# City Size, Monopsony, and the Employment Effects of Minimum Wages*

Priyaranjan Jha[1], Jyotsana Kala[2], David Neumark[3], and Antonio Rodriguez-Lopez[4]

[1]University of California, Irvine and CESifo
[2]University of California, Irvine
[3]University of California, Irvine, Hoover Institution, NBER, and CESifo
[4]University of California, Irvine and CESifo

May 2025

**Abstract**

We assess how minimum wage effects on restaurant employment in the U.S. vary with labor market size and monopsony power. Using city-level data, we construct monopsony proxies based on labor flows and concentration. Minimum wages bind less in larger cities, consistent with the urban wage premium, and omitting this relationship overstates how labor market power reduces adverse employment effects of minimum wages. Nonetheless, accounting for city size, lower job market fluidity is linked to weaker negative employment effects, consistent with search models. By contrast, traditional concentration measures do not consistently predict variation in the effects of minimum wages.

**JEL Classification**: J38, J42, R23

**Keywords**: minimum wage, employment, urban premium, labor market fluidity, concentration

*This research was partially supported by the International Center for Law and Economics.

# 1 Introduction

Standard economic theory predicts that in a perfectly competitive labor market, a binding minimum wage leads to job losses. However, under monopsony conditions—where firms face an upward-sloping labor supply—a minimum wage can increase wages and employment, mitigating or even reversing the textbook negative employment effect (Stigler, 1946). Using Quarterly Workforce Indicators (QWI), Job-to-Job (J2J), and National Establishment Time Series (NETS) data at the CBSA-by-state level, this paper assembles proxies for monopsony power—both labor-flow-based and concentration-based measures—to examine how minimum wages affect employment in the U.S. restaurant industry from 2001 to 2019. By interacting state-level minimum wages with our local monopsony proxies, we identify whether more competitive or more monopsonistic labor markets experience stronger or weaker employment responses to a minimum wage hike.

We distinguish between fluidity and concentration measures because they capture different channels through which employers exert monopsony power. Fluidity-based indicators—such as hiring and separation rates—reflect the ease with which workers move between employers. These measures originate from search and wage-posting models (e.g., Burdett and Mortensen, 1998; Manning, 2003), where employer wage-setting power depends on how quickly employees receive alternative offers and how likely they are to switch jobs in response to wage changes. By contrast, concentration metrics—such as the Herfindahl-Hirschman Index (HHI) or the number of employers per worker—stem from traditional oligopsony frameworks in which fewer competing firms means each faces a more inelastic labor supply, enabling them to suppress wages.

Although concentration measures like HHI are widely used, theoretical considerations suggest important limitations. In Burdett and Mortensen (1998), for example, an increase in the job offer rate—a sign of greater competition—can lead to higher concentration, as larger or more productive firms attract and retain a disproportionate share of workers. Similarly, in markets characterized by monopsonistic competition, firms may be atomistic and hold small market shares, yet still possess significant wage-setting power (e.g., Jha and Rodriguez-Lopez, 2021). In these cases, HHI fails to accurately capture the degree of monopsony power. Finally, a market with many establishments could still be monopsonistic if workers rarely move, while a concentrated market may not always imply high monopsony power if workers can easily switch jobs. Examining both dimensions provides a clearer picture of where and why minimum wages have stronger vs. weaker effects.

Before turning to our monopsony power analysis, we first show that accounting for differences in city size is crucial when estimating the employment effects of minimum wages.[1] We begin by documenting—consistent with the urban wage premium literature—that average earnings in both the U.S. restaurant industry and the overall private sector are positively related to city size, implying that minimum wages are less likely to bind in larger areas. We confirm this using two-way fixed effects (TWFE) regressions that include an interaction between the minimum wage and population size. These regressions show a negative and significant estimate for the minimum wage elasticity of employment in the average-sized city, and a positive and significant interaction coefficient, indicating that the elasticity moves closer to zero as population increases.[2]

We define 19 proxies for monopsony power: 15 labor market fluidity measures derived from Census J2J and QWI data, and 4 employment concentration measures constructed from the NETS database. All measures are defined such that higher values imply a "more competitive" labor market—either due to greater fluidity (e.g., higher hiring or separation rates) or lower concentration (e.g., more establishments or firms per worker). Using a TWFE model that interacts minimum wages with each monopsony proxy, our main finding is that more fluid (i.e., more competitive) labor markets are associated with more adverse employment effects of minimum wages, whereas less fluid labor markets exhibit elasticities that are closer to zero. These results provide empirical support for search-theoretic models that link monopsony power to labor market fluidity.

In contrast, concentration-based measures provide weaker and less consistent evidence linking lower concentration to stronger minimum wage effects. In TWFE regressions, more establishments or firms per worker—indicators of lower concentration that would typically imply greater employment losses—are instead associated with employment effects that are closer to zero. Similarly, although lower HHIs are conventionally interpreted as a sign of more competitive markets, we do not find significant evidence linking lower HHIs to more adverse minimum wage effects.

Our study relates to recent work on how monopsony power modulates the employment effects of minimum wages, but differs in both focus and empirical approach. Azar et al. (2024) examine the retail industry and find that minimum wage hikes lead to less negative/more positive employment effects in more concentrated labor markets, using HHIs as their main concentration-based proxy. They also report supplementary results for the restaurant industry, where, due to data limitations, they use the number of establishments as a proxy for concentration. In contrast, we construct direct HHI measures for the restaurant industry using NETS data, and find a much weaker link between

[1] Although Core-Based Statistical Areas are not cities per se, we refer to the labor markets defined by CBSAs as "cities" as a short-hand, and also because CBSAs are based on "urban clusters."

[2] We also show why TWFE specifications that ignore this relationship and use population weights tend to yield elasticity estimates much closer to zero, as these estimates are disproportionately influenced by the largest cities.

concentration and minimum wage effects. However, consistent with the broader interpretation of monopsony power, our results based on labor market fluidity show that minimum wage employment effects are less negative in more monopsonistic labor markets. Yet because city size is negatively correlated with labor market fluidity (as we document), omitting size interactions leads to biased estimates that overstate the extent to which monopsony power moderates job loss from higher minimum wages.

In sum, we make three key contributions to the literature on monopsony and minimum wages. First, we show that failing to account for the interaction between city size and the minimum wage substantially biases elasticity estimates toward zero because minimum wages are less binding in larger cities. Second, we highlight that different proxies for monopsony power—specifically, fluidity-based measures versus concentration-based measures—capture distinct dimensions of labor market frictions. Our results indicate that low labor market fluidity, rather than high concentration, is a more reliable indicator of monopsony power in the restaurant industry. Finally, failure to account for city size when estimating how monopsony influences minimum wage-employment effects leads to overstatement of how much labor market power mitigates job loss from higher minimum wages.

# 2 Data

## 2.1 Main Data Sources

We obtain employment and earnings at the CBSA-by-state level from the QWI for the 76 quarters between 2001 and 2019.[3] According to the U.S. Office of Management and Budget, a Core-Based Statistical Area (CBSA) consists of one or more counties with an urban core, linked by strong commuting patterns; CBSAs can be classified as either metropolitan or micropolitan statistical areas. Excluding Hawaii and Alaska, there are 919 CBSAs in our QWI data. Of these, 858 are single-state CBSAs, and 61 are multi-state CBSAs (MS-CBSAs). Because MS-CBSAs cross state boundaries, they create multiple CBSA-by-state geographies. Specifically, the 61 MS-CBSAs produce 133 CBSA-by-state geographies: $52 \times 2$ from two-state CBSAs, $7 \times 3$ from three-state CBSAs, and $2 \times 4$ from four-state CBSAs. Combined with the 858 single-state CBSAs, this yields a total of 991 CBSA-by-state geographies in the continental United States.[4] Although only 1,832

[3]The U.S. Census Bureau constructs the QWI from the linked employer-employee data of the Longitudinal Employer-Household Dynamics (LEHD). We start in 2001 because states enter the LEHD at different times and in large droves, and 2001 had a large drove that increased coverage to 92.63% of all CBSA-by-state entities. The largest changes occurred in the first quarter of 1995 (coverage reached 39.15%), the first quarter of 1998 (coverage reached 66.6%), and the first quarter of 2000 (coverage reached 83.15%). We end in 2019 to avoid the COVID-19 pandemic period, which caused severe and uneven disruptions to the restaurant industry, potentially confounding the effects of minimum wage changes.

[4]Figure A-1 in the Appendix shows our 919 CBSAs.

out of more than 3,100 U.S. counties form these 919 CBSAs, these counties accounted for 93.9% of the working-age population in the continental U.S. in 2001 and 94.9% in 2019.

For the restaurant industry (NAICS 722) and the entire private sector, we obtain employment—defined in the QWI as "the count of people employed in a firm at any time during the quarter"—and average monthly earnings for employees who remain with the same firm throughout the quarter. Quarterly state-level minimum wages are obtained from Vaghul and Zipperer (2022), and annual working-age population data at the CBSA-by-state level come from the Census Bureau's Population Estimates Program.[5] In total, our panel could contain up to 75,316 observations (991 geographies across 76 quarters). However, since some states entered the QWI data after 2001, our final panel includes 74,151 observations (98.45%) with complete employment data and 74,036 observations (98.3%) with complete earnings data.

## 2.2 Monopsony Power Proxies

We construct our time-invariant monopsony power proxies from three sources: labor market fluidity measures from the Census Bureau's J2J and QWI data, and concentration measures from the NETS database.

### 2.2.1 Labor Market Fluidity Measures

The search-theory monopsony model of Manning (2003), inspired by Burdett and Mortensen (1998), links the firm-level labor supply elasticity (a smaller elasticity implies more monopsony power) with the arrival rate of job offers for the firm's employees, so that higher hiring and separation rates indicate a more elastic labor supply. Ideally, one would calculate the labor supply elasticity of each firm from firm-level hiring and separation data—as done by Webber (2015) using restricted LEHD data—and then obtain an average elasticity across firms for each labor market. Nevertheless, the turnover measures in the J2J and QWI data are produced at the CBSA or CBSA-by-state levels from firm-level LEHD data, and hence should represent good measures of the labor market competitiveness faced by the average firm.

While both J2J and QWI data reflect worker flows from the same LEHD infrastructure, J2J is especially close to the Burdett–Mortensen–Manning concept of employees receiving competing offers and (nearly) immediately switching firms. That is because J2J data capture job-to-job transitions—i.e., moves without any intervening non-employment or only a brief (sub-quarter)

[5] In our quarterly regressions below, we interpolate annual population data into quarterly values using simple linear extrapolation. Because population typically changes slowly and smoothly over short periods, this method introduces minimal measurement error.

non-employment spell. This feature corresponds closely to the theoretical channel generating high labor supply elasticity at the firm level in search-and-matching models. QWI, by contrast, includes hires and separations to or from non-employment (of any duration), making it a broader turnover metric but still reflecting a substantial share of worker mobility. Thus, while J2J's job-switch rates best approximate the instantaneous offer-arrival framework in search-based theory, QWI turnover measures may also serve as good proxies for overall labor market fluidity.

Due to confidentiality restrictions, J2J data are only released at the CBSA level for the Accommodation and Food Services industry (NAICS 72), yielding data for 380 out of 919 CBSAs. Consequently, for multi-state CBSAs, when we use the J2J data we apply the same measure on both sides of the border, obtaining values for 436 of our 991 CBSA-by-state units. In contrast, QWI coverage is far more complete, producing measures for the restaurant industry (NAICS 722) at the CBSA-by-state level in 990 of the 991 geographies.

Each J2J or QWI measure is averaged over the four quarters of 2001 or—if a state enters the LEHD later—the first four quarters for which data are available.[6] Let $\theta^J_{k,i}$ and $\theta^Q_{k,i}$ denote fluidity measure $k$ for geography $i$ from J2J or QWI, respectively. Our eight J2J measures are the *(i)* total J2J hiring rate, *(ii)* total J2J separation rate, *(iii)* overall hiring rate, *(iv)* overall separation rate, *(v)* J2J continuous hiring rate, *(vi)* J2J continuous separation rate, *(vii)* J2J brief-nonemployment hiring rate, and the *(viii)* J2J brief-nonemployment separation rate. The "overall" rates include non–job-to-job transitions, resembling QWI, whereas the subdivided J2J rates isolate purely continuous vs. brief-nonemployment spells. Our seven QWI measures comprise the *(i)* hiring rate, *(ii)* separation rate, *(iii)* the replacement hiring rate (the fraction of workers being replaced), *(iv)* turnover (average of hiring and separation rates), *(v)* stable hires rate, *(vi)* stable separations rate, and *(vii)* stable turnover—where "stable" jobs last at least one full quarter.

Table 1 reports descriptive statistics for all these measures, as well as minimums and maximums for their demeaned values.[7] For each measure, larger values indicate a more fluid—and therefore more competitive—labor market, whereas smaller values suggest stronger monopsony power. Table A-1 in the Appendix presents the correlations among all J2J and QWI measures. The correlation among J2J measures ranges from 0.76 to 0.99, with an average of 0.88; among QWI measures, it

[6]We use monopsony power measures from the first year rather than contemporaneous or time-varying values. This choice aligns with standard practice in minimum wage studies that also use fixed initial weights (e.g., population or employment weights from the first year) to avoid endogenous shifts in composition over time. Time-varying monopsony measures are likely endogenous to the policy itself—minimum wage increases may influence hiring rates, separation rates, firm entry or exit, or employment shares, thereby creating a bias in the estimated effects. By holding monopsony power proxies fixed, we ensure that variation in the interaction term from section 4 reflects differences in pre-existing labor market structure, not responses to the minimum wage.

[7]For ease of interpretation, we use demeaned monopsony proxies in our specifications below.

Table 1: Descriptive statistics for monopsony power proxies

| | | Obs. | Mean | Std. dev. | $\theta_{k,i} - \bar{\theta}_k$ Min | Max |
|---|---|---|---|---|---|---|
| ***A. J2J fluidity measures*** | | | | | | |
| $\theta_1^J$ | Total J2J hiring rate | 436 | 0.103 | 0.019 | -0.068 | 0.045 |
| $\theta_2^J$ | Total J2J separation rate | 436 | 0.116 | 0.020 | -0.079 | 0.052 |
| $\theta_3^J$ | Overall hiring rate | 436 | 0.243 | 0.034 | -0.159 | 0.121 |
| $\theta_4^J$ | Overall separation rate | 436 | 0.234 | 0.033 | -0.149 | 0.129 |
| $\theta_5^J$ | J2J continuous hiring rate | 436 | 0.064 | 0.012 | -0.042 | 0.032 |
| $\theta_6^J$ | J2J continuous separation rate | 436 | 0.075 | 0.013 | -0.051 | 0.036 |
| $\theta_7^J$ | J2J brief-nonemp. hiring rate | 436 | 0.039 | 0.007 | -0.026 | 0.017 |
| $\theta_8^J$ | J2J brief-nonemp. sep. rate | 436 | 0.040 | 0.007 | -0.028 | 0.017 |
| ***B. QWI fluidity measures*** | | | | | | |
| $\theta_1^Q$ | Hiring rate | 990 | 0.266 | 0.035 | -0.131 | 0.123 |
| $\theta_2^Q$ | Separation rate | 990 | 0.262 | 0.033 | -0.099 | 0.144 |
| $\theta_3^Q$ | Replacement hiring rate | 990 | 0.187 | 0.030 | -0.101 | 0.107 |
| $\theta_4^Q$ | Turnover rate | 990 | 0.264 | 0.033 | -0.102 | 0.122 |
| $\theta_5^Q$ | Stable hires rate | 990 | 0.201 | 0.023 | -0.070 | 0.090 |
| $\theta_6^Q$ | Stable separations rate | 990 | 0.193 | 0.023 | -0.061 | 0.079 |
| $\theta_7^Q$ | Stable turnover | 990 | 0.197 | 0.022 | -0.063 | 0.078 |
| ***C. NETS concentration measures*** | | | | | | |
| $\theta_1^N$ | Establishments per worker | 991 | 0.070 | 0.018 | -0.050 | 0.095 |
| $\theta_2^N$ | Firms per worker | 991 | 0.066 | 0.020 | -0.047 | 0.099 |
| $\theta_3^N$ | $1 -$ HHI (estab.) | 991 | 0.973 | 0.047 | -0.973 | 0.027 |
| $\theta_4^N$ | $1 -$ HHI (firms) | 991 | 0.970 | 0.047 | -0.970 | 0.029 |

ranges from 0.71 to 0.98, with an average of 0.86. Correlations between J2J and QWI measures are also high, ranging from 0.56 to 0.80, with an average of 0.66. In the table's last row, we also report correlations of the labor fluidity monopsony measures with the log of working-age population in 2001; these are consistently negative, ranging from $-0.3$ to $-0.02$ and averaging $-0.21$. This points to an important feature of our data: larger cities are not necessarily more competitive according to labor fluidity measures—in fact, the simple correlations indicate the opposite.

### 2.2.2 Labor Market Concentration Measures

This section describes our concentration-based proxies for monopsony power. Much of the literature infers monopsony from market concentration measures, such as the HHIs, which reflect the degree to which a few large employers dominate local labor demand—see Azar and Marinescu (2024) for an extensive review of the theoretical and empirical literatures on HHIs as measures of wage-setting power.

Using 2001 data from NETS, we construct four measures of employment concentration at the CBSA-by-state level for the U.S. restaurant industry: establishments per worker, firms per worker, establishment-level HHI, and firm-level HHI. We distinguish between establishment- and firm-level metrics because a single company may operate multiple restaurants in the same labor market. If each restaurant hires and sets wages independently, then establishment-level measures should better capture competition for workers. However, if these restaurants effectively act as a single employer, firm-level measures are more relevant for gauging monopsony power.[8]

More establishments or firms per worker typically indicate a more competitive labor market, whereas a higher HHI indicates greater concentration. Although HHI is commonly reported in the 0 to $10,000$ range, we use a $(0, 1]$ normalization. For product markets, the *Horizontal Merger Guidelines* of the U.S. Department of Justice and the Federal Trade Commission classify a market as moderately concentrated if its HHI is between 0.15 and 0.25, and highly concentrated if it exceeds 0.25.[9] In our sample of 991 CBSA-by-state geographies, the mean employment-concentration HHIs for the restaurant industry are about 0.027 at the establishment level and 0.030 at the firm level, with corresponding 99th-percentile values of 0.139 and 0.147. Only eight geographies have establishment-level HHIs above 0.15, and nine exceed that threshold at the firm level. Hence, at least by product-market standards, most local restaurant labor markets appear competitive in terms of HHIs.

Let $\theta^N_{k,i}$ denote concentration measure $k$ for geography $i$ from the NETS database. We construct four such measures: *(i)* establishments per worker, *(ii)* firms per worker, *(iii)* $1 - \text{HHI}$ at the establishment level, and *(iv)* $1 - \text{HHI}$ at the firm level. We use $1 - \text{HHI}$ so that all fluidity and concentration measures are aligned: a higher value indicates a more competitive labor market, whereas a lower value indicates stronger monopsony power.

Table 1, referenced above, presents descriptive statistics for these concentration measures, including minimum and maximum values for the demeaned variables. We also report correlations between concentration and fluidity measures in Table A-1 in the Appendix. Notice that correlations are high between the establishment- and firm-level measures, but range from $-0.20$ to $-0.09$ between the employers-per-worker measures and the $1 - \text{HHI}$ measures; thus, more employ-

[8]Alternatively, some studies (e.g., Azar et al., 2024) proxy monopsony power using the log number of employers, assuming that fewer employers imply greater wage-setting power. We avoid this approach because, in our NETS data for the restaurant industry, the 2001 correlation between population and the log number of establishments (or firms) exceeds 0.98 across CBSA-by-state entities. In our main specification, such extreme multicollinearity would prevent reliable identification of the coefficients on the minimum wage interactions with population and monopsony power.

[9]See section 5.3 in the official guidelines.

ers per worker are not necessarily associated with smaller HHIs. Strikingly, all fluidity measures are negatively correlated with establishments- and firms-per-worker (correlations from $-0.47$ to $-0.14$, averaging $-0.24$).[10] As well, $1-\text{HHI}$ is negatively correlated with the J2J fluidity measures (average $-0.076$) and essentially uncorrelated with the QWI fluidity measures (average 0.022). Hence, labor markets that look competitive based on fluidity metrics can appear monopsonistic when judged by concentration metrics, and vice versa, underscoring why it is crucial to distinguish among different proxies for monopsony power. Lastly, the bottom row of Table A-1 shows that more populous markets have fewer employers per worker (correlations of $-0.33$ and $-0.43$)—indicating less competition—yet are less concentrated according to HHI metrics (correlations of 0.46 and 0.47 with $1-\text{HHI}$)—indicating more competition. Thus, depending on which monopsony measure is used, the relationship between city size and labor market power may lead to different conclusions about how monopsony shapes the employment effects of minimum wages.

# 3 Minimum Wage Effects and City Size

The classic paper by Glaeser and Maré (2001) on the urban wage premium begins by noting that the positive relationship between metropolitan area size and average annual earnings is not debatable, and that this relationship is "neither new nor temporary." Surprisingly, previous literature has paid little attention to how this relationship shapes employment responses to minimum wages. Before turning to our analysis of monopsony power, this section demonstrates that accounting for city size heterogeneity is crucial when estimating the effects of minimum wage policies. This is important to our inquiry given that city size is correlated with measures of monopsony.

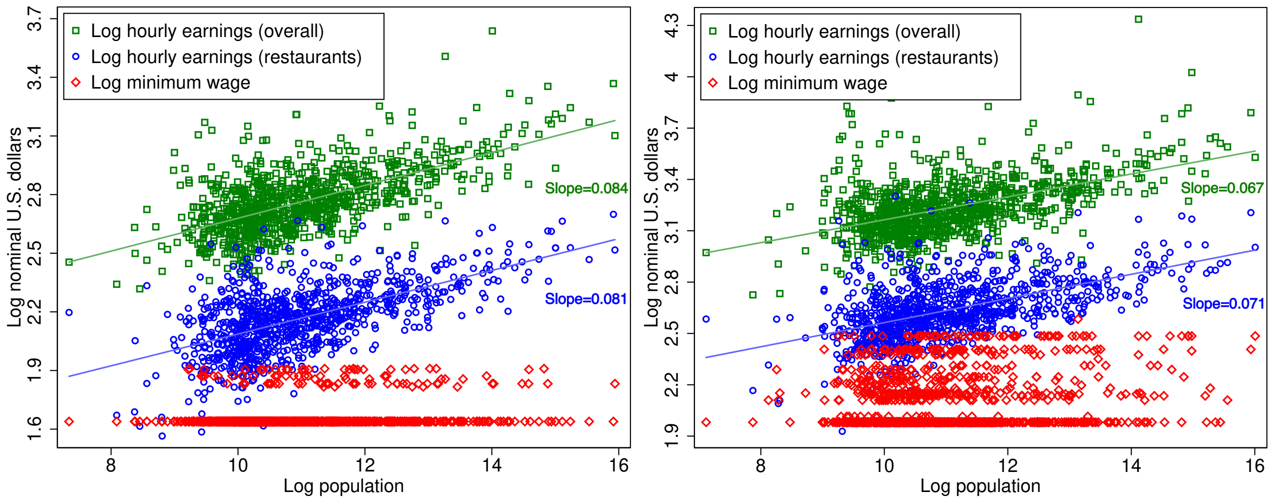
## 3.1 The Bite of Minimum Wages Across Labor Markets

If larger cities pay higher wages, but minimum wages vary less, then minimum wages will have a smaller bite in those areas. To provide evidence on this, we combine QWI data on average monthly earnings with average weekly hours from the BLS's Current Employment Statistics (CES) to construct hourly earnings for both the restaurant industry and the overall private sector.[11]

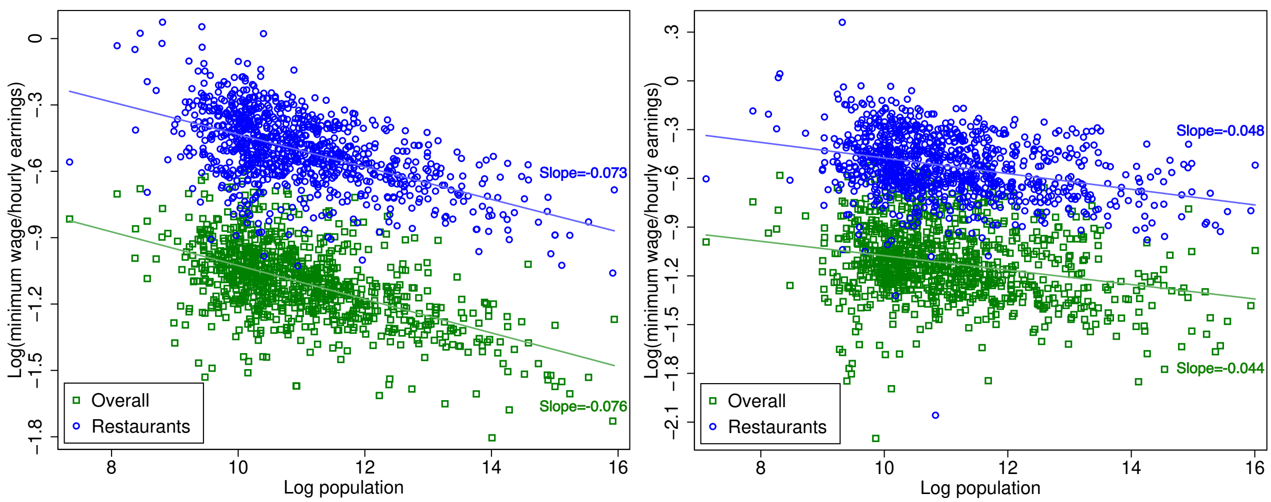
For each CBSA-by-state geography, Figure 1a plots log hourly earnings (for both restaurants

[10] In related work, Bagga (2023) uses MSA-by-industry panel data from 2000–2017 to show that firms per worker and job-to-job transitions may serve as alternative proxies for monopsony power. Although she finds that employers per worker and labor market fluidity are positively correlated at the aggregate level, we find the opposite pattern for the restaurant industry. This contrast reflects that relationships observed across industries in the aggregate do not necessarily hold within individual industries.

[11] According to the CES, average weekly hours from 2006–2019 were 25.3 in the restaurant industry and 34.4 in the overall private sector, remaining stable over time. Restaurant industry hours are reported at this link, and overall private sector hours at this link.




(a) Earnings and population — 2nd quarter of 2001 (left) and 2019 (right)




(b) Kaitz indexes and population — 2nd quarter of 2001 (left) and 2019 (right)

Figure 1: Minimum wages are less binding in larger CBSA-by-state entities

and the overall sector) and log minimum wages against log population, using data from the second quarters of 2001 and 2019 (the first and last years of our sample). In both years, there is a strong positive relationship between city size and hourly earnings not only overall (slopes of 0.084 in 2001 and 0.067 in 2019) but also in restaurants (slopes of 0.081 and 0.071). Because the relationship holds for both low-wage (restaurant) and broader private employment, it is unlikely to be driven by differences in industry composition across large and small areas. In addition, as log population increases, the gap between hourly earnings and the minimum wage becomes wider, indicating that the bite of the minimum wage is smaller in bigger labor markets.

Figure 1b further illustrates this point by plotting log Kaitz indexes—the log ratio of the minimum wage to average hourly earnings—against log population. A negative slope indicates that the minimum wage is less binding in larger markets. While the absolute value of the linear-fit slope has decreased—e.g., from $-0.073$ in 2001 to $-0.048$ in 2019 for the restaurant industry—there remains a clear pattern: the effects of the minimum wage are likely to be smaller in larger cities.

### 3.2 Estimating Minimum Wage Effects for Different City Sizes

To verify how minimum wage elasticities depend on city size, we estimate the following two-way fixed effects model:

$$\ln e_{it} = \alpha + \beta \ln MW_{it} + \gamma \Big[ \ln MW_{it} \times \big( \ln P_i - \overline{\ln P} \big) \Big] + \rho \ln E^{-}_{it} + \zeta \ln P_{it} + \eta_i + \tau_t + \nu_{it}, \qquad (1)$$

where $e_{it}$ denotes restaurant employment for CBSA-by-state $i$ in quarter $t$, $MW_{it}$ is the minimum wage, $P_i$ is the working-age population in 2001, and $\overline{\ln P}$ is the mean of $\ln P_i$ across all CBSA-by-state entities. The specification includes standard controls used in the minimum wage literature (e.g., Dube, Lester, and Reich, 2010; Azar et al., 2024): $E^{-}_{it}$, employment in all other industries (capturing local cyclical conditions), and $P_{it}$, the current working-age population. Terms $\eta_i$ and $\tau_t$ are entity and time fixed effects, respectively, and $\nu_{it}$ is the error term.

From (1), the minimum wage elasticity of employment for geography $i$ is

$$\beta + \gamma \big( \ln P_i - \overline{\ln P} \big),$$

so $\beta$ represents the elasticity for the average-size geography. We refer to $\ln P_i - \overline{\ln P}$ as the log population deviation. In our sample, the log population deviation ranges from $-3.64$ to $4.97$, with the 90th and 99th percentiles at 1.76 and 3.91, respectively. Table 2 reports results from estimating (1), where columns (1)–(2) impose the restriction $\gamma = 0$, and columns (3)–(4) estimate the unrestricted model. Columns (1) and (3) present unweighted estimates, while columns (2) and (4) use initial population weights (from 2001).

The first hint of the crucial effect of city size on the minimum wage elasticity of employment arises when comparing columns (1) and (2) in Panel A of Table 2. Notice that the estimated elasticity changes from a significant value of $-0.126$ to an insignificant and small positive estimate of 0.021 when population weights are used. This suggests that a few very populous geographies—where minimum wages bind the least—dominate the population-weighted estimate.

Once we estimate the unrestricted model, in columns (3) and (4), the estimates for $\beta$ and $\gamma$ yield similar results whether specification (1) is estimated without weights or with population weights. The estimate for $\beta$ is negative and significant (either $-0.132$ or $-0.186$), and the estimate for $\gamma$ is positive and significant (either 0.031 or 0.065). Hence, as geography size increases, the estimated

Table 2: TWFE estimation of minimum wage effects on restaurant employment and earnings using 2001-2019 QWI data

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| ***A. ln(employment)*** | | | | |
| ln(minimum wage) | -0.126*** | 0.021 | -0.132*** | -0.186*** |
| | (0.045) | (0.044) | (0.043) | (0.044) |
| $\ln \text{MW} \times (\ln P - \overline{\ln P})$ | | | 0.031*** | 0.065*** |
| | | | (0.010) | (0.015) |
| ln(employment$^-$) | 0.237*** | 0.503*** | 0.244*** | 0.452*** |
| | (0.028) | (0.145) | (0.029) | (0.094) |
| ln(population) | 0.801*** | 0.480** | 0.748*** | 0.449*** |
| | (0.065) | (0.184) | (0.069) | (0.148) |
| ***B. ln(earnings)*** | | | | |
| ln(minimum wage) | 0.202*** | 0.160*** | 0.211*** | 0.228*** |
| | (0.020) | (0.019) | (0.020) | (0.022) |
| $\ln \text{MW} \times (\ln P - \overline{\ln P})$ | | | -0.039*** | -0.021*** |
| | | | (0.006) | (0.006) |
| ln(earnings$^-$) | 0.327*** | 0.239*** | 0.301*** | 0.233*** |
| | (0.036) | (0.072) | (0.032) | (0.071) |
| ln(population) | 0.072* | -0.018 | 0.135*** | 0.007 |
| | (0.039) | (0.034) | (0.039) | (0.033) |
| CBSA-by-state effects | Y | Y | Y | Y |
| Quarter effects | Y | Y | Y | Y |
| Population weights | | Y | | Y |

Notes: This table reports $\hat{\beta}$, $\hat{\gamma}$, $\hat{\rho}$, and $\hat{\zeta}$ from the estimation of specification (1) for the restaurant industry using 2001-2019 quarterly data for 991 CBSA-by-state entities. In panel A, the dependent variable is log employment and uses 74,151 observations. In panel B the dependent variable is log earnings per worker and uses 74,036 observations. Columns (1)-(2) exclude the minimum wage/population interaction. Standard errors (in parentheses) are clustered at the state level. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

minimum wage elasticity of employment moves closer to zero, confirming that minimum wages bind less in more populous areas.[12]

If minimum wages bind less in more populous places, this should also imply that their earnings effects are weaker in larger areas. To verify this, Panel B in Table 2 estimates restricted and unrestricted versions of specification (1), but using log average monthly earnings in the restaurant industry as the dependent variable.[13] In the restricted regressions ($\gamma = 0$) in columns (1)–(2),

[12] Figure A-2a in the Appendix uses the estimates for $\beta$ and $\gamma$ from columns (3)-(4) in Panel A to show the estimated elasticities along the log population deviation range, including 90% confidence bands. For both specifications, the estimated elasticities are negative and significant for about 90% of our geographies. A positive and significant elasticity appears only for the top 1% of the largest geographies when using population weights.

[13] We use log average monthly earnings in the overall private sector as a control (ln(earnings)$^-$ in Table 2), which

the estimated earnings elasticities are positive and significant both cases, but the one estimated with population weights is smaller in magnitude than the other two (0.160 vs. 0.202), suggesting that minimum wage effects on earnings are smaller in the largest areas. This is confirmed in the unrestricted regressions in columns (3)–(4), which show positive, significant, and similar estimates for $\beta$ (either 0.211 or 0.228), and negative and significant estimates for $\gamma$ (either $-0.039$ or $-0.021$).[14]

The conclusion is clear: minimum wages bind less in larger cities, as reflected by minimum wage elasticities for both employment and earnings that move closer to zero as population size increases. Tying this analysis to the urban wage premium puts a more substantive interpretation on the sensitivity of minimum wage-employment effects to population size than the simple "mechanical" view that the estimates are sensitive to weighting.

# 4 Minimum Wages Effects and Monopsony Power

Having established that it is crucial to account for city size heterogeneity when estimating minimum wage elasticities, this section presents our main results on how these elasticities vary with different measures of monopsony power.

Our main specification is

$$\ln e_{it} = \alpha + \left[\beta + \gamma\left(\ln P_i - \overline{\ln P}\right) + \delta\left(\theta_i - \overline{\theta}\right)\right] \ln MW_{it} + \rho \ln E_{it}^{-} + \zeta \ln P_{it} + \eta_i + \tau_t + \nu_{it}, \qquad (2)$$

which expands specification (1) by adding $\delta\left(\theta i - \overline{\theta}\right) \ln MW_{it}$, where $\theta_i$ is the measure of monopsony power for CBSA-by-state $i$, and $\overline{\theta}$ is the average for that measure across geographies. As described in Sections 2.2.1 and 2.2.2 and summarized in Table 1, we use 19 different monopsony power proxies—15 fluidity measures and 4 concentration measures—constructed from J2J, QWI, and NETS data. For exposition purposes, the main text focuses on a selection of 9 of these measures: total J2J hiring rate ($\theta_1^J$), total J2J separation rate ($\theta_2^J$), QWI hiring rate ($\theta_1^Q$), QWI separation rate ($\theta_2^Q$), QWI replacement rate ($\theta_3^Q$), and the 4 NETS concentration metrics (from $\theta_1^N$ to $\theta_4^N$). All results for the remaining J2J and QWI fluidity metrics are presented in the Appendix; the conclusions are similar and if anything somewhat more stark than those discussed below.[15]

is obtained directly from the QWI.

[14] Using the estimates from Panel B in columns (3) and (4), Figure A-2b in the Appendix shows the estimated minimum wage elasticities for earnings along the log population deviation range, with 90% confidence bands. The estimated elasticities are positive across all geographies, and nonsignificant for fewer than 1% of them in the unweighted specification.

[15] While modern difference-in-differences estimators (e.g., Callaway and Sant'Anna, 2021) improve identification by allowing for treatment effect heterogeneity across cohorts, they are not designed to accommodate heterogeneity along continuous pretreatment characteristics, such as city size or monopsony power. One possible workaround is to split the sample into subgroups (e.g., high vs. low HHI), as in Azar et al. (2024), but applying this approach to

From (2), the minimum wage elasticity of employment for CBSA-by-state $i$ is given by

$$\beta + \gamma \left(\ln P_i - \overline{\ln P}\right) + \delta \left(\theta_i - \overline{\theta}\right),$$

so that if geography $i$ has an average level of monopsony power ($\theta_i = \overline{\theta}$), its elasticity is $\beta + \gamma \left(\ln P_i - \overline{\ln P}\right)$. Given that, for all our monopsony proxies, a higher value of $\theta$ indicates (in principle) a more competitive labor market—either a more fluid or less concentrated one—we expect the estimate for $\delta$ to be negative if more competitive markets exhibit stronger adverse employment responses to minimum wage hikes.

Table 3 presents $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the estimation of (2) using our selected 9 monopsony power proxies and weighting by population. To examine the impact of excluding the population-minimum wage interaction, we also show $\hat{\beta}$ and $\hat{\delta}$ from the estimation of (2) under the restriction that $\gamma = 0$—we refer to this as the "restricted" model.[16] The regressions with J2J fluidity measures in Panel A have fewer observations than those in Panels B and C (32,643 vs. 74,139 and 74,151), due to limited data availability from J2J. Recall from Table 1 that of 991 CBSA-by-state entities, we have J2J measures for 436, QWI measures for 990, and NETS measures for 991.

From Panels A and B, all coefficients in the fluidity specifications have the expected sign and are statistically significant. Hence, more fluid labor markets exhibit more adverse responses to minimum wage hikes. Comparing the restricted and unrestricted models, the estimate for $\beta$ is biased toward zero when the size–minimum wage interaction is ignored, while the estimate for $\delta$ is downward biased, leading to overstatement of the influence of labor market power on the employment effects of minimum wages. This downward bias is explained by the negative relationship between fluidity measures and population size.[17]

Panel C reports results using the four concentration measures from NETS. Focusing on the specifications using establishments per worker or firms per worker, $\theta_1^N$ and $\theta_2^N$, the estimates for $\delta$ are positive and significant in both the restricted and unrestricted models. This suggests that labor markets with more establishments or firms per worker do not face more adverse employment effects from minimum wages. Hence, lower concentration—as captured by larger values of $\theta_1^N$ and $\theta_2^N$—may not necessarily indicate greater labor market competitiveness. (Or, if they do, this competitiveness does not influence minimum wage-employment effects as expected.)

multiple continuous variables complicates implementation and interpretation. By contrast, our TWFE framework in specification (2) allows us to flexibly estimate interaction effects with both city size and monopsony measures in a unified setup.

[16] We do not report estimates for the control variables.

[17] Table A-2 in the Appendix shows the same story when using the remaining 10 fluidity measures: there is a robust negative association between labor market fluidity and the employment effects of minimum wages.

Table 3: TWFE estimation of minimum wage effects on restaurant employment for different monopsony power measures

| | **Restricted** ($\gamma = 0$) | | **Unrestricted** | | |
|---|---|---|---|---|---|
| | $\hat{\beta}$ | $\hat{\delta}$ | $\hat{\beta}$ | $\hat{\gamma}$ | $\hat{\delta}$ |
| ***A. Job-to-Job fluidity monopsony proxies (32,643 obs.)*** | | | | | |
| *J2J Hiring rate* ($\theta_1^J$) | -0.082** | -5.687*** | -0.179*** | 0.037*** | -4.208*** |
| | (0.033) | (1.135) | (0.044) | (0.011) | (0.819) |
| *J2J Sep. rate* ($\theta_2^J$) | -0.104*** | -5.805*** | -0.170*** | 0.027*** | -4.693*** |
| | (0.034) | (1.026) | (0.046) | (0.010) | (0.745) |
| ***B. QWI fluidity monopsony proxies (74,139 obs.)*** | | | | | |
| *Hiring rate* ($\theta_1^Q$) | -0.060* | -3.154*** | -0.174*** | 0.048*** | -1.661*** |
| | (0.033) | (0.778) | (0.045) | (0.014) | (0.425) |
| *Separation rate* ($\theta_2^Q$) | -0.064* | -3.135*** | -0.180*** | 0.051*** | -1.499*** |
| | (0.034) | (0.794) | (0.047) | (0.015) | (0.455) |
| *Replacement rate* ($\theta_3^Q$) | -0.080* | -4.164*** | -0.181*** | 0.042*** | -2.821*** |
| | (0.040) | (1.021) | (0.053) | (0.011) | (0.723) |
| ***C. NETS concentration monopsony proxies (75,151 obs.)*** | | | | | |
| *Est. p/worker* ($\theta_1^N$) | 0.057* | 6.128*** | -0.154*** | 0.067*** | 6.497*** |
| | (0.031) | (1.804) | (0.048) | (0.012) | (1.076) |
| *Firms p/worker* ($\theta_2^N$) | 0.067* | 5.051*** | -0.152*** | 0.073*** | 6.364*** |
| | (0.035) | (1.764) | (0.048) | (0.011) | (0.955) |
| $1 -$ *HHI (est.)* ($\theta_3^N$) | -0.033 | 2.356** | -0.178*** | 0.069*** | -0.958 |
| | (0.037) | (1.009) | (0.041) | (0.017) | (0.667) |
| $1 -$ *HHI (firm)* ($\theta_4^N$) | -0.043 | 2.719** | -0.182*** | 0.068*** | -0.552 |
| | (0.036) | (1.147) | (0.042) | (0.016) | (0.486) |

Notes: This table reports $\hat{\beta}$ and $\hat{\delta}$ from the estimation of specification (2) under $\gamma = 0$ (restricted model), and $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\delta}$ from the unrestricted estimation of specification (2) for the restaurant industry using 2001-2019 QWI data and different monopsony power proxies. Regressions are weighted by initial population. Standard errors (in parentheses) are clustered at the state level. The coefficients are statistically significant at the *10%, **5%, or ***1% level.

Another notable result from these specifications is the substantial upward bias in $\hat{\beta}$ in the restricted model, which yields positive and significant point estimates of 0.057 and 0.067 (these are estimates at the means of the concentration measures). These contrast sharply with the significant estimates of $-0.154$ and $-0.152$ obtained once the size–minimum wage interaction is accounted for.

For the $1 - \text{HHI}$ measures, $\theta_3^N$ and $\theta_4^N$, we find positive and significant estimates for $\delta$ in the restricted specifications, but negative and insignificant coefficients in the unrestricted specifications. Thus, lower concentration—reflected by lower HHIs and hence higher values of $\theta_3^N$ and $\theta_4^N$—is not significantly associated with more adverse minimum wages effects on employment. The upward bias in $\hat{\beta}$ under the restricted model is also evident, as shown by the contrast with the sizable and significant estimates for $\beta$ and $\gamma$ in the unrestricted specifications.

Overall, the results in Panel C do not support the hypothesis that greater concentration implies more monopsony power and therefore weaker employment effects of minimum wages. In fact, the establishments- and firms-per-worker measures suggest the opposite, while the HHI-based measures yield inconsistent and imprecise estimates. So either monospony power has the predicted effects but these measures are flawed, at least for the restaurant industry, or monopsony power does not have the predicted effects.[18]

Figure 2 illustrates how each of our 9 selected monopsony power proxies alters the distribution of predicted minimum wage elasticities across CBSA-by-state entities. For our CBSA-by-state geographies (436 when using J2J, 990 when using QWI, and 991 when using NETS), we use the estimates from Table 3 to calculate $\hat{\beta} + \hat{\delta}\left(\theta_i - \overline{\theta}\right)$ for the restricted specifications, and

$$\hat{\beta} + \hat{\gamma}\left(\ln P_i - \overline{\ln P}\right) + \hat{\delta}\left(\theta_i - \overline{\theta}\right)$$

for the unrestricted specifications. The boxplots labeled "0" show the baseline distribution of elasticities without monopsony power interactions (i.e., from Table 2, we use column (2) for the restricted case and column (4) for the unrestricted case). Each of the other boxplots reflects the distribution associated with a different monopsony proxy, with colors indicating the data source (J2J, QWI, or NETS).

The first takeaway from Figure 2 is the clear upward bias in estimated elasticities when using the restricted model ($\gamma = 0$), underscoring the findings from Section 3. While the J2J and QWI monopsony proxies help mitigate this bias to some extent, the NETS-based proxies continue to produce notably upward-biased elasticity estimates.

For the unrestricted-model boxplots—except for those based on the HHI measures, $\theta_3^N$ and $\theta_4^N$—adding the monopsony proxy interactions increases the spread of the elasticity distribution (both the interquartile range and the whiskers) and shifts the median slightly upward, relative to including only the city size interaction. The median changes from $-0.208$ in boxplot "0" to about $-0.155$ for the J2J and the $\theta_1^N$ and $\theta_2^N$ boxplots, and to about $-0.183$ in the QWI boxplots. The

[18] Section B in the Appendix presents a robustness check using a state-border pair identification strategy similar to that of Dube, Lester, and Reich (2010), who argue that conventional TWFE models may suffer from bias due to unobserved time-varying local shocks. Following Jha, Neumark, and Rodriguez-Lopez (2024), who focus on multi-state commuting zones, we construct pairs within multi-state CBSAs to more credibly define local labor markets and estimate a specification that includes pair-period fixed effects. The results generally reinforce our main findings: fluidity-based proxies consistently indicate more negative minimum wage employment effects in more competitive (i.e., more fluid) labor markets. By contrast, estimates for $\delta$ from concentration-based proxies remain mixed, with positive values for the employers-per-worker measures and negative but imprecise estimates for the HHI-based measures.

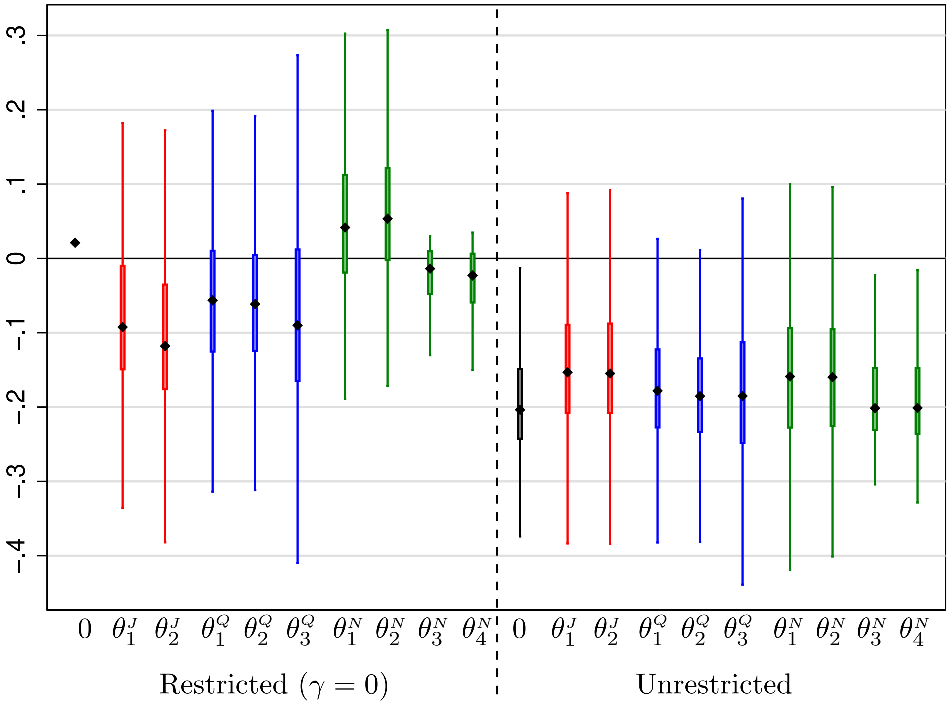



Figure 2: Boxplots of predicted minimum wage elasticities of employment for CBSA-by-state entities by monopsony power proxy

larger median shift in the J2J boxplots is a consequence of sample selection, as these regressions include primarily large CBSAs, which exhibit smaller employment responses to minimum wages.[19]

However, the key result is the difference between the box plots for the restricted and unrestricted models. The estimates that do not account for city size ("restricted") suggest that minimum wage effects are near-zero or positive in many places. In contrast, the boxplots that account for city size ("unrestricted") are shifted down, indicating that even after accounting for monopsony power, minimum wage effects are negative in most places.

## 5 Conclusion

One explanation for studies that find small and insignificant or even positive employment effects of minimum wages is the presence of monopsony power. Under monopsony, an employer does not face a perfectly elastic labor supply, which can soften or even reverse the classic negative employment response to a wage floor. Our results confirm a key prediction of search-theoretic monopsony models: in labor markets where workers readily move across employers (i.e., higher fluidity), minimum wage hikes induce more negative employment effects. By contrast, in less fluid

[19]Figure A-3 in the Appendix presents boxplots for the remaining J2J and QWI fluidity measures, which display patterns similar to those in Figure 2.

markets, the relationship between minimum wages and job losses is weaker.

At the same time, standard concentration measures do not appear to capture the same underlying frictions as fluidity-based proxies. In TWFE regressions, more establishments or firms per worker and lower HHIs—traditional indicators of less concentration—do not yield larger adverse employment effects of minimum wages. Overall, these patterns challenge the notion that labor market competitiveness can be assessed solely by counting employers or measuring how employment is distributed among them. Across specifications, low fluidity—not high concentration—emerges as a more reliable proxy for monopsony power in restaurant local labor markets.

In our view, our results have two implications for future research on the relationship between minimum wages and labor market power. First, unless there is strong prior information on where labor market concentration is associated with more vs. less labor market power, researchers should study this relationship using labor market fluidity measures instead of concentration measures. Second, regardless of the labor market power measure used, research needs to account for how the size of the labor market—acting through the urban wage premium and how binding the minimum wage is—influences the effects of minimum wages on employment. Otherwise, the effects of labor market power in mitigating the adverse employment effects of minimum wages are overstated. Indeed, accounting for city size, we find that although search-theoretic predictions of how minimum wages and labor market power interact are confirmed, minimum wages nevertheless have adverse employment effects in most markets.

## References

Azar, José, Emiliano Huet-Vaughn, Ioana Marinescu, Bledi Taska, and Till von Wachter. 2024. "Minimum Wage Employment Effects and Labour Market Concentration." *Review of Economic Studies* 91 (4):1843–1883. URL https://doi.org/10.1093/restud/rdad091.

Azar, José and Ioana Marinescu. 2024. "Chapter 10 - Monopsony Power in the Labor Market." In *Handbook of Labor Economics*, vol. 5, edited by Christian Dustmann and Thomas Lemieux. Elsevier, 761–827. URL https://www.sciencedirect.com/science/article/pii/S1573446324000099.

Bagga, Sadhika. 2023. "Firm Market Power, Worker Mobility, and Wages in the US Labor Market." *Journal of Labor Economics* 41 (S1):S205–S256.

Burdett, Kenneth and Dale T Mortensen. 1998. "Wage Differentials, Employer Size, and Unemployment." *International Economic Review* 39 (2):257–273. URL http://www.jstor.org/stable/2527292.

Callaway, Brantly and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225 (2):200–230. URL https://www.sciencedirect.com/science/article/pii/S0304407620303948. Themed Issue: Treatment Effect 1.

Dube, Arindrajit, T. William Lester, and Michael Reich. 2010. "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties." *Review of Economics and Statistics* 92 (4):945–964.

Glaeser, Edward L and David C Maré. 2001. "Cities and Skills." *Journal of Labor Economics* 19 (2):316–342.

Jha, Priyaranjan, David Neumark, and Antonio Rodriguez-Lopez. 2024. "What's Across the Border? Re-Evaluating the Cross-Border Evidence on Minimum Wage Effects." *Journal of Political Economy Microeconomics*, forthcoming.

Jha, Priyaranjan and Antonio Rodriguez-Lopez. 2021. "Monopsonistic Labor Markets and International Trade." *European Economic Review* 140:103939. URL https://www.sciencedirect.com/science/article/pii/S0014292121002373.

Manning, Alan. 2003. *Monopsony in Motion: Imperfect Competition in Labor Markets*. Princeton University Press.

Stigler, George J. 1946. "The Economics of Minimum Wage Legislation." *American Economic Review* 36 (3):358–365.

Vaghul, Kavya and Ben Zipperer. 2022. "Historical State and Sub-state Minimum Wages." https://github.com/benzipperer/historicalminwage/releases/tag/v1.4.0. Version 1.4.0.

Webber, Douglas A. 2015. "Firm Market Power and the Earnings Distribution." *Labour Economics* 35:123–134. URL http://www.sciencedirect.com/science/article/pii/S0927537115000706.