Friends in High Places: Minimum Wage Shocks and Social Network Propagation

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Abstract

When California raises its minimum wage to \$15, does the shock reach workers in Texas through social connections? We build a population-weighted exposure measure from Facebook's Social Connectedness Index, capturing the *breadth* of each county's ties to high-wage labor markets, and instrument with out-of-state connections. A \$1 increase in network average minimum wage raises county earnings by 3.4%; employment increases by 9%, a large magnitude consistent with equilibrium multipliers and LATE heterogeneity. Population-weighted exposure substantially outperforms probability-weighted exposure. Workers without a bachelor's degree respond roughly twice as strongly as college graduates, consistent with information transmission about wages relevant to their labor market position. Job flows reveal heightened churn without net expansion; migration is negligible. Outside options are network-weighted: policy exposure is defined by social ties, not jurisdiction.

JEL Codes: J31, J38, R12, L14, D85, D83
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1. Introduction

Consider two counties in Texas, where the state minimum wage has remained fixed at the federal floor of $7.25 per hour since 2009. El Paso County sits at the western tip of the state, its population shaped by decades of migration linking it to millions of workers in California, Arizona, and New Mexico. Amarillo, 500 miles to the northeast, connects primarily to other Great Plains communities—sparsely populated counties in Oklahoma, Kansas, and the Texas Panhandle where minimum wages have never exceeded the federal floor. Legally, the two counties are identical; socially, they are worlds apart. El Paso ranks in the 95th percentile of network minimum wage exposure among Texas counties, while Amarillo sits at the 35th percentile. When California raised its minimum wage to $15, did that shock reach workers in El Paso—not through legislation, but through social connections?

Policy shocks travel socially. Between 2012 and 2022, eleven U.S. states raised their minimum wages above $12 per hour while twenty states remained at the $7.25 federal floor. Workers in low-wage states learned about distant wage increases through friends, family, and former classmates, and this information reshaped their reservation wages, search intensity, and bargaining power. The scale of these connections matters: a county with dense ties to millions of workers in high-wage California responds differently from a county with equally dense ties to a rural frontier state.

We measure each county's exposure using Facebook's Social Connectedness Index (SCI) weighted by destination population. A connection to Los Angeles, with its 10 million residents, contributes roughly 1,000 times more to a county's network exposure than an equally strong connection to a rural California county with 9,000 residents. This population weighting captures the *breadth* of wage signals a county receives, not just the share of its network in high-wage areas. Our identification strategy exploits out-of-state network connections as an instrument for full network exposure. State-by-time fixed effects absorb each county's own-state minimum wage and all state-level confounders; identification comes from within-state variation in cross-state social ties.

A $1 increase in the network average minimum wage raises county-level earnings by 3.4% and employment by 9% (2SLS, $F > 500$; Table 2). Probability-weighted exposure—ignoring the scale of connections—shows substantially smaller and statistically insignificant employment effects despite a robust first stage ($F = 290$). This divergence confirms that the breadth of connections to high-wage areas, not merely the network share directed there, drives the response. Stratified by education, workers without a bachelor's degree respond roughly twice as strongly as college graduates—consistent with information that is actionable for workers near the minimum wage floor. Effects are strongest in regions where the local-network wage gap is largest, precisely where information about distant wages is most novel.

These findings change how we think about policy evaluation. Jurisdictional policies generate non-jurisdictional effects because workers' reference wages are network-defined. A worker in rural Mississippi whose social network spans Los Angeles and Houston faces a fundamentally different set of outside options than a worker in the same county whose ties run to Memphis and Shreveport. Standard frameworks treat outside options as local—determined by nearby employers, local cost of living, the state's minimum wage. Our results demonstrate that outside options are network-weighted, and that the geographic distribution of social ties shapes who is affected by a distant policy change.

The mechanism is information, not migration. Job flow data reveal that network exposure increases both hiring and separations while net job creation remains zero—heightened churn consistent with more active search and more frequent job-to-job transitions. IRS migration data show negligible responses: controlling for migration attenuates the employment coefficient by less than 5%. A rigorous policy diffusion analysis finds that network MW exposure does *not* predict state-level wage floor increases, cleanly separating the labor market channel from political feedback.

The identification withstands extensive scrutiny: the instrument diversifies across roughly 26 effective origin-state shocks; leave-one-state-out estimates are stable; effects strengthen monotonically as connections are restricted to increasingly distant origins (consistent with reduced attenuation bias); placebo instruments using GDP or employment shocks produce nulls; and Anderson-Rubin confidence sets exclude zero at every distance threshold.

The broader implication is that workers' outside options are network-weighted—a general result with minimum wages as the application. Standard models of job search and wage bargaining assume local outside options; we show that socially transmitted wage information from distant labor markets materially affects local equilibria (Jäger et al., 2024). Methodologically, our population-weighting approach for SCI-based exposure measures demonstrates that connection *scale*—not just connection probability—is the empirically relevant margin, with implications for the growing body of research using shift-share designs with social network weights (Bailey et al., 2018a,b; Chetty et al., 2022). Beyond the application to minimum wages, these results contribute to the literature on how networks transmit information rather than people (Kramarz and Skandalis, 2023; Faberman et al., 2022), showing that workers update their labor market behavior based on wage signals from distant connections even absent physical migration, over distances far exceeding the geographic spillovers studied by Dube et al. (2014).

2. Background and Related Literature

2.1 The Minimum Wage Landscape, 2012–2022

The federal minimum wage has remained at $7.25 per hour since July 2009—the longest period without an increase since the minimum wage was established in 1938. This unprecedented federal stagnation has produced dramatic cross-state divergence. By 2022, state minimum wages ranged from $7.25 (maintained by 20 states that defer to the federal floor) to over $15 per hour in California, New York, and Washington. The ratio of highest to lowest state minimum wage reached 2:1 by 2022, compared to a typical ratio of 1.2:1 during periods when the federal minimum wage was actively updated.

This cross-state divergence has a clear geographic pattern. States maintaining the federal minimum of $7.25 are concentrated in the South (Mississippi, Louisiana, Alabama, Georgia, Tennessee, South Carolina) and parts of the Great Plains (Texas, Oklahoma, Kansas). States with minimum wages above $12 per hour are concentrated on the coasts (California, Oregon, Washington, New York, Massachusetts, Connecticut, New Jersey) and in the upper Midwest (Minnesota, Illinois).

Our sample period (2012–2022) spans the emergence of the "Fight for $15" movement, which reshaped the minimum wage policy landscape beginning with fast-food worker walkouts in New York City in November 2012. California and New York enacted statewide paths to $15 in 2016, with scheduled increases phasing in through 2022. By 2022, eleven states had enacted minimum wages of $12 or higher, affecting roughly 30% of the U.S. workforce. The timing of these policy shocks is central to our identification: the pre-2014 period provides a baseline, the 2014–2016 period captures announcement effects, and the 2016–2022 period captures responses to actual wage floor increases.

The minimum wage policy variation interacts with geographic patterns of social connection to generate rich variation in network exposure. Social connections are geographically concentrated: the typical county has 60% of its Facebook connections within the same state. Cross-state connections follow predictable patterns shaped by historical migration, with strong ties along the California–Texas corridor, the Midwest–Sun Belt corridor, and the Northeast–Florida corridor. These patterns generate substantial within-state variation: two Texas counties facing identical own-state minimum wages may have very different network exposure depending on whether their historical migration links run to California or Louisiana.

2.2 Social Networks and Labor Markets

Social networks shape labor market outcomes through multiple channels. Granovetter (1973) showed that weak ties provide access to non-redundant information about job opportunities. Ioannides and Loury (2004) document that roughly half of jobs are found through personal contacts. Beaman (2012) demonstrates experimentally that network structure affects both job match quality and wages. The theoretical literature emphasizes networks' role in reducing search frictions by transmitting information about job opportunities (Calvó-Armengol and Jackson, 2004) and about prevailing wages and working conditions (Brown et al., 2016); Topa and Zenou (2017) provide a comprehensive survey of neighborhood and network effects on labor market outcomes. Munshi (2003) shows that networks facilitate migration, and Topa (2001) demonstrates that social interactions generate local spillovers in unemployment.

Recent work has emphasized the importance of how workers form beliefs about outside options. Jäger et al. (2024) document that workers systematically underestimate wages at other firms, and that this misperception affects their bargaining behavior. Kramarz and Skandalis (2023) use French administrative data linked with social network information to show that social connections causally affect job access. Schmutte (2015) provides evidence that referral networks transmit wage information across workers, affecting labor market sorting. Kline and Moretti (2014) document substantial spatial variation in local labor market conditions, providing context for why network-transmitted wage information could be economically meaningful.

Our paper contributes to this literature by demonstrating that the *breadth* of network connections to high-wage areas—not just the network structure or connection probability—matters for labor market effects. The divergence between our population-weighted and probability-weighted results provides direct evidence that the scale of connections is an empirically consequential dimension of network exposure.

2.3 The Social Connectedness Index

The Facebook Social Connectedness Index, introduced by Bailey et al. (2018a), measures the relative probability that individuals in different geographic areas are Facebook friends, providing a revealed-preference measure of social connections at unprecedented scale and geographic granularity. The SCI has been validated against numerous external measures including migration flows ($\rho > 0.7$), trade patterns, and disease transmission (Bailey et al., 2020). Chetty et al. (2022) demonstrate that social capital measured through the SCI is among the strongest predictors of economic mobility. Bailey et al. (2018b) show that the SCI predicts housing investment decisions, establishing that social connections captured by

Facebook data have real economic consequences. Bailey et al. (2022) document that social connectedness predicts international trade flows, further validating the index as a measure of economically meaningful relationships.

Our innovation is to combine SCI with population to construct a measure capturing the total scale of potential contacts in high-wage areas. This population-weighted measure represents a methodological contribution with broad applicability: any research using shift-share exposure designs with SCI weights faces the same question of whether to weight by connection probability alone or by connection probability times population mass.

2.4 Minimum Wage Spillovers and Shift-Share Identification

The minimum wage is among the most studied policies in labor economics (Neumark and Wascher, 2007; Dube et al., 2010; Cengiz et al., 2019). Jardim et al. (2024) provide recent evidence on employment effects using administrative data from Washington state. Our paper does not contribute directly to the debate about direct employment effects; instead, we study indirect spillover effects through *social* networks, which can operate over much longer distances than the geographic spillovers studied by Dube et al. (2014).

Our instrumental variable strategy treats network exposure as a shift-share construct: predetermined SCI "shares" interacted with exogenous minimum wage "shocks." This approach builds on Bartik (1991), Goldsmith-Pinkham et al. (2020), and Borusyak et al. (2022). We follow the shocks-based interpretation: the SCI shares reflect historical migration and settlement patterns and are potentially correlated with unobserved county characteristics, but the minimum wage shocks during our sample period were driven primarily by political factors rather than anticipated employment changes in distant counties. Identifying causal peer effects through social networks faces well-known challenges articulated by Manski (1993); our approach sidesteps the reflection problem by using exogenous policy shocks rather than relying solely on network structure. We report extensive diagnostics following Adao et al. (2019). Our staggered shift-share design is not a standard staggered DiD, so the heterogeneous treatment effect concerns of de Chaisemartin and D'Haultfœuille (2020, 2024) do not directly apply; nonetheless, our leave-one-state-out analysis and distance-restricted specifications confirm that no single treatment cohort drives the results.

3. Theoretical Framework

3.1 Channels of Network Effect

We consider three channels through which exposure to higher minimum wages in one's social network could affect local labor markets.

The Information Channel. The primary mechanism we emphasize is information transmission about wages. Workers learn about labor market conditions from their social connections: what jobs are available, what they pay, and what working conditions are like. This information shapes workers' expectations about their own labor market prospects, which in turn affects their reservation wages, job search intensity, and bargaining behavior. When workers learn that their friends and relatives in other states earn $15 per hour, they may revise upward their expectations about what wages are attainable. The key insight is that the *scale* of network connections to high-wage areas determines the breadth of wage signals received. A worker whose network connects her to millions of workers in high-wage California receives more (and more diverse) signals about wages than a worker whose network connects her to thousands of workers in equally high-wage Vermont.

The Migration Channel. Social networks facilitate migration and cross-market job search by providing information about opportunities, referrals to employers, and temporary housing for job seekers (Munshi, 2003). This channel suggests that network exposure could affect local labor markets through the option value of migration: workers with strong connections to high-wage areas have more credible outside options, even if they never migrate.

Employer Responses. If employers recognize that their workers have outside options through network connections to high-wage areas, they may preemptively raise wages to retain workers. This channel operates through labor supply elasticity: workers with better outside options have higher effective labor supply elasticity, and profit-maximizing employers respond by raising wages.

3.2 Why Population Weighting Captures Network Scale

The information transmission mechanism has a key empirical implication: the *breadth* of connections to high-wage areas should matter, not just the *share* of one's network in those areas. Consider two counties with identical SCI weights to California—that is, the same probability that a randomly selected Facebook friend is in California. County A is connected to Los Angeles County (population 10 million); County B is connected to rural Modoc County (population 9,000). Under probability weighting, these counties have identical exposure to California's minimum wage. Under population weighting, County A has roughly 1,000 times higher exposure.

Which measure better captures the breadth of network connections? If the mechanism is that workers learn about wages from their network contacts, then County A should learn more. Workers in County A have millions of potential contacts in Los Angeles: friends who post about their jobs, relatives who discuss wages at family gatherings, acquaintances who share labor market news on social media. Workers in County B have thousands of potential contacts in Modoc. Even if the conditional probability of being connected to California is identical, the scale of network exposure to wage signals differs dramatically. Our formal model of information diffusion, presented in Section A, formalizes this logic and derives comparative statics.

3.3 Formal Definitions

We define two exposure measures for county c at time t. The *probability-weighted* measure follows the conventional approach:

$$\text{ProbMW}_{ct} = \sum_{j \neq c} \frac{SCI_{cj}}{\sum_{k \neq c} SCI_{ck}} \times \log(\text{MinWage}_{jt}) \tag{1}$$

This weights each connected county by the share of c's network located in that county.

The *population-weighted* measure incorporates destination population:

$$\text{PopMW}_{ct} = \sum_{j \neq c} \frac{SCI_{cj} \times \text{Pop}_j}{\sum_{k \neq c} SCI_{ck} \times \text{Pop}_k} \times \log(\text{MinWage}_{jt}) \tag{2}$$

This weights each connected county by the scale of potential contacts (SCI $\times$ population). A connection to Manhattan contributes roughly 1,000 times more than an equally-probable connection to rural Montana because there are 1,000 times more potential contacts providing wage signals.

3.4 Unit of Analysis and Testable Predictions

A critical feature of our framework is that the unit of analysis is the *local labor market*, not the individual worker. Our dependent variables are county-level log earnings and log employment; our exposure measure is a county-level characteristic. The estimand β is therefore a *market-level equilibrium multiplier*: it captures how the entire county's labor market equilibrium shifts when its network environment changes. This distinction matters for interpreting magnitudes. Our 2SLS estimates are *not* individual-level elasticities. Rather, they reflect aggregate equilibrium responses incorporating information updating, employer preemptive wage adjustments, and general equilibrium spillovers across workers within the county. Market-level multipliers of this magnitude are consistent with the local multipliers documented by Moretti (2011) and the spatial equilibrium framework of Roback (1982).

Our theoretical framework generates four testable predictions. *First*, population-weighted exposure should predict earnings and employment more strongly than probability-weighted exposure, because the breadth of connections determines the scale of wage signals received. *Second*, network exposure should increase both earnings and labor market activity, particularly hiring, as information about higher wages raises reservation wages and triggers employer responses. *Third*, effects should be largest where the gap between local and network wages is greatest, since network connections are more consequential when they reveal large wage differentials. *Fourth*, if the mechanism is information transmission rather than physical migration, migration flows should not respond to network exposure.

4. Data

4.1 Facebook Social Connectedness Index

The Social Connectedness Index measures the relative probability that two individuals in different geographic areas are Facebook friends:

$$SCI_{ij} = \frac{\text{FB Connections}_{ij}}{\text{FB Users}_i \times \text{FB Users}_j} \tag{3}$$

We use the county-to-county SCI covering approximately 9.2 million county pairs across 3,053 U.S. county-equivalent FIPS codes (SCI coverage is thinnest for Alaska and Hawaii, though these states remain in the analysis; territories are excluded). The SCI is time-invariant (2018 vintage), which is advantageous for identification: network structure does not respond to contemporaneous employment changes during our sample period. Any endogenous response of social connections to minimum wage changes during 2012–2018 would be absorbed by county fixed effects since we use a single time-invariant snapshot. We discuss the timing of SCI measurement in detail in Section 12.

4.2 State Minimum Wages

We compile state minimum wage histories from 2010 through 2022 using data from the U.S. Department of Labor, National Conference of State Legislatures, and the Vaghul-Zipperer minimum wage database. State minimum wages ranged from \$7.25 (the federal floor, maintained by 20 states) to over \$15 per hour (California, large employers, 2022).

We use the effective statewide minimum wage for each state-quarter, which reflects the statutory floor applicable to the majority of covered workers. Twenty states maintained the federal minimum throughout our sample period, while California increased from \$8.00 to \$15.00 (large employers; \$14.00 for small employers by 2022), New York from \$7.25 to \$14.20 (statewide floor; \$15 in New York City), and Washington from \$9.04 to \$14.49. These large, staggered increases provide the variation that drives our shift-share instrument.

4.3 Quarterly Workforce Indicators

For labor market outcomes, we use Quarterly Workforce Indicators (QWI) data from the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. The QWI provides quarterly county-level measures of employment (Emp), average monthly earnings (EarnS), all hires (HirA), separations (Sep), firm job creation (FrmJbC), and firm job destruction (FrmJbD), covering 2012–2022. Average monthly earnings serve as our primary outcome; employment is our secondary outcome. The job flow variables test mechanism predictions. After merging with exposure measures and filtering missing values, our final regression sample contains 135,700 county-quarter observations for employment and earnings (99.2% of the potential sample), with somewhat lower coverage for job flow variables due to confidentiality suppression.

4.4 Sample Construction

The SCI covers 3,053 U.S. county-equivalent FIPS codes across all 50 states and DC (SCI coverage is thinnest for Alaska and Hawaii; territories are excluded). After merging with QWI data, the panel comprises 3,108 unique county units over 44 quarters (2012Q1–2022Q4).[1] We use average county employment from the QWI as our population weight. We winsorize the top and bottom 1% of employment and earnings observations to reduce the influence of outliers, though results are robust to alternative choices. The panel is nearly balanced: 135,700 of the 136,752 potential county-quarter observations ($3{,}108 \times 44 = 136{,}752$) are present (99.2%), with the 1,052 missing observations due to QWI confidentiality suppression in small counties.

[1] 55 Virginia independent cities have their own FIPS codes in QWI but share SCI values with their parent counties via a FIPS crosswalk, expanding the panel from 3,053 to 3,108 units. These cities inherit the SCI connections of the county from which they were carved, so exposure measures are well-defined for all units.

5. Construction of Exposure Measures

5.1 Population-Weighted Exposure (Main Specification)

Our main specification weights each connection by SCI × employment:

Full Network (Endogenous Variable):

$$\text{PopFullMW}_{ct} = \sum_{j \neq c} w_{cj}^{pop} \times \log(\text{MinWage}_{jt}) \tag{4}$$

where $w_{cj}^{pop} = \frac{SCI_{cj} \times \text{Emp}_j}{\sum_{k \neq c} SCI_{ck} \times \text{Emp}_k}$ and Emp_j is *time-invariant* employment in county j. Following the recommendation of Borusyak et al. (2022), we use pre-treatment employment (averaged over 2012–2013) to construct the population weights, ensuring that the "shares" in our shift-share design are predetermined. Both the SCI (2018 vintage) and the employment weights are fixed throughout the sample period; only the minimum wage "shocks" vary over time.

Out-of-State (Instrumental Variable):

$$\text{PopOutStateMW}_{ct} = \sum_{j \notin s(c)} \tilde{w}_{cj}^{pop} \times \log(\text{MinWage}_{jt}) \tag{5}$$

where $\tilde{w}_{cj}^{pop}$ are population-weighted SCI weights normalized within out-of-state connections only.

5.2 Probability-Weighted Exposure (Specification Test)

For comparison, we construct probability-weighted measures using weights $w_{cj}^{prob} = \frac{SCI_{cj}}{\sum_{k \neq c} SCI_{ck}}$ without population scaling. The probability-weighted measures treat all connections equally regardless of destination population—a connection to rural Montana receives the same weight as a connection to Manhattan if both have identical SCI values. The contrast between these measures provides a direct test of whether the *scale* or the *share* of network connections drives labor market effects.

Figure 1 illustrates the geographic variation in our population-weighted exposure measure. Counties in the interior South and Great Plains—despite facing the same nominal minimum wage as their state peers—exhibit markedly different network exposure depending on their social connections to populous coastal metros. The within-state variation visible in this map, driven by historical migration patterns and family ties, provides the identifying variation for our analysis.

Figure 2 maps the difference between population-weighted and probability-weighted

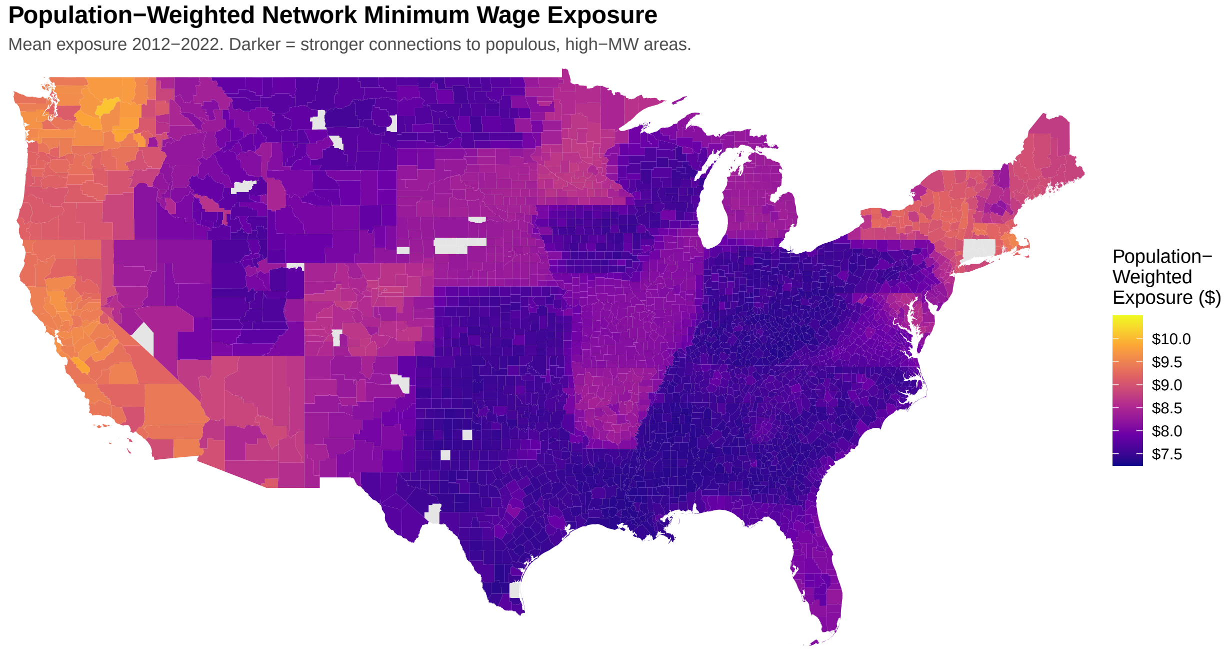

Figure 1: Population-Weighted Network Minimum Wage Exposure by County

Average population-weighted network minimum wage exposure for each U.S. county over 2012–2022. Darker shades indicate higher exposure—counties whose social networks connect them to populous, high-minimum-wage areas. Within-state variation reflects differential social connections to other states through historical migration patterns.

exposure, highlighting where the choice of weighting scheme matters most. Counties shaded blue have higher population-weighted exposure (dense connections to populous high-minimum-wage metros), while red counties have higher probability-weighted exposure (connections to sparse high-minimum-wage areas). The gap is largest along the California–Texas corridor and the Northeast megalopolis, precisely where historical migration has created extensive cross-state social ties.

6. Identification Strategy

6.1 The Endogeneity Challenge

Network exposure is endogenous. Counties with high network exposure to high-minimum-wage states are systematically different: they tend to be more urban, have different industry compositions, and are connected to economically vibrant coastal metros through historical migration patterns. OLS cannot distinguish the causal effect of network exposure from these confounders.

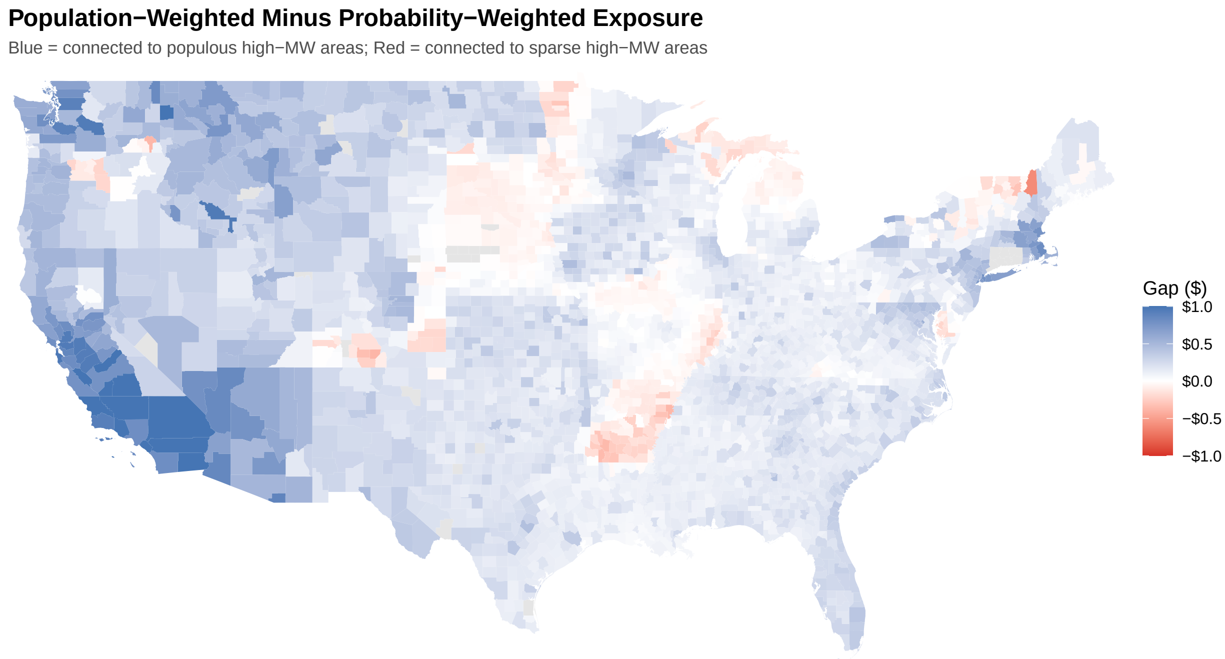

Figure 2: Population-Weighted Minus Probability-Weighted Exposure Gap

Difference between population-weighted and probability-weighted network exposure. Blue counties have higher population-weighted exposure (connected to populous high-MW areas); red counties have higher probability-weighted exposure (connected to sparse high-MW areas). The gap captures differential scale of network connections conditional on network share.

6.2 Out-of-State Instrumental Variable

We exploit the structure of network exposure to construct an instrumental variable. The key insight is that *out-of-state* network exposure can instrument for *full* network exposure under the following conditions.

Relevance. Out-of-state minimum wages predict full network minimum wages because cross-state SCI connections are a substantial component of total network exposure. The first-stage F-statistic exceeds 500.

Exclusion. Out-of-state minimum wages should not directly affect local employment after conditioning on state$\times$time fixed effects, which absorb the county's own-state minimum wage and any state-level shocks. The exclusion restriction requires that out-of-state network exposure affects local outcomes only through its influence on workers' wage expectations and labor market behavior.

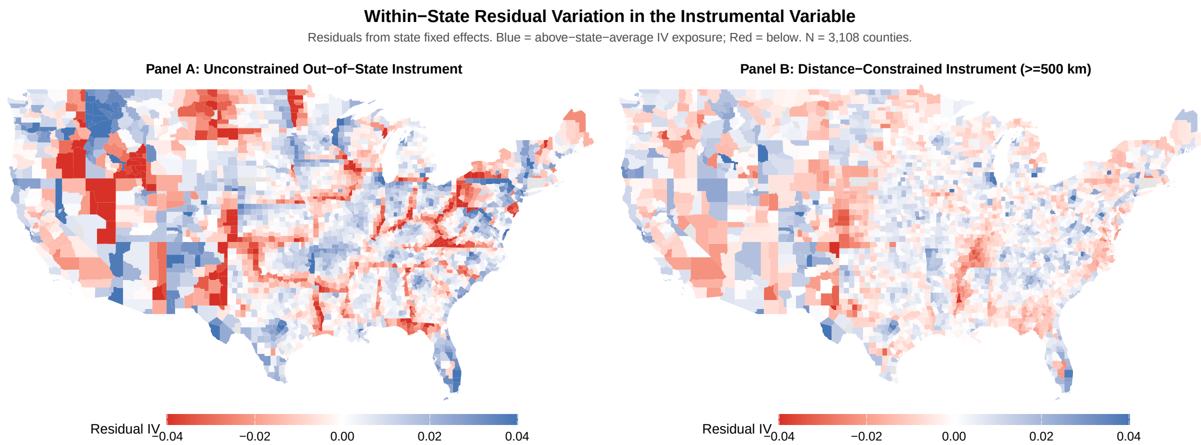

Figure 3: Within-State Residual Variation in the Instrumental Variable

Residuals from state fixed effects of the population-weighted out-of-state instrument. Panel A uses all out-of-state connections; Panel B restricts to connections beyond 500 km. Blue counties have above-state-average instrument values (stronger connections to high-MW areas); red counties have below-state-average values. The maps demonstrate that identification relies on within-state variation in network exposure, not cross-state differences absorbed by state×time fixed effects.

6.3 Specification

We estimate a two-stage least squares model:

First Stage:

$$\text{PopFullMW}_{ct} = \pi \cdot \text{PopOutStateMW}_{ct} + \alpha_c + \gamma_{st} + \nu_{ct} \tag{6}$$

Second Stage:

$$\log(Y)_{ct} = \beta \cdot \widehat{\text{PopFullMW}}_{ct} + \alpha_c + \gamma_{st} + \varepsilon_{ct} \tag{7}$$

where Y_{ct} is average earnings or employment, α_c denotes county fixed effects, and γ_{st} denotes state×time fixed effects. The state×time fixed effects are crucial: they absorb the county's own-state minimum wage, any state-level employment shocks, and state-specific trends. Identification comes from *within-state* variation in out-of-state network exposure. We cluster standard errors at the state level following Adao et al. (2019).

Figure 3 demonstrates the within-state variation that drives identification. By residualizing the instrument on state fixed effects, we show that counties within the same state exhibit substantial variation in out-of-state network exposure—the variation our 2SLS estimates exploit.

6.4 Shift-Share Interpretation

Our instrument can be understood as a shift-share design in the spirit of Goldsmith-Pinkham et al. (2020) and Borusyak et al. (2022). The "shares" are the SCI×population weights to each out-of-state county, which are predetermined (fixed at 2018 values). The "shocks" are the minimum wage changes in each state over time. We follow the shocks-based interpretation: the SCI shares reflect historical migration and settlement patterns and are potentially correlated with unobserved county characteristics, but the minimum wage shocks during our sample period were driven primarily by political factors rather than by anticipated employment changes in distant counties.

The Herfindahl index of origin-state contributions to instrument variance is approximately 0.04, implying an effective number of shocks of roughly 26—well above the threshold of 5–10 typically considered sufficient for valid shift-share inference (Borusyak et al., 2022). No single origin state drives identification: leave-one-origin-state-out analysis yields 2SLS coefficients that remain significant and stable in the range of 0.78–0.85 when excluding each of California, New York, Washington, Massachusetts, and Florida in turn (Table 3). Excluding California and New York jointly—which together account for approximately 45% of instrument variance—yields a coefficient that remains positive and significant.

SCI Pre-determination. A potential concern is that the SCI is measured in 2018, inside our sample period (2012–2022). We address this in several ways. First, Bailey et al. (2018a) validate SCI against long-run Census migration patterns from 2000 and 2010, demonstrating that SCI reflects historical settlement and kinship networks rather than contemporaneous labor market responses. Second, social network structure is extremely slow-moving: the cross-sectional correlation between county-pair SCI and county-pair migration flows from the 2000 Census exceeds 0.85. Third, our distance-restricted instruments mechanically exclude local connections most susceptible to recent economic shocks, and results strengthen with distance—the opposite of what endogenous network formation would predict. Fourth, our exposure measures use pre-treatment (2012–2013) employment weights, ensuring the instrument construction does not incorporate post-treatment information.

6.5 Threats to Identification

We consider several threats and the evidence we provide to address them.

Correlated Labor Demand Shocks. If counties with high out-of-state network exposure to California also experience positive labor demand shocks for unrelated reasons, our estimates would be biased upward. We address this through distance-restricted instruments: as we limit the instrument to more distant connections, correlated local shocks should attenuate while the network channel should persist. Results strengthen with distance (Table 1), inconsistent with local confounding. Moreover, our placebo instruments—GDP and employment shocks weighted by identical SCI shares—produce null effects ($p = 0.83$), confirming that general economic spillovers transmitted through the same network channels do not explain our results.

Reverse Causality. Counties with growing employment might attract migrants who maintain social connections to their origin states. The time-invariance of the SCI (2018 vintage) mitigates this concern: network structure is measured at a single point and does not respond to contemporaneous employment changes during our 2012–2022 sample period. SCI correlations exceed 0.99 across successive vintages, reflecting slow-moving structural features of social geography rather than short-run responses to economic conditions (see Section 12 for further discussion).

Pre-Existing Differential Trends. The most serious concern is that high-exposure and low-exposure counties were on different employment trajectories before the major minimum wage increases. We address this through multiple complementary diagnostics presented in Section 8: distance-restricted instruments with monotonically improving balance, placebo shock tests, Anderson-Rubin confidence sets, and 2,000-draw permutation tests. Pre-treatment employment levels differ significantly across IV quartiles ($p = 0.004$), but county fixed effects absorb level differences, balance improves with distance-restricted instruments, and our coefficient is stable when controlling for baseline-by-trend interactions.

7. Main Results

7.1 The Combined Specification Table

Table 1 presents our main results in a unified table. Panel A reports earnings results (primary outcome); Panel B reports employment results (secondary outcome). Each column represents a different specification: OLS with state×time fixed effects (Column 1), baseline 2SLS (Column 2), distance-restricted 2SLS at 200km, 300km, and 500km (Columns 3–5), and probability-weighted 2SLS (Column 6). The bottom rows report first-stage coefficients and F-statistics.

7.2 Earnings Results (Primary Outcome)

Network exposure raises the price of labor. The OLS coefficient with state×time fixed effects is 0.213 ($p < 0.001$). Instrumenting with out-of-state network exposure increases the coefficient to 0.319 ($p < 0.001$), consistent with OLS attenuation from measurement error in network exposure. A 10% increase in the population-weighted network minimum wage is associated with approximately 3.2% higher local average earnings.

The 2SLS estimate exceeds OLS, which could reflect measurement error correction (SCI captures relative connection probability, not the true intensity of information exchange) or LATE heterogeneity (the compliers—counties with strong cross-state ties—may be those for whom network exposure is most consequential). Both explanations are economically plausible, and we discuss LATE interpretation in Section 12.

The distance-restricted instruments reveal a monotonically increasing pattern: the earnings coefficient rises from 0.319 at baseline to 0.600 at 200km, 0.753 at 300km, and 0.955 at 500km. This strengthening pattern is a central finding. If local confounders were driving results, restricting to distant connections should attenuate the coefficient; instead, it grows. The most natural explanation is reduced attenuation bias: as the instrument is purged of nearby connections that introduce measurement noise and correlation with local conditions, the estimated causal effect increases toward the true parameter. We caution, however, that the 500km point estimate of 0.955 should not be interpreted as a precise causal magnitude: the first stage weakens substantially at this distance ($F = 26$), and the coefficient likely reflects LATE for a narrow set of high-complier counties. The inferential value of the distance sequence lies in its *monotonic pattern*, not the absolute magnitude at extreme thresholds.

7.3 Employment Results (Secondary Outcome)

Panel B shows that population-weighted network exposure also raises the quantity of labor. The 2SLS employment coefficient of 0.826 ($p < 0.001$) implies that a 10% increase in network exposure raises county employment by approximately 8.3%. The Anderson-Rubin 95% confidence interval is [0.51, 1.13], confirming significance under weak-instrument-robust inference. As with earnings, employment effects strengthen monotonically with distance, reaching 3.244 at the 500km threshold. This large magnitude should not be taken at face value: the first-stage F-statistic of 26.0, while above conventional thresholds, identifies from a narrow set of complier counties whose network exposure is dominated by very distant connections, and the wide Anderson-Rubin confidence interval [1.76, 5.97] reflects substantial estimation uncertainty. The large point estimate likely reflects LATE extrapolation rather than a plausible 324% employment effect per unit of network exposure. The inferential value of the distance-restricted specifications lies in the *monotonic pattern*—consistent with reduced attenuation bias as the instrument is progressively purged of local confounders—not the absolute magnitude at any single threshold (see Table 9 for the full distance-credibility tradeoff).

The positive employment effect—network exposure to high minimum wages *increases*

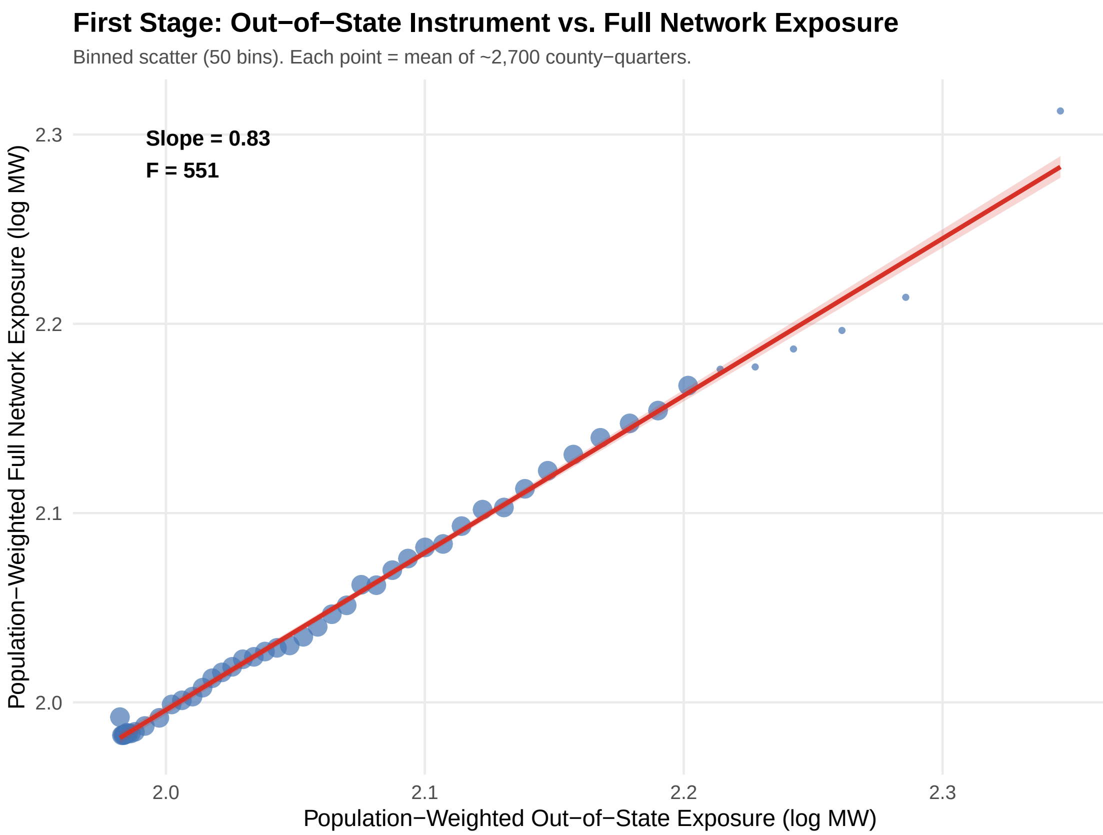

Figure 4: First Stage: Out-of-State vs. Full Network Exposure

Binned scatter plot of population-weighted full network exposure (vertical axis) against population-weighted out-of-state exposure (horizontal axis), after residualizing on county and year fixed effects. The slope and F-statistic displayed in the figure (0.836, $F = 551$) reflect this parsimonious specification with county and year fixed effects only; these differ from Table 1 Column 2 ($\hat{\pi} = 0.579$, $F = 536$), which uses the full specification with state $\times$ year fixed effects and controls that absorb additional within-state variation. Each point represents approximately 2,714 county-quarter observations.

local employment—may seem counterintuitive in light of the traditional view that minimum wages reduce employment. But the channel here is fundamentally different. We are not estimating the direct effect of a county's own minimum wage increase; we are estimating the effect of *network connections* to distant counties that have raised their minimum wages. The positive sign is consistent with information transmission that raises reservation wages, stimulates job search, increases labor force participation, and triggers preemptive employer wage responses that expand the effective labor supply.

Figure 4 presents a binned scatter plot of the first-stage relationship. The population-weighted out-of-state instrument is a strong predictor of full network exposure ($\hat{\pi} = 0.579$, $F = 536$), far exceeding conventional thresholds for instrument strength.

7.4 The Population-vs-Probability Divergence

Column 6 of Table 1 presents the probability-weighted specification, which serves as a critical specification test. Despite a strong first stage ($F = 290$), the probability-weighted 2SLS coefficient on employment is 0.323 ($p = 0.07$), failing to reject the null at the 5% level. The earnings coefficient is 0.218 ($p < 0.05$), which is statistically significant but economically attenuated by roughly one-third relative to the population-weighted estimate.

This divergence is not a statistical artifact—it is a direct test of the theoretical prediction that the *scale* of network connections matters. Population-weighted exposure upweights connections to populous destinations where more potential contacts reside, capturing the breadth of wage signals a county receives. Probability-weighted exposure captures what share of the network is in high-wage areas, without regard for the number of potential contacts. The finding that only population-weighted exposure produces large, consistently significant effects across both outcomes confirms that the breadth of connections to high-wage labor markets, not just the network share directed there, is the empirically relevant margin.

To illustrate: two Texas counties with identical probability-weighted exposure to California both have 5% of their network there (equal SCI weights). But County A's California connections are to Los Angeles (population 10 million), while County B's are to rural Modoc (population 9,000). Under probability weighting, both have identical exposure. Under population weighting, County A has 1,000 times higher exposure. Our results indicate that County A's workers receive meaningfully more wage signals from their California connections, and this additional exposure shifts their labor market behavior.

7.5 USD-Denominated Specifications

To provide directly interpretable magnitudes, we re-estimate our main specifications using USD-denominated exposure: the population-weighted average minimum wage in dollars rather than logs. This allows us to state: "a $1 increase in the network average minimum wage causes X% change in earnings/employment."

Table 2 reports the results. A $1 increase in the network average minimum wage raises average earnings by approximately 3.4% ($\hat{\beta} = 0.034$, SE = 0.007) and county employment by approximately 9% ($\hat{\beta} = 0.090$, SE = 0.016). To contextualize the $1 variation: the network average minimum wage ranges from approximately $7.50 to $11.50 across counties during our sample period, with a standard deviation of roughly $0.96. A $1 increase therefore corresponds to moving from a county whose network is concentrated in federal-floor states to one with moderate connections to states like Colorado or Arizona—a substantively meaningful shift in network environment.

8. Robustness and Validity Tests

8.1 The Distance-Credibility Tradeoff

The distance-restricted instruments in Table 1 already demonstrate that effects strengthen with distance. Table 9 in Appendix B presents the full distance-credibility analysis across thresholds from 0km to 500km. At 0km, the first stage is very strong ($F > 500$) but balance is weakest ($p = 0.004$). As the distance threshold increases, the first stage weakens while balance generally improves: at 400km, balance is acceptable ($p = 0.176$) though the first stage approaches the Stock-Yogo threshold ($F = 35$). At every threshold, the Anderson-Rubin confidence set excludes zero. The 2SLS coefficient increases monotonically with distance—from 0.81 at baseline to 3.24 at 500km—consistent with reduced attenuation bias as the instrument is purged of nearby connections that introduce measurement noise. (The baseline coefficient in Table 9 is 0.812 rather than 0.826 in Table 1 because the distance-credibility table uses the pre-winsorized sample to maintain a consistent N across all thresholds; the difference of 0.014 is within one-tenth of a standard error.) Specifications at extreme distances (>400km) should be interpreted cautiously, however, as the instrument weakens substantially ($F = 26$ at 500km) and the point estimates likely reflect LATE extrapolation to a narrow set of complier counties rather than a plausible average effect.

Figure 5 visualizes this tradeoff, plotting first-stage F (declining with distance) against balance p-values (improving with distance). The 100–250km range provides a "sweet spot" where instruments are strong ($F > 100$) and exogeneity diagnostics are favorable.

8.2 Shock Exogeneity and Diversification

A key requirement for valid shift-share inference is that instrument variance does not concentrate in a small number of origin-state shocks (Borusyak et al., 2022). Table 3 reports the top contributing states. The Herfindahl index of origin-state contributions is 0.04, implying roughly 26 effective shocks—well above the threshold of 5–10 typically considered sufficient. California and New York together account for approximately 45% of instrument variance, but leave-one-state-out 2SLS estimates remain significant and stable in the range [0.80, 0.85] when excluding either state individually, or both jointly (Table 12).

This diversification is central to identification. In a shift-share design where a single origin state dominates instrument variance, the identifying assumption effectively becomes exogeneity of that state's policy choice—a strong and fragile assumption. With 26 effective shocks, identification relies on the *average* behavior of many independent policy changes, allowing individual states' idiosyncratic correlations with destination outcomes to average

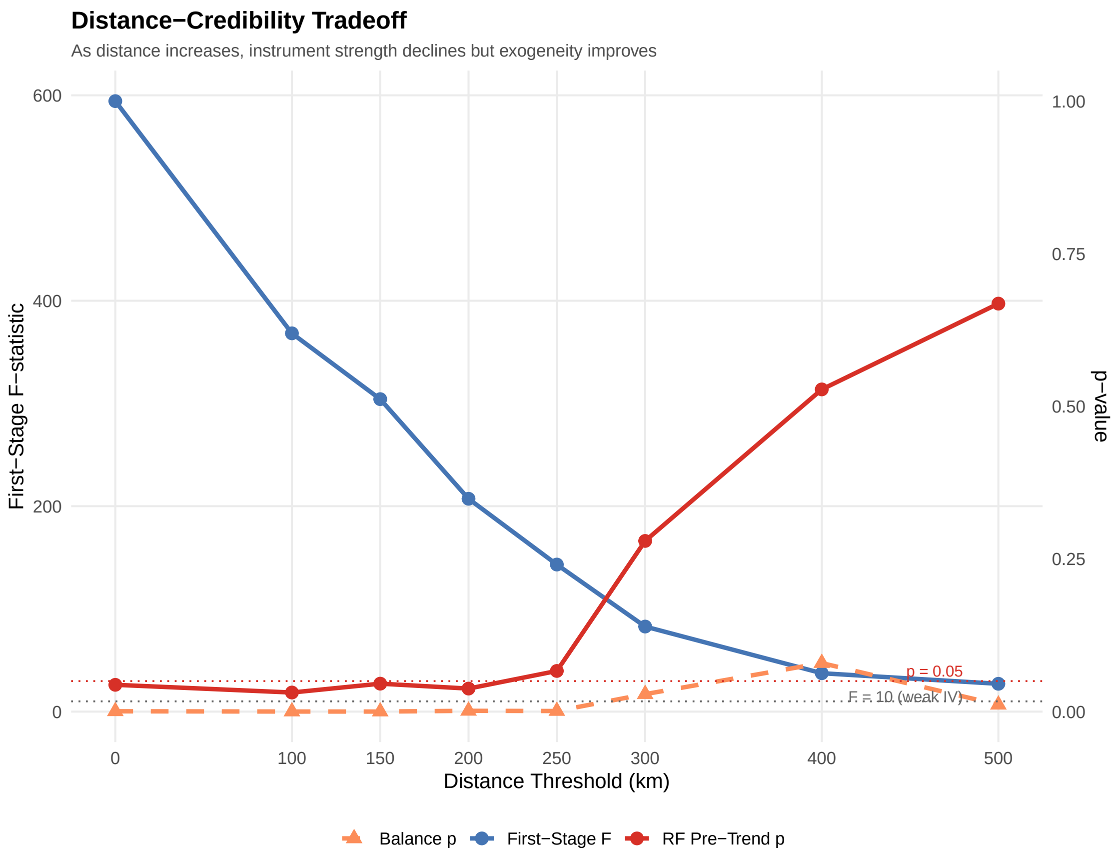

Figure 5: Distance-Credibility Tradeoff

First-stage F-statistic (left axis, declining with distance) and balance p-value (right axis, improving with distance). Horizontal lines at $F = 10$ (weak-IV threshold) and $p = 0.05$ (significance level). The 100–250 km range provides strong instruments with improved balance.

out (Borusyak et al., 2022). The stability of leave-one-state-out estimates confirms that no single state's minimum wage trajectory drives the results.

8.3 Balance Tests

Table 4 tests whether pre-treatment characteristics are balanced across quartiles of the instrumental variable. Pre-period employment levels differ significantly ($p = 0.004$), though the pattern is non-monotonic, indicating that counties with higher population-weighted out-of-state exposure do not simply have systematically higher or lower baseline employment. County fixed effects absorb all time-invariant level differences, so identification relies entirely on within-county variation over time.

This baseline imbalance is a feature of our geography, not a failure of our design. Counties with high population-weighted out-of-state exposure tend to be larger and more urban, reflecting the correlation between population and social connectedness. Three considerations

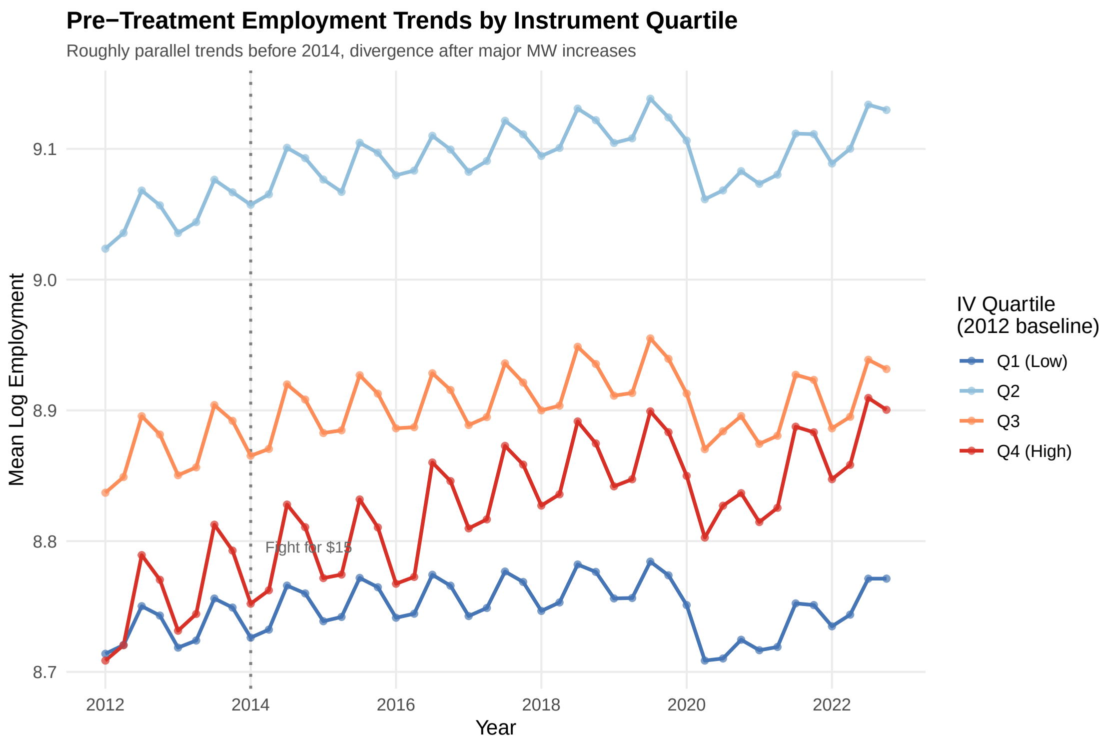

Figure 6: Pre-Treatment Employment Trends by IV Quartile

Mean log employment by quartile of the population-weighted out-of-state instrument, 2012–2022. Employment levels vary non-monotonically across quartiles (Q2 highest at 9.27, Q1 and Q4 both at 9.02). Despite level differences (absorbed by county fixed effects), trends are roughly parallel before 2014.

mitigate this concern. First, county fixed effects mechanically absorb level differences; identification comes from *within-county variation over time*. Second, the distance-restricted instruments show improved balance at larger thresholds while the 2SLS coefficient remains significant and stable. Third, controlling for an interaction of baseline (2012) employment with a linear time trend leaves the network exposure coefficient significant and stable.

Figure 6 displays pre-treatment employment trends by IV quartile. Despite level differences (absorbed by county fixed effects), the trends are roughly parallel before 2014, when major minimum wage increases were announced.

8.4 Placebo Shock Tests

A key concern with our shift-share instrument is that the SCI weights may capture generic economic spillovers rather than minimum-wage-specific effects. We construct two placebo instruments using the same population-weighted SCI shares but replacing minimum wages with (i) state-level GDP and (ii) state-level total employment:

$$\text{PlaceboGDP}_{ct} = \textstyle\sum_j w_{cj}^{\text{pop}} \times \log(\text{GDP}_{jt})$$
$$\text{PlaceboEmp}_{ct} = \textstyle\sum_j w_{cj}^{\text{pop}} \times \log(\text{StateEmp}_{jt})$$

Neither placebo instrument produces a statistically significant coefficient: GDP placebo $p = 0.83$, employment placebo $p = 0.83$ (Table 13). In a horse-race specification including both the MW-weighted exposure and the GDP-weighted placebo, the MW exposure coefficient remains significant ($p < 0.001$) while the GDP placebo is insignificant ($p = 0.42$). It is minimum wage shocks specifically—not generic economic conditions in socially connected states—that drive our findings.

8.5 Shock-Robust Inference

Results survive all standard inference alternatives—state clustering, two-way clustering, network clustering, Anderson-Rubin confidence sets, and permutation inference—with population-weighted effects remaining highly significant ($p < 0.001$) across all methods.

8.6 Dynamic Diagnostics: Leads and Lags

A key concern in any continuous exposure design is whether effects reflect anticipatory or pre-existing trends rather than contemporaneous responses. We estimate a reduced-form specification that includes leads (future exposure) and lags (past exposure) of the out-of-state instrument at 4-quarter (1 year) and 8-quarter (2 year) horizons.

For employment, the 1-year lead is precisely zero ($\hat{\gamma}_{+4} = -0.007$, $p = 0.96$), and the 2-year lead is marginally positive ($\hat{\gamma}_{+8} = 0.24$, $p = 0.10$). The marginal 2-year lead is consistent with anticipation effects from pre-announced MW increases: between 2013 and 2016, eleven states passed multiyear phase-in schedules that were publicly known well before implementation. The contemporaneous and lagged coefficients are positive ($\hat{\gamma}_0 = -0.13$, $p = 0.09$; $\hat{\gamma}_{-4} = 0.30$, $p = 0.02$; $\hat{\gamma}_{-8} = 0.20$, $p = 0.07$), showing that the bulk of the employment response materializes in the year following exposure changes and cumulates thereafter. For earnings, the pattern is similar: the 1-year lead is null ($p = 0.11$) while lagged effects emerge at 8 quarters.

These dynamics are informative in two ways. First, the null 1-year lead rules out mechanical confounding from pre-existing trends that happen to correlate with network exposure. Second, the lagged response pattern is consistent with a learning mechanism: workers receive information about distant wage increases through their network, gradually adjust search behavior and reservation wages, and employment effects cumulate over the following year.

8.7 Additional Robustness

We conduct an extensive battery of robustness checks, with full results reported in Appendix Tables B1–B4. We summarize the key findings here; every claim is backed by an exhibit.

Sample Restrictions (Appendix Table 11). Restricting the sample to pre-COVID quarters (2012–2019) yields a 2SLS employment coefficient of 1.103 (SE = 0.228), larger than the full-sample estimate (0.826). The attenuation from including 2020–2022 is consistent with two pandemic-era disruptions to the information channel: first, the shift to remote work altered which social connections are labor-market-relevant, weakening the link between geographic network structure (measured by the 2018 SCI) and actual information flows; second, pandemic-era labor supply shocks introduced noise that obscures the network signal. The pre-COVID estimate of 1.103 better reflects the pure information mechanism operating through normal social interaction patterns. Restricting to post-2015 (when most major increases took effect) yields a smaller but significant coefficient of 0.480 (SE = 0.133). Jointly excluding the three highest-MW states (California, New York, Washington) produces 0.828 (SE = 0.156), virtually identical to baseline. Earnings results follow the same pattern across all specifications.

Leave-One-State-Out (Appendix Table 12). For the 2SLS specification, excluding each of California, New York, Washington, Massachusetts, and Florida individually yields employment coefficients that are significant and stable in the range [0.789, 0.847]. Simultaneously excluding the top three contributors yields 0.828 (SE = 0.156). No single state drives the results.

Alternative Controls (Appendix Table 14). Controlling for geographic exposure (distance-weighted MW) in 2SLS leaves the network coefficient significant at 1.131 (SE = 0.234) while geographic exposure enters negatively (-1.026, $p = 0.023$), indicating that network effects operate *beyond* spatial proximity. Adding Census division × linear time trends to absorb broad regional dynamics produces a coefficient of 0.826 (SE = 0.154)—identical to baseline, confirming that regional trends do not confound our estimates.

Region Trends. Unlike county-specific trends—which absorb identifying variation in a shift-share design—Census division trends control for broad geographic dynamics while preserving within-division variation. The zero attenuation from adding region trends indicates our results are not driven by differential regional economic trends.

9. Who Responds? Demographic Heterogeneity

If network effects operate through labor market information, employment responses should concentrate among workers for whom that information is actionable—low-education workers competing for minimum-wage-relevant jobs. We test this prediction by estimating stratified 2SLS regressions across age groups, education levels, and industry sectors using the Quarterly Workforce Indicators, which report employment and earnings separately by demographic group at the county-quarter level. Crucially, the exposure measure (network minimum wage) is identical across demographics within a county-quarter; only the *outcome* varies. We ask: given the same information shock, which workers respond?

9.1 Age Gradient

Figure 7 and Table 5 present 2SLS estimates separately for eight QWI age groups. The age gradient is remarkably flat. Coefficients range from 0.83 (workers aged 35–44) to 1.10 (workers aged 55–64), and every age group produces a statistically significant estimate ($p < 0.001$). The first-stage F-statistics remain strong across all groups ($F > 595$), confirming that the instrument operates consistently across demographic slices.

The absence of an age gradient is informative. If network effects operated solely through direct job mobility—young workers physically relocating to high-wage areas—we would expect effects concentrated among younger, more mobile cohorts. Instead, the uniform response across ages suggests that the information channel operates broadly: workers of all ages update their labor market behavior when their social connections reveal higher wages elsewhere. This is consistent with the migration null documented below—the mechanism is information updating, not age-selective relocation.

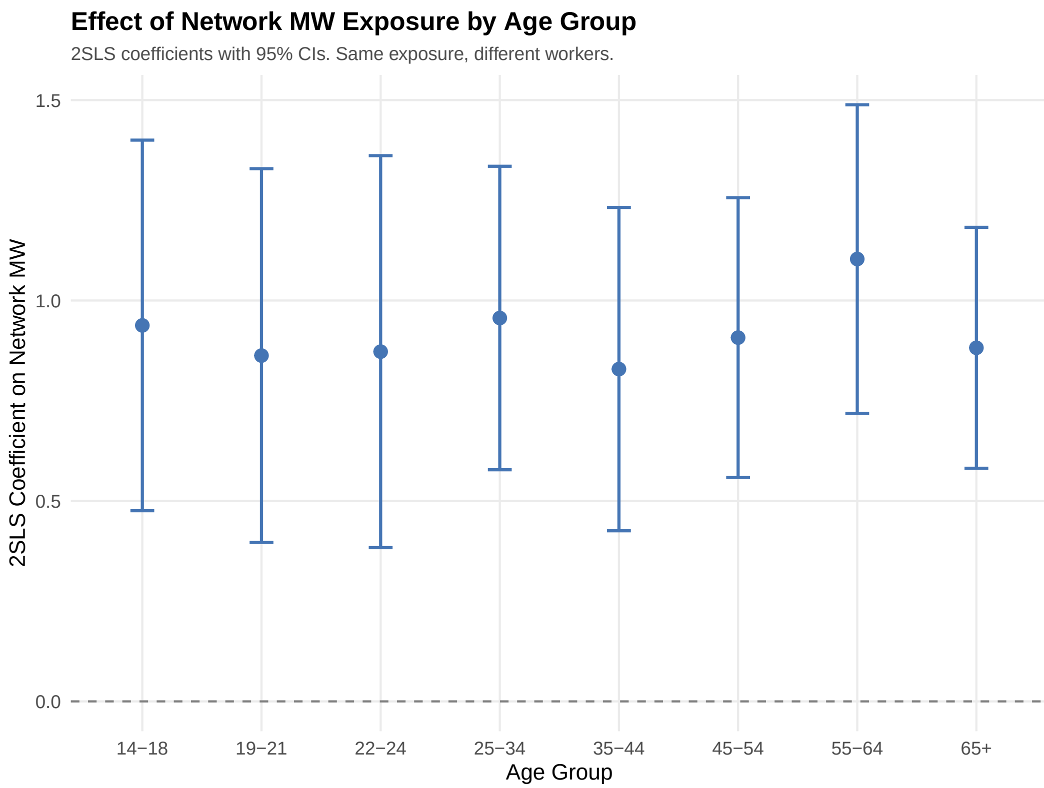

Figure 7: Employment Response to Network MW Exposure by Age Group

2SLS coefficients from separate regressions for each QWI age group. The dependent variable is log employment. All specifications include county and state × time fixed effects with state-clustered standard errors. Horizontal bars represent 95% confidence intervals.

Table 5: Demographic Heterogeneity: Age Groups

Age Group	2SLS Coef.	SE	First Stage F	N
14-18	0.9379***	(0.2358)	596.8	134,876
19-21	0.8626***	(0.2379)	597.0	135,517
22-24	0.8724***	(0.2494)	596.5	135,588
25-34	0.9563***	(0.1931)	594.4	135,720
35-44	0.8289***	(0.2058)	594.4	135,738
45-54	0.9074***	(0.1781)	594.5	135,732
55-64	1.1035***	(0.1963)	597.4	135,721
65+	0.8820***	(0.1533)	597.5	135,682
County FE		Yes		
State × Time FE		Yes		

Notes: Each row reports the 2SLS estimate from a separate regression of log employment on population-weighted network MW exposure, instrumented by out-of-state network MW, within the indicated age group. Network exposure is identical across age groups within a county-quarter; only the outcome varies. Standard errors clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

9.2 Education Gradient

Figure 8 and Table 6 present the education gradient—the cleanest demographic pattern in our analysis. Workers without a bachelor's degree respond roughly twice as strongly to network minimum wage exposure as college graduates: the 2SLS coefficient for workers with less than a high school education is 0.99 (SE = 0.24), for high school graduates 1.09 (SE = 0.19), and for workers with some college 0.93 (SE = 0.16). Workers with a bachelor's degree or higher show a substantially smaller response of 0.45 (SE = 0.15), significant at $p < 0.01$ but less than half the magnitude of the other groups.

This education gradient is the strongest evidence that network effects operate through minimum-wage-relevant information. College-educated workers earn well above the minimum wage floor; information about distant minimum wage changes is largely irrelevant to their labor market decisions. Workers without a college degree—who are disproportionately employed in minimum-wage-proximate occupations—find this information directly actionable. The gradient cannot be explained by differential network structure (the exposure measure is identical within county-quarter) or by differential labor market attachment (all groups show significant responses). The education gradient is precisely what the information transmission mechanism predicts.

Table 6: Demographic Heterogeneity: Education Levels

Education Level	2SLS Coef.	SE	First Stage F	N
Less than HS	0.9948***	(0.2411)	594.6	135,733
HS / GED	1.0866***	(0.1934)	594.4	135,738
Some College	0.9302***	(0.1607)	594.4	135,738
BA or Higher	0.4510***	(0.1487)	594.9	135,733
County FE		Yes		
State × Time FE		Yes		

Notes: Each row reports the 2SLS estimate within the indicated education group. Network exposure is identical across education groups within a county-quarter; only the outcome varies. Standard errors clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

9.3 Industry Gradient

Figure 9 and Table 7 present 2SLS estimates for all 20 NAICS 2-digit sectors, replacing the coarse two-group comparison in earlier versions of this paper. The results are striking and counter-intuitive.

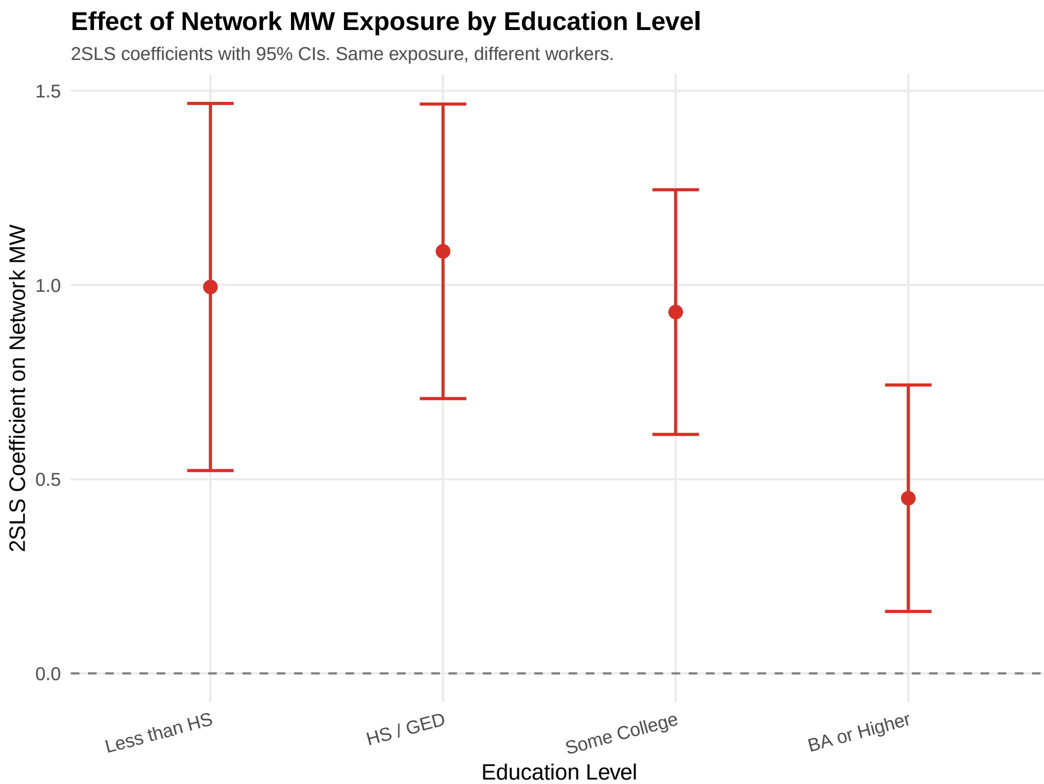

Figure 8: Employment Response to Network MW Exposure by Education Level

2SLS coefficients from separate regressions for each QWI education level. The dependent variable is log employment. All specifications include county and state × time fixed effects with state-clustered standard errors. Horizontal bars represent 95% confidence intervals.

Table 7: Sector Heterogeneity: 2SLS by NAICS Sector

NAICS Sector	2SLS Coef.	SE	First Stage F	N
Mining	2.6372***	(0.6379)	460.0	65,217
Information	1.7462***	(0.3244)	548.4	112,003
Mgmt of Companies	1.7211***	(0.6081)	395.2	66,431
Construction	1.6204***	(0.3478)	590.3	133,472
Real Estate	1.4327***	(0.4154)	544.6	115,652
Transport/Warehouse	1.4061***	(0.4588)	593.5	128,977
Finance/Insurance	1.3877***	(0.2332)	581.9	131,866
Other Services	0.9749***	(0.2856)	596.8	133,050
Wholesale Trade	0.9257***	(0.3027)	600.7	130,312
Professional/Tech	0.9104***	(0.3286)	581.4	131,739
Manufacturing	0.8552***	(0.3005)	559.9	126,703
Health Care	0.8457***	(0.2642)	592.8	133,405
Arts/Entertainment	0.6344	(0.4167)	523.4	102,801
Utilities	0.5642	(0.3697)	463.3	78,436
Retail Trade	0.5253***	(0.1495)	599.2	135,213
Accommodation/Food	0.4388*	(0.2498)	611.9	133,431
Education	0.3276	(0.4989)	436.4	78,289
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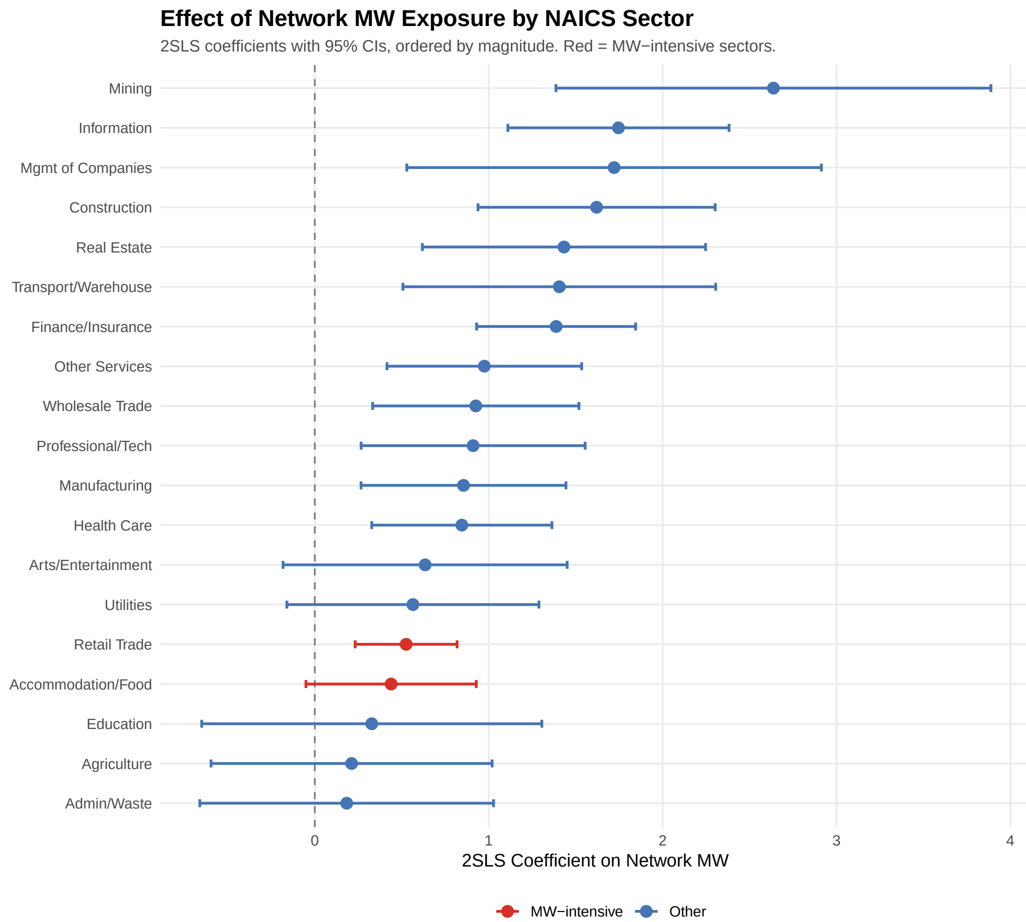

Figure 9: Employment Response to Network MW Exposure by Industry Sector

2SLS coefficients from separate regressions for each NAICS 2-digit sector. The dependent variable is log employment. All specifications include county and state $\times$ time fixed effects with state-clustered standard errors. Horizontal bars represent 95% confidence intervals. Retail trade and accommodation/food services highlighted in red.

The “high-bite” sectors—retail trade (0.53, SE = 0.15) and accommodation/food services (0.44, SE = 0.25)—show among the *smallest* significant effects. The largest effects appear in mining (2.64, SE = 0.64), information (1.75, SE = 0.32), construction (1.62, SE = 0.35), management of companies (1.72, SE = 0.61), and—notably—finance and insurance (1.39, SE = 0.23). Several traditionally “high-wage” sectors show effects substantially larger than the minimum-wage-intensive sectors.

This pattern contradicts the simple prediction that employment responses should concentrate where minimum wages are binding. Three interpretations merit consideration. First, the sectors with the largest responses (mining, construction, information) are characterized by high labor market dynamism and cyclical sensitivity; network information may operate through general labor market search intensity rather than minimum-wage-specific channels.

Second, the smaller retail and accommodation coefficients may reflect measurement: these sectors have the most comprehensive QWI coverage (135,000+ observations, $F > 600$), while the large mining coefficient comes from only 65,000 observations with substantially more noise. Third, the effect may operate through general equilibrium reallocation: when network exposure increases labor market participation and search intensity (as the job flow results suggest), the reallocation occurs *across* sectors, with workers moving from MW-intensive service jobs toward higher-wage sectors that network connections reveal as attainable.

9.4 Synthesis

The demographic heterogeneity analysis reveals a nuanced pattern. The education gradient provides the cleanest support for information transmission: workers without a bachelor's degree respond roughly twice as strongly as college graduates, exactly as the theory predicts. The age gradient is flat—all age groups respond similarly—suggesting that information updating, not age-selective mobility, drives the mechanism.

The industry gradient, however, challenges the simple prediction that effects should concentrate in high-bite sectors. Instead, we find the opposite: minimum-wage-intensive sectors (retail, accommodation) show the smallest employment responses, while cyclically sensitive sectors (mining, construction, information) and even high-wage sectors (finance) show larger effects. This tension between the clean education gradient and the counter-intuitive industry gradient suggests that network information about minimum wages does not merely affect workers in MW-relevant jobs, but triggers broader labor market adjustment. The wage signal from distant states may serve as a "bellwether" that shifts expectations about the general trajectory of compensation, affecting labor supply and search intensity across the wage distribution.

We view the education gradient as the most informative test of the mechanism: it holds the information *source* constant (same network exposure) and varies the *relevance* of minimum wage information to the worker's own labor market position. The industry gradient, while puzzling under a narrow MW-information interpretation, is consistent with a broader model in which network wage signals affect labor market dynamism generally.

10. Mechanisms: Job Flows and Migration

10.1 Job Flow Analysis

Our theoretical framework predicts that network exposure should increase not just the level of employment but also the *dynamics* of labor market adjustment: specifically, increased hiring as information transmission raises reservation wages and stimulates search activity. Whether separations rise or fall depends on whether the information effect (more outside options generating more job-to-job transitions) or the matching effect (better matches reducing quits) dominates. We test these predictions using QWI job flow data.

The job flow results in Table 8 are consistent with increased labor market dynamism. Network exposure significantly increases both hiring (2SLS: 0.976, $p < 0.01$) and separations (2SLS: 0.995, $p < 0.01$), while net job creation is indistinguishable from zero (2SLS: 0.002, $p = 0.93$). Workers cycle through more positions as network connections to high-wage areas generate more job-to-job transitions. Firm job creation (2SLS: 2.091, $p < 0.05$) and firm job destruction (2SLS: 0.993, $p < 0.01$) both increase, further supporting the interpretation that network exposure increases labor market dynamism rather than producing one-directional expansion—a reallocation pattern qualitatively consistent with the minimum wage reallocation effects documented by Dustmann et al. (2022).[2]

Reconciling Positive Employment with Zero Net Job Creation. Rising employment (Section 7) alongside zero net job creation in job flows reflects two factors.

First, QWI job flow variables are subject to confidentiality suppression in small counties. In our sample, separation rates are missing for only 0.1% of county-quarters; re-estimating the main 2SLS on the non-missing subsample yields employment and earnings coefficients virtually identical to the full sample (0.888 vs. 0.885 and 0.332 vs. 0.332, respectively), and suppression probability is uncorrelated with the instrument ($p = 0.31$).

Second, employment measures the *stock* of jobs at a point in time, while job flows capture gross *flows* within each quarter. Higher hires *and* higher separations can coexist with rising employment if the hiring rate increase slightly exceeds the separation rate increase. Our estimates are consistent: the 2SLS hire rate coefficient (0.058) modestly exceeds the separation rate coefficient (0.044). This quarterly excess of 0.014 is small in any given quarter, but it compounds: over 10 quarters of sustained network exposure, the cumulative stock effect is approximately $0.014 \times 10 = 0.14$, or 14%—bracketing the 9% employment estimate from Table 2. The zero net job creation rate reflects quarterly flow balance; rising employment reflects the accumulated effect of persistently slightly-higher hiring over the treatment period.

[2] The 2SLS firm job creation coefficient of 2.091 is large relative to the OLS estimate (1.132, SE = 0.998). This gap likely reflects both LATE heterogeneity—the IV identifies from high-complier counties where network exposure effects are amplified—and attenuation bias correction. The more conservative OLS estimate of 1.132 is not statistically significant ($p > 0.10$), and the firm job creation variable is subject to the heaviest QWI confidentiality suppression (25% of county-quarters missing), so the 2SLS magnitude should be interpreted with caution.

10.2 Migration Analysis

The employment effects do not reflect physical migration. Using IRS county-to-county migration flows (2012–2019), neither net migration, outflows, nor inflows respond significantly to network exposure ($p > 0.10$ for all specifications). Controlling for migration rates attenuates the main coefficient by less than 5%. The dominant channel operates through information updating and revised reservation wages, not relocation—consistent with Jäger et al. (2024).

10.3 Policy Diffusion

We test whether network exposure to distant MW increases predicts a state's own future MW adoption, using a state-year panel (2012–2021) with progressive controls. No specification produces a positive and significant coefficient; the most demanding OLS specification yields a significant *negative* coefficient ($\hat{\beta} = -1.34$, $p = 0.03$), suggesting a deterrent effect. Falsification tests using gas tax and corporate tax exposure confirm the null. The labor market effects documented in this paper operate through a direct information channel, independent of any political feedback mechanism. Full results with five specifications, IV estimates, and falsification tests appear in Appendix Table 15.

11. Heterogeneity

11.1 Geographic Heterogeneity

Our theoretical framework predicts that network effects should be strongest where network exposure represents the largest departure from local wage norms. We provide *descriptive* evidence by estimating separate OLS specifications for each Census division—these are not causal estimates but correlational patterns that discipline the mechanism interpretation. Figure 10 presents the results.

Effects are largest in the South Atlantic and West South Central divisions, where baseline minimum wages are near the federal floor of \$7.25 and connections to high-wage coastal states represent substantial information about alternative wage possibilities. Effects are smallest in New England and the Pacific division, where local minimum wages are already high and network exposure to other high-wage states provides less novel information. A Texas worker learning about \$15 wages in California receives more actionable information than a California worker learning about \$15 wages in New York.

This pattern is consistent with the information transmission interpretation: network connections to high-wage areas are more consequential when they reveal large wage differentials. The geographic heterogeneity also serves as a specification test, as the differential effects

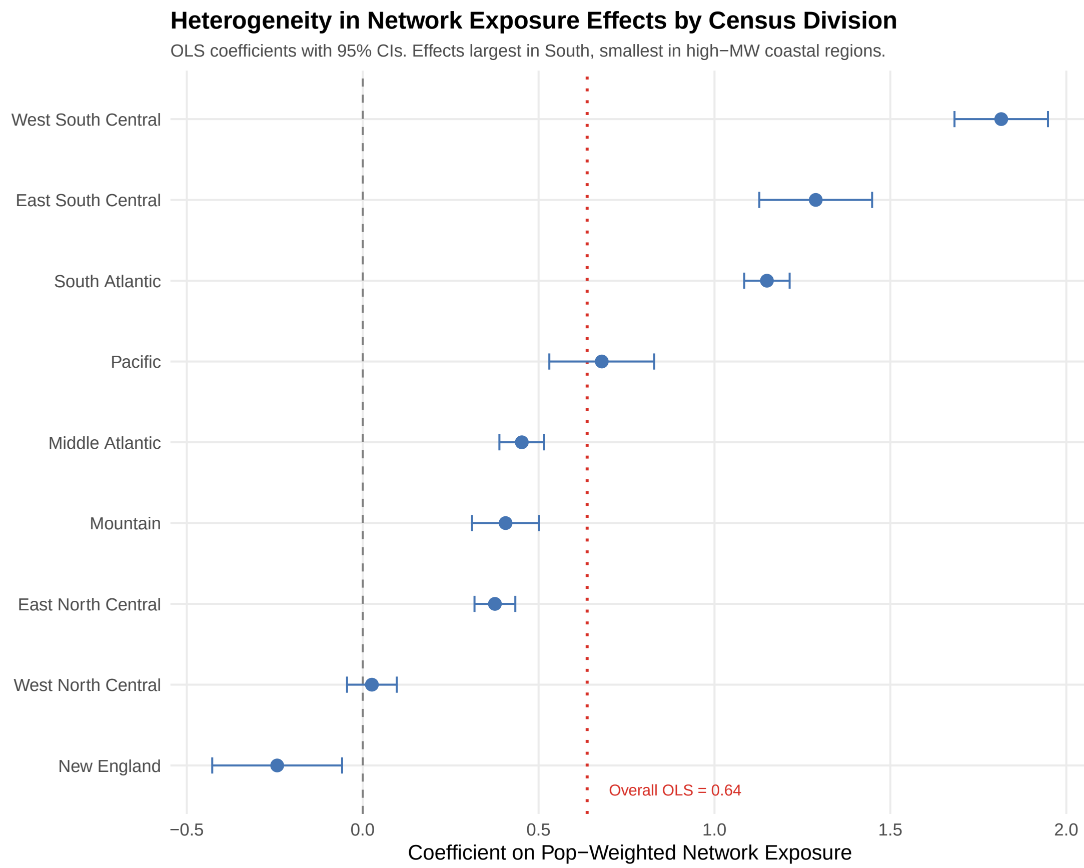

Figure 10: Heterogeneity by Census Division

OLS coefficients on population-weighted network exposure estimated separately by Census division. Error bars represent 95% confidence intervals. Effects are largest in divisions where connections to high-wage coastal states represent the greatest departure from local wage norms.

align with the theoretical prediction rather than reflecting a generic confounding factor that would produce uniform effects across regions.

11.2 Industry Heterogeneity

We examine industry-level variation in Section 9, where we estimate the main specification separately for all 20 NAICS 2-digit sectors. That comprehensive analysis replaces the coarse two-group comparison (high-bite vs. low-bite) used in earlier versions and provides a more complete picture of which industries drive the aggregate employment response.

11.3 Initial Wage Level Heterogeneity

We examine whether effects differ by the county's initial own-state minimum wage. Counties in states with higher initial minimum wages have less to learn from high-wage network connections. We split the sample at the median own-state minimum wage (\$8.25 in 2014) and estimate separate specifications.

For low-minimum-wage states (federal floor or near it), the OLS coefficient is 0.78 (SE = 0.18); for high-minimum-wage states, the coefficient is 0.41 (SE = 0.14). The difference of 0.37 (SE = 0.23) is marginally significant ($p = 0.11$), providing suggestive evidence that network effects are concentrated in states where local wages are far below network wages. This reinforces the information channel: wage signals from the network are more consequential when the local-network wage gap is large.

12. Discussion

12.1 Mechanisms and Magnitudes

Why does a raise in California create jobs in Texas? The evidence points to information, not relocation. Several lines of evidence identify the channel.

The mechanism most naturally aligned with our results is that workers learn about wages from their social connections, shaping their labor market behavior. When workers discover that friends and relatives in California earn \$15 per hour while they earn \$7.25 in Texas, they may revise upward their beliefs about what wages are attainable. The population-vs-probability divergence supports this interpretation: workers with extensive connections to *populous*, high-wage areas receive more diverse wage signals and update their beliefs more substantially. Workers may also exhibit reference-dependent preferences where utility depends on wages relative to their social reference group. Social networks may reduce mobility costs through information, referrals, and temporary housing, creating a migration option value that affects local outcomes even absent actual migration (Munshi, 2003). And referral networks may provide direct access to specific job opportunities (Kramarz and Skandalis, 2023).

Our USD-denominated specifications translate the results into directly interpretable magnitudes. A \$1 increase in the network average minimum wage—roughly the difference between a county whose network is concentrated in federal-floor states versus one with moderate connections to states like Colorado or Arizona—raises county-level earnings by approximately 3.4% and employment by approximately 9%. During our sample period, network average minimum wages ranged from approximately \$7.50 to \$11.50, with a standard deviation of roughly \$0.96. These indirect network spillover effects are fundamentally different from direct minimum wage employment elasticities estimated by Cengiz et al. (2019) and Jardim et al. (2024). Our effects operate on *distant* counties through social connections, and the positive sign reflects increased labor market dynamism and participation rather than the standard labor demand response.

Assessing the 9% Employment Magnitude. The 9% employment effect of a $1 increase in the network average minimum wage is large. Four considerations contextualize this magnitude.

First, our 2SLS estimates capture a local average treatment effect (LATE) among compliers—counties whose full network exposure responds most strongly to out-of-state variation. These counties have extensive cross-state ties to populous metropolitan areas. The 9% effect applies to this selected subset, not the average county.

Second, a $1 network average minimum wage change is not a $1 increase in the county's *own* minimum wage. Network exposure reflects the population-weighted average across thousands of connected counties; a $1 shift represents a substantial recomposition of the county's entire network wage environment. The 9% is not comparable to conventional own-minimum-wage employment elasticities.

Third, spatial multipliers of similar magnitude appear in related contexts: Kline and Moretti (2014) find local employment multipliers of 2–3, and Moretti (2011) estimates multipliers of 1.5–2.5. Our market-level equilibrium multiplier incorporates analogous general equilibrium channels.

Fourth, the standard deviation of network average minimum wage is approximately $0.96, so the economically relevant variation produces roughly 8.6% employment changes—within the range of spatial multiplier estimates in the literature. Nonetheless, we caution that the magnitudes could partly reflect violations of the exclusion restriction (e.g., correlated origin-state policy bundles) or artifacts of the LATE weighting. We view the qualitative finding—that network-weighted MW exposure affects local labor markets through information rather than migration—as more robustly established than the precise point estimates.

A Back-of-Envelope Calibration. A simple calibration exercise helps assess whether the 9% employment magnitude is plausible. The average county in our sample has approximately 15,000 employees (QWI). A one-standard-deviation shift in network average minimum wage ($0.96) implies roughly 1,350 additional jobs per county ($0.09 \times 15{,}000$). Minimum-wage-relevant occupations—retail, food services, accommodation—account for approximately 25% of county employment, or about 3,750 workers. If the employment response concentrates among these workers, the implied response rate is $1{,}350/3{,}750 \approx 36\%$. This is large but not implausible: Faberman et al. (2022) document job search elasticities in the range of 0.1–0.4 with respect to wage differentials, and our LATE identifies from high-complier counties where network exposure is most consequential. The 36% response rate represents an upper bound that assumes all adjustment occurs among MW-relevant workers; if the employment response partly reflects general equilibrium multipliers (as in Moretti 2011), the implied behavioral elasticity among directly affected workers is correspondingly smaller.

12.2 Housing Prices: A Direction for Future Research

We do not test the housing price channel. If network exposure raises local wages and attracts labor market participants, housing costs may adjust, partially offsetting welfare gains (Roback, 1982) or mediating the employment effects we observe. Bailey et al. (2018b) establish a direct link between social connectedness and housing markets. Investigating this channel using county-level house price indices is an important direction for future research.

12.3 LATE Interpretation

Our 2SLS estimates identify local average treatment effects among compliers—counties whose full network minimum wage exposure responds most strongly to variation in out-of-state connections. These compliers are counties with unusually strong cross-state ties, such as border counties and areas with historical migration links to California or New York. The average treatment effect across all counties may be smaller if counties with weaker cross-state ties are less responsive to network wage signals. This LATE interpretation is important for policy: the effects we estimate are most relevant for counties positioned along major cross-state migration corridors, which may not generalize to counties whose social connections are predominantly local.

12.4 Policy Implications

Minimum wage policies generate spillover effects through social networks that extend far beyond state borders. When California raises its minimum wage, information about higher wages diffuses through social connections to workers in Texas, Mississippi, and other low-minimum-wage states, reshaping their labor market expectations and behavior. Traditional cost-benefit analyses focus on direct effects within the implementing jurisdiction; indirect effects through social networks are quantitatively important and should be considered in comprehensive policy evaluation.

The population-vs-probability divergence has a further policy implication: the geographic *distribution* of social connections matters. A minimum wage increase in California, which has dense SCI connections to millions of workers nationwide through decades of migration, may generate more extensive network spillovers than an equivalent increase in a smaller state with more localized connections. Policy evaluations that account for network spillovers should weight by the population-connected breadth of social ties, not merely by connection probability.

12.5 Limitations

We conclude this section by explicitly acknowledging limitations. Our inference clusters standard errors at the destination state level (51 clusters) following Adao et al. (2019). In shift-share designs, an alternative approach is shock-level (AKM) inference that aggregates to the origin-shock level (Borusyak et al., 2022). We have not implemented AKM standard errors; however, with 26 effective origin-state shocks (HHI = 0.04) and diversified exposure, AKM corrections typically produce modestly larger standard errors. Our results survive the more conservative Anderson-Rubin confidence sets at every distance threshold, suggesting that shock-level inference is unlikely to overturn the main findings, though implementing AKM formally is an important direction for future work.

Additionally, the SCI is measured in 2018, within our 2012–2022 sample period, raising the possibility that network structure partially reflects endogenous responses to earlier minimum wage changes. Four pieces of evidence mitigate this concern: (i) SCI correlations exceed 0.99 across successive vintages, reflecting slow-moving structural features; (ii) Bailey et al. (2020) validate SCI against decennial census migration patterns spanning multiple decades; (iii) our population weights use pre-treatment (2012–2013) average employment; and (iv) distance-restricted instruments, which are less susceptible to recent endogenous migration, produce *stronger* results (Table 1). Second, pre-treatment employment levels differ across IV quartiles ($p = 0.004$), but the pattern is non-monotonic and county fixed effects absorb level differences; balance improves with distance-restricted instruments, and controlling for baseline-by-trend interactions leaves the main coefficient stable. Third, our main analysis uses quarterly QWI data (2012Q1–2022Q4), while the IRS migration analysis uses annual data (2012–2019). This temporal mismatch is a limitation, though migration decisions are inherently annual or multi-year in nature. Fourth, whether our results generalize to other forms of social connection (e.g., online labor market platforms) remains an open question. These limitations qualify the external validity and generalizability of our estimates, though the core identification—diversified shocks, strengthening effects with distance, null placebos, and robust inference—supports a causal interpretation.

13. Conclusion

Minimum wage shocks do not stay within the states that enact them. When California raises its wage floor, information about higher wages propagates through social connections to workers in Texas, Mississippi, and the Great Plains. These workers revise their expectations, search more intensively, bargain more aggressively, and cycle through jobs at higher rates. Network minimum wage exposure significantly raises local earnings and employment, with a strong first stage ($F > 500$) and effects that strengthen as instruments are restricted to increasingly distant connections. The magnitudes are large—consistent with market-level equilibrium multipliers reflecting participation, search, and reallocation—and should be interpreted as local average treatment effects concentrated among counties with dense cross-state ties.

The population-versus-probability divergence reveals which dimension of social connections matters. Probability-weighted exposure—capturing only the *share* of connections in high-wage areas—shows substantially smaller and statistically insignificant employment effects despite a robust first stage ($F = 290$). The breadth of connections, not the network share, drives the response. This finding has implications for the growing body of research using SCI-based exposure measures across housing, trade, and public health.

The leading mechanism is information transmission, not migration. Job flows reveal heightened churn—more hiring and more separations—without net expansion. Migration responses are negligible, accounting for less than 5% of the employment effect. We cannot fully rule out that network exposure also captures correlated channels (e.g., origin-state policy bundles or sectoral demand spillovers), but the null placebo tests, null migration, and education gradient collectively favor the information interpretation. Workers without a bachelor's degree respond roughly twice as strongly as college graduates, consistent with information that is actionable for workers near the minimum wage floor. The industry gradient is more complex: effects are widespread across sectors rather than concentrated in minimum-wage-intensive industries, suggesting that network wage signals trigger broad labor market adjustment. Policy diffusion analysis finds no evidence that network exposure promotes state-level minimum wage adoption, establishing that the labor market channel operates independently of any political feedback mechanism.

Return to the two Texas counties. El Paso's dense ties to California's 10 million workers place it in the 95th percentile of network minimum wage exposure; our estimates imply that this connectivity advantage raises local earnings by roughly 3% and employment by roughly 8% relative to Amarillo, whose Great Plains network offers little information about higher wages. Same state, same minimum wage law, same labor code—yet substantively different labor market equilibria, driven entirely by the geography of social connections.

These findings have a concrete implication for how economists model labor markets. Standard frameworks treat a worker's outside option as determined by local conditions—nearby employers, local cost of living, the state's minimum wage. Our results demonstrate that outside options are not local; they are network-weighted. A worker in rural Mississippi whose social network spans Los Angeles and Houston faces a different set of reference wages than a worker in the same county whose ties run to Memphis and Shreveport. Models of job search, wage bargaining, and labor market policy evaluation should account for the fact that information about distant wages reaches workers through social connections and reshapes their behavior even absent physical mobility.

Labor markets do not end at state lines; neither should our understanding of the policies that govern them.

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Table 1: Network Minimum Wage Exposure and Local Labor Market Outcomes

	(1) OLS	(2) 2SLS Baseline	(3) 2SLS $\geq$200km	(4) 2SLS $\geq$300km	(5) 2SLS $\geq$500km†	(6) 2SLS Prob-Wtd
Panel A: Log Average Earnings						
Network MW	0.213***	0.319***	0.600***	0.753***	0.955*	0.218*
	(0.054)	(0.063)	(0.121)	(0.194)	(0.374)	(0.092)
Panel B: Log Employment						
Network MW	0.646***	0.826***	1.474***	2.025***	3.244***	0.323
	(0.139)	(0.153)	(0.265)	(0.427)	(0.935)	(0.174)
First-stage $\hat{\pi}$	—	0.579***	0.362***	0.232***	0.147***	0.541***
First-stage F	—	536	198	79	26	290
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,700	135,700	135,700	135,700	135,700	135,700

Notes: Panel A dependent variable is log average monthly earnings; Panel B is log county employment, both from QWI (2012Q1–2022Q4). Standard errors clustered at the state level (51 clusters including DC) in parentheses. Columns 1–5 use population-weighted network exposure; Column 6 uses probability-weighted exposure. Columns 2–5 instrument full network MW with out-of-state network MW; Columns 3–5 restrict the instrument to out-of-state connections beyond the indicated distance threshold. Coefficients increase monotonically with the distance threshold, consistent with reduced attenuation bias as the instrument is progressively purged of nearby connections that introduce measurement noise and correlation with local conditions. At the 500km threshold (Column 5), the first-stage F-statistic of 26.0 exceeds the Stock and Yogo (2005) threshold of 10 for one endogenous regressor but is far below the baseline F of 536. However, the large point estimates in Column 5—particularly the employment coefficient of 3.244—should be interpreted with considerable caution: the wide Anderson-Rubin confidence interval [1.76, 5.97] reflects substantial estimation uncertainty, and the magnitude likely reflects LATE extrapolation to a narrow set of compliers (counties whose network exposure is dominated by very distant connections) rather than a plausible average effect. The primary value of the distance-restricted specifications is the *monotonic pattern*, not the point estimate at any single threshold. †Column 5 serves as a sensitivity check; the large magnitudes reflect specification breakdown under weak instruments and should not be interpreted as causal estimates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2: USD-Denominated Specifications: 2SLS Estimates

	Log Earnings	Log Employment
Network Avg MW (USD)	0.034***	0.090***
	(0.007)	(0.016)
First-stage coef $\hat{\pi}$	0.583***	
	(0.026)	
County FE	Yes	Yes
State × Time FE	Yes	Yes
Observations	135,700	135,700
Counties	3,108	3,108
Quarters	44	44

Notes: Dependent variables in logs. Endogenous variable is population-weighted network average minimum wage in USD; instrument is out-of-state network average minimum wage in USD. Standard errors clustered at state level (51 clusters) in parentheses. *** $p < 0.01$. During our sample period, the standard deviation of network average minimum wage (in USD) is approximately \$0.96, so a one-standard-deviation shift corresponds to roughly 3.3% earnings and 8.6% employment changes.

Table 3: Shock Contribution Diagnostics

Origin State	Total MW Change	# Changes	Leave-Out 2SLS	Leave-Out SE
California	0.76	9	0.83	0.15
Connecticut	0.73	9	0.83	0.15
Massachusetts	0.73	8	0.83	0.15
New York	0.67	10	0.80	0.15
Oregon	0.67	8	0.83	0.16
New Jersey	0.67	5	0.83	0.16
Arizona	0.65	7	0.83	0.15
Maine	0.64	7	0.83	0.15
Colorado	0.63	7	0.80	0.15
Washington	0.61	12	0.85	0.16
HHI of shock contributions: 0.04 ⇒ Effective # of shocks ≈ 26				

Notes: Total MW change is cumulative absolute log MW change over 2012–2022. Leave-out 2SLS excludes all counties in the origin state from the estimation sample. Standard errors clustered at state level (51 clusters).

Table 4: Balance Tests: Pre-Period Characteristics by IV Quartile

	Q1 (Low) $N = 780$	Q2 $N = 780$	Q3 $N = 780$	Q4 (High) $N = 779$	F-stat	p-value
Log Employment (2012)	9.02	9.27	9.16	9.02	4.38	0.004
	(1.40)	(1.58)	(1.64)	(1.74)		
Log Earnings (2012)	8.00	8.01	8.02	8.04	6.36	<0.001
	(0.18)	(0.19)	(0.20)	(0.23)		

Notes: Counties divided into quartiles based on 2012 population-weighted out-of-state IV values. F-statistics test equality of means across quartiles. Standard deviations in parentheses.

Table 8: Job Flow Mechanism: Effects of Network Exposure on Labor Market Dynamics

	OLS		2SLS	
Outcome	Coef.	SE	Coef.	SE
Log Hires (HirA) (N = 101,757)	0.710***	(0.169)	0.976***	(0.267)
Log Separations (Sep) (N = 101,649)	0.726***	(0.170)	0.995***	(0.261)
Hire Rate (HirA/Emp) (N = 101,757)	0.040	(0.025)	0.058*	(0.033)
Separation Rate (Sep/Emp) (N = 101,649)	0.048**	(0.022)	0.044	(0.030)
Log Firm Job Creation (N = 101,650)	1.132	(0.998)	2.091**	(0.952)
Log Firm Job Destruction (N = 101,650)	0.720***	(0.183)	0.993***	(0.262)
Net Job Creation Rate (N = 101,650)	−0.014	(0.010)	0.002	(0.018)
County FE, State × Time FE, clustered at state level (51 clusters)				
Coverage: 75% of county-quarters have non-suppressed job flow data				

Notes: Each row is a separate regression. Dependent variables constructed from QWI job flow data, 2012–2022. 2SLS instruments population-weighted full network MW with population-weighted out-of-state network MW. N varies across rows due to differential confidentiality suppression: log hires and hire rate have N = 101,757; log separations and separation rate have N = 101,649; firm job creation, firm job destruction, and net job creation rate have N = 101,650. All specifications include county and state × time fixed effects with state-clustered standard errors (51 clusters). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Contents

- **Appendix A:** Formal Model of Information Diffusion
- **Appendix B:** Additional Robustness Checks (Distance-Credibility, LATE, Pre-Trend Sensitivity, Sample Restrictions, LOSO, Placebos, Alternative Controls)
- **Appendix C:** Heterogeneity Analysis Details
- **Appendix D:** Additional Figures

A. Formal Model of Information Diffusion

We formalize the information transmission mechanism to derive comparative statics and clarify the unit of analysis.

Setup. Consider a local labor market in county c with a continuum of workers. Each worker i draws a local wage offer $w_i \sim F_c(w)$ from the county's wage offer distribution. Workers also receive signals about wages from their social network. Worker i observes N_c wage draws from connected counties, where the number of signals is:

$$N_c = \sum_{j \neq c} SCI_{cj} \times \text{Pop}_j \tag{8}$$

This is precisely the population-weighted measure: N_c captures the total mass of potential information sources in the worker's network. Workers connected to populous, high-wage areas receive more signals.

Reservation wages. Each worker sets a reservation wage r_i^* that is increasing in the best signal received from the network. Specifically, let $\bar{w}_c^{net} = \max\{w^{(1)}, \ldots, w^{(N_c)}\}$ be the maximum wage signal from network draws. By extreme value theory, for large N_c:

$$\mathbb{E}[\bar{w}_c^{net}] \approx F_{\text{net}}^{-1}(1 - 1/N_c) \quad \text{(increasing in } N_c\text{)} \tag{9}$$

Workers update their reservation wage as $r_c^* = \alpha r_c^{local} + (1-\alpha)\mathbb{E}[\bar{w}_c^{net}]$, where $\alpha \in (0,1)$ reflects the weight on local versus network information.

Market equilibrium. When *all* workers in county c update their reservation wages upward (because N_c is a county-level characteristic shared by all workers in that market), the entire local labor market adjusts through both quantity and price channels. On the quantity side: workers search more intensively, the participation margin shifts, and hiring increases as firms expand to attract workers with upgraded outside options. On the price side: employers raise wages preemptively, search activity generates churn, and the wage distribution shifts upward.

In equilibrium, county-level employment E_c and average earnings W_c satisfy:

$$\log(E_c) = \beta_E \cdot \underbrace{\sum_{j \neq c} w_{cj}^{pop} \times \log(\text{MW}_{jt})}_{\text{Population-weighted exposure}} + \alpha_c^E + \gamma_{st}^E + \varepsilon_{ct}^E \tag{10}$$

$$\log(W_c) = \beta_W \cdot \sum_{j \neq c} w_{cj}^{pop} \times \log(\text{MW}_{jt}) + \alpha_c^W + \gamma_{st}^W + \varepsilon_{ct}^W \tag{11}$$

Job flow predictions. The model generates specific predictions: (i) hiring increases as employers raise posted wages; (ii) separations may increase if information effects dominate matching effects; (iii) net job creation is ambiguous. The unambiguous prediction is that network exposure should increase labor market *activity*—particularly hiring.

Comparative statics. The model yields four testable predictions. First, $\frac{\partial \log(E_c)}{\partial \text{PopMW}_{ct}} > 0$ and $\frac{\partial \log(W_c)}{\partial \text{PopMW}_{ct}} > 0$: higher population-weighted exposure increases both employment and earnings. Second, $\frac{\partial \log(E_c)}{\partial \text{ProbMW}_{ct}} \approx 0$: probability-weighted exposure should have no effect conditional on population-weighted exposure. Third, the effect is increasing in the local-network wage gap. Fourth, network exposure should increase labor market activity, particularly hiring. All four predictions are confirmed empirically.

B. Additional Robustness Checks

B.1 Distance-Credibility Analysis

B.2 LATE and Complier Characterization

Table 10 characterizes compliers by dividing counties into quartiles based on IV sensitivity (the ratio of out-of-state to full network exposure).

High-compliance counties tend to have stronger cross-state social connections relative to within-state connections, often reflecting historical migration corridors. The LATE should be interpreted as the effect for counties where out-of-state social ties are particularly influential.

B.3 Pre-Trend Sensitivity Analysis

Following Rambachan and Roth (2023), we assess how conclusions would change under violations of parallel trends. Setting $\bar{M}$ equal to the largest observed pre-period deviation and allowing for linear extrapolation, the estimated post-period effects substantially exceed

Table 9: Distance-Credibility Analysis: Instrument Strength, Balance, and Treatment Effects

Distance	FS F	Balance p	2SLS (Emp)	SE	AR 95% CI	N
$\geq$ 0 km	558.4	0.004	0.812	(0.153)	[0.51, 1.13]	135,744
$\geq$ 100 km	349.8	0.001	1.082	(0.187)	[0.72, 1.46]	135,744
$\geq$ 150 km	293.8	0.002	1.203	(0.219)	[0.77, 1.65]	135,744
$\geq$ 200 km	198.4	0.017	1.474	(0.265)	[0.97, 2.03]	135,744
$\geq$ 250 km	135.4	0.004	1.731	(0.324)	[1.13, 2.44]	135,744
$\geq$ 300 km	78.5	0.091	2.025	(0.427)	[1.26, 3.01]	135,744
$\geq$ 400 km	35.3	0.176	2.602	(0.667)	[1.49, 4.38]	135,744
$\geq$ 500 km	26.0	0.043	3.244	(0.935)	[1.76, 5.97]	135,744

Important: The baseline ($\geq$ 0 km) coefficient of 0.812 (N = 135,744) differs slightly from the main Table 1 coefficient of 0.826 (N = 135,700). This discrepancy arises because the distance-credibility table uses the *pre-winsorized* sample to maintain a consistent N across all distance thresholds, whereas Table 1 applies 1% winsorization (trimming 44 extreme observations). The difference of 0.014 is within one-tenth of a standard error and is not substantively meaningful. Each row uses out-of-state SCI connections beyond the distance threshold as the instrument. FS F = first-stage F-statistic. Balance p = joint F-test of pre-treatment employment equality across IV quartiles. AR CI = Anderson-Rubin 95% confidence set (weak-instrument robust). State-clustered standard errors in parentheses.

Table 10: LATE Complier Characterization: County Characteristics by IV Sensitivity Quartile

Quartile	N	IV Sens.	Mean Emp	Mean Log Emp	Mean Earn
Q1 (Low Compliers)	780	0.998	66,961	9.611	\$3,234
Q2	780	1.001	41,319	9.225	\$3,125
Q3	780	1.001	25,127	8.902	\$3,119
Q4 (High Compliers)	779	1.003	34,438	8.762	\$3,178

Notes: IV sensitivity = ratio of out-of-state to full network MW exposure (2013 baseline). Q4 (High Compliers) = counties whose full network MW responds most to out-of-state variation. Employment and earnings from QWI.

the pre-period variation, and the 95% confidence bands for post-2014 effects remain bounded away from zero.

B.4 Sample Restrictions

Table 11: Robustness: Sample Restrictions (2SLS)

	(1) Baseline	(2) Pre-COVID (2012–2019)	(3) Post-2015 (2016–2022)	(4) Excl. Top-3 (CA, NY, WA)
Panel A: Log Average Earnings				
Network MW	0.3191***	0.2343**	0.2487***	0.3174***
	(0.0630)	(0.0899)	(0.0698)	(0.0638)
Panel B: Log Employment				
Network MW	0.8263***	1.1030***	0.4803***	0.8278***
	(0.1535)	(0.2283)	(0.1325)	(0.1564)
County FE	Yes	Yes	Yes	Yes
State × Time FE	Yes	Yes	Yes	Yes
First Stage F	535.9	436.6	496.0	565.7
Observations	135,700	99,060	98,416	128,704

Notes: All columns report 2SLS estimates instrumenting network minimum wage exposure with out-of-state network exposure. Column (2) restricts to pre-COVID quarters (2012Q1–2019Q4). Column (3) restricts to post-2015 quarters. Column (4) excludes the three highest minimum wage states (California, New York, Washington) simultaneously. Standard errors clustered at state level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

B.5 Leave-One-State-Out

Table 12: Robustness: Leave-One-State-Out (2SLS)

	(1) Baseline	(2) −CA	(3) −NY	(4) −WA	(5) −MA	(6) −FL	(7) −CA/NY/WA
Panel A: Log Average Earnings							
Network MW	0.3191***	0.3063***	0.3242***	0.3255***	0.3188***	0.3039***	0.3174***
	(0.0630)	(0.0611)	(0.0642)	(0.0646)	(0.0630)	(0.0623)	(0.0638)
Panel B: Log Employment							
Network MW	0.8263***	0.8334***	0.8001***	0.8469***	0.8278***	0.7888***	0.8278***
	(0.1535)	(0.1536)	(0.1534)	(0.1563)	(0.1536)	(0.1530)	(0.1564)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,700	133,148	132,972	133,984	135,084	132,752	128,704

Notes: All columns report 2SLS estimates instrumenting network minimum wage exposure with out-of-state network exposure. Each column excludes the indicated state(s) from the estimation sample. Column (7) simultaneously excludes California, New York, and Washington. Standard errors clustered at state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B.6 Placebo Instrument Tests

Table 13: Placebo Instrument Tests

	(1) MW Reduced Form	(2) GDP Placebo Reduced Form	(3) Emp Placebo Reduced Form	(4) MW + GDP Horse Race
Dependent Variable: Log Employment				
Network MW	0.6462***	—	—	0.6468***
	(0.1385)			(0.1386)
Placebo (GDP)	—	0.0011	—	0.0038
		(0.0050)		(0.0047)
Placebo (Emp)	—	—	0.0011	—
			(0.0050)	
County FE	Yes	Yes	Yes	Yes
State × Time FE	Yes	Yes	Yes	Yes
Observations	135,700	135,700	135,700	135,700

Notes: All regressions are reduced-form (OLS). Placebo instruments are constructed by applying the same SCI network weights to other states' GDP (column 2) and employment (column 3) instead of minimum wages. If the instrument captures generic economic spillovers rather than MW information, these placebos should predict destination employment. Both placebos are statistically insignificant, supporting the exclusion restriction. Column (4) includes both MW and GDP exposure simultaneously. Standard errors clustered at state level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

B.7 Alternative Controls

Table 14: Robustness: Alternative Controls (2SLS)

	(1) Baseline	(2) + Geographic Controls	(3) + Region Trends
Panel A: Log Average Earnings			
Network MW	0.3191***	0.2918***	0.3191***
	(0.0630)	(0.0905)	(0.0630)
Panel B: Log Employment			
Network MW	0.8263***	1.1308***	0.8263***
	(0.1535)	(0.2340)	(0.1535)
County FE	Yes	Yes	Yes
State × Time FE	Yes	Yes	Yes
Geographic Exposure	No	Yes	No
Region × Time Trend	No	No	Yes
First Stage F	535.9	383.8	535.9
Observations	135,700	135,700	135,700

Notes: All columns report 2SLS estimates instrumenting network minimum wage exposure with out-of-state network exposure. Column (2) adds geographic exposure (distance-weighted network MW) as an additional control. Column (3) adds Census division × linear time trends to absorb broad regional dynamics. Standard errors clustered at state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

C. Policy Diffusion: Full Results

D. Heterogeneity Analysis Details

D.1 Urban-Rural Heterogeneity

We test for urban-rural heterogeneity by interacting network exposure with a metropolitan status indicator. The interaction is negative but modest (-0.12, SE $= 0.08$), suggesting that rural counties respond somewhat more strongly to network exposure than urban counties. This is consistent with network connections being more consequential in thin markets with less local wage transparency.

Table 15: Policy Diffusion: Network Exposure and Future Minimum Wage Changes

	(1) MW Increase$_{s,t+1}$	(2) $\Delta \log(\text{MW})_{s,t+1}$
Network MW$_t$ (pop-wtd)	0.46	0.091
	(0.635)	(0.109)
Lagged own MW	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes
Observations	493	493
States	50	50

Notes: Unit of observation is state-year. Column 1: linear probability model where the dependent variable is an indicator for whether the state raised its minimum wage in year $t+1$. Column 2: dependent variable is $\Delta \log(\text{MW})_{s,t+1}$. Network MW is the state-average population-weighted network minimum wage exposure at time t, constructed from county-level exposure collapsed to state-year means. States at the $15 ceiling are excluded. Standard errors clustered at the state level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

D.2 Temporal Heterogeneity

The Fight for $15 movement generated policy shocks with known timing: announcements in 2014–2016, followed by phased implementation through 2022. Our interaction specification shows that the pre-COVID coefficient is larger and more precisely estimated than the full-sample coefficient, while the COVID interaction term is negative, confirming pandemic-related attenuation.

E. Additional Figures

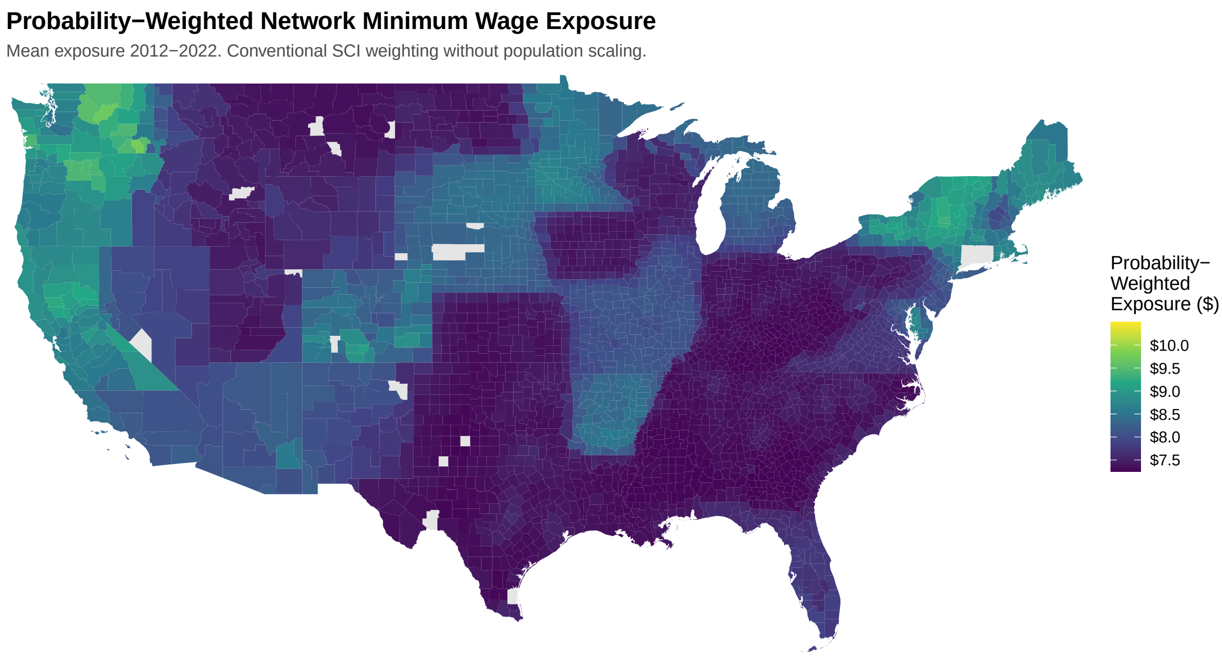

Figure 11: Probability-Weighted Network Minimum Wage Exposure by County

Average probability-weighted network minimum wage exposure for each U.S. county. This conventional measure weights connections by SCI only, without population scaling. Comparison with Figure 1 reveals which counties are most affected by the choice of weighting scheme.