background. This report examines the effects of minimum wage increases on the labor market outcomes of hourly wage, jobs, hours worked, and wage earnings between 2018Q1 and 2022Q4. The administrative data for our analyses come from the Department of Employment and Economic Development (DEED). This report updates results discussed in the 2023 report and will be followed by four additional evaluation reports to be provided to the City of Minneapolis. The study will last until December 1, 2028.

What's New in This Report? Due to delays in receiving new rounds of data, the scope of this report is limited to extending the results of the previous report by adding one additional year of data and providing more robustness results. This additional year of data allows us to examine the effects of the policy change as the economy saw further recovery from the pandemic in 2022.

Using the same methodology as the previous report, our aggregate estimates suggest that higher minimum wages increased wages and lowered employment. Industries with a lower median wage in 2017 experience more positive wage responses and more negative jobs responses. Similarly, the effects were larger for establishments and workers that were more

exposed to the minimum wage. In this report, we collect additional data and extend our methodology to directly adjust our estimates for pandemic and civil unrest conditions in the sample of other U.S. cities and find that these adjustments make very little difference for the estimated effects of the minimum wage on jobs.

The stability and robustness in our estimates give us confidence that our empirical evidence is capturing the effects of the minimum wage increase rather than the potential effects stemming from other developments, such as the pandemic or the civil unrest of 2020. Our current establishment-level and worker-level analyses point to significant differential effects of minimum wage increases across groups. Further exploration of similar effects, such as those on workers of different skills or on businesses of different sizes, will provide a more comprehensive assessment of the differential effects of this policy change.

Future Reports. Using additional data received from the Department of Human Services (DHS) and the Department of Revenue (DOR), we will examine several outcomes at a disaggregated level, such as establishments' substitution of labor with other factors of production, the substitution of employees with contractors, and changes in firms' profits. Moreover, our analysis of changes in social benefits following the minimum wage increases will begin to shed light on the fiscal implications of the policy change.

2 Purpose and Scope of the Study

Due to delays in receiving new rounds of data, the scope of this report is limited to extending the results of the previous report by adding one additional year of data and providing more robustness results. This report examines the labor market effects of the minimum wage ordinance in Minneapolis up to 2022Q4. We document the effects on hourly wages, jobs, hours worked, and worker earnings. We use two methods to estimate the causal effect of the minimum wage increase on these labor market outcomes. First, following a standard approach in the minimum wage literature, we use time series variation to compare these out-

Table 1: Minimum Wage Changes in Minnesota 2000-2022 (Dollars)

(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^*

Notes: * 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.

comes in Minneapolis with those of appropriate control cities within Minnesota or the rest of the country. This analysis provides us with aggregate effects of minimum wage increase on these outcomes in Minneapolis. Second, we exploit differential exposure of establishments and workers to the minimum wage increase within Minneapolis to estimate the labor market effects at the establishment and worker levels.

This analysis is based on data received from the Department of Employment and Economic Development (DEED). This is the third of a series of annual reports we will be providing to the City of Minneapolis; the final report will be delivered in 2028. The future reports will use additional data received from the Department of Human Services (DHS) and the Department of Revenue (DOR). Our ability to merge the DEED-DHS-DOR datasets will allow us to examine several outcomes at a disaggregated level, such as effects on social benefits received by workers, the establishments' substitution of labor with other factors of production, the substitution of employees with contractors, and changes in firms' profits.

Policy context. In 2022, the state minimum wage increased to $8.42 for small firms (firms with an annual revenue less than 500,000 dollars) and $10.33 for large firms. In July of 2022,

Table 2: Minimum Wage Policy Change in Minneapolis (Dollars)

Date	Small Firms (<100 Employees)	Large Firms (100+ Employees)
2018 (Jan)		10.00
2018 (July)	10.25	11.25
2019 (July)	11.00	12.25
2020 (July)	11.75	13.25
2021 (July)	12.50	14.25
2022 (July)	13.50	15.00*
2023 (July)	14.50	
2024 (July)	Equal to large firms	

Notes: * 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 minimum wage in Minneapolis increased to $13.50 for firms with less than 100 employees while it reached $15 for firms with more than 100 employees. Table 1 provides the details of changes in Minnesota's minimum wages over time while the details of the phased implementation of the Minneapolis minimum wage ordinance, which began in January 2018, are presented in Table 2. The minimum wage will be indexed to inflation once the target level of 15 dollars per hour is reached. This makes the changes both large and permanent. Our analysis will examine the economic impact of the minimum wage increases in Minneapolis since 2018. Throughout our period of study, the state minimum wage applies to all cities in Minnesota outside of the Twin Cities.

3 Data Sources

We use two main sources of data on workers and firms for our analyses of the effects of the minimum wage increase on labor market outcomes. Both sources are administrative and non-publicly-available data that were made available to us by Minnesota's Department of Employment and Economic Development (DEED). The first is individual-level data of workers from Unemployment Insurance (UI). Minnesota requires most employers to file quarterly

unemployment wage detail reports 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 hourly wages for each worker by dividing total quarterly earnings by quarterly hours.[1] 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 a large part of employment is generated in multi-establishment firms.

The UI data do not contain information on the location of the establishments, which is necessary in order to identify which establishments were affected by the minimum wage increase. To overcome this problem, we merge the UI data 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 the state of Minnesota. From these data, we observe the six-digit North American Industry Classification System code for the industry that the establishment operates in, the location of the establishment, and the firm to which the establishment belongs. The location data consist of both the city and the zip code in which the establishment operates.

The merged data result in a quarterly dataset that spans 2001Q1 to 2022Q4 and provides us with information on workers' hours and wages, as well as the establishments at which they are employed, by industry, zip code, and city. Our dataset improves measurement relative to that of previous studies along three dimensions. First, using administrative sources, we provide estimates for the effects of minimum wage increase on hours worked.[2] Second, Minnesota is unique in that it records employee hours worked at the establishment level within firms. Thus, we include in our analysis firms with multiple establishments across city borders. Finally, we leverage detailed location data at the zip code level to increase the precision of our estimates.

[1] For calculating hourly wages, we exclude roughly 5 percent of observations that reported zero hours worked. We keep these observations for calculating other outcomes.

[2] Oregon, Rhode Island, and Washington are the three other states in the U.S. that collect hours worked in the matched employer-employee administrative data.

4 Time Series Analysis: Aggregate Effects

In this section, we use a standard approach in the minimum wage literature that exploits time series variation to estimate the effects of a minimum wage increase. The main challenge of causal inference in our context is that we observe wages, employment, hours, and worker earnings in Minneapolis before and after the policy change but do not observe what the economic outcomes in Minneapolis would have been in the absence of an increase in the minimum wage. To answer the question of what the effect of the minimum wage increase is, one needs to know the difference between the actual outcomes (which are observed) and the counterfactual outcomes (which are not observed).

We use two approaches in creating the counterfactual. The first approach creates a comparison group using zip codes in Minnesota outside of Minneapolis and Saint Paul. The second approach uses cities from the rest of the country that are similar in size to Minneapolis. In both cases, the comparison group is constructed by choosing weights across geographic regions so that the weighted average across regions approximates as closely as possible Minneapolis before 2018 on the observable dimensions that are relevant for the analysis. The details of our methodology are explained in our previous report Karabarbounis et al. (2023).

The time series analysis focuses on the two-digit industries in which 30 percent or more of workers earned below 15 dollars per hour in 2017. The six industries that satisfy this criterion are retail trade (44); administration and support (56); health care and social assistance (62); arts, entertainment, and recreation (71); accommodation and food services (72); and other services[3] (81). As requested by the City of Minneapolis, we separately analyze full-service restaurants (722511) and limited-service restaurants (722513). These industries have a high fraction of potentially impacted workers and have been studied extensively in the literature.[4]

[3]The other services sector consists of repair and maintenance shops, personal and laundry services, and various civic, professional, and religious organizations.

[4]We also analyzed other industries and did not find statistically or economically significant responses. As

Table 3: Effects of the Minimum Wage Increase

	Wage	Jobs	Hours	Earnings
Retail Trade (44)	8.4	-34.2	-20.0	-18.4
	(0.0)	(0.6)	(1.4)	(8.2)
Administration and Support (56)	5.6	22.0	19.4	19.9
	(12.4)	(15.2)	(27.0)	(23.2)
Health Care and Social Assistance (62)	-6.0	-11.1	-10.7	-10.8
	(0.0)	(23.2)	(24.6)	(15.8)
Arts, Entertainment and Recreation (71)	-7.9	11.0	14.8	4.8
	(0.0)	(21.2)	(3.6)	(92.5)
Accommodation and Food Services (72)	2.6	-19.6	-28.5	-17.5
	(6.0)	(0.8)	(0.0)	(4.8)
Other Services (81)	8.4	-0.4	-10.8	4.8
	(0.0)	(68.5)	(20.4)	(39.0)
Full-Service Restaurants (722511)	6.7	-44.2	-55.5	-48.7
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	6.0	-38.9	-24.5	-18.8
	(0.0)	(0.2)	(4.6)	(7.6)

Notes: Average hourly wage, excluding the highest-paying 10 percent of jobs. The estimates are in log points, multiplied by 100, and represent cumulative effects of minimum wage until 2022Q4. Entries in parentheses are *p*-values using the placebo method.

Table 3 presents results for these low-wage industries and those for restaurants. In this table, Minneapolis is compared with a synthetic control consisting of cities outside of Minneapolis and Saint Paul but within Minnesota. The columns present different outcome variables. For example, the first row shows that the increase in the minimum wage in Minneapolis caused an 8.4 log points increase in the wage and a 34.2 log points 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 retail example, note that the placebo method produces a *p*-value of 0 for the wage and 0.6 for jobs, and thus we conclude

we explain in section 5, we use data from these other industries in our analysis of the cross section.

that both the wage effect and the job effect are precisely estimated. As another example, the p-value for the hours effect in the administration and support industry is 27, and thus we conclude that the hours effect is not very precisely estimated and cannot be statistically distinguished from zero at conventional levels of significance.

We estimate wage increases with low p-values for retail, accommodation and food services, other services, and restaurants. Among industries with statistically significant increases, we document increases that range between 6.0 and 8.4 log points. For all other industries, we find either statistically insignificant wage changes or small declines. We find these wage increases reasonable. The difference between the minimum wage in Minneapolis and the one in the control cities is 50 percent. 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.

Turning to the second column, we find negative job effects for retail trade and accommodation and food services. Within accommodation and food services, we find a 44 log points jobs decline for full-service restaurants and a 39 log points jobs decline for limited-service restaurants. The third column presents results for total hours. For retail trade, the estimated effects for hours are smaller than those for jobs. In contrast, for accommodation and food services and full-service restaurants, we detect a decline in hours greater than the decline in jobs. We find a statistically significant increase in hours worked in arts, entertainment, and recreation. The variation in the effects of minimum wage across industries is correlated to how exposed they are to the policy change: industries with a lower median wage in 2017 experienced more positive wage responses and more negative jobs responses.

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

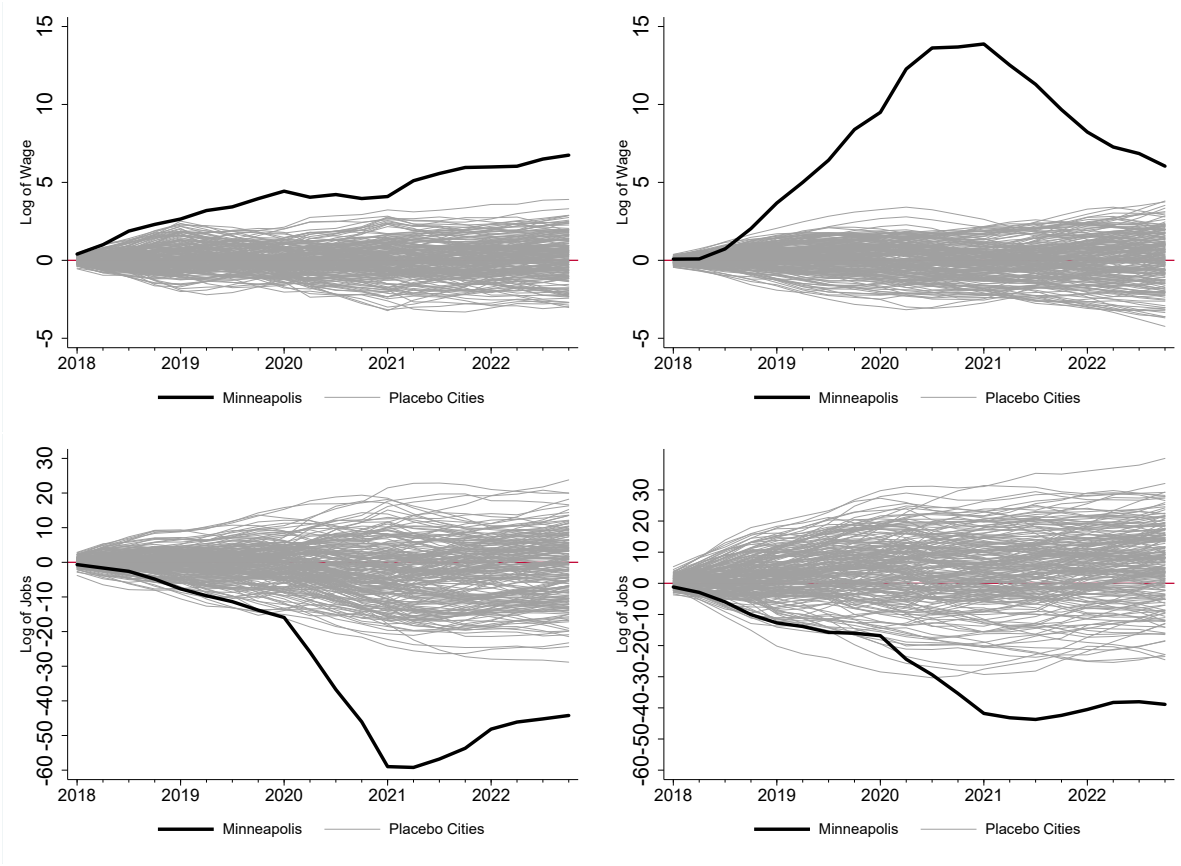
The above results describe the effects of minimum wage until 2022Q4. In the Appendix, we present the time series over the period 2001Q1 to 2022Q4 for wages and jobs in Minneapolis, for the Minnesota average of other cities, and for the synthetic control. See Figure A.1 for retail; Figure A.2 for administration and support; Figure A.3 for health care and social assistance; Figure A.4 for arts, entertainment, and recreation; Figure A.5 for accommodation and food services; Figure A.6 for other services; Figure A.7 for full-service restaurants; and Figure A.8 for limited-service restaurants.

We next examine the time variation of the estimated effects for the two industries with the most negative job effects, the restaurant industries. Figure 1 plots the quarterly cumulative wage and job effects of the minimum wage increase for full-service and limited-service restaurants in Minneapolis. Along with our estimated effects, we plot placebo effects for 1000 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 top panels of Figure 1 show that the wage increase for restaurants in Minneapolis began soon after the minimum wage ordinance went into effect. The bottom panels plot the quarterly cumulative job effects of the minimum wage increase for full-service and limited-service restaurants. While the job declines in limited-service restaurants are relatively stable over time, the job declines in full-service restaurants accelerated significantly after the first quarter of 2020, when the pandemic hit, and then stabilized from 2021Q4 on.

Our results are robust when we repeat 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 Minneapolis to neighboring cities. From the perspective 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

Figure 1: Time-Varying Effects in Minneapolis Restaurants



Full-Service Restaurants Limited-Service Restaurants

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. Table A.2 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.

Our results are also robust to an alternative method of constructing the comparison group. In the above analysis, the control group is constructed by choosing weights across geographic regions so that the weighted average across regions approximates as closely as possible Minneapolis before 2018 on the observable dimensions that are relevant for the analysis. In the

robustness analysis, time weights are used in addition to weights across geographic units. The time weights are chosen to make the control group's average pre-treatment growth as similar as possible to the average post-treatment growth. Thus, this exercise allows us to examine the robustness of our results when we place more weight on periods when the synthetic control experiences large negative growth, like the pandemic. Even when we re-weight the data, the results in Table A.3 are similar to the baseline results, though there are a few differences worth emphasizing. First, the wage gain for administration and support is now statistically significant at the 5 percent level. Second, we find that the increase in hours worked for the arts, entertainment, and recreation sector is no longer statistically significant. Finally, the earnings decline in health care and social assistance is statistically significant while we find an increase in earnings in administration and support and arts, entertainment, and recreation.

Evidence from Other U.S. Cities While some of our estimated negative job effects following the minimum wage increase in Minneapolis 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 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. For example, it may be that the enforcement and economic impact of lockdowns was larger in more densely populated cities than in smaller cities.

To address this concern, we now extend our analysis to use variation from other U.S. cities of similar size to Minneapolis. Using these cities to construct our synthetic control addresses the concern that our control from Minnesota may not be appropriate during the pandemic recession because other large, densely populated U.S. cities faced similar or more stringent lockdowns than Minneapolis. Additionally, using other U.S. cities allows us to control for nationwide changes in economic conditions such as the substitution of services prone to virus transmission with online shopping, the rise of gig work, and labor shortages in low-wage

industries.

For our analysis 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.[5] 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.

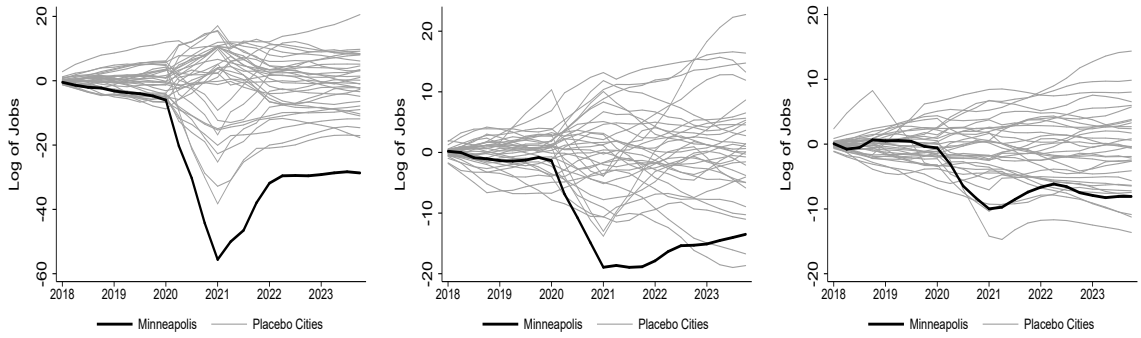
We note two differences between the research design using the QCEW data and that of our previous analyses using the DEED data. First, the QCEW does not have a measure of hours, and the wage measure differs from that in the DEED data. Thus, we analyze only jobs and not hours or wages. Second, the unit of analysis in the QCEW data is other U.S. cities of similar size to Minneapolis, whereas in the DEED data, we used zip code within a city as our unit of analysis.

Figure 2 presents our estimates from the QCEW for the restaurants and retail industries, using only the unit weights to construct the synthetic control.[6] 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, we continue to estimate negative jobs effects. The jobs declines in the sample of other U.S. cities using the

[5]Before the minimum wage increases, Minneapolis employment was roughly 280,000. We include in the control group only cities whose employment is between half and double that of Minneapolis. This restriction results in a sample of 36 cities for the control group. Table A.4 shows the U.S. cities included in the control group. 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.

[6]In Figure 2, 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 2: Time-Varying Jobs Effects, Cities with Comparable Employment

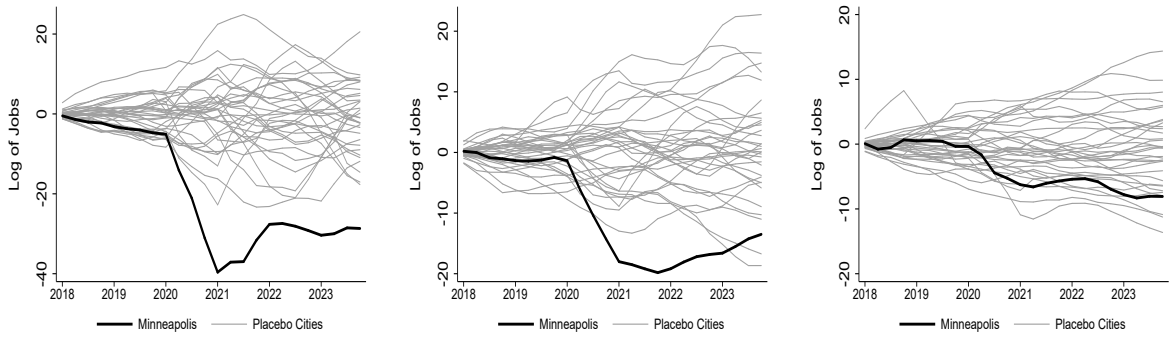


Full-service restaurants Ltd-service restaurants Retail trade

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 35 log points in the DEED. For retail, the declines are roughly 10 log points in the QCEW as opposed to 27 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 us to difference out other factors affecting jobs that are contemporaneous to the minimum wage change.

To examine more formally 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 et al. (2024) 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

Figure 3: Time-Varying Jobs Effects, Adjusted for Retail and Recreation Mobility



Full-service restaurants Ltd-service restaurants Retail trade

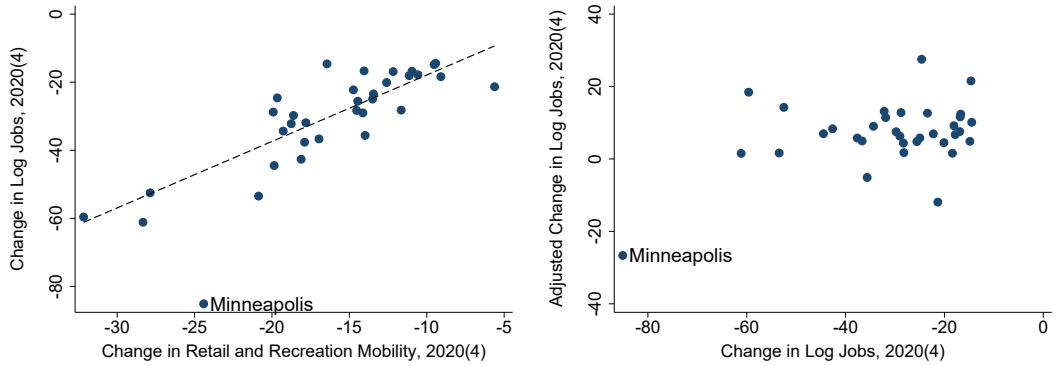
adjust for civil unrest conditions similar to those in the Twin Cities during 2020.

Our methodology of adjusting for pandemic and civil unrest conditions proceeds in three steps. First, we regress the mobility or protest variables on outcomes to obtain the direct effect of these variables on our outcome of interest. Second, we subtract this direct effect of the pandemic and protests from the outcome to get the adjusted outcomes. Third, we repeat our synthetic difference-in-differences methodology using the adjusted outcomes. Thus, our methodology allows us to examine the sensitivity of our estimates in the case that jobs for all cities, including Minneapolis, are adjusted for pandemic and civil unrest conditions.[7]

Figure 3 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 2 and the adjusted estimates in Figure 3 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 of 2021 and throughout 2022 are very similar to the unadjusted estimates, for all industries including full-service restaurants. Appendix Figures A.9, A.10, and A.11 also show the stability of our results when adjusting our estimates for workplace mobility, violent protests, and total protests.

[7] See Karabarbounis et al. (2022) for the details of this methodology.

Figure 4: Adjustments for Retail and Recreation Mobility



Why does adjusting for pandemic and civil unrest conditions make little difference for the estimated effects of the minimum wage on jobs? While Minneapolis was more exposed to pandemic restrictions and civil unrest than the synthetic control, its observed jobs decline was an outlier relative to the jobs decline 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 4 that uses as an example the case of full-service restaurants in 2020. The left panel displays 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, Minneapolis is an outlier relative to the predicted relationship between these two variables. That is, Minneapolis experienced a larger decline in jobs relative to the decline we would expect based on their reduced mobility during the pandemic and civil unrest. The right panel formalizes this by plotting that adjusted jobs decline against the observed jobs decline. Even after adjusting for pandemic and civil unrest conditions, the jobs decline is among the largest in the cross-section of U.S. cities. Appendix Figures A.12, A.13, and A.14 illustrate that the same result applies when using the other three indicators to adjust for pandemic and civil unrest conditions.

5 Cross-Section Analysis: Establishment Effects

In this section, we use variation from the cross sections of establishments within a city to examine how differential exposure to the minimum wage change affects their outcomes. Unlike our analysis from the time series, which focuses on industries with a high share of affected workers, our analysis in this section includes all industries in our sample. This is appropriate, because even within industries that are relatively less exposed to the minimum wage, there exist establishments with high exposure to the minimum wage.

Our measure of establishments' exposure to the minimum wage is the increase in labor costs they need to incur to adhere to the target increase in minimum wage. More specifically, we measure the exposure of each establishment to the minimum wage policy with the increase in its labor cost from having to pay all workers at least 15 dollars, adjusted for inflation. We call this difference for an establishment j in sector s in zip code z its GAP_{jsz}.[8] In our regressions, our GAP measure is calculated in 2017.

We estimate how differential exposure of establishments to the minimum wage affects their wage, jobs, hours, and workers' earnings. The establishments we include in our sample are located only within Minneapolis and have to exist in the sample in 2017 and all the way up to 2022. Our strategy exploits variation across establishments in exposure to the minimum wage within industry s, within zip code z, and within year t. This is implemented by including a sector–zip code–time fixed effect term in the regression of the outcomes on the GAP measure. The fixed effect allows us to capture any common shock, such as the pandemic recession or civil unrest, shared by these establishments. [9]

[8] See our previous report for more details (Karabarbounis et al. (2023)).

[9] In our cross-sectional analysis, we control for establishment dynamics unrelated to exposure that may introduce a spurious correlation between exposure and various outcomes. For example, smaller establishments pay lower wages and thus have larger gaps. At the same time, smaller establishments tend to exit at faster rates, which may exert a negative effect for jobs, hours, and earnings. If we do not take into account these establishment dynamics, we may incorrectly attribute the observed effects to the variation in exposure. In order to control for these dynamics, we include a term for exposure in the period before minimum wage was implemented. In our baseline estimates, our period of analysis begins in 2010, providing us with seven years of data before the policy was implemented.

We measure the outcomes as percent changes with respect to 2017. Specifically, we use the arc percent change, which is defined as $Y_{jszt} = \frac{y_{jszt} - y_{jsz,2017}}{(1/2)(y_{jszt} + y_{jsz,2017})}$. Comparing each post-treatment period to 2017 allows us to examine the short run and the longer run effects of the minimum wage. Note that in previous reports, our focus was on three year arc percent changes and we examined how it changed over time. If we used the three year differences to examine the effect of the minimum wage period in 2022, then we would be looking at changes between two years that are both in the post policy period. Since this would not be a meaningful measure, we present results where outcomes are measured using percentage changes with respect to 2017. For continuity, we also present the results until 2020 for specifications where outcomes are measured as three-year arc percent changes.

Table 4 presents estimates for effects of exposure to the minimum wage increase on wage, jobs, hours, and earnings. The entries 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 a 100 percent increase in its wage bill due to the minimum wage.[10] The maximum GAP is around 100 percent, and the average GAP is around 4 percent. We will later use these statistics 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 percentages associated with each coefficient. We cluster standard errors at the establishment level.

The top panel of Table 4 presents the estimates when the arc percent changes in outcomes are measured with respect to 2017 while the bottom panel presents the estimates when the outcome is measured in three year arc percent changes. In 2018, the effect of exposure to minimum wage on one year change in wages was 6.6 percent while the effect on the three

[10] Our sample includes many establishments with a zero GAP. The average outcome of these establishments is absorbed in the estimates of the fixed effect constant. 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 our main regression specification, we have repeated our regressions by excluding establishments with a zero GAP. We find no significant differences in our results.

Table 4: Labor Market Effects of Minimum Wage Increases: Cross Section of Establishments

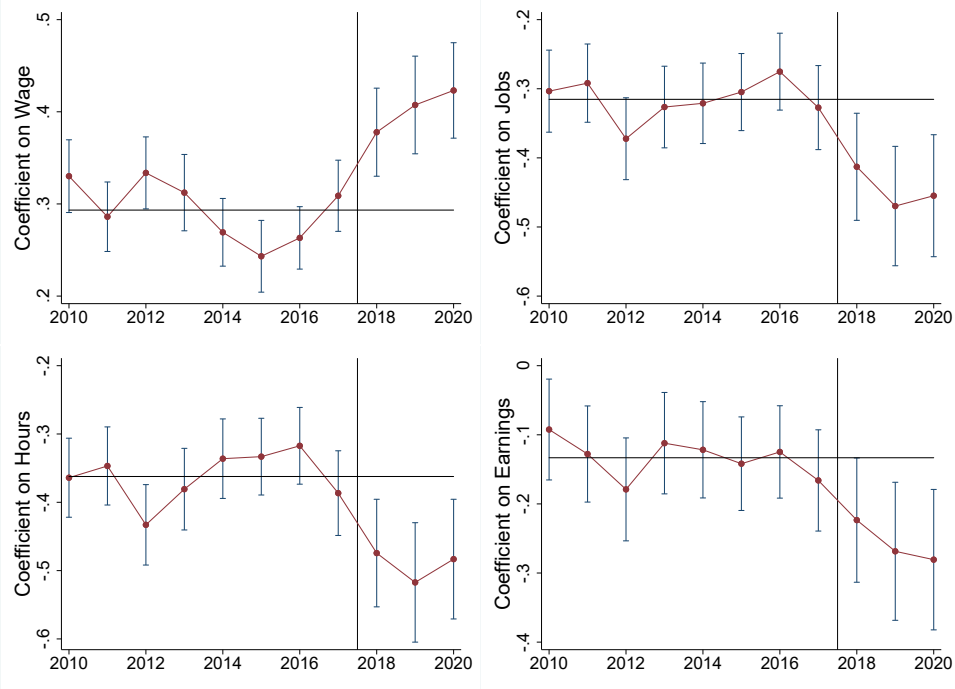
Change Since 2017	Wage	Jobs	Hours	Earnings
2017 - 2018	6.6	−5.3	−4.4	−2.1
	(0.1)	(19.6)	(30.7)	(64.7)
2017 - 2019	7.4	−3.6	−3.5	−1.3
	(0.7)	(48.6)	(50.9)	(82.2)
2017 - 2020	12.5	−13.4	−12.2	−13.7
	(0.0)	(1.3)	(2.5)	(3.0)
2017 - 2021	15.9	−15.9	−15.4	−18.0
	(0.0)	(0.7)	(0.9)	(1.0)
2017 - 2022	17.6	−20.8	−19.3	−23.1
	(0.0)	(0.1)	(0.3)	(0.3)
Three-Year Changes Over Time	Wage	Jobs	Hours	Earnings
2015 - 2018	8.0	−9.2	−11.3	−8.0
	(0.5)	(4.4)	(1.5)	(13.9)
2016 - 2019	11.0	−14.9	−15.6	−12.5
	(0.1)	(0.6)	(0.4)	(4.7)
2017 - 2020	12.5	−13.4	−12.2	−13.7
	(0.0)	(1.3)	(2.5)	(3.0)

Notes: The estimates are in percentages. Entries in parentheses are *p*-values using standard errors clustered at the establishment level.

year change was 8 percent. While the effects on jobs and hours are small and statistically insignificant if we look at a one year change between 2017 and 2018, the bottom panel shows that declines for three year changes in jobs and hours are statistically significant at 9 percent and 11 percent respectively between 2015 and 2018. We find a similar pattern for 2019 estimates.

The 2022 estimates from the top panel of Table 4 show that wages increased significantly by 18 percent between 2017 and 2022. We also find statistically significant declines in jobs, hours, and earnings at 21 percent, 19 percent, and 23 percent respectively.

Figure 5: Cross-Sectional Responses Over Time: Three-Year Changes



Notes: The figure shows estimates for an event analysis regression together with 95 percent confident intervals. The horizontal line represents the average of the estimated coefficients between 2010 and 2017.

The estimated responses of the wage, jobs, hours, and earnings are above and beyond those generated by typical establishment dynamics because our regression 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 estimated coefficients for three year changes to vary over time for all periods. Figure 5 shows that there is no noticeable trend in these estimated coefficients before the minimum wage increase. The coefficients for all outcomes in 2018, 2019, and 2020 are statistically different from the average coefficient before the minimum wage increase, as indicated by the horizontal line. Note that the estimated coefficients on all variables are quite stable between 2019 and 2020. We find the stability of the estimated coefficients reassuring and conclude that our identification strategy from the cross section of establishments isolates differential exposure to the minimum wage rather than other forces contemporaneous with

the minimum wage change.

We provide two robustness checks to our results. The top panel of Table A.5 presents estimated coefficients in a regression 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, where magnitudes are lower. In the lower panel of Table A.5, we include three additional years of data before the minimum wage increase to get a longer pre-period and find similar results.

6 Cross-Section Analysis: Worker Effects

As outlined in our previous report, 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 moved from high to low GAP establishments, then we would be double-counting the effects of the minimum wage increase on establishments' employment. Another challenge arises from reallocation outside of Minneapolis, because our estimates could reveal negative employment effects from the minimum wage even if all affected workers find jobs outside of Minneapolis. Thus, while our estimates directly speak to the outcomes of establishments that were located in Minneapolis 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.

We use variation in wage gaps across workers and track workers' outcomes directly over time, irrespective of whether workers moved to other establishments in or outside of Minneapolis. The treatment is defined at the establishment-level; we regress worker-level outcomes on their establishments' gaps instead of workers' own gaps. This is because using worker-level gaps may lead to a concern that any differences in employment effects may cap-

Table 5: Labor Market Effects of Minimum Wage Increases: Cross Section of Workers

Change Since 2017	Wage	Hours	Earnings
2017 - 2018	1.6	−7.9	−8.2
	(10.9)	(1.0)	(1.0)
2017 - 2019	4.3	−10.2	−8.0
	(0.0)	(0.1)	(1.1)
2017 - 2020	−2.2	−11.7	−10.8
	(18.8)	(0.0)	(0.1)
2017 - 2021	8.7	−4.5	−2.4
	(0.0)	(12.4)	(43.8)
2017 - 2022	15.1	2.7	8.4
	(0.0)	(36.8)	(0.8)

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

ture low-wage workers' difficulty in finding jobs due to the pandemic or civil unrest, rather than their difficulty in finding jobs due to the minimum wage.

Our analysis now includes the fixed effects at the industry-time level, and this intercept absorbs all time effects common to workers belonging to the same industry. We allow the workers to work multiple jobs across sectors and geographies. Compared with the establishment-level regressions, this approach results in two differences in the intercept: (1) the fixed effects do not include the within-zip-code variation, and (2) we interact the fixed effects intercept with the share of workers' employment in the industry. The other difference relative to our specification for establishments is that now we include in the regression the lagged outcome for workers. Thus, we interpret the coefficients as the percent change in worker outcomes resulting from a higher exposure for workers with the same growth rate in the period immediately preceding the wage increase and after differencing out the common effect that workers in the same industry experience, and any effects we would detect because of typical worker

dynamics.

Table 5 presents estimates of the regression coefficients applied to wages, hours, and earnings.[11] We measure the outcomes as percent changes with respect to 2017. For 2018, 2019, and 2020, we find a statistically significant decline with respect to hours and earnings but, except for 2019, an insignificant increase in workers' wage. In 2022, the magnitude of the wage increase is comparable to the establishment-level regressions while we do not find comparable declines in hours and earnings. In fact, we find an increase in workers' earnings in 2022.

7 Summary of Estimates

Table 6 summarizes our estimates. In the first row, we present the average hourly wage increases in Minneapolis in 2022. The average of time series estimates shows a wage decline of 0.1 percent. We calculate this number as the average wage increase across all two-digit industries, where wage gains are weighted with the employment share of the corresponding industry in total Minneapolis employment before the minimum wage increase. We include only industries with statistically significant changes in wages. The fourth column presents the estimate of average wage gains using variation from the cross section. The average estimate is 0.7 percent. We calculate this number by multiplying the 2022 average of the coefficients from establishments and workers regressions (roughly 12 percent) with the employment-weighted average GAP (roughly 4 percent) in Minneapolis in 2022. The fifth column takes a simple average of the average time series estimate in column three and the average cross-section estimate in column four. Similar calculations are made for average jobs, average hours worked, and average earnings estimates in the second, third, and fourth rows, respectively.

For each outcome, "most exposed" summarizes estimates from the industries and estab-

[11] As in the time series analysis, we exclude workers with a wage below the youth minimum wage for Minnesota. For the worker-level analysis, we include only workers with a wage below 45 dollars per hour and run the regression at the yearly frequency.

Table 6: Effects of Minimum Wage Increases: Summary of Estimates (Percentages)

Outcome in 2022 (1)	Estimate Type (2)	Time Series (3)	Cross Section (4)	Average (5)
Hourly Wages	Average	-0.1	0.7	0.3
	Most Exposed	1.9	15.7	8.8
Jobs	Average	-1.9	-0.9	-1.4
	Most Exposed	-59.5	-20.0	-39.7
Hours Worked	Average	-1.1	-0.4	-0.8
	Most Exposed	-55.3	-8.0	-31.6
Wage Earnings	Average	-0.9	-0.3	-0.6
	Most Exposed	-38.3	-7.1	-22.7

Notes: Average from the time series includes only industries with statistically significant changes, weighted by employment shares. "Most Exposed" from the time series uses the estimates for the restaurant industries. The estimates for the cross section multiply the 2022 coefficients from the establishments' and workers' regressions with the weighted average and maximum GAP measure.

lishments that were most exposed to the minimum wage increase. For the time series, we use the estimates for restaurants and conclude that the largest wage gains are 1.9 percent. For the cross section, we multiply the 2022 average of the coefficients from the establishments' and workers' regressions with the maximum GAP in 2022. We use the maximum GAP so that we can get a comparable estimate of the largest wage gains effects. This yields an estimated wage gain of 15.7 percent. The fifth column takes a simple average of the time series estimate in column three and the cross-section estimates in column four. Similar calculations are made for jobs, hours worked, and earnings estimates in the second, third, and fourth rows, respectively.

Reconciling the Time Series with the Cross Section. Table 6 shows that the time series estimates are generally larger in magnitude than the cross-sectional estimates. There are three reasons why this is the case.[12] First, the time series effects of the minimum wage on employment are at the industry level and sum up employment effects at the intensive margin (existing establishments hiring fewer workers), effects arising from the exit of establishments, and effects arising from a reduction in the 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.

Second, 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. 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 increase on employment.

Finally, despite our efforts to difference out other shocks, Minneapolis 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 address this directly by adjusting our outcomes for mobility and protest measures; and find that the adjusted estimates by the end of 2021 and throughout 2022 are very similar to the unadjusted estimates. The cross-sectional estimates do not suffer from this concern, to the extent that Minneapolis shocks are differenced out across establishments in the same industry and zip code or across workers in the same industry.

[12] See Karabarbounis, Lise, and Nath (2022) for a technical discussion of why time series estimates differ from those that use variation from the cross section.

A Appendix

A.1 Additional Figures

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

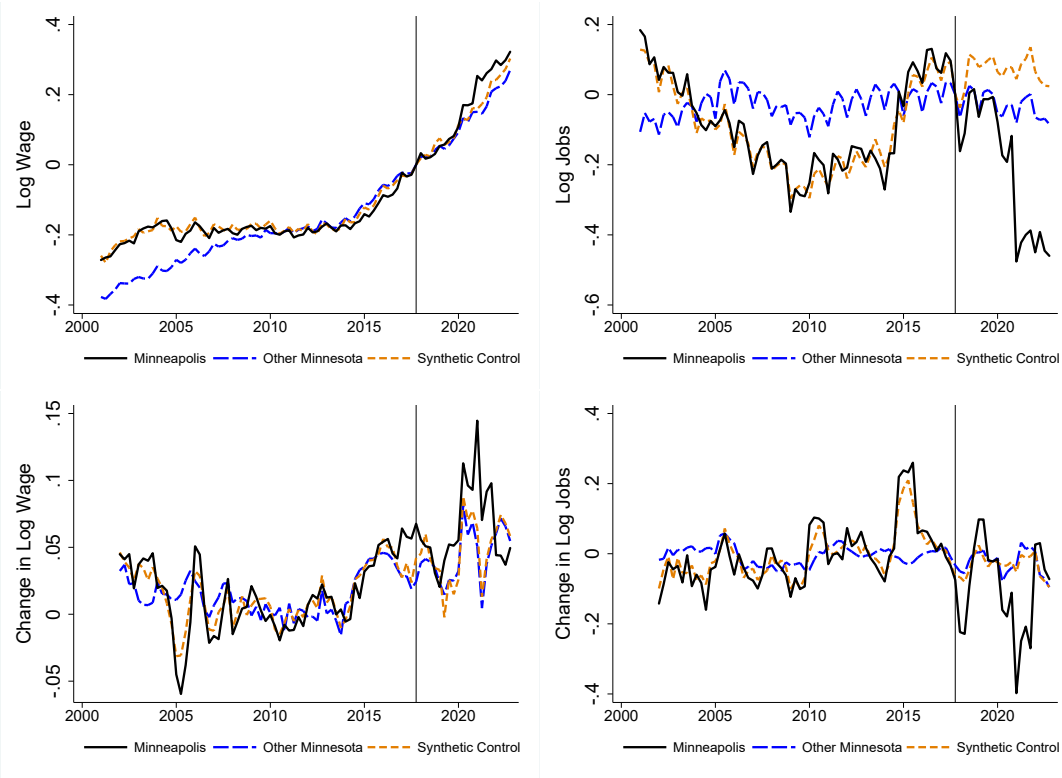


Figure A.2: Time Series of Administration and Support in Minneapolis

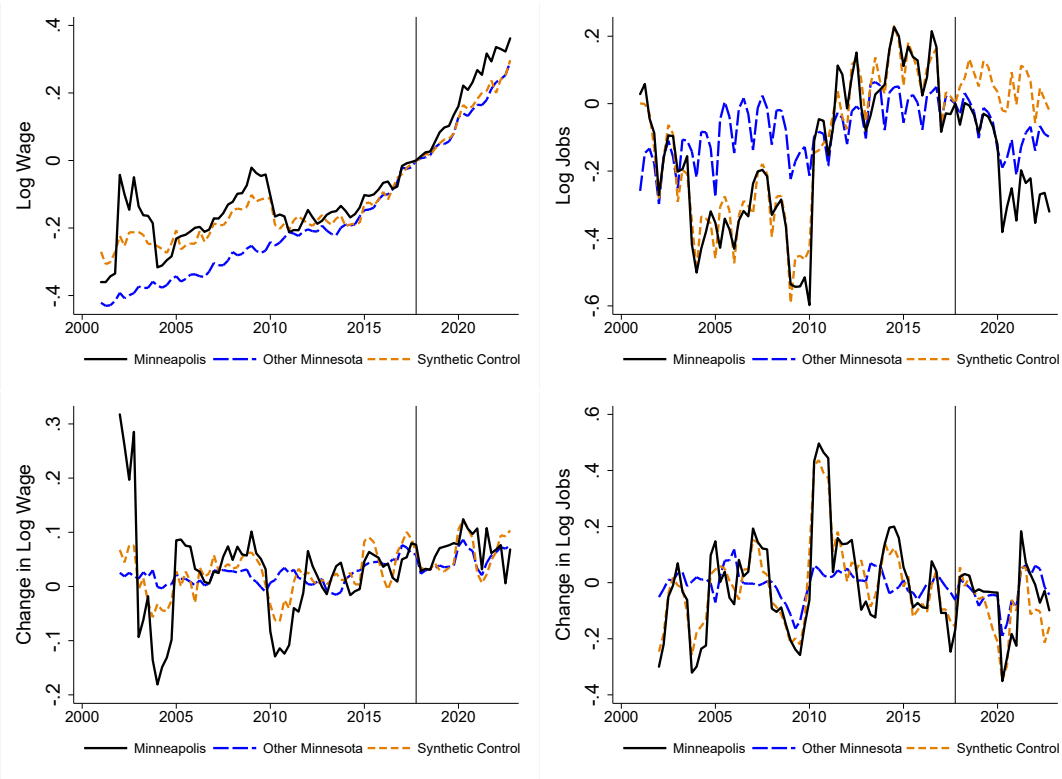


Figure A.3: Time Series of Health Care and Social Assistance in Minneapolis

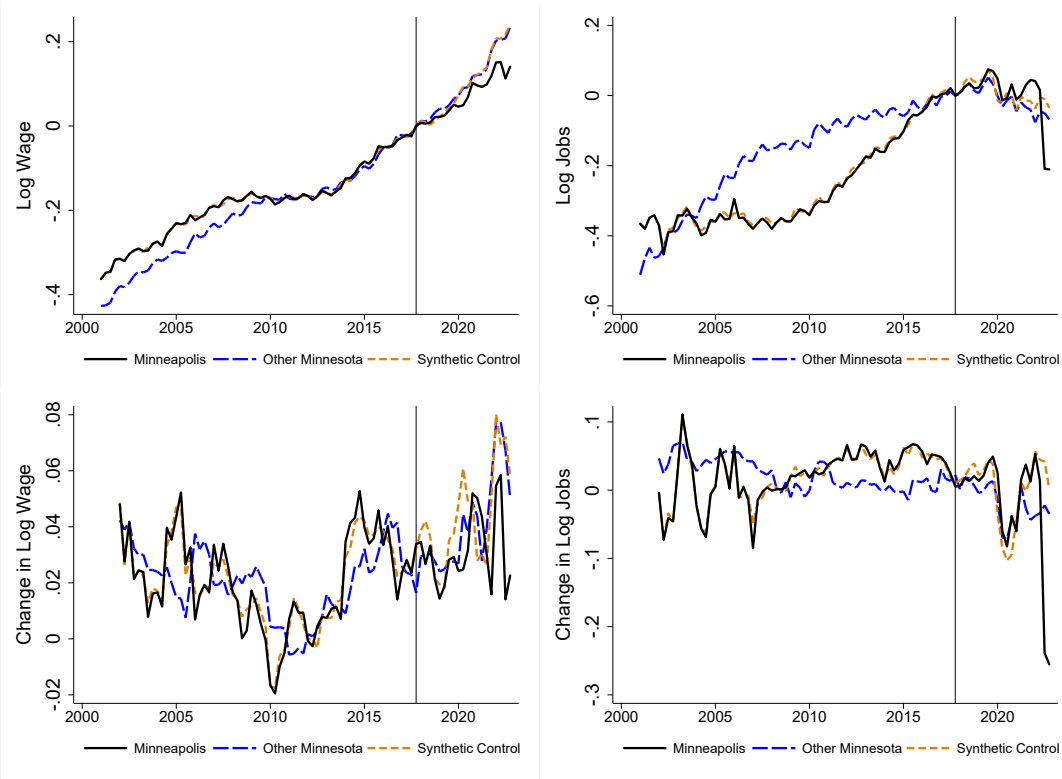


Figure A.4: Time Series of Arts, Entertainment, and Recreation in Minneapolis

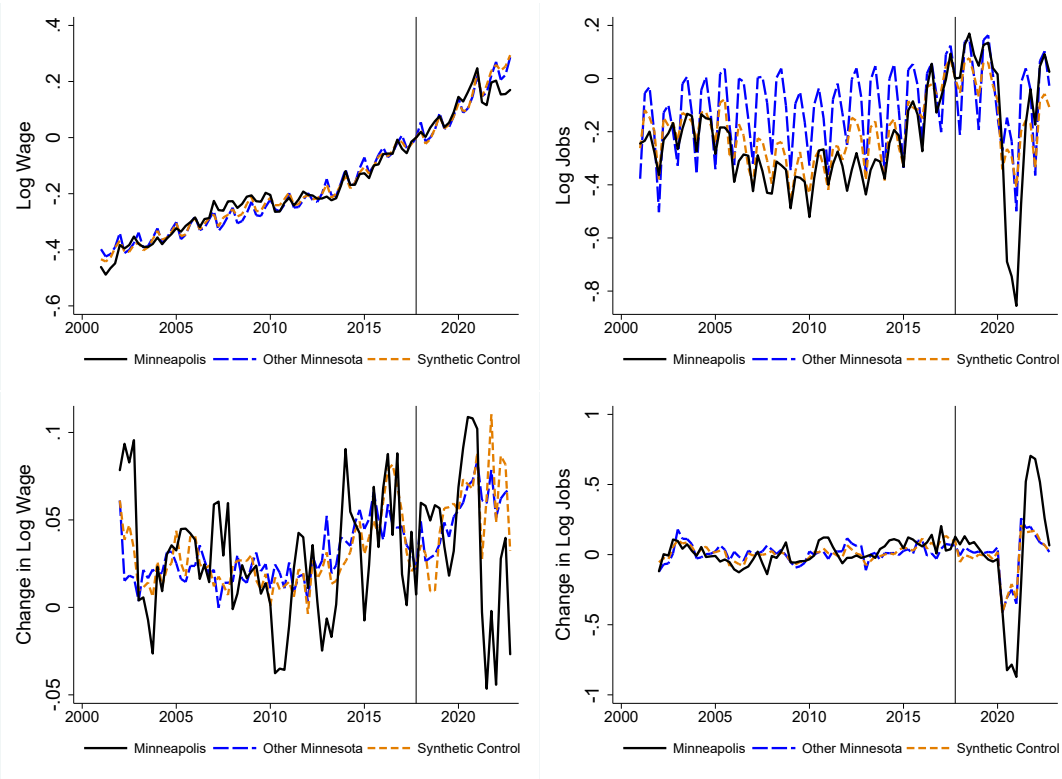


Figure A.5: Time Series of Accommodation and Food Services in Minneapolis

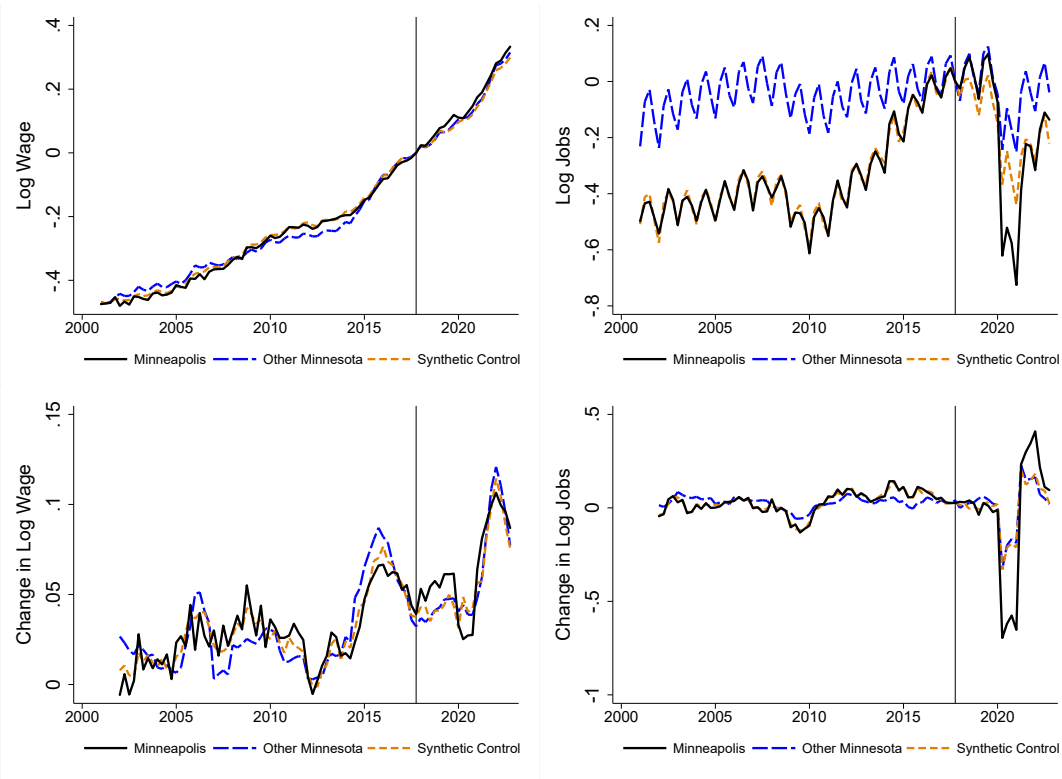


Figure A.6: Time Series of Other Services in Minneapolis

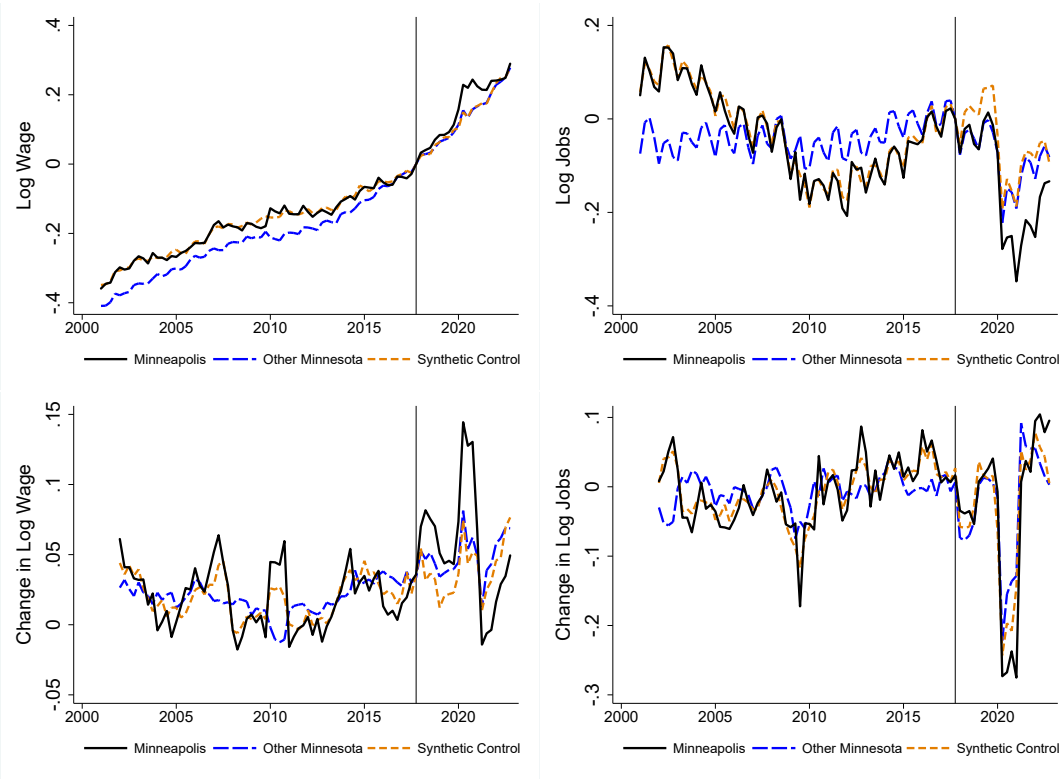


Figure A.7: Time Series of Full-Service Restaurants in Minneapolis

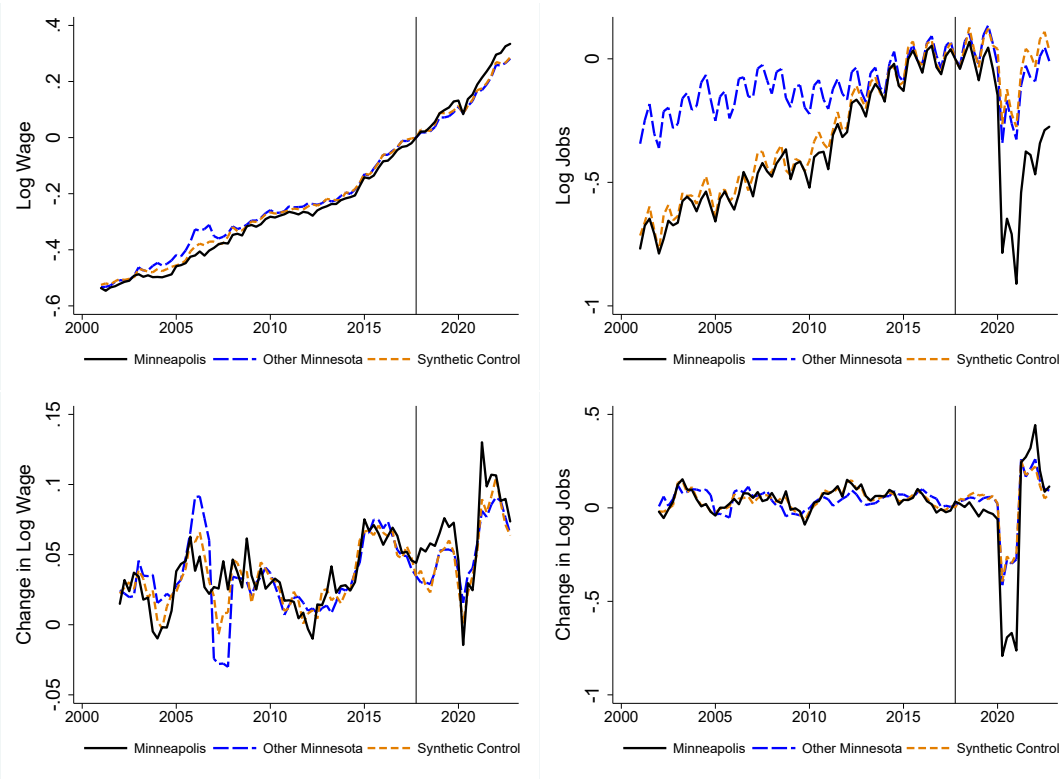


Figure A.8: Time Series of Limited-Service Restaurants in Minneapolis

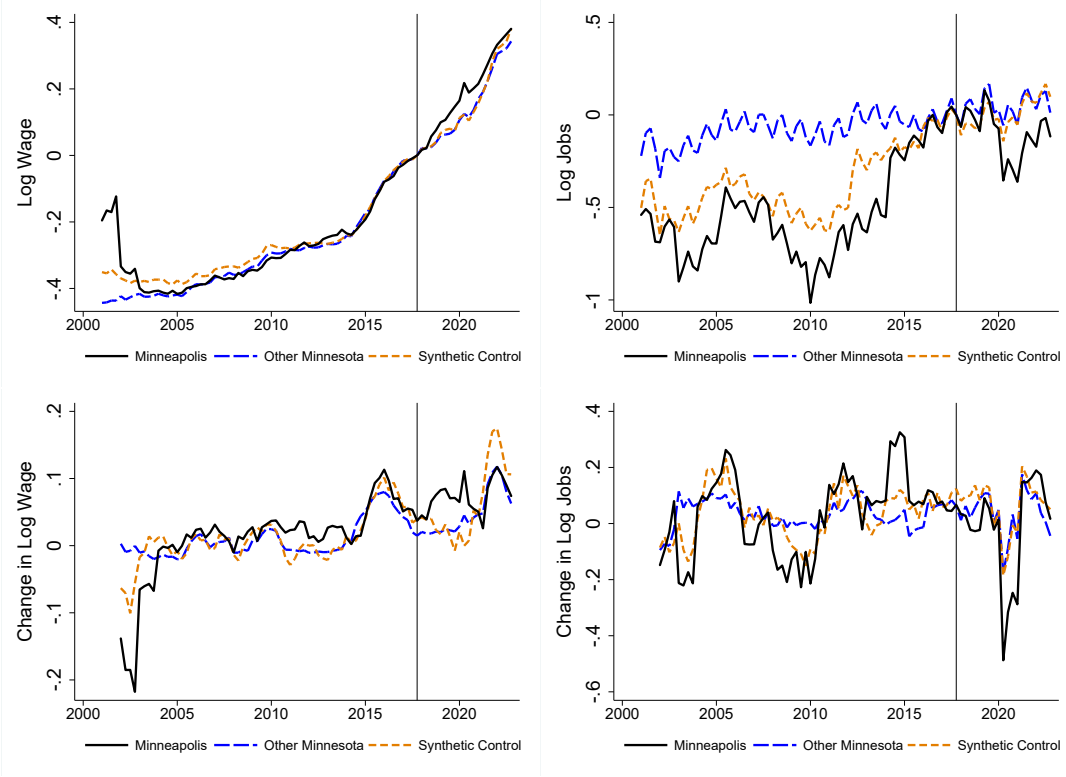
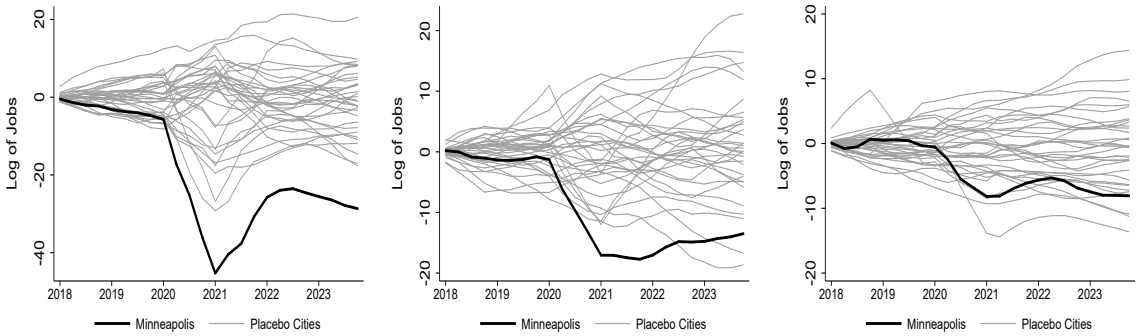
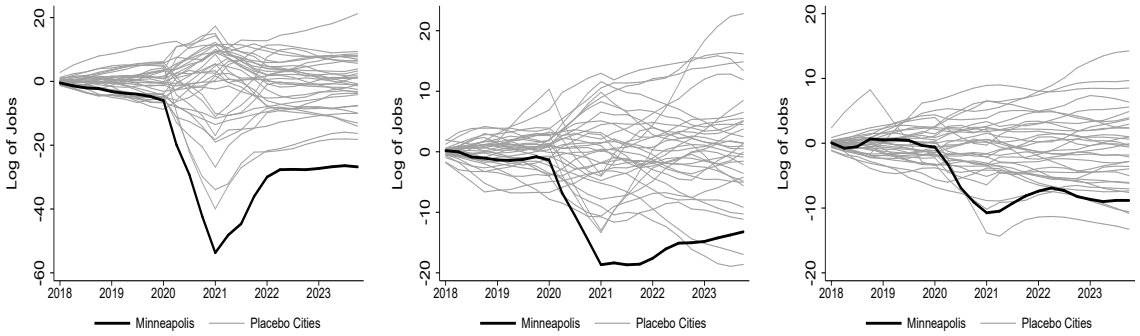


Figure A.9: Time-Varying Jobs Effects, Adjusted for Workplace Mobility



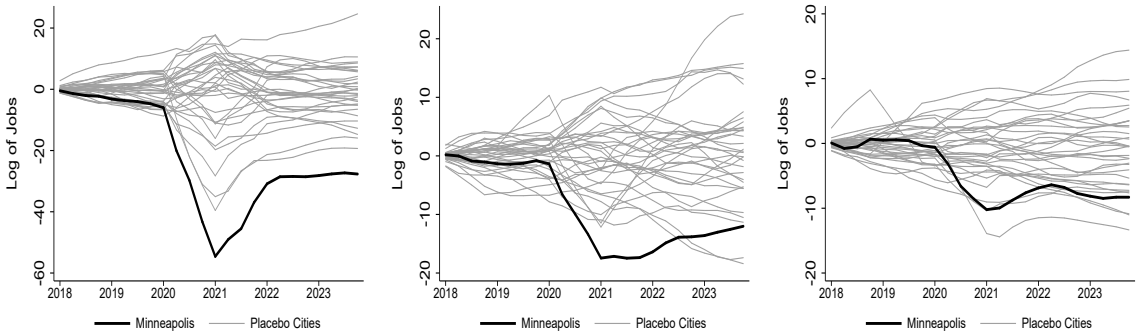
Full-service restaurants Ltd-service restaurants Retail trade

Figure A.10: Time-Varying Jobs Effects, Adjusted for Violent Protests



Full-service restaurants Ltd-service restaurants Retail trade

Figure A.11: Time-Varying Jobs Effects, Adjusted for Total Protests



Full-service restaurants Ltd-service restaurants Retail trade

Figure A.12: Adjustments for Workplace Mobility

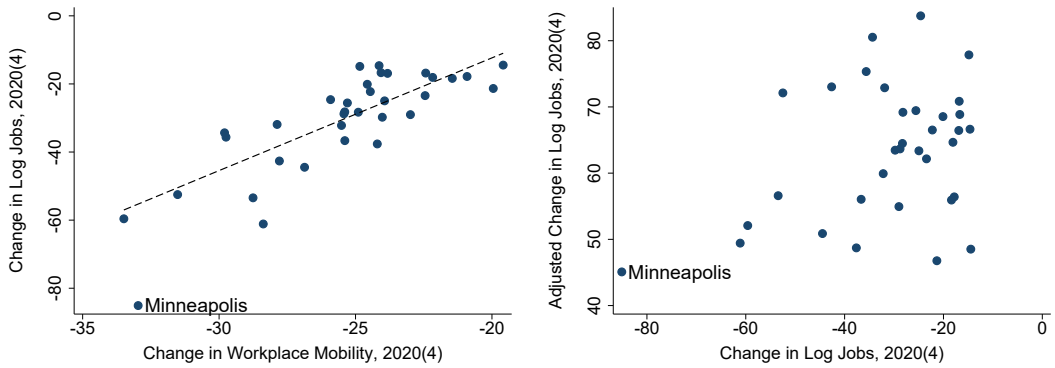


Figure A.13: Adjustments for Violent Protests

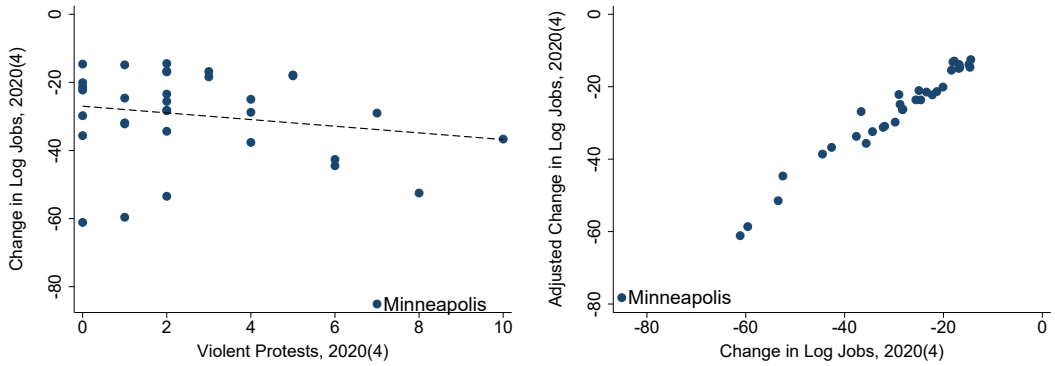
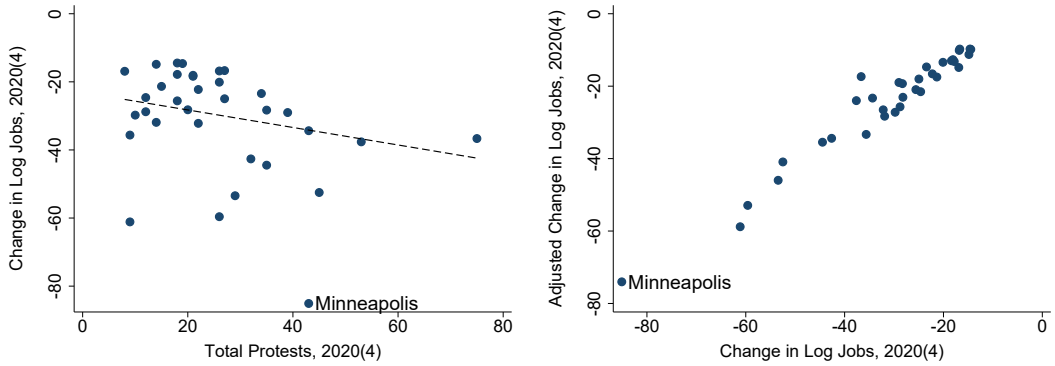


Figure A.14: Adjustments for Total Protests



A.2 Additional Tables

Table A.1: Pre-Treatment Fit: Synthetic Control versus Differences-in-Differences

	Wage		Jobs		Hours		Earnings	
(R-squared, percent)	SC	DD	SC	DD	SC	DD	SC	DD
Retail Trade (44)	84	30	86	0	80	6	69	1
Administration and Support (56)	52	3	88	14	76	14	83	20
Health Care and Social Assistance (62)	94	25	96	7	88	17	94	8
Arts, Entertainment and Recreation (71)	31	8	40	8	32	11	19	6
Accommodation and Food Services (72)	86	57	96	44	96	33	97	58
Other Services (81)	68	1	82	5	83	4	87	16
Full-Service Restaurants (722511)	65	31	88	24	85	36	82	23
Limited-Service Restaurants (722513)	63	30	58	9	54	3	51	3

Notes: Average hourly wage, excluding the highest-paying 10 percent of jobs. SC: synthetic control. DD: difference-in-differences.

Table A.2: Excluding Neighboring Cities from the Control Group

	Wage	Jobs	Hours	Earnings
Retail Trade (44)	8.8	−33.2	−20.1	−24.7
	(0.0)	(1.6)	(2.4)	(3.6)
Administration and Support (56)	7.0	21.2	25.9	21.4
	(4.2)	(17.8)	(14.0)	(20.0)
Health Care and Social Assistance (62)	−7.3	−10.4	−14.1	−10.3
	(0.0)	(27.8)	(11.6)	(18.4)
Arts, Entertainment and Recreation (71)	−7.7	10.3	15.9	7.7
	(0.0)	(35.4)	(2.6)	(94.1)
Accommodation and Food Services (72)	2.5	−26.6	−15.5	−18.4
	(11.4)	(0.0)	(4.6)	(4.2)
Other Services (81)	8.3	6.9	−9.5	3.6
	(0.0)	(18.6)	(24.4)	(45.6)
Full-Service Restaurants (722511)	6.7	−40.3	−44.3	−45.1
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	6.0	−38.9	−24.5	−18.8
	(0.0)	(0.2)	(5.0)	(6.0)

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

Table A.3: Adding Time Weights in the Synthetic Difference-in-Differences Estimation

	Wage	Jobs	Hours	Earnings
Retail Trade (44)	10.4	−41.0	−32.6	−25.4
	(0.0)	(0.0)	(0.0)	(0.0)
Administration and Support (56)	9.9	12.9	23.8	24.2
	(0.0)	(41.2)	(13.0)	(10.0)
Health Care and Social Assistance (62)	−3.9	−0.7	−6.3	−10.6
	(1.4)	(99.7)	(45.6)	(9.4)
Arts, Entertainment and Recreation (71)	−8.8	15.7	16.4	40.9
	(3.0)	(56.3)	(33.0)	(5.6)
Accommodation and Food Services (72)	2.9	−17.4	−24.2	−27.6
	(0.6)	(0.8)	(0.0)	(0.0)
Other Services (81)	4.7	−3.4	−8.2	−7.0
	(1.6)	(74.5)	(31.0)	(52.1)
Full-Service Restaurants (722511)	5.7	−49.8	−48.1	−40.7
	(0.0)	(0.0)	(0.0)	(0.0)
Limited-Service Restaurants (722513)	6.8	7.9	22.3	10.7
	(0.0)	(73.5)	(10.8)	(49.0)

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

Table A.4: Cities of Similar Size to Minneapolis

City	Jobs (000's)	City	Jobs (000's)
Washington, DC	533	Baltimore, MD	276
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
Portland, OR	433	Wichita, KS	220
Louisville, KY	425	Little Rock, AR	201
Montgomery County, MD	380	St. Louis, MO	197
Honolulu, HI	380	Reno, NV	193
Oklahoma City, OK	374	New Orleans, LA	170
Tulsa, OK	322	Fort Wayne, IN	169
Kansas City, MO	314	Winston-Salem, NC	167
Fresno, CA	310	Lexington, KY	159
Omaha, NE	301	Huntsville, AL	155
Tucson, AZ	299	Virginia Beach, VA	149
Aurora, CO	295	Springfield, MO	147
Minneapolis, MN	**280**		

Table A.5: Robustness: Labor Market Effects from Cross Section of Establishments

Add Lagged Growth	Wage	Jobs	Hours	Earnings
2017 - 2018	3.5	−5.2	−4.4	−1.9
	(8.6)	(20.1)	(31.0)	(68.8)
2017 - 2019	−0.1	−3.3	−3.4	−0.2
	(97.9)	(51.7)	(52.4)	(96.7)
2017 - 2020	5.8	−13.2	−12	−13.7
	(11.4)	(1.5)	(2.7)	(2.7)
2017 - 2021	11.5	−15.7	−15.2	−16.9
	(0.9)	(0.8)	(1.0)	(1.5)
2017 - 2022	14.5	−20.7	−19.1	−21.5
	(0.8)	(0.1)	(0.4)	(0.6)
Extended Pre-Sample	Wage	Jobs	Hours	Earnings
2017 - 2018	5.9	−5.2	−4.2	−2.8
	(0.3)	(20.3)	(32.7)	(54.8)
2017 - 2019	7.3	−4.1	−3.8	−2.1
	(0.7)	(42.4)	(47.8)	(72.6)
2017 - 2020	11.9	−13.3	−11.5	−14.0
	(0.0)	(1.4)	(3.4)	(2.6)
2017 - 2021	14.8	−14.8	−13.7	−17.9
	(0.0)	(1.2)	(2.1)	(1.0)
2017 - 2022	15.3	−18.3	−16.4	−21.7
	(0.0)	(0.3)	(1.0)	(0.4)

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

B Acknowledgments

We thank Evan Cunningham for excellent research assistance; Marc Nelson, Brooke Tosi, and Toua Vang for technical and organizational support; and Oriane Casale, Mustapha Hammida, and Steve Hine for help with the administrative data used in this paper. All results have been reviewed by the Minnesota Department of Employment and Economic Development to ensure that no confidential information has been revealed. Any errors are the authors' sole responsibility.

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