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

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FEDERAL RESERVE BANK
OF MINNEAPOLIS

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1 Executive Summary

Key Findings

The minimum wage in Minneapolis increased by 50 percent from 2017 to 2021. Our analysis finds that this increase resulted in higher wages and lower employment. Specifically, by 2021Q4, the increase in the minimum wage was associated with an average increase in hourly wages of 0.7 percent, an average decline in jobs of 1.7 percent, an average decline in hours worked of 1.3 percent, and an average decline in wage earnings of 1 percent. The largest effects are found in the restaurant and the retail industries, in lower-paying establishments, and for lower-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 2021Q4. The administrative data for our analyses come from the Department of Employment and Economic Development (DEED). This report updates results discussed in the 2021 report and will be followed by five 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? Relative to our first report, this report uses two more years of data and adds new methods to examine the labor market effects of higher minimum wages. The longer data series allows us to examine the effects of the policy change as the economy was recovering from the pandemic in 2021. In addition, while our previous estimates were obtained from the comparison of trends in Minneapolis with trends in other cities within Minnesota that did not experience a minimum wage increase, in this report, we discuss estimates from three new approaches. First, we present results from an analysis that compares Minneapolis with other cities in the United States with comparable size and exposure to the pandemic but no changes in their minimum wage policies. Second, we look at Minneapolis establishments that belong to the same industry and zip code at a point in time to ascertain how differences in their exposure to the minimum wage increases affected their labor market outcomes. Third, we follow workers over time to examine how workers' exposure to the minimum wage increases affected their labor market outcomes.

Our aggregate estimates suggest that higher minimum wages increased wages and lowered employment. 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 following the murder of George Floyd. 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 of the Study

The City of Minneapolis commissioned a study of the economic impacts of the minimum wage ordinance passed in 2017. The phased implementation of the minimum wage ordinance began in 2018. The minimum wage reached 15 dollars in July 2022 for large firms and is scheduled to reach 15 dollars in July 2024 for small firms. The principal investigators of the study, hosted by the Federal Reserve Bank of Minneapolis, are providing to the City

Table 1: Minimum Wage Changes in Minnesota 2000-2020 (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^*

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.

of Minneapolis the impact evaluation results for the 2018, 2019, 2020, and 2021 minimum wage increases.

Minnesota first introduced a statewide minimum wage in 1974 and has since updated the wage floor periodically. In the period of our study (2001-2021), the latest policy-driven increase in the state minimum wage was in August 2014. The minimum wage rate was set to increase in stages; the first was in August 2014 and set the rate to 6.5 dollars for small firms and youth employees and to 8 dollars for large firms. Small firms are defined as ones earning an annual revenue less than 500,000 dollars, and large firms are ones that earn an annual revenue higher than this threshold. The rates were set to eventually reach 7.75 and 9.5 dollars per hour by 2016 for small and large firms, respectively.[1] From 2018 onward, the rate was indexed to the price deflator for personal consumption expenditure, with annual increases capped at 2.5 percent of the previous rate. Table 1 provides the details of these changes over time.

[1]Gratuities are not applied to the minimum wage, implying that employers have to pay their employees a wage rate above minimum wage before tips. The Minneapolis minimum wage ordinance adopted a similar policy with respect to gratuities.

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.

After the 2014 increase in the statewide minimum wage, the City of Minneapolis began discussing raising the city minimum wage to 15 dollars per hour. In 2016, the mayor announced support for a city-wide minimum wage hike, the first major step toward a policy change. In 2017, the Minneapolis City Council passed a minimum wage ordinance that aimed to increase the minimum wage rate to 15 dollars. This increase was set to be implemented in phases starting in 2018 that would reach 15 dollars in July 2022 for large firms and in July 2024 for small firms. Unlike the definition of firm size used by the State of Minnesota, which is based on revenues, the Minneapolis ordinance's definition is based on employment. A firm is defined as "small" if it employs fewer than 100 persons and "large" if it employs 100 or more. The details of the phased implementation of the 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 these 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 Scope of the Study

This report examines the labor market effects of the minimum wage ordinance in Minneapolis up to 2021Q4. 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 outcomes 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 second 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.

4 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.[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 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 2021Q4. Our geographic unit of analysis is a zip code within a city. This allows the same zip code to be affected differently by the treatment if the zip code 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 average wages, aggregate number of jobs (sum of full-time and part-time jobs), aggregate hours, and aggregate worker earnings paid within geographic units for each quarter. Finally, we aggregate all units that have fewer 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 are able to create a dataset on workers' hours and wages, as well as the establishments at which they are employed, by industry, zip code, and city. Our dataset improves

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

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.[3] 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.

Table 3 reports the industry distribution of employment shares and the fraction of workers earning below 15 dollars in 2017 by industry.[4] There are six industries in which 30 percent or more of workers earn below 15 dollars per hour: 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 (81).[5] We also show the composition of full-service and limited-service restaurants, as requested by the City. Restaurants account for 6 percent of total employment and have a high fraction of potentially impacted workers.[6]

[3]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]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).

[5]"Other services" consists of repair and maintenance shops, personal and laundry services, and various civic, professional, and religious organizations.

[6]The fraction of workers earning below 15 dollars reported in Table 3 for the restaurant industries is a lower bound for the fraction of workers who are affected by the minimum wage increase. This is because the wages reported to DEED include tips, and the minimum wage ordinance excludes tips.

Table 3: Employment Shares and Fraction of Workers Earning below 15 Dollars

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

Note: "MPLS" denotes Minneapolis and "Other MN" denotes the sum of all other cities in Minnesota except for Minneapolis and Saint Paul.

5 Time Series Analysis: Aggregate Effects

At the core of any policy evaluation lies the fundamental problem of causal inference. In this section, we use a standard approach in the minimum wage literature that exploits time series variation to estimate the effects of minimum wage increase. It compares outcomes in Minneapolis with those of appropriate control cities within Minnesota or the rest of the country.

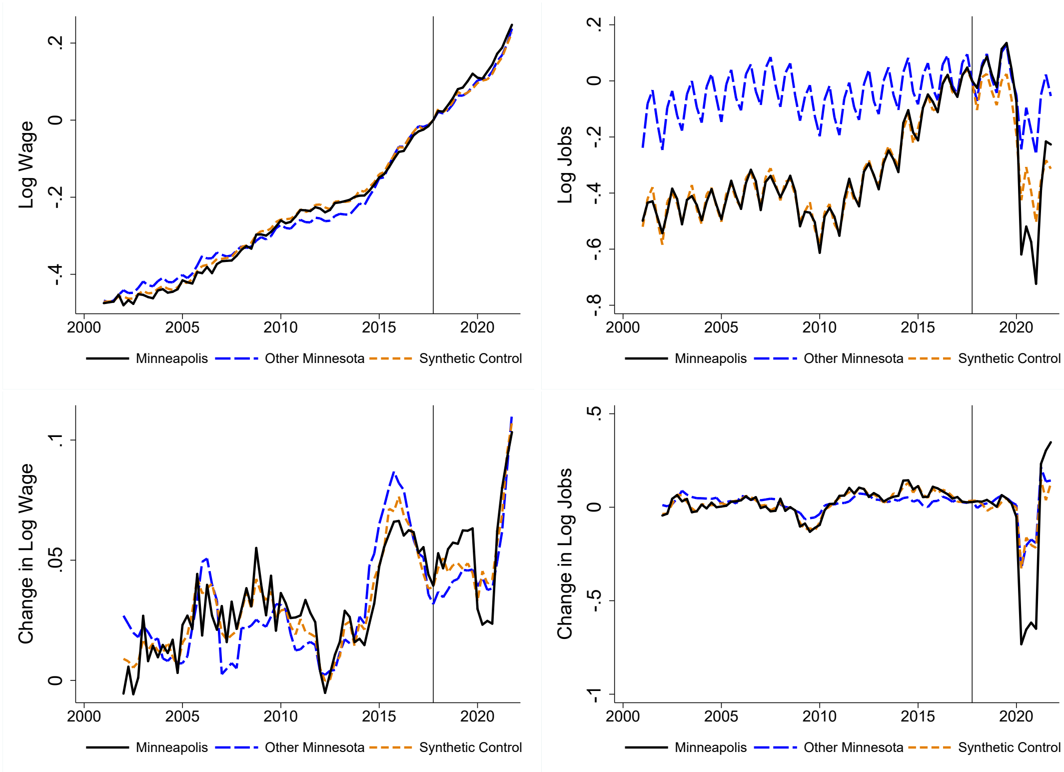
5.1 Methodology

The minimum wage increase was implemented on January 1, 2018. We observe wages, employment, hours, and worker earnings in Minneapolis before and after the minimum wage increase. However, researchers do not observe the counterfactual of 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). The key to evaluating the policy is to construct counterfactual outcomes in a credible manner.

To construct counterfactuals, we use synthetic control methods (Abadie and Gardeazabal (2003), Abadie, Diamond, and Hainmueller (2015)) as augmented by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) with fixed effects. The synthetic control approach takes a weighted average of the geographical units outside Minneapolis to construct the counterfactual. The statistical tool chooses weights so that the synthetic control looks like Minneapolis (in a statistical sense) in terms of outcome variables before 2018. For example, weights would be found so that the synthetic control's time series for the economic outcome before 2018 matches as closely as possible the same time series in Minneapolis. The counterfactual is built from other geographical regions, but they are averaged in such a way that they approximate as closely as possible Minneapolis before 2018 on the observable dimensions that are relevant for the analysis. This method produces a counterfactual that responds to economic shocks in a way similar to how Minneapolis does in the period before the minimum wage increase.[7] See Section A.1 for the technical details of this methodology.

[7] To infer the statistical significance of the estimated impact effects, we use the "placebo method." The method takes all non-treated units and estimates the treatment effect in these samples, with each sample generated under a placebo treatment of a subset 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 exact implementation details to construct the placebo standard errors.

Figure 1: Illustration of the Synthetic Control Method



As an example, Figure 1 illustrates this method in the context of the accommodation and food services sector. The upper panels of the figure plot quarterly time series of the average hourly wage and the total number of jobs during the period with data coverage between 2001Q1 and 2021Q4. All series are in logs and normalized to 0 in 2017Q4, which is the last quarter before the minimum wage increased in Minneapolis.

The long-dashed blue lines show the evolution of wages 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. This specification would estimate the effect of a minimum wage increase by comparing the changes in outcomes over time between Minneapolis and the average of other cities. The trends in Minneapolis are significantly different from those of other cities in Minnesota.

The dashed orange line shows the evolution of wages and jobs for the synthetic control of Minneapolis, which is the weighted average of cities in Minnesota other than Minneapolis and Saint Paul.[8] By design, the methodology weights more heavily cities with similar pre-treatment trends and less heavily cities with different pre-treatment trends. As seen in the figure, the time series for the synthetic control reproduce very closely the time series of wages and jobs in Minneapolis in the pre-treatment period. Using synthetic difference-in-differences, we can visualize the treatment effect of the minimum wage increase as the difference between the dashed orange line and the solid line in the post-2018 period.

The empirical estimates presented in Section 5.2 will focus on outcome variables that are expressed in yearly growth rates.[9] The lower panels of Figure 1 demonstrate that wages and jobs growth in accommodation and food services are substantially more volatile in Minneapolis than in the rest of Minnesota. For the synthetic control, we reestimate the weights in the growth specification of the outcome variable. As with the levels specification, the fit during the pre-treatment period is significantly improved relative to that of the unweighted average that underlies the difference-in-differences specification.

5.2 Results

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. As we show in Table 3, 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 (81), which consists of repair and maintenance shops, personal and laundry services, and various civic, professional, and religious organizations. 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

[8] We exclude Saint Paul from the construction of the synthetic control of Minneapolis because Saint Paul began discussing a minimum wage increase in 2018 and implemented the increase in 2020.

[9] There are two reasons why we prefer a specification in growth rates to a specification in levels. First, using a unit fixed effect in a growth specification removes heterogeneity in average growth rates that may be correlated with the treatment of increasing the minimum wage. Second, using yearly growth rates allows us to remove quarterly seasonal variation, thus improving the efficiency of our estimates.

Table 4: Effects of the Minimum Wage Increase

	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)

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 2021Q4. Entries in parentheses are *p*-values using the placebo method.

impacted workers and have been studied extensively in the literature.[10]

Table 4 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.[11] Entries are equal to the log point change in outcomes in 2021Q4 due to the minimum wage increase.[12] 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 increase in the wage and a 34 log points decrease in

[10]We also analyzed other industries and did not find statistically or economically significant responses. As we explain in section 6, we use data from these other industries in our analysis of the cross section.

[11]Table A.2 provides a list of Minnesota zip codes and the corresponding city names for the largest synthetic control weights in retail trade. Similarly, details of the largest synthetic control weights are provided in Table A.3 for administration and support; Table A.4 for health care and social assistance; Table A.5 for arts, entertainment, and recreation; Table A.6 for accommodation and food services; Table A.7 for other services; Table A.8 for full-service restaurants; and Table A.9 for limited-service restaurants.

[12]Log point changes approximate percent changes when changes are small.

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 for jobs, and thus we conclude that both the wage effect and job effect are precisely estimated. As another example, the p-value for job effects in the administration and support industry is 46.8, and thus we conclude that the jobs 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, administration and support, other services, and restaurants. Among industries with statistically significant increases, we document increases that range between 6 and 12 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 41 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; arts, entertainment, and recreation; and accommodation and food services. Within accommodation and food services, we find a 52 log points jobs decline for full-service restaurants and a 36 log points jobs decline for limited-service restaurants.

The third column presents results for total hours. For retail trade; arts, entertainment, and recreation; and restaurants, the estimated effects for hours are smaller than those for jobs. In contrast, for other services, we detect a decline in hours, whereas we did not find a significant declines in jobs.

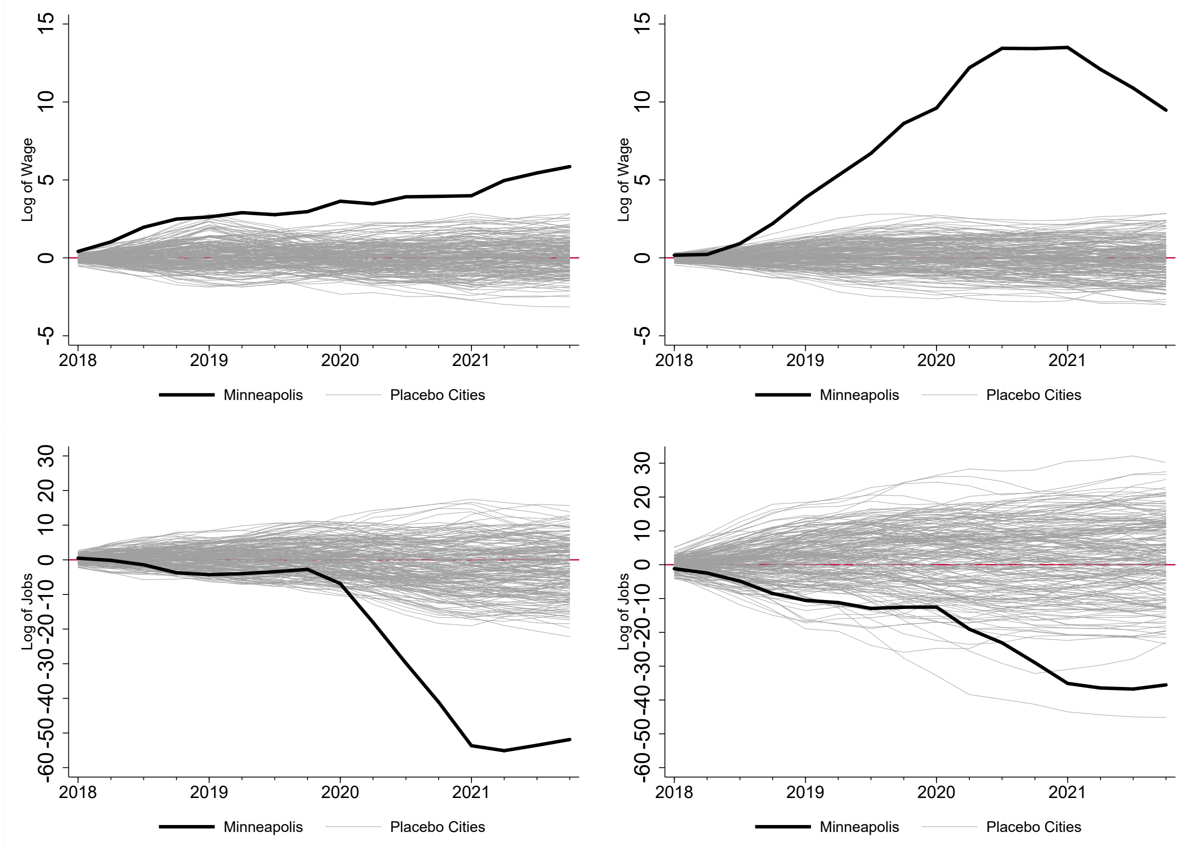
The last column of the table presents results for worker earnings. We detect statistically significant declines in earnings for retail trade, accommodation and food services, and restaurants. Given the modest wage gains for all industries and the significant negative effects on employment for some industries, 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 2021Q4. In the Appendix, we present the time series over the period 2001Q1 to 2021Q4 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 2 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 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 top panels of Figure 2 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.

Figure 2: Time-Varying Effects in Minneapolis Restaurants



Full-Service Restaurants Limited-Service Restaurants

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 the Twin Cities 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 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.10 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.11 are similar to the baseline results, though there are a few differences worth emphasizing. First, when we use the time weights, we find a statistically significant decline in wages of 2.3 log points for the health care and social assistance sector. Second, the wage gain for accommodation and food services is now statistically significant at the 5 percent level. Finally, the hours decline in other services is no longer statistically significant.

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.[13]

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.[14] 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. 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.

Table 5 presents our estimates from the QCEW until 2021Q4. The estimates from the

[13]For each industry, Table A.13 presents the synthetic control weights assigned to the U.S. cities in our sample.

[14]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.12 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.

Table 5: Job Effects of Minimum Wage Increases: Cities with Comparable Employment

	Job Effects
Retail Trade (44)	-3.2
	(64.9)
Administration and Support (56)	2.5
	(97.3)
Health Care and Social Assistance (62)	-2.5
	(64.9)
Arts, Entertainment, and Recreation (71)	-12.4
	(32.4)
Accommodation and Food Services (72)	-25.2
	(5.4)
Other Services (81)	-9.6
	(34.5)
Full-Service Restaurants (722511)	-38.5
	(5.4)
Limited-Service Restaurants (722513)	-19.0
	(5.4)

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

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, the job effects we estimate for Minneapolis using variation across U.S. cities are similar to those we estimated previously using within-Minnesota variation, with three exceptions. First, the jobs estimate for retail is smaller in magnitude and is not statistically significant. Second, the estimate for arts, entertainment, and recreation is similar to the DEED estimate, but is not statistically significant. Finally, even though we find job declines for accommodation and food services of similar magnitude to the job declines we documented before in the DEED data, the magnitude for restaurant industries is smaller.

To summarize, using additional variation from outside of Minnesota, we conclude that our results are not driven by the pandemic recession, which may have impacted control regions within Minnesota differently than Minneapolis.

Interpreting the Time Series Results- The time series methods attribute any differences after 2018 between outcomes in Minneapolis and those in the control group to the minimum wage increase. Our starting point is to use the synthetic control from the state of Minnesota to difference out any effects unrelated to the minimum wage. Despite how well the synthetic control fits the time series of the treated units in the pre-treatment period, it may be that in the post-treatment period, Minneapolis's sensitivity to aggregate shocks changed relative to that of the synthetic control. As we argued, using time weights or a synthetic control from other U.S. cities alleviates this concern because these strategies make it more plausible that we are differencing out pandemic effects in the post-treatment period that are unrelated to the minimum wage increase.

As with any research design that uses time series variation, it may still be case that Minneapolis experienced idiosyncratic shocks, such as civil unrest, that cannot be differenced out in the post-treatment period. Using the QCEW sample of other U.S. cities, we find a persistent jobs decline through the end of 2021, when it is reasonable to assume that civil unrest was no longer impacting Minneapolis differently than other cities. However, perhaps the negative impacts of civil unrest propagated in 2021 through mechanisms other than the minimum wage. For this reason, we now use variation from the cross sections of establishments and workers within Minneapolis.

6 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.

6.1 Methodology

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. Consider a full-service restaurant, Restaurant A. It is located on the fictitious Plain Street and pays all of its workers at least 16 dollars per hour in 2017. This restaurant is not directly exposed to the increase in minimum wage, because all its workers are already earning a wage above 15 dollars. Next, consider another full-service restaurant on Plain Street, Restaurant B, which pays all its workers in 2017 an hourly wage of 7.5 dollars. Restaurant B is highly exposed to the minimum wage increase, because to continue to operate using the same workforce, it needs to increase wages for all workers.

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 at time period t its GAP_{jszt}. In the example, the GAP measure for Restaurant A in 2017 is zero, because the restaurant already pays all its workers a wage above 15 dollars. The GAP measure for Restaurant B is one, because it needs to double the wage of all its workers in order to continue to operate using the same workforce.

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 three years before the time period under consideration. 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. Continuing with our example, we are comparing outcomes in the same quarter t between Restaurant A and Restaurant B, which belong in the same industry j and the same zip code z. Therefore, any common shock that these restaurants face will be differenced out. The only difference remaining between these two restaurants is their differential exposure to the minimum wage.[15]

6.2 Results

We now present the results from the analysis of cross section of establishments. Table 6 presents estimates for effects of exposure to the minimum wage increase on wage, jobs, hours, and earnings.[16] 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.[17] 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.

[15] 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. See Section A.1 for the technical details of the regression specification.

[16] We measure the outcomes in three-year percent changes. Specifically, we use the arc percent change, which is defined as $Y_{jszt} = \frac{y_{jszt} - y_{jszt-3}}{(1/2)(y_{jszt} + y_{jszt-3})}$. 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. We make this transformation of the outcome variables to capture potential changes in the propensity of establishments to exit in response to the minimum wage increase.

[17] 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 6: Labor Market Effects of Minimum Wage Increases: Cross Section of Establishments

Year	Wage	Jobs	Hours	Earnings
2018	11.5	−11.1	−12.8	−8.0
	(0.0)	(1.2)	(0.4)	(12.5)
2019	13.5	−15.8	−16.3	−11.6
	(0.0)	(0.4)	(0.4)	(7.5)
2020	15.1	−14.6	−13.3	−13.1
	(0.0)	(1.3)	(2.4)	(5.7)
2021	14.7	−14.6	−15.8	−16.1
	(0.0)	(1.9)	(1.2)	(2.7)

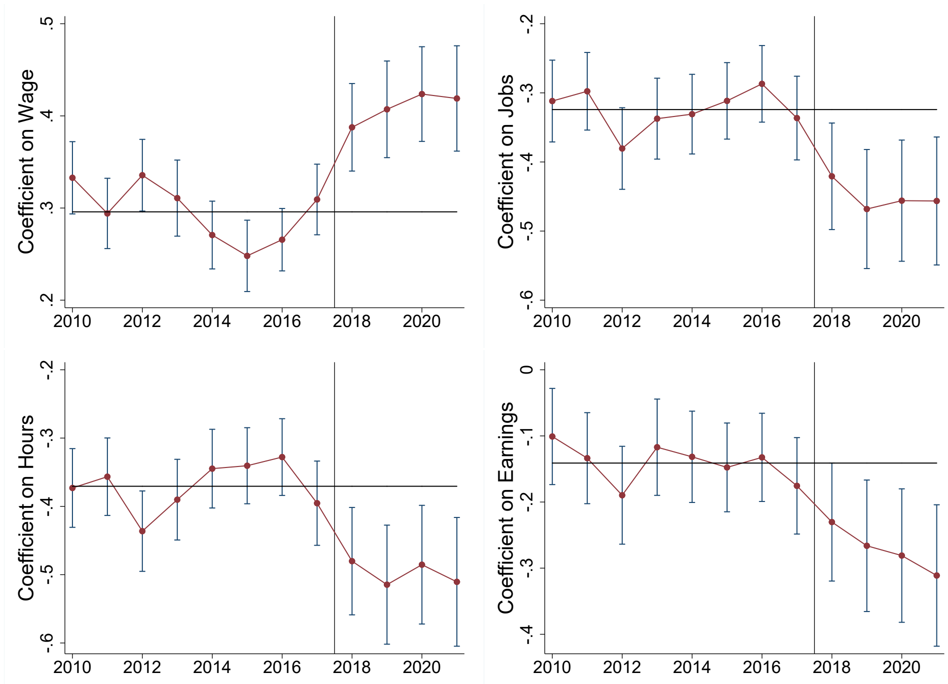
Notes: The estimates are in percentages. Entries in parentheses are *p*-values using standard errors clustered at the establishment level.

Beginning with the wage in the first column, we estimate a wage growth between 12 percent and 15 percent for establishments with GAP of 1 relative to establishments with GAP of 0. For the employment responses, we find declines of jobs and hours that range between 11 percent and 16 percent across establishments. We estimate negative relationships between exposure to the minimum wage and earnings of workers at the establishment level. The earnings coefficients are generally smaller in absolute value and less statistically significant than employment coefficients, reflecting the positive effects we estimate for the wage.

A reasonable concern about our cross-sectional results in 2020 and 2021 is whether our strategy identifies establishments' sensitivity to the minimum wage or the sensitivity of smaller establishments with larger GAP exposure to the pandemic recession or civil unrest. However, 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.

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

Figure 3: Cross-Sectional Responses Over Time



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.

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 to vary over time for all periods. Figure 3 shows that there is no noticeable trend in these estimated coefficients before the minimum wage increase. The coefficients for all outcomes are statistically different from the average coefficient before the minimum wage increase in 2018, indicated by the horizontal line.[18]

[18]The estimated coefficients in Table 6 represent the differences between the 2018, 2019, 2020, and 2021 estimates in the figure and the horizontal line. Thus, the estimates denote the effects of the minimum wage beyond any effects we would estimate owing to typical establishment dynamics.

We provide two robustness checks to our results. The top panel of Table A.14 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, which are lower by 4 to 9 percentage points. In the lower panel of Table A.14, 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 we wish to control for typical establishment dynamics during a period close to the minimum wage increase. However, our results are not sensitive to this robustness check.

7 Cross-Section Analysis: Worker Effects

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.

7.1 Methodology

In this section, 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 capture 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.

7.2 Results

Table 7 presents estimates of the regression coefficients applied to wages, hours, and earnings.[19] For 2018, we do not detect statistically significant responses with respect to hours, and we find a small decrease in workers' wage. This is in contrast to the establishment-level results, which show both a significant increase in wages and declines in hours. For 2019, we find a small but statistically signifiant increase in workers' wage. The declines in hours and earnings are similar to estimates from the establishment-level regressions. The 2020 and 2021 results for workers in Minneapolis are comparable to the results for establishments.

[19] 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 7: Labor Market Effects of Minimum Wage Increases: Cross Section of Workers

Workers' Establishment GAP	Wage	Hours	Earnings
2018	-4.0	-1.2	-6.1
	(0.1)	(67.1)	(3.3)
2019	5.7	-14.7	-12.1
	(0.0)	(0.0)	(0.0)
2020	4.9	-12.7	-9.5
	(0.1)	(0.0)	(0.4)
2021	13.6	-14.1	-8.1
	(0.0)	(0.0)	(1.8)

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

8 Summary of Estimates

Table 8 summarizes our estimates. In the first row, we present the average hourly wage increases in Minneapolis in 2021. The average of time series estimates of the wage increase is 0.8 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 0.5 percent. We calculate this number by multiplying the 2021 average of the coefficients from establishments and workers regressions (roughly 12.3 percent) with the employment-weighted average GAP (roughly 4 percent) in Minneapolis in 2021. 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. Note that for the average jobs effect, we average our estimates between the DEED and the QCEW data sources.

Table 8: Effects of Minimum Wage Increases: Summary of Estimates (Percentages)

Outcome in 2021 (1)	Estimate Type (2)	Time Series (3)	Cross Section (4)	Average (5)
Hourly Wages	Average	0.8	0.5	0.7
	Most Exposed	7.4	11.9	9.6
Jobs	Average	-2.7	-0.6	-1.7
	Most Exposed	-32.0	-14.0	-23.0
Hours Worked	Average	-2.2	-0.4	-1.3
	Most Exposed	-33.8	-8.4	-21.1
Wage Earnings	Average	-1.7	-0.4	-1.0
	Most Exposed	-33.7	-8.6	-21.2

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 2021 coefficients from the establishments' and workers' regressions with the weighted average and maximum GAP measure.

For each outcome, "most exposed" summarizes estimates from the industries and establishments 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 7.4 percent. For the cross section, we multiply the 2021 average of the coefficients from the establishments' and workers' regressions with the maximum GAP in 2021. 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 11.9 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 8 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.[20] 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. 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.

[20] 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 Technical Appendix

This section provides a technical discussion of our time series methodology outlined in Section 5.1 and our cross-section methodologies outlined in Section 6.1 and Section 7.1.

Time Series Methodology

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).

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}. After 2018, we have multiple treated units because the unit of analysis is a zip code within a city. All zip codes in Minneapolis are treated with a minimum wage increase.

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:

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

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 (A.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. This "parallel trends" assumption cannot be tested in the post-treatment period, because we do not observe the counterfactual for treated units. 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 $t = 1, \dots, T_{\text{pre}}$.

Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) propose a synthetic difference-in-differences methodology that uses estimating equation (A.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 [1]

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

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

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

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

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

We express outcome variables Y_{it} in equation (A.3) in growth rates. We prefer a specification in growth rates to a specification in levels for two reasons.[2] If the Twin Cities

[1] 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 those of treated units.

[2] Another popular specification in the minimum wage literature is to add unit-specific linear time trends to equation (A.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

implemented a minimum wage policy because they were growing at a different rate than that of other cities, that would invalidate the identifying assumption that the treatment effect is independent to other determinants of outcome variables.[3] 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

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

In equation (A.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.[4] 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 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 (A.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 ef-

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.8 for the time series in the other low-wage industries that are included in our analyses.

[3] 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 pre-treatment periods goes to infinity.

[4] 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 (A.3).

fect 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.

We now discuss the synthetic control method's performance in accounting for the growth of the treated unit, Minneapolis, in the period before the minimum wage increase. In Table A.1, we present R-squared coefficients from regressions of growth in Minneapolis 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 for restaurants, the synthetic control accounts for a substantial fraction of the variation of growth of Minneapolis 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. 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.[5] Additionally, in robustness checks detailed in the report, 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

[5] Using growth rates means that we are requiring that the synthetic control fit 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.

is a linear factor model, we perform Monte Carlo simulations to assess the size of the bias in the presence of imperfect fit, and we conclude that the bias in our context is generally small.

Cross-Sectional Methodology

Our starting point is the statistical model

$$Y_{jszt} = \gamma_{szt} + \sum_{t=2018}^{2021} \tau_t \left(\text{GAP}_{jszt-3} \cdot d_t\right) + u_{jszt}, \tag{A.5}$$

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 and have to exist in the sample in period $t-3$.

In regression (A.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 arising from the pandemic recession or civil unrest in 2020 for each industry within a zip code.

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

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

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.[6] In equation (A.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}, τ_{2020}, and τ_{2021} as the difference in establishment outcomes arising from differences in their exposure to the minimum wage increase in 2018, 2019, 2020, and 2021 after differencing out any common time effect that these establishments share with other establishments in the same zip code and industry.[7] 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 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

$$Y_{jszt} = \gamma_{szt} + \sum_{t=2018}^{2021} \tau_t \left(\text{GAP}_{jszt-3} \cdot d_t\right) + \tau_0 \text{GAP}_{jszt-3} + u_{jszt}, \tag{A.7}$$

where τ_0 controls for any correlation between GAP and outcomes due to typical establish-

[6]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).

[7]We run regression (A.5) 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 (A.5).

ment 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}, τ_{2020}, and τ_{2021} as the difference in establishment outcomes due to differential exposure to the minimum wage increase.

In the section with cross-sectional analysis of worker effects, we use variation in wage gaps across workers and track workers' outcomes directly over time, irrespective of whether workers reallocated to other establishments in or outside of the Twin Cities. Our first specification is

$$Y_{it} = \sum_{s} \gamma_{st} X_{ist} + \sum_{t=2018}^{2021} \tau_t \left(\text{GAP}_{it-3} \cdot d_t \right) + \tau_0 \text{GAP}_{it-3} + \rho Y_{it-1} + u_{it}, \tag{A.8}$$

where the dependent variable for worker i in period t, Y_{it}, is defined as the arc percent change over three years and the wage gap over three years is calculated at the worker level:

$$\text{GAP}_{it} = \frac{\max(15/(1+\pi_{t,2017}) - w_{it}, 0)}{w_{it}}. \tag{A.9}$$

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. The other difference relative to our specification for establishments is that now we include in the regression the lagged outcome Y_{it-1} for workers. Thus, we interpret the τ coefficients as the percent change in worker outcomes resulting from a higher wage gap 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, γ_{st}, and any effects we would detect that are due to typical worker dynamics, τ_0.

To address the concern that roughly half of the employment effects capture low-wage workers' difficulty finding jobs due to the pandemic or civil unrest, rather than their difficulty finding jobs due to the minimum wage, we consider a final specification:

$$Y_{it} = \sum_{s} \gamma_{st} X_{ist} + \sum_{t=2018}^{2021} \tau_t \left(\overline{\text{GAP}}_{it-3} \cdot d_t \right) + \tau_0 \overline{\text{GAP}}_{it-3} + \rho Y_{it-1} + u_{it}, \tag{A.10}$$

where the wage gap over three years now becomes

$$\overline{\text{GAP}}_{it} = \frac{1}{\# J_t(i)} \sum_{j \in J_t(i)} \text{GAP}_{jt}, \tag{A.11}$$

where $\# J_t(i)$ 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 (A.6). The only difference relative to our previous worker-level specification in equation (A.8) is that we treat workers with their establishments' gaps instead of their own gaps. This specification combines elements from both the establishment-level regressions and the worker-level regressions we ran previously. It 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 and 2021 is because of the pandemic or civil unrest.

A.2 Additional Figures

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

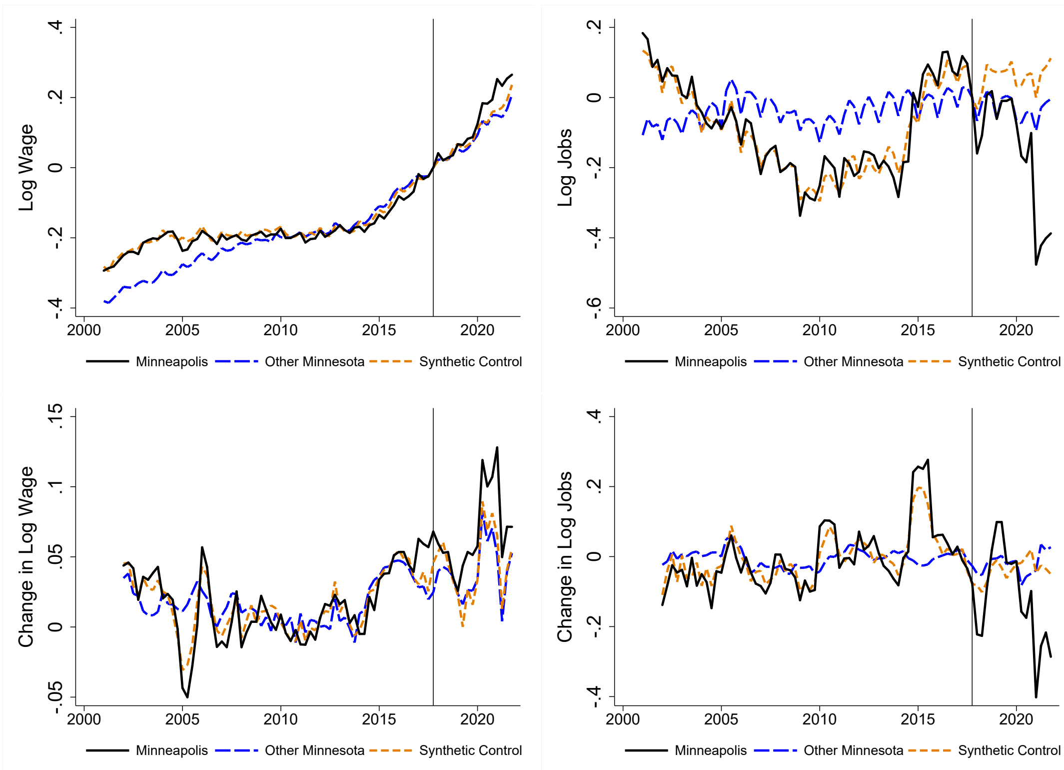


Figure A.2: Time Series of Administration and Support in Minneapolis

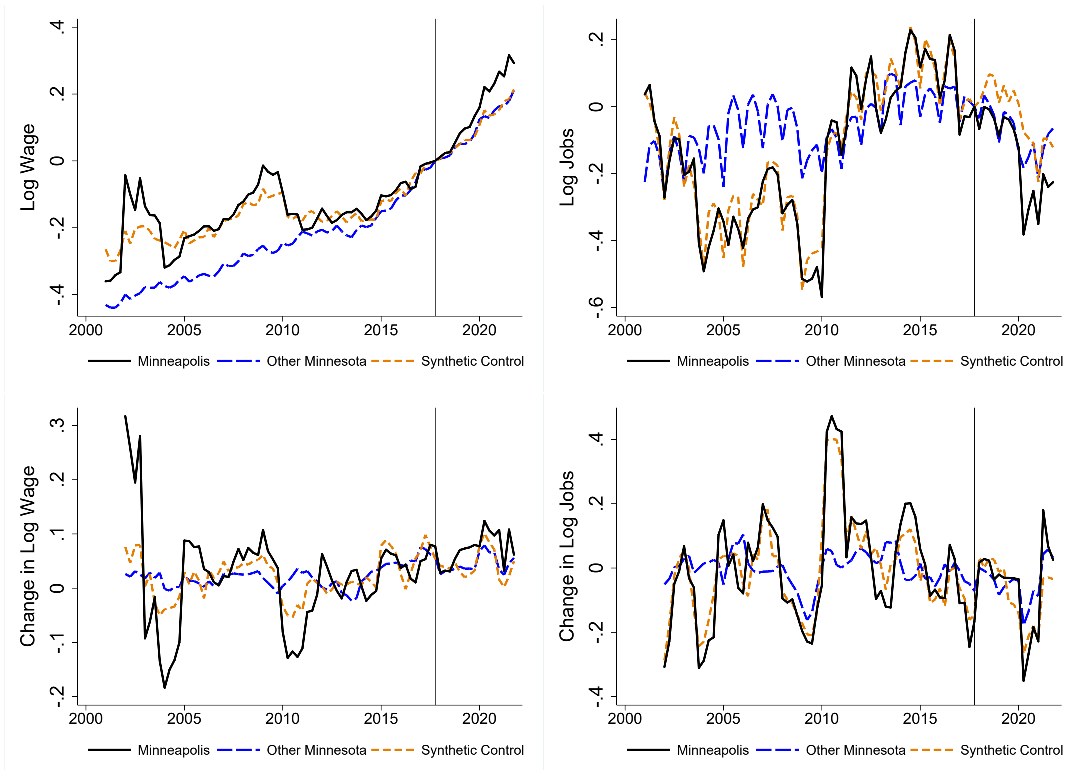


Figure A.3: Time Series of Health Care and Social Assistance in Minneapolis

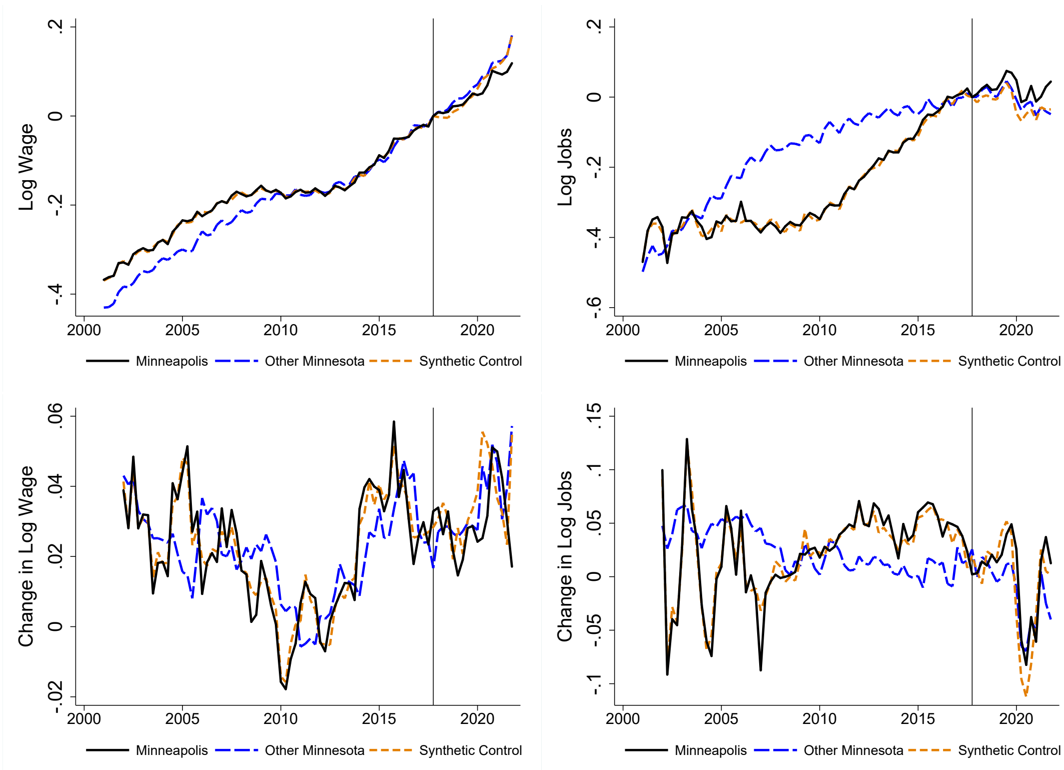


Figure A.4: Time Series of Arts, Entertainment, and Recreation in Minneapolis

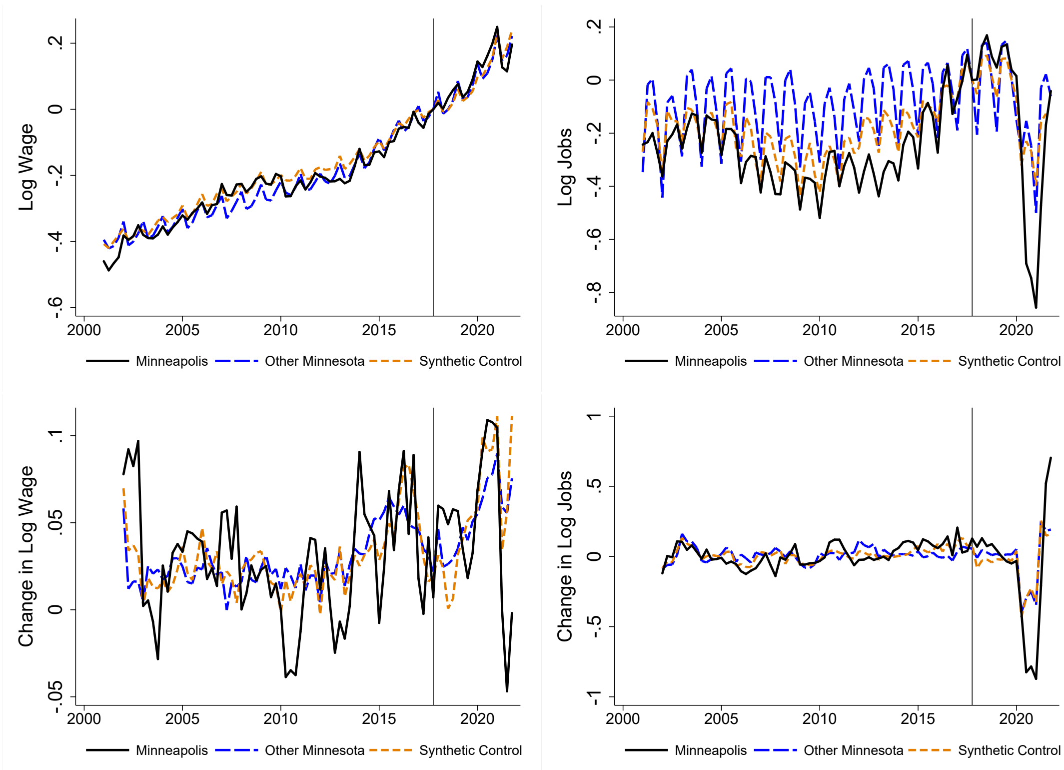


Figure A.5: Time Series of Accommodation and Food Services in Minneapolis

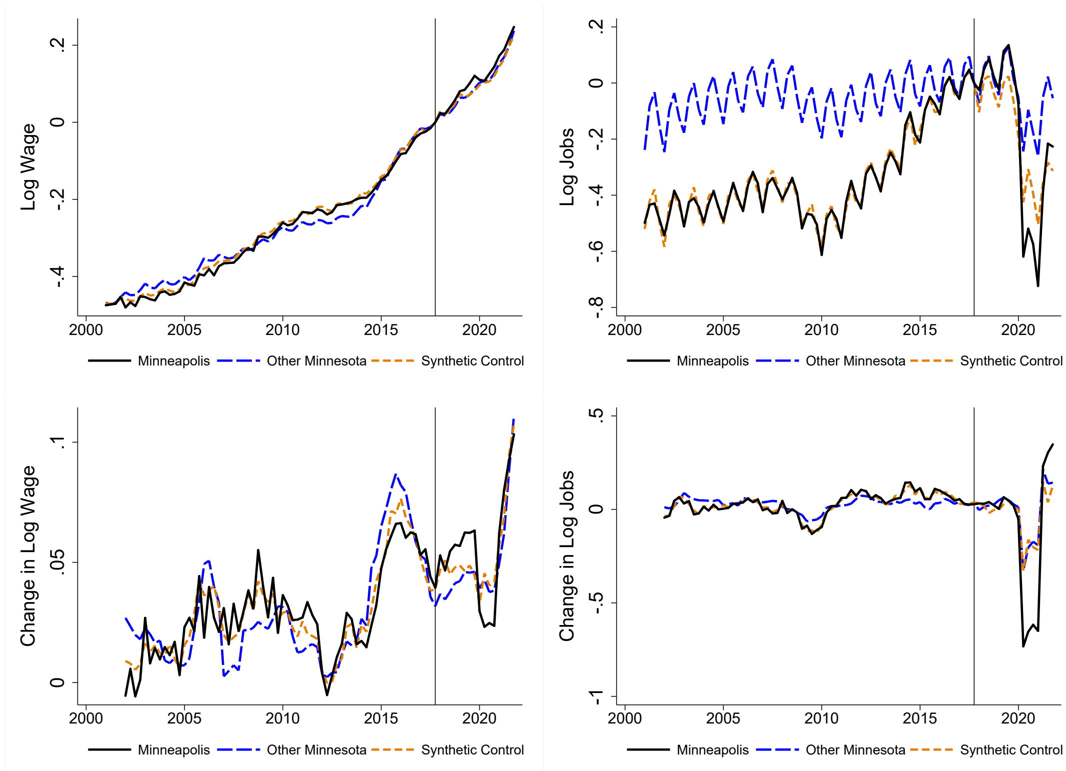


Figure A.6: Time Series of Other Services in Minneapolis

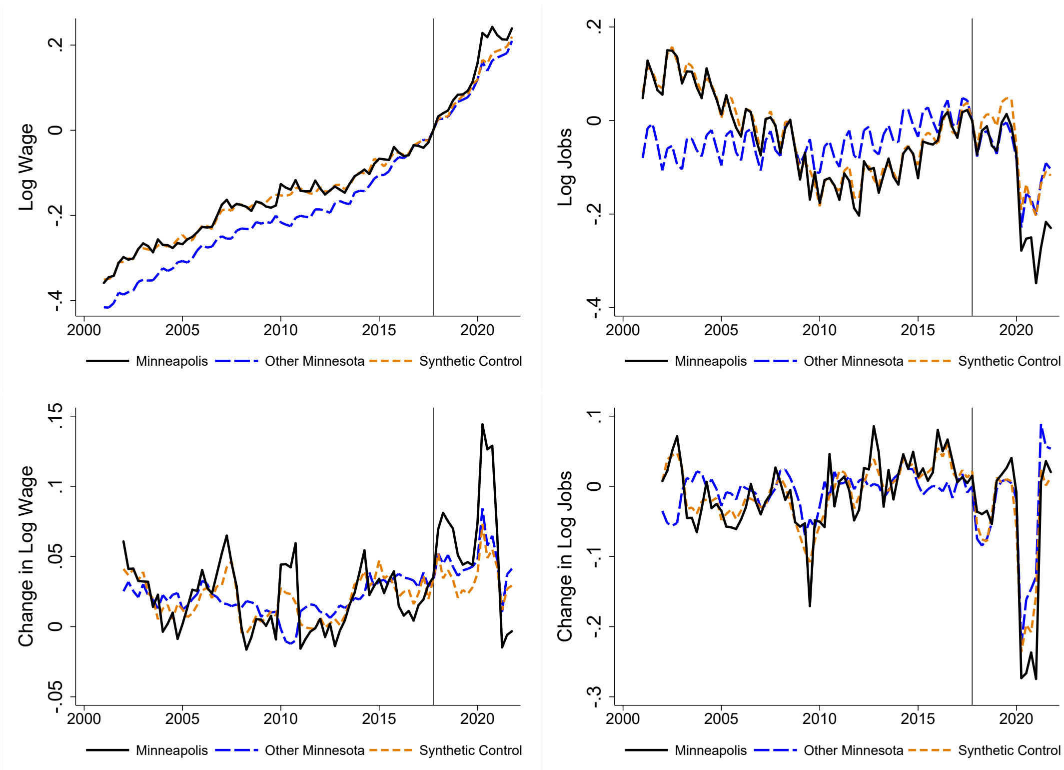


Figure A.7: Time Series of Full-Service Restaurants in Minneapolis

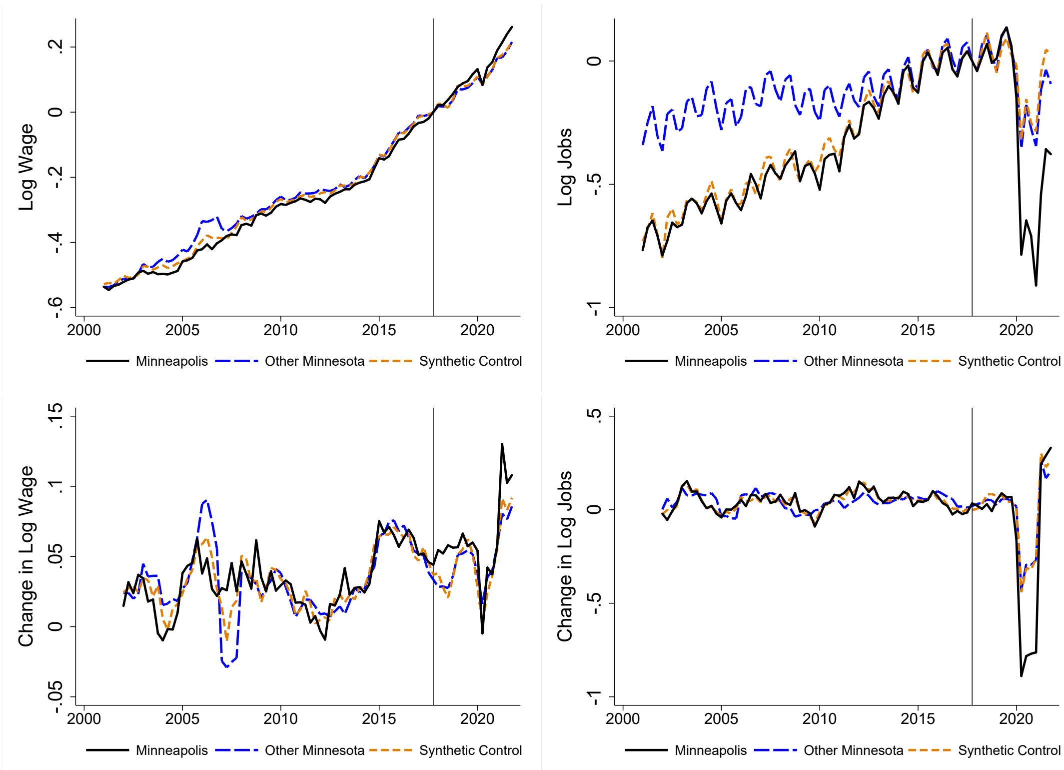
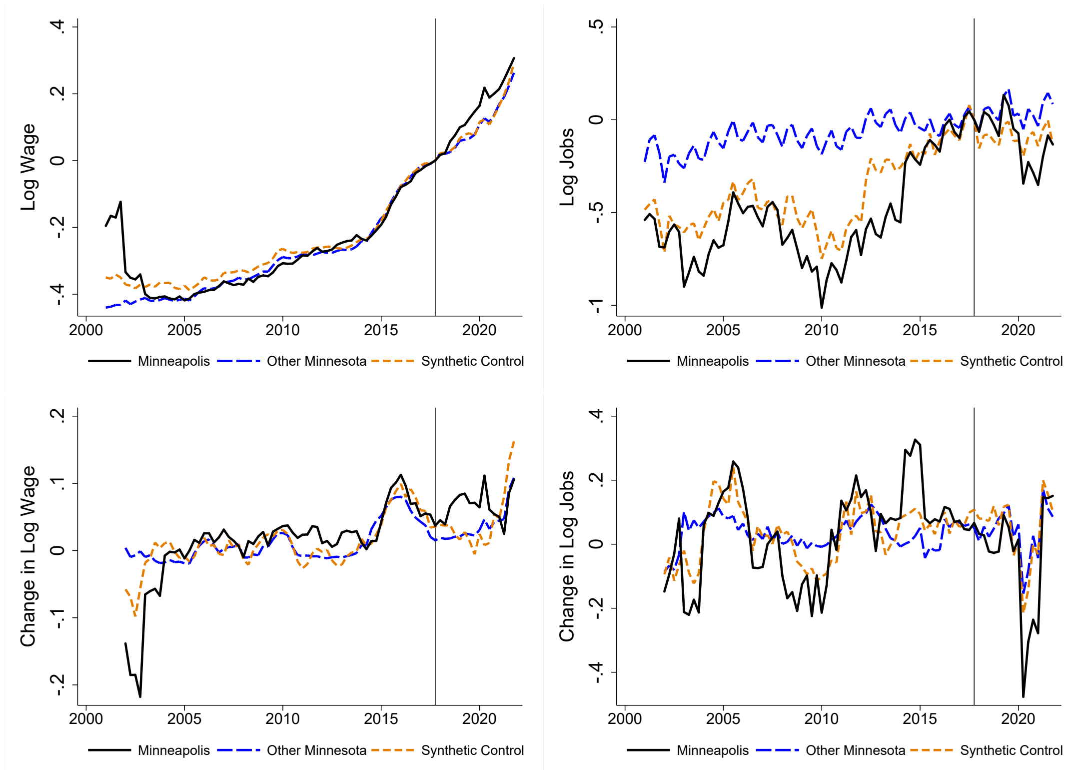


Figure A.8: Time Series of Limited-Service Restaurants in Minneapolis



A.3 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)	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

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

Table A.2: Synthetic Control Weights for Minnesota Zip Codes (Retail Trade)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
56364, PIERZ	0.17	55082, STILLWATER	0.21	55371, PRINCETON	0.12	55428, BROOKLYN PARK	0.12
55441, PLYMOUTH	0.09	55371, PRINCETON	0.13	55428, BROOKLYN PARK	0.12	55311, MAPLE GROVE	0.11
55372, PRIOR LAKE	0.07	55303, ANOKA	0.12	55311, MAPLE GROVE	0.11	55920, BYRON	0.10
55421, FRIDLEY	0.06	55428, BROOKLYN PARK	0.09	55976, STEWARTVILLE	0.09	56721, EAST GRAND FORKS	0.08
55442, PLYMOUTH	0.06	55391, WAYZATA	0.08	55391, WAYZATA	0.09	55811, DULUTH	0.08
56501, DETRIOT LAKES	0.05	55811, DULUTH	0.07	55443, BROOKLYN PARK	0.07	55443, BROOKLYN PARK	0.07
55805, DULUTH	0.05	55077, INVER GROVE HEIGHTS	0.06	55042, LAKE ELMO	0.05	56304, ST CLOUD	0.05
55428, CRYSTAL	0.05	56721, EAST GRAND FORKS	0.06	55331, EXCELSIOR	0.05	55331, EXCELSIOR	0.05
55082, OAK PARK HEIGHTS	0.05	55976, STEWARTVILLE	0.05	56374, ST JOSEPH	0.04	55082, STILLWATER	0.04
55369, OSSEO	0.05	55311, MAPLE GROVE	0.04	56721, EAST GRAND FORKS	0.04	55371, PRINCETON	0.04

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.3: Synthetic Control Weights for Minnesota Zip Codes (Administration and Support)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
55121, EAGAN	0.21	55343, MINNETONKA	0.15	55044, LAKEVILLE	0.03	56073, NEW ULM	0.19
55305, MINNETONKA	0.20	55423, RICHFIELD	0.13	55110, WHITE BEAR LAKE	0.05	55426, ST LOUIS PARK	0.17
55987, WINONA	0.18	55303, ANOKA	0.09	55118, WEST SAINT PAUL	0.04	55113, ROSEVILLE	0.13
55318, CHASKA	0.09	55438, BLOOMINGTON	0.09	55124, APPLE VALLEY	0.18	55378, SAVAGE	0.06
55792, VIRIGINA	0.09	55113, ROSEVILLE	0.09	55303, ANOKA	0.10	55432, FRIDLEY	0.05
55303, RAMSEY	0.09	55117, LITTLE CANADA	0.08	55343, HOPKINS	0.04	55343, MINNETONKA	0.05
55378, SAVAGE	0.08	55439, EDINA	0.06	55343, MINNETONKA	0.07	56187, WORTHINGTON	0.05
		55369, MAPLE GROVE	0.06	55391, WAYZATA	0.07	55369, MAPLE GROVE	0.04
		56303, ST CLOUD	0.04	55423, RICHFIELD	0.08	55044, LAKEVILLE	0.04
		56560, MOORHEAD	0.04	55438, BLOOMINGTON	0.07	55117, LITTLE CANADA	0.03

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.4: Synthetic Control Weights for Minnesota Zip Codes (Healthcare and Social Assistance)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
56085, SLEEPY EYE	0.08	55109, MAPLEWOOD	0.04	99999, POOLED	0.07	56762, WARREN	0.12
56011, BELLE PLAINE	0.08	55120, MENDOTA HEIGHTS	0.01	55124, APPLE VALLEY	0.07	56649, INTERNATIONAL FALLS	0.09
99999, POOLED	0.06	55121, EAGAN	0.04	55421, COLUMBIA HEIGHTS	0.07	99999, POOLED	0.08
55121, EAGAN	0.06	55126, SHOREVIEW	0.03	56762, WARREN	0.06	55971, RUSHFORD	0.06
56572, PELICAN RAPIDS	0.06	55128, OAKDALE	0.07	56649, INTERNATIONAL FALLS	0.06	56001, MANKATO	0.05
55425, BLOOMINGTON	0.05	55303, RAMSEY	0.05	55442, PLYMOUTH	0.04	55427, GOLDEN VALLEY	0.04
56232, DAWSON	0.04	55372, PRIOR LAKE	0.11	55123, EAGAN	0.04	55421, COLUMBIA HEIGHTS	0.04
55343, MINNETONKA	0.04	55398, ZIMMERMAN	0.04	55109, MAPLEWOOD	0.04	55427, NEW HOPE	0.04
56001, MANKATO	0.04	55421, COLUMBIA HEIGHTS	0.03	55427, NEW HOPE	0.03	55120, MENDOTA HEIGHTS	0.04
56440, CLARISSA	0.03	55430, BROOKLYN CENTER	0.03	56283, REDWOOD FALLS	0.03	55372, PRIOR LAKE	0.03

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.5: Synthetic Control Weights for Minnesota Zip Codes (Arts, Entertainment and Recreation)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
55790, TOWER	0.30	99999, POOLED	0.29	55987, WINONA	0.19	55987, WINONA	0.38
55082, STILLWATER	0.17	55066, RED WING	0.16	55124, APPLE VALLEY	0.19	55436, EDINA	0.27
56701, THIEF RIVER FALLS	0.16	55436, EDINA	0.09	56701, THIEF RIVER FALLS	0.15	56701, THIEF RIVER FALLS	0.21
55391, WAYZATA	0.11	55044, LAKEVILLE	0.08	55436, EDINA	0.10	55802, DULUTH	0.04
55449, BLAINE	0.08	55082, STILLWATER	0.08	55449, BLAINE	0.10		
55124, APPLE VALLEY	0.07	55033, HASTINGS	0.08	55044, LAKEVILLE	0.06		
55033, HASTINGS	0.04	55449, BLAINE	0.07	55901, ROCHESTER	0.05		
		55124, APPLE VALLEY	0.05	55066, RED WING	0.05		
		55790, TOWER	0.02	55033, HASTINGS	0.04		

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.6: Synthetic Control Weights for Minnesota Zip Codes (Accommodation and Food Services)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
56011, BELLE PLAINE	0.21	55309, BIG LAKE	0.09	55615, TOFTE	0.09	99999, POOLED	0.06
55447, PLYMOUTH	0.12	55371, PRINCETON	0.07	55051, MORA	0.07	55434, BLAINE	0.06
55309, BIG LAKE	0.09	55303, ANOKA	0.06	55387, WACONIA	0.06	55343, MINNETONKA	0.05
55021, FAIRBAULT	0.08	55075, SOUTH ST PAUL	0.05	55309, BIG LAKE	0.05	56716, CROOKSTON	0.05
55330, ELK RIVER	0.06	55439, EDINA	0.05	55343, MINNETONKA	0.05	55431, BLOOMINGTON	0.04
55615, TOFTE	0.06	56334, GLENWOOD	0.05	55075, SOUTH ST PAUL	0.05	56401, BRAINERD	0.04
55906, ROCHESTER	0.05	55313, BUFFALO	0.04	55431, BLOOMINGTON	0.03	55371, PRINCETON	0.03
55016, COTTAGE GROVE	0.04	56751, ROSEAU	0.04	55317, CHANHASSEN	0.03	55112, NEW BRIGHTON	0.03
56031, FAIRMONT	0.04	55343, MINNETONKA	0.04	56156, LUVERNE	0.03	55362, MONTICELLO	0.03
55720, CLOQUET	0.04	55063, PINE CITY	0.04	55434, BLAINE	0.03	55904, ROCHESTER	0.03

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.7: Synthetic Control Weights for Minnesota Zip Codes (Other Services)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
55350, HUTCHINSON	0.17	55421, COLUMBIA HEIGHTS	0.10	55421, COLUMBIA HEIGHTS	0.09	55337, BURNSVILLE	0.11
55128, OAKDALE	0.15	56501, DETRIOT LAKES	0.09	55318, CHASKA	0.07	55806, DULUTH	0.09
55447, PLYMOUTH	0.09	55309, BIG LAKE	0.07	55432, FRIDLEY	0.06	55318, CHASKA	0.09
56258, MARSHALL	0.09	55426, GOLDEN VALLEY	0.07	56303, ST CLOUD	0.05	56537, FERGUS FALLS	0.07
55443, BROOKLYN PARK	0.07	55318, CHASKA	0.06	55309, BIG LAKE	0.05	55902, ROCHESTER	0.06
56082, ST PETER	0.07	55124, APPLE VALLEY	0.06	55435, EDINA	0.05	55443, BROOKLYN PARK	0.05
55987, WINONA	0.06	55378, SAVAGE	0.05	55362, MONTICELLO	0.05	55309, BIG LAKE	0.05
55318, CHASKA	0.06	55391, WAYZATA	0.05	55369, MAPLE GROVE	0.04	55807, DULUTH	0.04
55379, SHAKOPEE	0.05	55436, EDINA	0.05	55317, CHANHASSEN	0.04	55033, HASTINGS	0.04
55441, PLYMOUTH	0.04	55811, DULUTH	0.05	55434, BLAINE	0.04	56345, LITTLE FALLS	0.04

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.8: Synthetic Control Weights for Minnesota Zip Codes (Full-Services Restaurants)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights	Zip Code, City	Weights
55128, OAKDALE	0.16	55082, STILLWATER	0.10	99999, POOLED	0.15	99999, POOLED	0.21
56601, BEMIDJI	0.13	55433, COON RAPIDS	0.08	55902, ROCHESTER	0.11	55744, GRAND RAPIDS	0.09
55316, CHAMPLIN	0.11	56401, BRAINERD	0.07	56560, MOORHEAD	0.06	55305, MINNETONKA	0.09
55125, WOODBURY	0.10	99999, POOLED	0.07	55987, WINONA	0.05	55122, EAGAN	0.08
55057, NORTHFIELD	0.08	55350, HUTCHINSON	0.06	56308, ALEXANDRIA	0.05	55021, FAIRBAULT	0.07
56308, ALEXANDRIA	0.08	55902, ROCHESTER	0.06	55122, EAGAN	0.05	55350, HUTCHINSON	0.06
56721, EAST GRAND FORKS	0.08	55125, WOODBURY	0.06	55021, FAIRBAULT	0.05	55044, LAKEVILLE	0.06
55912, AUSTIN	0.06	55337, BURNSVILLE	0.06	55303, ANOKA	0.04	55369, MAPLE GROVE	0.05
55448, COON RAPIDS	0.05	55122, EAGAN	0.06	55124, APPLE VALLEY	0.04	55057, NORTHFIELD	0.05
56401, BRAINERD	0.05	56301, ST CLOUD	0.05	55060, OWATONNA	0.04	56601, BEMIDJI	0.05

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.9: Synthetic Control Weights for Minnesota Zip Codes (Limited-Services Restaurants)

Wage Rate		Jobs		Hours		Earnings	
Zip Code, City	Wage Rate	Zip Code, City	Jobs	Zip Code, City	Hours	Zip Code, City	Earnings
55122, EAGAN	0.74	56007, ALBERT LEA	0.13	55313, BUFFALO	0.23	56007, ALBERT LEA	0.15
55362, MONTICELLO	0.26	55362, MONTICELLO	0.12	55125, WOODBURY	0.16	55060, OWATONNA	0.15
		55313, BUFFALO	0.11	56007, ALBERT LEA	0.16	55125, WOODBURY	0.14
		55912, AUSTIN	0.08	55362, MONTICELLO	0.11	55313, BUFFALO	0.11
		56073, NEW ULM	0.07	56201, WILLMAR	0.08	56201, WILLMAR	0.09
		55987, WINONA	0.07	55060, OWATONNA	0.05	55912, AUSTIN	0.07
		55125, WOODBURY	0.07	55912, AUSTIN	0.05	55362, MONTICELLO	0.06
		55113, ROSEVILLE	0.06	55433, COON RAPIDS	0.05	56073, NEW ULM	0.06
		55420, BLOOMINGTON	0.05	55330, ELK RIVER	0.04	55420, BLOOMINGTON	0.05
		55433, COON RAPIDS	0.04			55330, ELK RIVER	0.03

Notes: This table lists the Minnesota zip codes outside the Twin Cities with top synthetic control weights.

Table A.10: Excluding Neighboring Cities from the Control Group

	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)

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

Table A.11: Adding Time Weights in the Synthetic Difference-in-Differences Estimation

	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)

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

Table A.12: 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.13: Synthetic Control Weights for U.S. Cities

	Retail Trade	Administration and Support	Health Care and Social Assistance	Art, Entertainment and Recreation	Accommodation and Food Services	Other Services	Full-Service Restaurants	Limited-Service Restaurants
Albuquerque, NM							0.01	0.07
Aurora, CO							0.08	
Baltimore, MD	0.03						0.12	
Baton Rouge, LA							0.12	0.03
Colorado Springs, CO						0.39		
Denver, CO		0.42				0.05		0.24
El Paso, TX				0.05				0.08
Fort Wayne, IN								
Fresno, CA			0.11		0.06			
Greensboro, NC					0.12			
Honolulu, HI		0.12						0.33
Huntsville, AL								
Indianapolis, IN	0.01				0.07		0.24	
Jacksonville, FL		0.07		0.05				
Kansas City, MO		0.11			0.11	0.13	0.1	
Lexington, KY								
Little Rock, AR	0.12							
Louisville, KY							0.16	
Memphis, TN				0.07				0.17
Milwaukee, WI								
Montgomery County, M								
Nashville, TN	0.01			0.1		0.2	0.03	0.07
New Orleans, LA	0.03		0.04	0.01			0.03	
Oklahoma City, OK								
Omaha, NE								
Portland, OR				0.07	0.13			
Prince George's Coun				0.03				
Reno, NV					0.03			0.01
Springfield, MO		0.15		0.01				
St. Louis, MO					0.13		0.02	
Tucson, AZ					0.31	0.01		
Tulsa, OK			0.26		0.03			
Virginia Beach, VA			0.22					
Washington, DC	0.29		0.24	0.54				
Wichita, KS							0.09	
Winston-Salem, NC	0.51	0.13	0.13	0.06		0.22		

Table A.14: Robustness: Labor Market Effects from Cross Section of Establishments

Add Lagged Growth	Wage	Jobs	Hours	Earnings
2018	7.5	−10.8	−12.7	−8.1
	(1.1)	(1.4)	(0.5)	(11.3)
2019	8.4	−15.3	−16.1	−11.5
	(2.0)	(0.6)	(0.4)	(7.1)
2020	6.0	−14.4	−13.3	−13.6
	(11.7)	(1.5)	(2.5)	(4.6)
2021	8.5	−14.7	−15.8	−15.6
	(4.8)	(1.8)	(1.2)	(3.1)
Pre-Sample to 6 Years	**Wage**	**Jobs**	**Hours**	**Earnings**
2018	9.9	−9.1	−10.9	−8.2
	(0.1)	(4.9)	(2.1)	(13.0)
2019	11.9	−13.8	−14.4	−11.8
	(0.0)	(1.2)	(1.0)	(6.4)
2020	13.6	−12.6	−11.4	−13.3
	(0.0)	(2.7)	(4.4)	(4.4)
2021	13.1	−12.7	−14.0	−16.3
	(0.0)	(3.5)	(2.3)	(2.0)

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

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