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

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December 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. We thank two anonymous referees for helpful comments and suggestions that improved this paper.

1 Introduction

Standard economic theory predicts that in a perfectly competitive labor market, a binding minimum wage leads to job losses. A large empirical literature has debated the magnitude—and in some cases even the sign—of these employment effects, with estimates varying across time periods, sectors, and local labor markets. One prominent explanation for muted or heterogeneous employment responses is monopsony power: when firms face upward-sloping labor supply, a minimum wage can increase wages and employment, mitigating or even reversing the textbook negative employment effect (Stigler, 1946). In this paper we examine how minimum wages affect employment in the U.S. restaurant industry from 2001 to 2019, emphasizing two sources of heterogeneity: systematic differences in how binding minimum wages are across cities of different sizes, and differences in monopsony power across local labor markets.

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 regressions that include an interaction between the minimum wage and city population. 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 and can even change sign as city population increases.[2]

Having established that minimum wage employment effects vary systematically with city size, we then ask how these effects vary with monopsony power. Using Job-to-Job (J2J), Quarterly Workforce Indicators (QWI), and National Establishment Time Series (NETS) data at the CBSA-by-state level, this paper assembles proxies for monopsony power, using both labor-flow-based and concentration-based measures. 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

[1] Although Core-Based Statistical Areas (CBSAs) 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 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.

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 oligopsony frameworks in which fewer competing firms means each faces a more inelastic labor supply, enabling them to suppress wages. Examining both dimensions provides a clearer picture of where and why minimum wages have stronger vs. weaker effects.

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 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 our results, 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.

Although concentration measures like HHI are widely used as measures of monopsony power, 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.[3]

[3]Using matched employer–employee data from Oregon for 2000–2017, Bassier, Dube, and Naidu (2022) cast doubt

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 are able to construct direct HHI measures for the restaurant industry using NETS data, and find a much weaker link between 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.[4] According to the U.S. Office of Management and Budget, a Core-Based

on the use of concentration measures as proxies for monopsony power. They state: "... when we calculate commuting zone by industry by year HHI, we find no evidence that labor supply elasticities are decreasing with concentration, as measured using either payroll or employment. This stands as a cautionary note on the strategy of using labor market concentration to proxy for monopsony power."

[4]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

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: 52×2 from two-state CBSAs, 7×3 from three-state CBSAs, and 2×4 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.[5] Although only 1,832 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.[6] 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 Burdett and Mortensen (1998) equilibrium search model, as summarized in Manning (2003), provides a clear theoretical link between worker mobility and monopsony power: the amount of wage-setting power a firm has depends on how easily its workers can find and take jobs elsewhere.

period, which caused severe and uneven disruptions to the restaurant industry, potentially confounding the effects of minimum wage changes.

[5] Appendix Figure A-1 shows our 919 CBSAs.

[6] 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.

When employed workers frequently receive outside job offers, they have better alternatives and the labor supply facing the firm is more elastic. A worker switching directly from one employer to another is the empirical counterpart of this mechanism. This idea underlies much of the modern empirical monopsony literature. Webber (2015), using LEHD microdata, infers firm-level labor-supply elasticities from patterns of quits and job-to-job moves. Using data from Oregon, Bassier, Dube, and Naidu (2022) estimate labor supply elasticities from the responses of workers' job-switching behavior to wage differences. Guided by this theoretical and empirical work, we use J2J and QWI mobility measures as reduced-form indicators of differences in how easily workers are able to leave their current employers, and thus of variation in monopsony power across local labor markets.

Although both J2J and QWI are derived from the underlying LEHD worker-flow infrastructure, they capture different components of mobility and therefore relate somewhat differently to search-theoretic notions of monopsony. J2J transitions measure direct moves between employers without intervening nonemployment (or, at most, a brief sub-quarter nonemployment spell), and thus most closely reflect the Burdett–Mortensen–Manning mechanism of workers receiving competing offers while employed. In this sense, J2J mobility provides the clearest reduced-form analogue of the job-offer arrival rate to employed workers. QWI hires and separations, by contrast, include transitions to and from nonemployment regardless of duration, and thus provide a broader measure of turnover. While J2J transitions most directly reflect the offer-arrival mechanism emphasized in search-based models, both J2J and QWI turnover measures provide useful information about variation in the fluidity of local labor labor markets that can arise from differences in job offer arrival rates or other sources.[7] Since our analysis relies on cross-market differences in worker mobility rather than structural estimation of firm-level labor supply elasticities, these measures offer practical, theory-motivated proxies for differences in monopsony power across CBSAs.

One limitation of our J2J and QWI transition measures is that they may also capture reallocation toward higher-productivity firms when productivity shocks vary across local markets, rather than only the job-switching behavior emphasized in search-based models of monopsony. This can result in higher J2J and QWI transition rates in more volatile markets, even if underlying frictions—and thus monopsony power—are similar. An important exception is the QWI replacement-hiring

[7] Our interest is in CBSA-level variation in the availability of outside options for workers in the restaurant industry. A higher J2J or QWI transition rate for a CBSA, for example, will capture better outside options for workers and consequently a more competitive labor market. It is not clear to us whether the hiring rate or separation rate is better at capturing this, and thus we present results with both (along with other fluidity measures). The correlation between the J2J hiring and separation rates is 0.94, and the correlation between the QWI hiring and separation rates is 0.92 (see Appendix Table A-1).

rate (one of our fluidity measures), which tracks how often firms hire new workers to fill vacated positions. This measure is less likely to respond to separations driven by negative productivity shocks: when a firm is hit by a negative shock and chooses to contract, it typically does not replace departing workers, so these reallocative separations do not raise the replacement-hiring rate. Instead, replacement hiring is more common when workers voluntarily leave their jobs, especially when they move to better outside opportunities. This type of separation is directly related to the monopsony mechanism emphasized in search models. Thus, by filtering out separations driven by firms' negative productivity shocks, the replacement-hiring rate is unlikely to be contaminated by reallocation dynamics and may provide a cleaner indicator of workers' willingness to leave their current employers.[8]

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.[9] 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.

[8]Below we show that the results using the replacement-hiring rate are similar to those using our other fluidity measures, suggesting that the reallocation mechanism is not a first-order concern empirically.

[9]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 in Section 4 reflects differences in pre-existing labor market structure, not responses to the minimum wage.

Table 1: Descriptive statistics for monopsony power proxies

		Obs.	Mean	Std. dev.	$\theta_{k,i} - \bar{\theta}_k$ Min	Max
A. J2J fluidity measures						
θ_1^J	Total J2J hiring rate	436	0.103	0.019	-0.068	0.045
θ_2^J	Total J2J separation rate	436	0.116	0.020	-0.079	0.052
θ_3^J	Overall hiring rate	436	0.243	0.034	-0.159	0.121
θ_4^J	Overall separation rate	436	0.234	0.033	-0.149	0.129
θ_5^J	J2J continuous hiring rate	436	0.064	0.012	-0.042	0.032
θ_6^J	J2J continuous separation rate	436	0.075	0.013	-0.051	0.036
θ_7^J	J2J brief-nonemp. hiring rate	436	0.039	0.007	-0.026	0.017
θ_8^J	J2J brief-nonemp. sep. rate	436	0.040	0.007	-0.028	0.017
B. QWI fluidity measures						
θ_1^Q	Hiring rate	990	0.266	0.035	-0.131	0.123
θ_2^Q	Separation rate	990	0.262	0.033	-0.099	0.144
θ_3^Q	Replacement hiring rate	990	0.187	0.030	-0.101	0.107
θ_4^Q	Turnover rate	990	0.264	0.033	-0.102	0.122
θ_5^Q	Stable hires rate	990	0.201	0.023	-0.070	0.090
θ_6^Q	Stable separations rate	990	0.193	0.023	-0.061	0.079
θ_7^Q	Stable turnover	990	0.197	0.022	-0.063	0.078
C. NETS concentration measures						
θ_1^N	Establishments per worker	991	0.070	0.018	-0.050	0.095
θ_2^N	Firms per worker	991	0.066	0.020	-0.047	0.099
θ_3^N	$1 - \text{HHI}$ (estab.)	991	0.973	0.047	-0.973	0.027
θ_4^N	$1 - \text{HHI}$ (firms)	991	0.970	0.047	-0.970	0.029

Table 1 reports descriptive statistics for all these measures, as well as minimums and maximums for their demeaned values.[10] For each measure, larger values indicate a more fluid—and therefore more competitive—labor market, whereas smaller values suggest stronger monopsony power. Appendix Table A-1 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 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 last row of Appendix Table A-1, 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:

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

larger cities are not necessarily more competitive according to labor fluidity measures, and in fact the simple correlations indicate the opposite. This finding is surprising if we think in terms of scale. However, search frictions can come from other sources. One source of frictions may be lack of information about pay at other firms (Manning, 2003). In a smaller labor market, a worker's network may provide information about pay at a larger share of employers, and more of those employers may be close by. A second source of frictions is workers' preferences, such as tastes for non-wage amenities or relationships with colleagues and managers (e.g., Card, Cardoso, Heining, and Kline, 2018; Berger, Herkenhoff, and Mongey, 2022). Caldwell, Haegele, and Heining (2025) present survey evidence that workers often prefer to remain at their current job over a higher-paying alternative, citing a reluctance to undergo change, firm amenities, and social connections. We do not think there is an a priori predicted sign for the relationship between these sources of frictions and market size. Another factor may be distance; the "scale" of the labor market in a large city may be much more limited by travel time or constraints owing to public transportation networks.

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.[11]

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*

[11] 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.

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.[12] 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 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 Appendix Table A-1. 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 employers 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).[13] 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.

[12]See Section 5.3 in the official guidelines. Of course, these thresholds are derived for product-market concentration rather than labor markets, so we view them as at best a useful benchmark rather than a criterion based on direct evidence on monopsony power.

[13]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.

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.

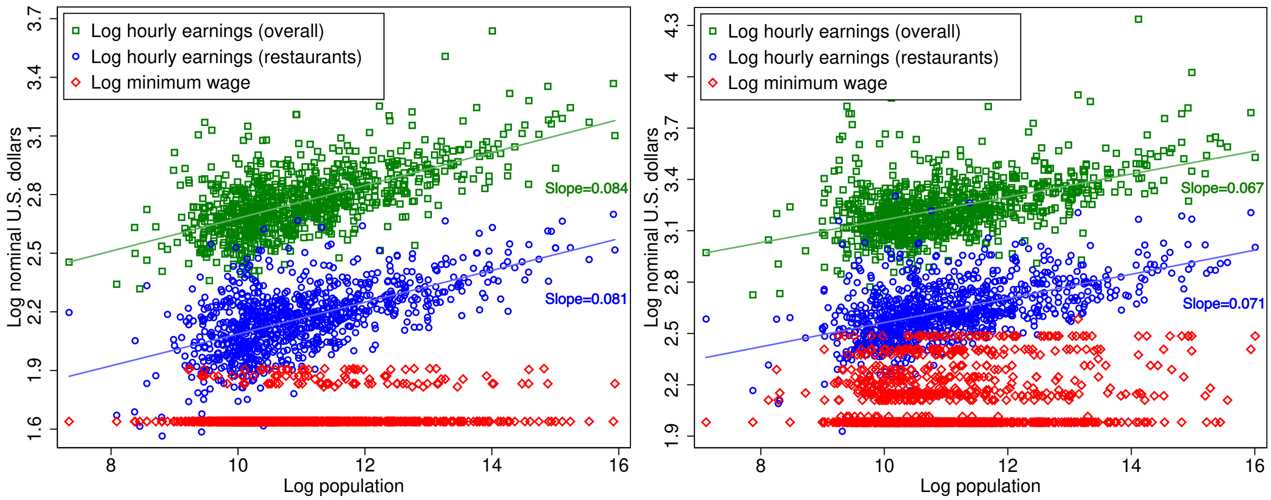
3.1 The Bite of Minimum Wages Across Labor Markets

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.[14]

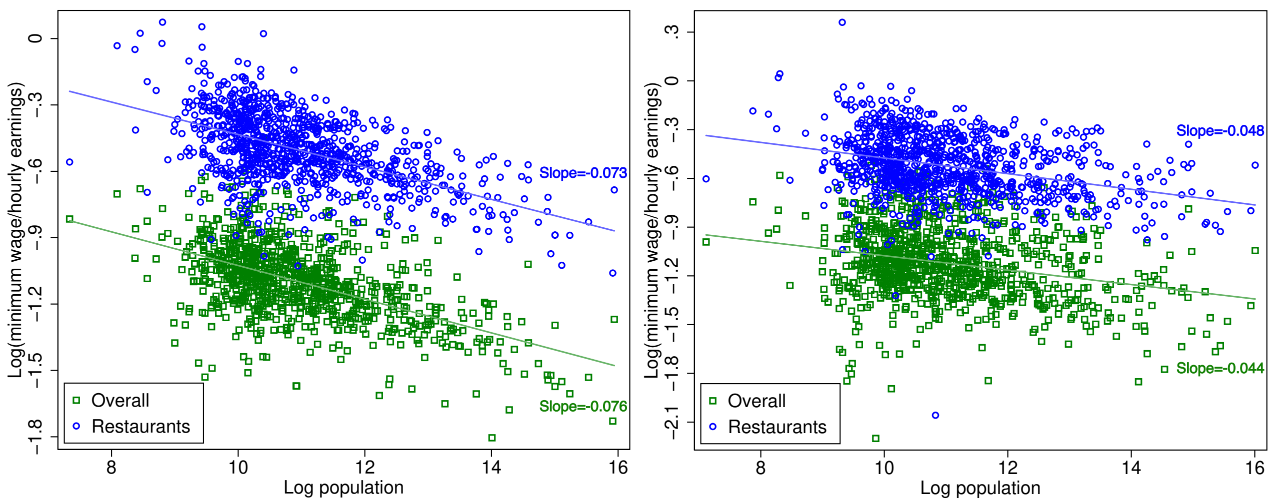
For each CBSA-by-state geography, Figure 1a plots log hourly earnings (for both restaurants and the overall sector) and log minimum wages against log population, using data from the second quarters of 2001 and 2019 (the first and last years of our sample). In both years, there is a strong positive relationship 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

[14] 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

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 η_i and τ_t are entity and time fixed effects, respectively, and ν_{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 β 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 β and γ yield similar results whether specification (1) is estimated without weights or with population weights. The estimate for β is negative and significant (either −0.132 or −0.186), and the estimate for γ is positive and significant (either 0.031 or 0.065). Hence, as population size increases, the estimated minimum wage elasticity of employment moves closer to zero, confirming that minimum wages bind less in more populous areas.[15]

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.[16] In the restricted regressions ($\gamma = 0$) in columns (1)–(2), 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

[15]Appendix Figure A-2a uses the estimates for β and γ 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.

[16]We use log average monthly earnings in the overall private sector as a control ($\ln(\text{earnings})^{-}$ in Table 2), which is obtained directly from the QWI.

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.

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 β (either 0.211 or 0.228), and negative and significant estimates for γ (either -0.039 or -0.021).[17]

To aid comparability across minimum wage studies, Dube and Zipperer (2024) suggest reporting the ratio of the minimum wage elasticity of employment to the minimum wage elasticity of earnings, commonly labeled as the own-wage elasticity (OWE). From Table 2, the implied OWEs when

[17] Using the estimates from Panel B in columns (3) and (4), Appendix Figure A-2b 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.

ignoring the city-size interaction are -0.624 in column (1) and 0.131 in column (2). Once we account for the interaction, the OWEs for a geography with mean log population are -0.626 in column (3) and -0.816 in column (4). Using the coefficients in column (4) to calculate the predicted OWEs for the 991 CBSA-by-state geographies, the median OWE is -0.873 and the interquartile range is $(-0.986, -0.690)$, so for the median geography roughly 87 percent of the earnings gains from higher minimum wages are offset by employment losses.

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. Given that monopsony power measures also vary with city size, it is therefore critical to include city-size minimum wage interactions in models attempting to estimate the effects of monopsony-minimum wage interactions.

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 θ_i is the measure of monopsony power for CBSA-by-state i, and $\overline{\theta}$ is the average for that measure across geographies. For exposition purposes, the main text focuses on a selection of 9 of our monopsony power proxies: total J2J hiring rate (θ_1^J), total J2J separation rate (θ_2^J), QWI hiring rate (θ_1^Q), QWI separation rate (θ_2^Q), QWI replacement rate (θ_3^Q), and the 4 NETS concentration metrics (from θ_1^N to θ_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.

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 θ indicates (in principle) a more competitive labor market—either a more fluid or less concentrated one—we expect the estimate for δ 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.[18] 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.

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 β is biased toward zero when the size–minimum wage interaction is ignored, while the estimate for δ 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.[19]

Panel C reports results using the four concentration measures from NETS. Focusing on the specifications using establishments per worker or firms per worker, θ_1^N and θ_2^N, the estimates for δ 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 θ_1^N and θ_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.)[20]

Another notable result from these specifications is the substantial upward bias in $\hat{\beta}$ in the restricted model, even yielding positive and significant point estimates of 0.057 and 0.067 (these are

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

[19] Appendix Table A-2 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.

[20] There may be concerns that the NETS data, which are not administrative, are more prone to measurement error, which can generate attenuation bias. However, the NETS is intended to capture the universe of business establishments, and hence there should not be attenuation bias from sampling error. Prior work tells us more. First, as documented in Neumark, Zhang, and Wall (2007), the NETS measures employment stock quite well, while it is less accurate at capturing short-term flows (based on correlations with levels and changes in data from the QCEW). We use only information on the number of workers in each establishment in the initial year to construct our HHI; i.e., a measure based on stocks. Second, the NETS captures more establishments than other company/employer data sets, but this is largely attributable to the fact that it captures non-employer businesses (Barnatchez, Crane, and Decker, 2017); this should not be an issue in the restaurant sector. Third, Neumark, Zhang, and Wall (2007) report some evidence of rounding of employment in the NETS, and imputation of employment is more common in the early years of an establishment's appearance. These could create some measurement error in the concentration measure, but the attenuation bias should be just that, bias towards zero, rather than bias that causes signs to flip.

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 (θ_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 (θ_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 (θ_1^Q)	-0.060*	-3.154***	-0.174***	0.048***	-1.661***
	(0.033)	(0.778)	(0.045)	(0.014)	(0.425)
Separation rate (θ_2^Q)	-0.064*	-3.135***	-0.180***	0.051***	-1.499***
	(0.034)	(0.794)	(0.047)	(0.015)	(0.455)
Replacement rate (θ_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 (θ_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 (θ_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.)* (θ_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)* (θ_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.

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, θ_3^N and θ_4^N, we find positive and significant estimates for δ 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 θ_3^N and θ_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 β and γ in the unrestricted specifications.

Thus, 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. One interpretation is that monopsony power has the predicted effects, but these concentration measures are relatively uninformative or flawed. A second interpretation is that monopsony power does not, in practice, have the predicted moderating effect on minimum wage employment responses. The latter seems unlikely, given that the fluidity proxies do deliver the expected pattern. Regarding the former interpretation, we cannot rule out the possibility that concentration measures are uninformative for the restaurant industry, where more than 99 percent of our CBSA-by-state geographies would be classified as competitive labor markets under the DOJ-FTC's product-market HHI thresholds (i.e., levels below 0.15).

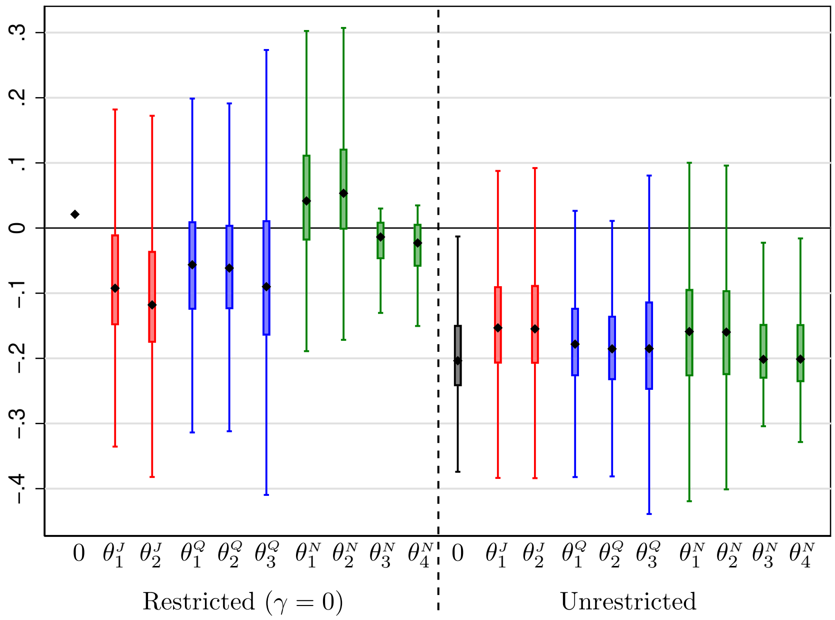
Using boxplots, Figure 2 illustrates how each of our 9 selected monopsony power proxies alters the distribution of predicted minimum wage elasticities of employment and OWEs across CBSA-by-state entities. For our CBSA-by-state geographies (436 when using J2J, 990 when using QWI, and 991 when using NETS),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

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

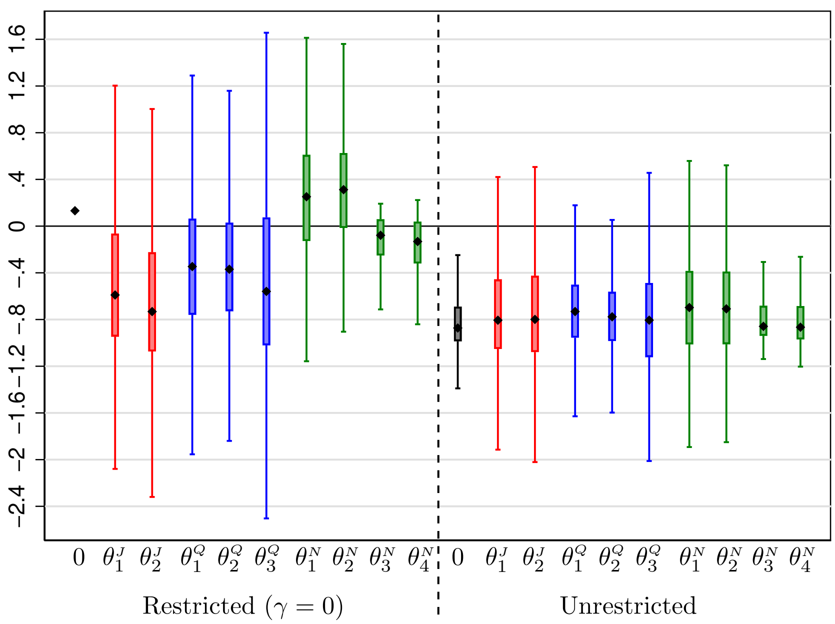
for the unrestricted specifications. The boxplots labeled "0" show the baseline distribution of elasticities without monopsony power interactions (i.e., from Table 2, we use column (2) for the restricted case and column (4) for the unrestricted case). Each of the other boxplots reflects the distribution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

The first takeaway from Figure 2a 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, θ_3^N and θ_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 θ_1^N and θ_2^N boxplots, and to about -0.183 in the QWI boxplots. The larger median shift in the J2J boxplots is a consequence of sample selection, as these regressions



(a) Predicted minimum wage elasticities of employment



(b) Predicted own-wage elasticities (OWEs)

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

Notes: Each boxplot summarizes the distribution of estimates across CBSA-by-state geographies by showing the median (diamond marker inside the box), the interquartile range from the 25th to the 75th percentile (height of the box), and whiskers that extend to the most extreme observations lying within 1.5 times the interquartile range from the box; we omit outliers.

include primarily large CBSAs, which exhibit smaller employment responses to minimum wages.

However, the key result is the difference between the boxplots 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 even positive in many places. In contrast, the boxplots that account for city size ("unrestricted") are shifted downward, indicating that even after accounting for monopsony power, minimum wage effects are negative in most places. The same story holds when we look at OWEs in Figure 2b: ignoring the city-size interaction ($\gamma = 0$) produces substantial upward bias, whereas under the unrestricted model, OWEs are negative in most places across all monopsony power proxies. Moreover, the OWEs are generally quite large; the median OWEs across the nine proxies average -0.783 and range between -0.866 and -0.697 (when ignoring monopsony power, boxplot "0", the median OWE is -0.873).[21]

As robustness checks, Appendix Section B presents two complementary state-border discontinuity designs. First, following Dube, Lester, and Reich (2010), we implement a state-border pair identification strategy that addresses potential bias in conventional TWFE models from 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 specifications with pair–period fixed effects. Second, following Dube, Reich, Bhatt, and Sosinskiy (2025b), we develop a border-approach implementation of the local-projections DiD event-study design of Dube, Girardi, Jordà, and Taylor (2025a). Our event study defines events as sustained changes in cross-border minimum-wage differentials and traces out the dynamic responses of employment and their interactions with city size and monopsony proxies. The results reinforce our main findings: ignoring city size leads to understating the adverse employment effects of minimum wages; and fluidity-based proxies consistently indicate more negative minimum wage employment effects in more competitive (i.e., more fluid) labor markets, while estimates for δ based on concentration proxies remain mixed—positive for the employers-per-worker measures and negative but imprecise for the HHI-based measures.

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

[21] To obtain the predicted OWEs across CBSA-by-state geographies, we first computed the predicted minimum wage elasticities of earnings. Appendix Tables A-3 and A-4 report the TWFE estimates for earnings for the different monopsony proxies, and Figure A-4 shows the corresponding boxplots. Appendix Figure A-3 presents boxplots for the remaining J2J and QWI fluidity measures, which display patterns similar to those in Figure 2.

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 markets, the relationship between minimum wages and job losses is weaker, although the effect of minimum wages generally remains negative in all but the largest cities.

At the same time, standard concentration measures do not appear to capture the same underlying frictions as fluidity-based proxies in the restaurant industry. We find that 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, rather than 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, as we have shown, when we account for city size we find that search-theoretic predictions of how minimum wages and labor market power interact are confirmed, but minimum wages nevertheless have adverse employment effects in most markets.

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