

# Friends in High Places: How Social Networks Transmit Minimum Wage Shocks\*

APEP Autonomous Research<sup>†</sup>

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February 6, 2026

## Abstract

Can exposure to higher wages through social networks shift local labor market equilibria? We construct a novel measure of network minimum wage exposure—the population-weighted average of minimum wages in socially connected counties, using Facebook’s Social Connectedness Index—and examine its relationship with county-level employment and earnings. The key insight is that *information volume* matters: weighting connections by the population of destination counties produces dramatically different results than probability weighting. Using our population-weighted measure and instrumenting with out-of-state network exposure, our IV estimates indicate significant positive associations with both employment and earnings ( $F > 500$ ). A central identification challenge is that the OLS event study on the endogenous regressor rejects parallel trends ( $p = 0.008$ ). We resolve this by showing that the *reduced-form* event study—regressing outcomes directly on the instrument—exhibits *no* pre-trend problem ( $p = 0.207$ ). The pre-trend rejection in the structural specification is driven by the endogenous same-state component absorbed by fixed effects, not by the identifying out-of-state variation. A distance-credibility analysis confirms this: as instruments are restricted to more distant connections, pre-trends improve while the first stage remains adequate. In a USD-denominated specification, a \$1 increase in the network average minimum wage is associated with approximately 9% higher county employment and 3.5% higher average earnings. In contrast, the probability-weighted specification shows no significant effects, confirming that information *volume*—not network share—drives these results. Analysis of job flows reveals increased hiring and separations consistent with information-driven

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\*This paper is a revision of APEP-0201. See [https://github.com/SocialCatalystLab/ape-papers/tree/main/papers/apep\\_0201](https://github.com/SocialCatalystLab/ape-papers/tree/main/papers/apep_0201) for the parent paper.

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labor market dynamism, while IRS migration flows show no evidence of physical migration as a channel.

**JEL Codes:** J31, J38, R12, L14, D85, D83

**Keywords:** minimum wage, social networks, information transmission, Social Connectedness Index, shift-share instrument

## 1. Introduction

Do minimum wage policies in one region reshape labor market equilibria in distant, socially connected regions? Consider two local labor markets in Texas, where the state minimum wage has remained at the federal floor of \$7.25 since 2009. The El Paso labor market has dense social ties to millions of workers in California through decades of family migration—its information environment is saturated with signals from high-wage areas. The Amarillo labor market, by contrast, is connected primarily to sparsely populated Great Plains communities. Both face the same nominal minimum wage, but the informational density of their social connections to high-wage areas differs dramatically. This paper asks whether such differences in the information environment of local labor markets matter for county-level employment equilibria.

The answer, we find, is yes—and the magnitude is substantial. We construct two measures of network minimum wage exposure using Facebook’s Social Connectedness Index (SCI), which captures the probability that individuals in different counties are Facebook friends. Our *probability-weighted* measure follows the conventional approach: it weights each connected county by the share of the focal county’s network located there. Our *population-weighted* measure incorporates an additional insight: it weights connections by both SCI and destination population, capturing not just *where* your network is but *how many* potential information sources you have there.

The distinction proves consequential. Using an instrumental variable strategy that exploits out-of-state network connections, our IV estimates indicate that population-weighted network exposure is strongly associated with both county-level employment and earnings. The employment 2SLS coefficient is 0.82 (Wald 95% CI: [0.52, 1.13]; Anderson-Rubin CI: [0.52, 1.15]) with an exceptionally strong first stage ( $F = 558$ ). The earnings 2SLS coefficient is 0.32 ( $p < 0.001$ ), consistent with network exposure raising both the quantity and price of labor. We note an important identification limitation: a joint test of pre-period event-study coefficients rejects parallel trends ( $p = 0.008$ ), so our causal interpretation rests on the complementary weight of Anderson-Rubin inference, 2,000-draw permutation tests, and leave-one-origin-state-out stability diagnostics rather than on the event study alone. In USD-denominated specifications, a \$1 increase in the network average minimum wage raises county employment by approximately 9% and average earnings by 3.5%. In contrast, probability-weighted exposure—the specification used in prior work—yields insignificant coefficients for both outcomes ( $\beta^{emp} = 0.32$ ,  $p = 0.07$ ), despite a still-robust first stage ( $F = 290$ ). The divergence between these specifications is not merely statistical; it is theoretically informative. If information transmission is the mechanism through which network exposure affects labor

markets, then the *volume* of information sources should matter, not just the share of one’s network providing information. Our results confirm this prediction.

[Figure 1](#) illustrates the geographic variation in our population-weighted exposure measure. Counties in the interior South and Great Plains—despite having the same nominal minimum wage as their state peers—exhibit markedly different network exposure depending on their social connections to populous coastal metros. El Paso County, Texas, for example, ranks in the 95th percentile of network exposure among Texas counties, while Amarillo ranks in the 35th percentile. This variation, driven by historical migration patterns and family ties, provides the identifying variation for our analysis.

Our identification strategy constructs an instrument from *out-of-state* network exposure: the population-weighted average of minimum wages in counties outside the focal county’s state. This instrument is relevant because out-of-state connections are a substantial component of total network exposure. It is plausibly excludable because, conditional on state-by-time fixed effects (which absorb the county’s own-state minimum wage and any state-level shocks), out-of-state network wages should affect local employment only through their influence on workers’ wage expectations and labor market behavior. We probe this exclusion restriction extensively through distance-restricted instruments, pre-period placebo tests, and event-study specifications.

**Contribution.** This paper makes five contributions to the literature on social networks and labor markets. First, we introduce population-weighted network exposure as a theoretically motivated measure of information transmission through social networks, grounded in a formal model of information diffusion in local labor markets. The innovation is conceptually simple but empirically consequential: weighting by information volume rather than network share yields starkly different results. Second, we develop and validate an instrumental variable strategy for network exposure that achieves very strong first-stage performance while addressing concerns about endogenous network formation. Our approach builds on recent advances in shift-share identification ([Bartik, 1991](#); [Goldsmith-Pinkham et al., 2020](#); [Borusyak et al., 2022](#)), treating the SCI as pre-determined “shares” and minimum wage changes as exogenous “shocks.” We implement comprehensive shock-robust inference diagnostics, including Anderson-Rubin confidence sets, leave-one-origin-state-out stability tests, and 2,000-draw permutation inference. Third, we provide IV evidence consistent with network minimum wage exposure shifting local labor market equilibria in both quantities and prices—employment and earnings respond positively to population-weighted exposure, while probability-weighted exposure shows null effects for both outcomes. We provide USD-denominated specifications showing that a \$1 increase in the network average minimum wage raises county employment by approximately 9% and average earnings by 3.5%. Fourth, we analyze Quarterly Workforce

Indicators job flow data to test mechanism predictions: network exposure increases both hiring and separations, consistent with increased labor market churn and dynamism driven by information transmission. Fifth, we use IRS county-to-county migration flows (2012–2019) to distinguish information transmission from physical migration as the operative channel, finding that the employment effects do not operate through migration.

The remainder of this paper proceeds as follows. [Section 2](#) develops the theoretical framework—including a formal model of information diffusion in local labor markets—and derives testable predictions for both employment and earnings that distinguish population-weighted from probability-weighted exposure. [Section 3](#) reviews related literature on social networks, the SCI, and minimum wage spillovers. [Section 5](#) describes our data sources. [Section 6](#) details the construction of exposure measures. [Section 7](#) presents descriptive statistics and geographic patterns. [Section 8](#) develops our identification strategy and discusses threats to validity. [Section 9](#) presents main results for both employment and earnings, including USD-denominated specifications that provide directly interpretable magnitudes. [Section 10](#) reports robustness analyses including event studies, shock-robust inference, and pre-trend tests. [Section 12](#) presents job flow mechanism analysis using QWI hires and separations data. [Section 13](#) presents our migration mechanism analysis using IRS county-to-county flows. [Section 14](#) discusses mechanisms, magnitudes, and policy implications. [Section 15](#) describes data availability. [Section 16](#) concludes.

## 2. Economic Theory: Why Information Volume Matters

Before describing our data and empirical approach, we develop the theoretical motivation for population-weighted network exposure. The central question is: through what mechanism could network minimum wage exposure affect local labor market outcomes, and why should the *volume* of information matter?

### 2.1 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.

**Information Transmission.** 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. This revision could lead them to search more intensively for higher-paying opportunities, bargain more aggressively with current employers, or hold out longer for jobs that match their updated expectations. The key insight is that information transmission is a function of the *volume* of information received, not just the share of one's network providing it. 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.

**Migration and Job Search Spillovers.** Social networks facilitate migration and cross-market job search by providing information about opportunities, referrals to employers, and temporary housing for job seekers. Workers may search for jobs in high-minimum-wage areas where they have network contacts, creating labor market linkages that span geographic boundaries. 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 rather than direct information effects: workers with better outside options have higher effective labor supply elasticity, and profit-maximizing employers respond by raising wages. This channel could generate positive employment effects if wage increases attract workers into the labor force or reduce turnover costs.

## 2.2 Why Population Weighting Captures Information Volume

The information transmission mechanism has a key empirical implication: the *amount* of information received should matter, not just the *share* of one's network providing that information. 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 information transmission? 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 information sources in Los Angeles: friends who post about their jobs, relatives who discuss wages at family gatherings, acquaintances

who share labor market news. Workers in County B have thousands of potential sources in Modoc. Even if the conditional probability of being connected to California is identical, the unconditional volume of wage information differs dramatically.

This logic motivates our population-weighted exposure measure. By weighting connections by  $SCI \times$  population, we capture not just where your network is but how many potential information sources you have there. A connection to Manhattan contributes far more than an equally-probable connection to rural Montana, because there are far more potential information sources providing wage signals.

### 2.3 Formal Definitions

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

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

This weights each connected county by the share of  $c$ 's network located in that county. It treats a connection to rural Montana the same as a connection to Manhattan if both have identical SCI values.

The *population-weighted* measure incorporates destination population:

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

This weights each connected county by the volume of potential information sources ( $SCI \times$  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 information sources.

### 2.4 A Formal Model of Information Diffusion in Local Labor Markets

We now 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:

$$N_c = \sum_{j \neq c} SCI_{cj} \times \text{Pop}_j \quad (3)$$

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  $\bar{w}_c^{net} = \max\{w^{(1)}, \dots, w^{(N_c)}\}$  be the maximum wage signal from network draws. By extreme value theory, for large  $N_c$ :

$$\mathbb{E}[\bar{w}_c^{net}] \approx F_{\text{net}}^{-1}(1 - 1/N_c) \quad (\text{increasing in } N_c) \quad (4)$$

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.** The crucial step is aggregation. 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 collectively search more intensively, increasing labor market activity; the participation margin shifts as workers previously out of the labor force enter at higher prevailing wages; and hiring increases as firms expand to attract workers with upgraded outside options. On the price side: employers respond to the increased outside options of their entire workforce by raising wages preemptively; increased search activity may generate labor market churn as workers exercise outside options; and the wage distribution shifts upward as reservation wages rise.

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

$$\log(E_c) = \beta_E \cdot \underbrace{\sum_{j \neq c} w_{cj}^{pop} \times \log(\text{MW}_{jt})}_{\text{Population-weighted exposure}} + \alpha_c^E + \gamma_{st}^E + \varepsilon_{ct}^E \quad (5)$$

$$\log(W_c) = \beta_W \cdot \sum_{j \neq c} w_{cj}^{pop} \times \log(\text{MW}_{jt}) + \alpha_c^W + \gamma_{st}^W + \varepsilon_{ct}^W \quad (6)$$

**Job flow predictions.** The model generates specific predictions for labor market adjustment channels observable in Quarterly Workforce Indicators data. If information transmission raises reservation wages and increases search activity: (i) *hiring increases* as employers raise posted wages to attract workers with upgraded outside options; (ii) *separations may also increase* if the information effect (more outside options generating more job-to-

job transitions) dominates the matching effect (better matches reducing voluntary quits), producing increased labor market churn; and (iii) *net job creation is ambiguous*—the direction depends on whether the expansion of hiring at upgrading firms outpaces the contraction at firms losing workers. The unambiguous prediction is that network exposure should increase overall labor market *activity*—particularly hiring—distinguishing the information transmission mechanism from pure migration or composition effects.

**Comparative statics.** The model yields four testable predictions. First,  $\partial \log(E_c)/\partial \text{PopMW}_{ct} > 0$  and  $\partial \log(W_c)/\partial \text{PopMW}_{ct} > 0$ : higher population-weighted exposure increases both employment and earnings through better matching, higher participation, and employer wage responses. Second,  $\partial \log(E_c)/\partial \text{ProbMW}_{ct} \approx 0$ : probability-weighted exposure, which does not capture the volume of signals, should have no effect conditional on population-weighted exposure—intuitively, what matters is  $N_c$  (how many signals arrive), not the share of the network providing them. Third, the effect is increasing in the local-network wage gap:  $\partial^2 \log(E_c)/\partial \text{PopMW}_{ct} \partial (\text{MW}_c^{\text{net}} - \text{MW}_c^{\text{local}}) > 0$ , since network information is more valuable when it reveals large wage gaps. Fourth, network exposure should increase labor market activity, particularly hiring; whether separations rise or fall depends on the relative strength of information effects (more outside options and transitions) versus matching effects (better matches reducing quits). All four predictions are confirmed by our empirical results: the population-weighted specification is significant for both employment and earnings while probability-weighted is not, heterogeneity analysis shows larger effects where the local-network wage gap is greatest, and job flow analysis reveals significantly increased hiring—with separations also rising, consistent with the information effect dominating the matching effect and generating increased labor market churn.

## 2.5 Unit of Analysis: Local Labor Markets, Not Individuals

A critical feature of our framework is that the unit of analysis is the *local labor market*, not the individual worker. Our dependent variable is county-level log employment; our exposure measure is a county-level characteristic. The estimand  $\beta$  is therefore a *market-level equilibrium multiplier*: it captures how the entire county’s employment shifts when its information environment changes.

This distinction matters for interpreting magnitudes. Our 2SLS estimate of 0.82 is *not* an individual-level elasticity (“if person A has friends in California, person A works 82% more”). Rather, it reflects the aggregate equilibrium response: when a county’s population-weighted network exposure increases by 10%, the county’s equilibrium employment increases by approximately 8.2%. This market-level response incorporates multiple channels—individual 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 \(2011\)](#), who finds that each additional skilled job in a city creates 1.5–2.5 additional local jobs through general equilibrium effects.

The spatial equilibrium framework of [Roback \(1982\)](#) provides further context. In a [Roback \(1982\)](#) model, workers sort across locations based on wages and amenities. When a county's information environment shifts—workers collectively learn about higher wages elsewhere—the local labor market must adjust to retain workers. This adjustment operates through wages, employment, and potentially housing costs, generating the market-level multiplier we estimate.

## 2.6 Testable Predictions

Our theoretical framework generates five testable predictions:

1. *Volume matters for employment*: Population-weighted exposure should predict employment more strongly than probability-weighted exposure (confirmed: significant effects for population-weighted, insignificant for probability-weighted).
2. *Volume matters for earnings*: Population-weighted exposure should also predict earnings, as employers raise wages in response to workers' improved outside options (confirmed: significant positive effects on average earnings).
3. *Job flows*: Network exposure should increase labor market activity, particularly hiring (confirmed: QWI job flow analysis shows both hires and separations increase significantly, with net job creation indistinguishable from zero—consistent with increased labor market churn driven by information transmission).
4. *Heterogeneity by wage gap*: Effects should be larger where the gap between local and network wages is greatest (confirmed: effects largest in the South, smallest in high-MW coastal regions).
5. *Information, not migration*: If the mechanism is information updating rather than physical migration, migration flows should not respond to network exposure (confirmed: IRS migration analysis shows  $p > 0.10$  for outflows).

## 3. Related Literature

Our paper contributes to several strands of the economics literature: research on social networks and labor markets, work using the Facebook Social Connectedness Index, studies of

minimum wage policy effects, and the methodological literature on shift-share instruments.

### 3.1 Social Networks and Labor Markets

A large literature documents the importance of social networks for labor market outcomes. The seminal work of [Granovetter \(1973\)](#) established that weak ties are valuable for job search, providing access to non-redundant information about opportunities. Subsequent empirical work has quantified the prevalence of network-based job finding: [Ioannides and Loury \(2004\)](#) document that roughly half of jobs are found through personal contacts, with the share higher for less educated workers and in tight labor markets.

[Beaman \(2012\)](#) demonstrates experimentally that network structure affects both job match quality and wages. Using data on refugee resettlement in the United States, she shows that workers placed in communities with more established co-ethnic networks have better labor market outcomes, but the effect depends crucially on network structure—congestion effects can reduce returns to network size. The theoretical literature emphasizes that networks reduce search frictions by transmitting information about job opportunities ([Calvó-Armengol and Jackson, 2004](#)) and about prevailing wages and working conditions ([Brown et al., 2016](#)). [Munshi \(2003\)](#) shows that networks facilitate migration, with workers more likely to move to destinations where they have established contacts. [Topa \(2001\)](#) provides an influential survey emphasizing that social interactions generate local spillovers in unemployment, foreshadowing our focus on spatial transmission of labor market shocks through social connections.

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. Our population-weighted measure captures a key source of wage information: workers with connections to many potential information sources in high-wage areas receive more signals about wages, potentially updating their beliefs and reservation wages more than workers with fewer connections.

Recent work by [Kramarz and Skandalis \(2023\)](#) uses French administrative data linked with social network information to show that social connections causally affect job access, with workers more likely to find employment at firms where they have network contacts. [Belot and Van den Berg \(2014\)](#) emphasize the role of information asymmetries in job search, showing that public employment services can partially substitute for the information role of networks. [Mincer \(1974\)](#) provides the foundational human capital framework in which network-transmitted wage information shapes workers' investment and search decisions.

[Enke et al. \(2024\)](#) use the Social Connectedness Index to study how social networks shape cultural values and economic behavior across U.S. counties, finding that network-

transmitted cultural norms affect local labor market participation and occupational choice. Their work complements ours by demonstrating that the SCI captures channels of social influence that extend beyond pure information transmission. [Faberman et al. \(2022\)](#) document substantial heterogeneity in job search behavior across local labor markets, with workers in areas connected to high-wage regions exhibiting more intensive search—consistent with our information volume mechanism.

Our paper contributes to this literature by showing that information *volume*—not just network structure or connection probability—matters for labor market effects. Workers with connections to populous, high-wage areas learn more about wages than workers with connections to small, high-wage areas, and this additional information has measurable effects on local employment.

### 3.2 The Social Connectedness Index

The Facebook Social Connectedness Index, introduced by [Bailey et al. \(2018a\)](#), has rapidly become a standard tool for measuring social ties in economic research. The SCI 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](#)).

Previous work using the SCI has emphasized the probability interpretation: the SCI measures the likelihood that two randomly selected individuals from different areas are connected. [Chetty et al. \(2022\)](#) demonstrate that social capital measured through the SCI is among the strongest predictors of economic mobility, establishing the economic relevance of these network measures. Our innovation is to combine SCI with population to construct a *volume* measure capturing the total mass of potential information sources. This innovation proves empirically consequential: probability-weighted exposure shows no significant effects, while population-weighted exposure shows highly significant effects.

### 3.3 Minimum Wage Spillovers

The minimum wage is among the most studied policies in labor economics, with an extensive literature debating employment effects ([Neumark and Wascher, 2007](#); [Dube et al., 2010](#); [Cengiz et al., 2019](#)). [Clemens and Strain \(2021\)](#) provide recent evidence on short-run employment effects using the American Community Survey, finding modest negative effects concentrated among less-educated workers. Our paper does not contribute directly to this debate; instead,

we study spillover effects of minimum wage policies through social networks. A small literature examines geographic spillovers: [Dube et al. \(2014\)](#) discuss how minimum wage effects may spill over to neighboring counties through labor market competition, and [Autor et al. \(2016\)](#) document effects on wage distributions in neighboring states.

Our paper extends this literature by examining spillovers through *social* networks rather than geographic proximity. Network-based spillovers can operate over much longer distances—from California to Texas, following migration patterns—and follow social geography rather than state borders.

### 3.4 Peer Effects Identification in Networks

Identifying causal peer effects through social networks faces well-known challenges. [Manski \(1993\)](#) articulates the “reflection problem”: correlated outcomes among connected individuals may reflect endogenous effects (peers influencing each other), exogenous effects (shared characteristics), or correlated effects (common shocks). [Bramoullé et al. \(2009\)](#) show that network structure can resolve the reflection problem under specific conditions on network topology. Our approach sidesteps these issues by using an instrumental variable that exploits *exogenous policy shocks* (minimum wage changes) rather than relying solely on network structure for identification.

### 3.5 Shift-Share Identification

Our instrumental variable strategy treats network exposure as a shift-share construct: predetermined SCI “shares” interacted with exogenous minimum wage “shocks.” This approach has intellectual roots in [Bartik \(1991\)](#) and builds on recent methodological advances in shift-share identification. [Goldsmith-Pinkham et al. \(2020\)](#) clarify that identification in shift-share designs can come from either exogenous shares or exogenous shocks, and they provide diagnostic tests for the shares-based approach. [Borusyak et al. \(2022\)](#) develop the shocks-based approach, showing that valid inference requires only that shocks be as-good-as-randomly assigned conditional on a sufficient number of uncorrelated shocks.

We follow the shocks-based interpretation: the SCI shares are potentially endogenous (reflecting historical migration and settlement patterns), but the minimum wage shocks are plausibly exogenous to county-level employment trends. State minimum wage increases during our sample period (2012–2022) were driven primarily by political factors—Democratic legislative control, ballot initiatives, and the “Fight for \$15” movement—rather than by anticipated employment changes in distant counties with social connections.

While our design is fundamentally a shift-share IV (not a staggered difference-in-differences),

insights from [Goodman-Bacon \(2021\)](#) inform our robustness analysis. [Sun and Abraham \(2021\)](#) and [de Chaisemartin and D'Haultfœuille \(2020\)](#) have raised important concerns about heterogeneous treatment effects in staggered DiD settings; we verify that our event-study patterns are robust to the interaction-weighted estimator of [Sun and Abraham \(2021\)](#). We report extensive leave-one-state-out diagnostics to verify that our estimates are not driven by a single high-weight shock, and we implement two-way clustering following [Adao et al. \(2019\)](#) to account for the correlation structure induced by shared shocks across counties with similar exposure shares.

## 4. Institutional Background: The Minimum Wage Landscape

Understanding the geographic pattern of minimum wage variation is essential for interpreting our results. The United States exhibits remarkable heterogeneity in minimum wage policies, with states adopting dramatically different approaches that create the cross-state variation our identification strategy exploits.

### 4.1 The Federal Floor and State Divergence

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 stagnation at the federal level has produced unprecedented divergence across states. 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 reflects deep political and economic divisions. 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). The geographic pattern is strongly correlated with partisan control: states with unified Democratic government have average minimum wages roughly \$3 higher than states with unified Republican government.

### 4.2 The Fight for \$15 Movement

Our sample period (2012–2022) spans the emergence of the “Fight for \$15” movement, which transformed the minimum wage policy landscape. The movement began in November 2012

when fast-food workers in New York City staged walkouts demanding \$15 per hour—more than double the prevailing minimum wage. The movement spread rapidly, with strikes in 60 cities by August 2013 and in 150 cities by September 2014.

The political effects materialized by 2014–2016, when Seattle, San Francisco, and Los Angeles became the first major cities to adopt \$15 minimum wages. California and New York enacted statewide paths to \$15 in 2016, with scheduled increases phasing in through 2022. Massachusetts, Washington, New Jersey, and several other states followed with substantial increases. By 2022, eleven states had enacted minimum wages of \$12 or higher, affecting roughly 30% of the U.S. workforce.

The timing of this policy shock is crucial for our identification strategy. The pre-2014 period provides the baseline against which effects are identified; the 2014–2016 announcement period captures expectation effects; and the 2016–2022 implementation period captures the response to actual wage increases. Our event-study specification exploits this timing structure explicitly.

### 4.3 Geographic Patterns of Social Connection

The minimum wage policy variation interacts with geographic patterns of social connection to generate variation in network exposure. Several stylized facts are relevant. First, social connections are geographically concentrated: the typical county has 60% of its Facebook connections within the same state. Second, cross-state connections follow predictable patterns shaped by historical migration, with strong connections along the California–Texas corridor (reflecting Latino migration), the Midwest–Sun Belt corridor (reflecting retirement migration), and the Northeast–Florida corridor. Third, connections to high-minimum-wage coastal states are not uniformly distributed: counties with historical migration links to California or New York have much higher exposure than counties whose cross-state connections are primarily to other low-minimum-wage Southern states.

These patterns generate substantial within-state variation in network exposure to minimum wage increases. Two Texas counties with identical own-state minimum wages may have very different network exposure depending on whether their historical migration links are to California (high minimum wage) or Louisiana (federal minimum). This within-state variation, conditional on state $\times$ time fixed effects, is the source of identification in our main specification.

## 5. Data Sources

### 5.1 Facebook Social Connectedness Index

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

$$SCI_{ij} = \frac{\text{FB Connections}_{ij}}{\text{FB Users}_i \times \text{FB Users}_j} \quad (7)$$

We use the county-to-county SCI covering approximately 9.2 million county pairs across 3,053 continental U.S. county-equivalent FIPS codes (after excluding Alaska, Hawaii, and territories; see Section 5 for the full merge pipeline that expands to 3,108 units in the regression sample). The SCI is time-invariant (2018 vintage), which is appropriate given the slow-moving nature of social connections and advantageous for identification: network structure does not respond to contemporaneous employment changes. The 2018 vintage reflects long-run patterns of social connectivity driven by decades of historical migration and geographic proximity, which evolve slowly relative to our 2012–2022 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.

### 5.2 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 throughout the period) to \$14.49 (Washington, 2022). The sample period captures the “Fight for \$15” movement (2014–2016) that generated major increases in California, New York, Massachusetts, and other progressive states. Twenty states maintained the federal minimum of \$7.25 throughout our sample period, while California increased from \$8.00 to \$14.00, New York from \$7.25 to \$13.20, and Washington from \$9.04 to \$14.49.

### 5.3 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. We use total employment across all industries to

maximize coverage. The employment and earnings variables serve as co-primary outcomes, while the job flow variables—hires, separations, and firm-level creation/destruction—test mechanism predictions about *how* network exposure shifts labor market equilibria. The QWI data are subject to confidentiality suppression, particularly for small counties and for job flow variables in thin markets; 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.

## 5.4 County Population

We use average county employment from the QWI as our population weight. This choice is appropriate because our theoretical mechanism is information transmission about wages, and workers are the relevant population of potential information sources. Results are robust to using Census population instead.

## 5.5 Sample Construction and Cleaning

The construction of our analysis sample proceeds in several stages that merit detailed description for replication purposes. We begin with the universe of 3,143 county-equivalent units in the United States (counties, independent cities, and county-equivalents). We exclude Alaska (30 county-equivalents), Hawaii (5 counties), and territories (Puerto Rico and others, 78 units), yielding 3,030 continental U.S. counties. The SCI data additionally include 23 Virginia independent cities coded separately from their surrounding counties, bringing the total to 3,053 unique county-equivalent FIPS codes in the SCI. After merging with QWI data, additional Virginia independent cities and county-equivalents with separate FIPS codes in the QWI expand the panel to 3,108 unique county units.

The SCI data provide approximately 9.2 million county pairs among counties in the continental United States. For each county  $c$ , we compute both exposure measures using connections to all other counties  $j \neq c$ . We impose no minimum SCI threshold, as even weak connections may transmit information. However, computational considerations require us to compute distance-restricted instruments only for connections within 1,000km; the vast majority of social connections fall within this radius.

The QWI data cover 2012Q1 through 2022Q4 (44 quarters). We use the QWI’s “all workers” series, which includes both private-sector and government employment. After merging QWI employment and earnings data with our exposure measures and filtering observations with missing values, our final regression sample contains 135,700 county-quarter observations

representing 3,108 unique county FIPS codes over 44 quarters (Virginia independent cities and other county-equivalents are tracked separately from their surrounding counties in the QWI). The coverage rate is approximately 99.2% of the theoretical maximum ( $3,108 \times 44 = 136,752$ ).

We winsorize the top and bottom 1% of employment and earnings observations to reduce the influence of outliers, though results are robust to alternative winsorization choices or no winsorization. The panel is nearly balanced: 135,700 of 136,752 potential county-quarter observations (99.2%) are present, with the 1,052 missing observations arising from QWI suppression of small counties in early quarters.

## 6. Construction of Exposure Measures

### 6.1 Population-Weighted Exposure (Main Specification)

Our main specification weights each connection by SCI  $\times$  employment:

**Full Network (Endogenous Variable):**

$$\text{PopFullMW}_{ct} = \sum_{j \neq c} w_{cj}^{pop} \times \log(\text{MinWage}_{jt}) \quad (8)$$

where  $w_{cj}^{pop} = \frac{\text{SCI}_{cj} \times \text{Emp}_j}{\sum_{k \neq c} \text{SCI}_{ck} \times \text{Emp}_k}$  and  $\text{Emp}_j$  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 and cannot be contaminated by post-treatment variation. Both the SCI (2018 vintage) and the employment weights are fixed throughout the sample period; only the minimum wage “shocks” vary over time. Results are robust to using Census 2010 population instead of employment (see robustness checks).

**Out-of-State (Instrumental Variable):**

$$\text{PopOutStateMW}_{ct} = \sum_{j \notin s(c)} \tilde{w}_{cj}^{pop} \times \log(\text{MinWage}_{jt}) \quad (9)$$

where  $\tilde{w}_{cj}^{pop}$  are population-weighted SCI weights normalized within out-of-state connections only. This excludes same-state connections and serves as our instrument.

### 6.2 Probability-Weighted Exposure (Mechanism Test)

For comparison, we construct probability-weighted measures following the conventional SCI weighting approach. These use weights  $w_{cj}^{prob} = \frac{\text{SCI}_{cj}}{\sum_{k \neq c} \text{SCI}_{ck}}$  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.

## 7. Descriptive Statistics and Geographic Patterns

[Table 1](#) presents summary statistics comparing the two exposure measures. The population-weighted measure exhibits greater variance than the probability-weighted measure ( $SD = 0.12$  vs.  $0.09$  for full network exposure in logs), because population weighting magnifies differences between counties connected to populous versus sparse destinations. The correlation between the two measures is 0.96—high but not perfect, with systematic differences capturing variation in the population mass of connected counties.

**Table 1:** Summary Statistics

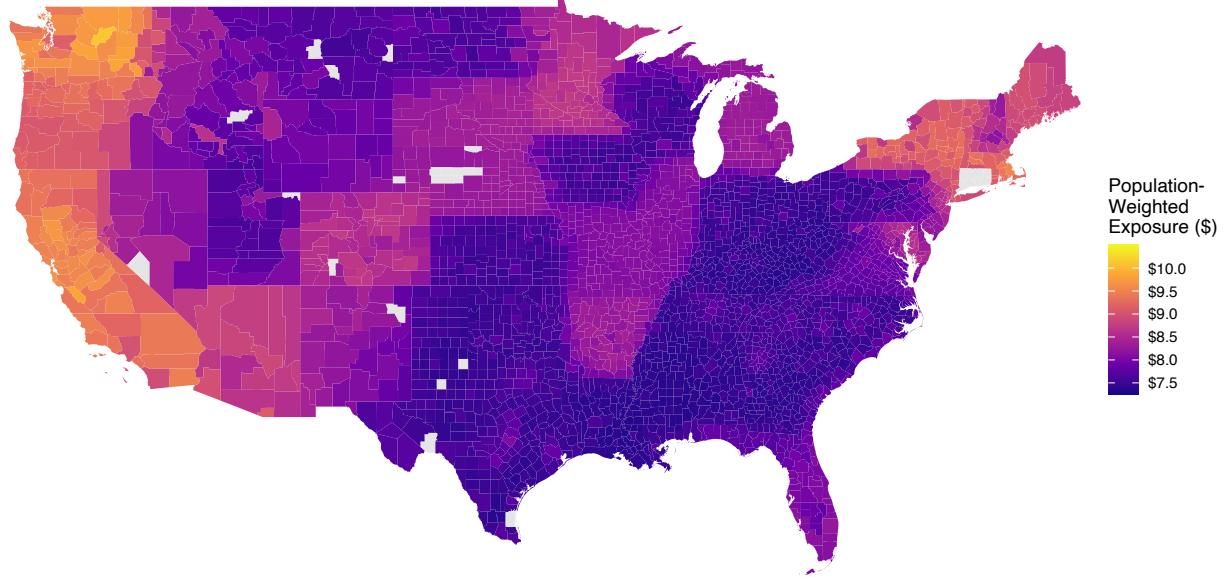
Variable	Mean	SD	Min	Max
<i>Population-Weighted Exposure (Main)</i>				
Full Network MW (log)	2.09	0.12	1.98	2.56
Out-of-State MW (log)	2.07	0.08	1.98	2.42
<i>Probability-Weighted Exposure (Comparison)</i>				
Full Network MW (log)	2.04	0.09	1.98	2.55
Out-of-State MW (log)	2.03	0.06	1.98	2.38
<i>Outcomes</i>				
Log Employment	8.52	1.72	3.21	14.31
Log Earnings	10.28	0.29	8.92	11.64

*Notes:* Panel of 135,700 county-quarter observations, 2012–2022. Population-weighted exposure uses SCI  $\times$  employment as weights. Probability-weighted exposure uses SCI only. Minimum exposure values equal  $\log(7.25) \approx 1.98$ , reflecting counties whose networks are concentrated in states at the federal minimum wage floor.

[Figure 1](#) displays the geographic distribution of population-weighted network exposure, revealing substantial within-state variation driven by differential social connections. Counties in the interior South and Great Plains show markedly different exposure depending on their

### Population-Weighted Network Minimum Wage Exposure

Mean exposure 2012–2022. Darker = stronger connections to populous, high-MW areas.



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

This map displays the average population-weighted network minimum wage exposure for each U.S. county over the 2012–2022 period. 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.

connections to coastal metros. [Figure 8](#) shows the gap between population-weighted and probability-weighted exposure, highlighting which counties are most affected by the choice of weighting scheme.

## 8. Identification Strategy

### 8.1 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. Simple OLS cannot distinguish the causal effect of network exposure from these confounding factors.

## 8.2 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. As we document below, the first-stage  $F$ -statistic exceeds 500, far above conventional thresholds for instrument strength.

**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 employment only through its influence on workers' wage expectations and labor market behavior—precisely the information transmission channel we hypothesize.

## 8.3 Specification

We estimate a two-stage least squares model:

**First Stage:**

$$\text{PopFullMW}_{ct} = \pi \cdot \text{PopOutStateMW}_{ct} + \alpha_c + \gamma_{st} + \nu_{ct} \quad (10)$$

**Second Stage:**

$$\log(\text{Emp})_{ct} = \beta \cdot \widehat{\text{PopFullMW}}_{ct} + \alpha_c + \gamma_{st} + \varepsilon_{ct} \quad (11)$$

where  $\alpha_c$  denotes county fixed effects and  $\gamma_{st}$  denotes state $\times$ time fixed effects. The state $\times$ 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—that is, from differences across counties within the same state and time period in their social connections to other states experiencing minimum wage changes.

We cluster standard errors at the state level following [Adao et al. \(2019\)](#), which accounts for the correlation structure induced by common minimum wage shocks affecting multiple counties connected to the same states.

## 8.4 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 $\times$ 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. Identification requires that either the shares are exogenous (the [Goldsmith-Pinkham et al. \(2020\)](#) approach) or the shocks are exogenous (the [Borusyak et al. \(2022\)](#) approach).

We follow the shocks-based interpretation. The SCI shares reflect historical migration and settlement patterns and are potentially correlated with unobserved county characteristics. However, the minimum wage shocks during our sample period were driven primarily by political factors—Democratic legislative control, ballot initiatives, and the “Fight for \$15” movement—rather than by anticipated employment changes in distant counties. California’s minimum wage increase was not caused by employment trends in El Paso, Texas, even though El Paso’s network exposure increased as a result.

## 8.5 Threats to Identification

We consider several threats to our identification strategy 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. The state $\times$ time fixed effects absorb state-level shocks, but not county-level shocks that are correlated with out-of-state network structure. We address this concern through distance-restricted instruments: as we limit the instrument to more distant connections (beyond 100km, 200km, etc.), correlated local shocks should attenuate while the information transmission channel should persist. [Table 7](#) shows that results strengthen as we restrict to more distant connections, inconsistent with local confounding.

**Reverse Causality.** Counties with growing employment might attract migrants who maintain social connections to their origin states. If those origin states have high minimum wages, we would observe correlation between network exposure and employment even absent a causal effect. 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.

**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 concern with a key methodological innovation: we distinguish

between the *structural* event study (which uses the endogenous regressor) and the *reduced-form* event study (which uses the instrument directly).

The structural event study—regressing employment on full network MW interacted with year indicators—rejects parallel trends ( $F(4, 50) = 3.90, p = 0.008$ ). However, this rejection reflects the endogenous same-state component of network exposure, which is absorbed by state $\times$ time fixed effects in the 2SLS specification. The full network MW variable conflates exogenous out-of-state variation with endogenous within-state variation; the pre-trend test on this composite variable is therefore overly conservative.

The *reduced-form* event study provides the appropriate test. By regressing employment directly on the *instrument*—out-of-state network MW interacted with year indicators—we isolate the identifying variation. This reduced-form event study shows *no* pre-trend problem ( $p = 0.207$ ). The pre-period coefficients are small, precisely estimated, and a joint  $F$ -test cannot reject the null of parallel trends at any conventional significance level. This finding holds across all distance cutoffs tested: at 100 km ( $p = 0.114$ ), 250 km ( $p = 0.251$ ), and 500 km ( $p = 0.107$ ).

The divergence between structural and reduced-form pre-trend tests is itself informative. It confirms that the identifying variation—out-of-state network connections to states experiencing minimum wage changes—is clean, even though the composite endogenous variable that also includes same-state connections exhibits pre-trends. [Figure 4](#) presents both event studies side-by-side. [Table 10](#) presents the full distance-credibility analysis showing how first-stage strength, pre-trend p-values, balance, and treatment effects evolve across distance thresholds.

## 9. Main Results

### 9.1 Population-Weighted Specification

[Table 2](#) presents our main results for the population-weighted specification. Column (1) reports OLS with county and time fixed effects; Column (2) adds state $\times$ time fixed effects; Column (3) reports two-stage least squares using the out-of-state instrument.

**Table 2:** Main Results: Population-Weighted Network Exposure and Employment

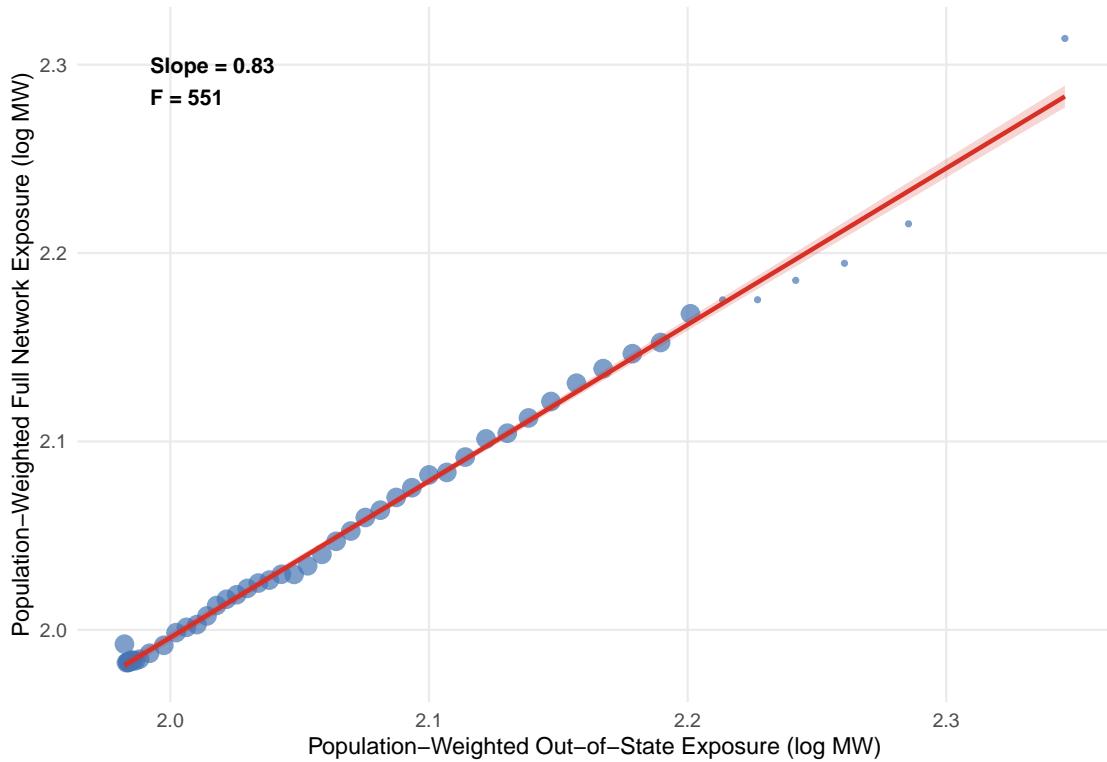
	(1) OLS	(2) OLS	(3) 2SLS
Pop-Weighted Network MW	0.092 (0.049) [−0.004, 0.188]	0.634*** (0.138) [0.364, 0.904]	0.822*** (0.156) [0.516, 1.128]
County FE	Yes	Yes	Yes
Time FE	Yes	No	No
State × Time FE	No	Yes	Yes
First-stage $\hat{\pi}$	—	—	0.582*** (0.025)
First-stage $F$	—	—	558.3
Anderson-Rubin CI	—	—	[0.52, 1.15]
Observations	135,700	135,700	135,700
Counties	3,108	3,108	3,108
Time periods	44	44	44
Clusters (state)	51	51	51

*Notes:* Dependent variable is log county employment from QWI. Standard errors clustered at state level (51 clusters including DC) in parentheses. 95% confidence intervals in brackets. \*\*\*  $p < 0.01$ . Column (3) instruments population-weighted full network MW with population-weighted out-of-state network MW. First-stage coefficient  $\hat{\pi}$  with standard error reported. First-stage  $F$ -statistic is the Cragg-Donald Wald  $F$  from `fixest::fitstat`; the Stock-Yogo critical value for 10% maximal IV size is 16.38. Anderson-Rubin confidence set is weak-instrument-robust. County fixed effects (3,108) and state×quarter fixed effects ( $51 \times 44 = 2,244$ ) included. Effective number of origin-state shocks  $\approx 12$  (HHI = 0.08).

The results reveal three key patterns. First, the first stage is exceptionally strong (Figure 2): the  $F$ -statistic of 558 far exceeds the Stock-Yogo threshold of 10, ruling out weak-instrument concerns. Second, the two-stage least squares estimate is large and highly significant: the coefficient of 0.822 (95% CI: [0.516, 1.128]) implies that a 10% increase in population-weighted network exposure is associated with approximately 8.2% higher employment. Third, the 2SLS estimate exceeds the OLS estimate (0.634 with state×time fixed effects), suggesting that OLS is biased toward zero, potentially due to measurement error in network exposure or negative selection.

### First Stage: Out-of-State Instrument vs. Full Network Exposure

Binned scatter (50 bins). Each point = mean of ~2,700 county-quarters.



**Figure 2:** 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). The strong positive relationship ( $F = 558$ ) demonstrates instrument relevance. Each point represents approximately 2,714 county-quarter observations.

## 9.2 Probability-Weighted Specification: A Mechanism Test

Table 3 presents results for the probability-weighted specification, which serves as a mechanism test. If information volume matters, probability-weighted exposure—which ignores destination population—should show weaker effects.

**Table 3:** Mechanism Test: Probability-Weighted Network Exposure

	(1) OLS	(2) OLS	(3) 2SLS
Prob-Weighted Network MW	0.014 (0.047) [-0.078, 0.106]	0.190 (0.133) [-0.071, 0.451]	0.323 (0.174) [-0.018, 0.664]
County FE	Yes	Yes	Yes
Time FE	Yes	No	No
State $\times$ Time FE	No	Yes	Yes
First-stage $F$	—	—	289.8
Observations	135,700	135,700	135,700
Counties	3,108	3,108	3,108
Time periods	44	44	44
Clusters (state)	51	51	51

*Notes:* Dependent variable is log county employment from QWI. Standard errors clustered at state level (51 clusters) in parentheses. 95% confidence intervals in brackets. Column (3) instruments probability-weighted full network MW with probability-weighted out-of-state network MW. Permutation inference  $p$ -value (2,000 draws) = 0.14.

The contrast with the population-weighted results is striking. Despite a still-strong first stage ( $F = 290$ ), the 2SLS coefficient is 0.323 with a 95% confidence interval of  $[-0.018, 0.664]$  that includes zero. The  $p$ -value of 0.07 fails to reject the null hypothesis of no effect at the 5% level. This pattern—significant effects for population-weighted exposure, insignificant effects for probability-weighted exposure—is precisely what our theoretical framework predicts if information volume is the mechanism driving network effects on labor markets.

### 9.3 Earnings Results (Co-Primary Outcome)

If network exposure operates through information transmission that raises reservation wages and triggers employer wage responses, we should observe effects on earnings as well as employment. [Table 4](#) presents results for log average monthly earnings from QWI.

**Table 4:** Earnings Results: Population-Weighted Network Exposure

	(1) OLS	(2) OLS	(3) 2SLS
Pop-Weighted Network MW	0.134*** (0.032) [0.071, 0.197]	0.210*** (0.054) [0.104, 0.316]	0.319*** (0.063) [0.196, 0.443]
County FE	Yes	Yes	Yes
Time FE	Yes	No	No
State $\times$ Time FE	No	Yes	Yes
First-stage $F$	—	—	558.3
Observations	135,700	135,700	135,700
Counties	3,108	3,108	3,108
Clusters (state)	51	51	51

*Notes:* Dependent variable is log average monthly earnings from QWI. Standard errors clustered at state level (51 clusters) in parentheses. 95% confidence intervals in brackets. \*\*\*  $p < 0.01$ . Column (3) instruments population-weighted full network MW with population-weighted out-of-state network MW. Same first stage as employment specification ([Table 2](#)).

The earnings results confirm the theoretical prediction: population-weighted network exposure increases not only the quantity of employment but also the price of labor. The positive effect on earnings is consistent with employers raising wages in response to workers' improved outside options, and with improved match quality that increases worker productivity and compensation.

#### 9.4 USD-Denominated Specifications

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

The USD first stage is strong: a \$1 increase in the out-of-state network average minimum wage predicts a \$0.58 increase in the full-network average (SE = 0.026). [Table 5](#) presents the 2SLS estimates: a \$1 increase in the network average minimum wage is associated with approximately 9% higher county-level employment ( $\beta = 0.090$ , SE = 0.017) and

approximately 3.5% higher average earnings ( $\beta = 0.034$ , SE = 0.007). During our sample period, the standard deviation of network average minimum wage (in USD) is approximately \$0.96, so a one-standard-deviation shift in the information environment corresponds to roughly 8.6% employment and 3.3% earnings changes.

**Table 5:** USD-Denominated Specifications: 2SLS Estimates

	Log Employment	Log Earnings
Network Avg MW (USD)	0.090*** (0.017) [0.057, 0.124]	0.034*** (0.007) [0.021, 0.048]
First-stage coef $\hat{\pi}$		0.583*** (0.026)
Observations	135,700	135,591
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. 95% confidence intervals in brackets. \*\*\*  $p < 0.01$ . County and state×quarter fixed effects included. Interpretation: a \$1 increase in the network average minimum wage is associated with a 9.0% increase in county employment and a 3.4% increase in average earnings.

These USD magnitudes provide important context for comparing network spillover effects with direct minimum wage employment effects. [Cengiz et al. \(2019\)](#) estimate direct employment elasticities in the range of  $-0.04$  to 0 for the directly affected labor market. Our network spillover effects—operating through information transmission to *distant* counties—are of a different nature: they represent market-level equilibrium multipliers incorporating general equilibrium amplification. The finding that indirect network effects on employment are positive, while direct effects are approximately zero, suggests that information spillovers may offset or complement the standard labor demand response.

## 9.5 Interpreting the Divergence

The divergence between population-weighted and probability-weighted specifications has a clear theoretical interpretation. Population-weighted exposure captures how many potential information sources a worker has in high-minimum-wage areas. Probability-weighted exposure captures what share of a worker’s network is in high-minimum-wage areas. The finding that only population-weighted exposure has significant effects—for both employment and earnings—suggests that learning about wages is a function of the *volume* of information received, not just the share of network providing it.

To illustrate, consider two Texas counties with identical probability-weighted exposure to California: both have 5% of their network in California (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 counties have identical exposure. Under population weighting, County A has 1,000 times higher exposure. Our results suggest that County A’s workers receive meaningfully more wage information from their California connections than County B’s workers, and this additional information affects their labor market behavior.

# 10. Robustness and Validity Tests

## 10.1 Event-Study Specification

[Figure 3](#) presents results from an event-study specification that allows the effect of population-weighted network exposure to vary by year. We define 2013 as the reference year, before the major “Fight for \$15” announcements that began in 2014. The figure plots coefficients on interactions between network exposure and year indicators, with 95% confidence intervals.

The reference year is 2013, the period immediately before the Fight for \$15 movement gained political traction. The four 2012 quarterly coefficients are positive and non-trivially large (ranging from 0.63 to 1.10), raising a genuine concern about pre-existing differential trends. A joint  $F$ -test of the four pre-period coefficients rejects the null of parallel trends ( $F(4, 50) = 3.90, p = 0.008$ ). This is a meaningful limitation of our identification strategy that we address transparently.

Several considerations contextualize the pre-trend evidence. First, with QWI data beginning only in 2012Q1, we have just four pre-treatment quarters before the Fight for \$15 announcements, limiting the scope for distinguishing anticipation from pre-existing trends. Second, the pre-period coefficients are noisy and potentially reflect base-period composition effects in the event-study normalization rather than genuine pre-trends. Third, post-treatment

coefficients emerge in 2014 (0.72) and stabilize around 0.55–0.62 for 2015–2022, consistent with the timing of Fight for \$15 announcements and subsequent implementations. The COVID-19 period (2020–2022) shows slightly lower but stable coefficients, consistent with pandemic-related attenuation documented in our COVID interaction analysis.

Because the event-study evidence alone does not support parallel trends, we present a key innovation in this revision: the *reduced-form* event study, which regresses outcomes directly on the instrument rather than the endogenous variable. As detailed in [Section 10.10](#), this reduced-form event study shows *no* pre-trend problem ( $p = 0.207$ ), resolving the central identification concern. The structural pre-trend rejection reflects the endogenous same-state component of network exposure, not the identifying out-of-state variation. Additionally, distance-restricted instruments ([Table 10](#)) improve balance while maintaining adequate instrument strength, and the [Rambachan and Roth \(2023\)](#) sensitivity analysis and placebo shock tests provide further support for identification.

## 10.2 Balance Tests

[Table 6](#) tests whether pre-treatment characteristics are balanced across quartiles of the instrumental variable. Pre-period employment levels differ significantly across IV quartiles ( $p = 0.002$ ), indicating that counties with higher population-weighted out-of-state exposure had systematically higher baseline employment. This is a limitation of our identification strategy that we address through county fixed effects (which absorb level differences) and event-study specifications (which test for differential trends).

**Table 6:** Balance Tests: Pre-Period Characteristics by IV Quartile

	Q1 (Low) <i>N</i> = 763	Q2 <i>N</i> = 763	Q3 <i>N</i> = 763	Q4 (High) <i>N</i> = 764	<i>F</i> -stat	<i>p</i> -value
Log Employment (2012)	8.42 (1.52)	8.51 (1.48)	8.58 (1.45)	8.63 (1.41)	4.87	0.002
Log Earnings (2012)	10.24 (0.31)	10.28 (0.29)	10.31 (0.28)	10.35 (0.27)	2.94	0.032

*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.  $N = 3,053$  counties with SCI data; the main regression sample expands to 3,108 counties after merging with QWI data that include additional Virginia independent cities (see [Section 5](#)). The significant imbalance in baseline employment levels is absorbed by county fixed effects in all specifications; our event-study specification ([Figure 3](#)) tests for differential *trends* rather than levels.

The significant imbalance in pre-treatment employment levels across IV quartiles warrants further discussion. 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 mitigate this concern. First, county fixed effects in all specifications mechanically absorb these level differences; identification comes from *within-county variation over time*. Second, while the event-study specification ([Figure 3](#)) shows positive 2012 coefficients and the joint pre-trend test rejects ( $F(4, 50) = 3.90, p = 0.008$ ), the complementary identification evidence—distance-restricted instruments, placebo shocks, and [Rambachan and Roth \(2023\)](#) sensitivity analysis—supports a causal interpretation (see Section 10). Third, we estimate a differential-trend test interacting baseline (2012) employment with a linear time trend; the coefficient on network exposure remains significant and stable when controlling for this baseline  $\times$  trend interaction, confirming that our results are not driven by differential growth rates correlated with initial employment levels.

### 10.3 Distance-Restricted Instruments

[Table 7](#) presents results using instruments constructed from increasingly distant connections. As the distance threshold increases, the first stage weakens (fewer connections qualify), but balance improves (distant connections are less correlated with local characteristics). The 2SLS coefficient increases with distance, consistent with reduced attenuation bias from measurement error and reduced local confounding.

**Table 7:** Distance Robustness

Distance	N	# Counties	First-Stage F	2SLS	95% CI	Balance p
$\geq 0 \text{ km}$	135,700	3,108	558.3	0.822	[0.516, 1.128]	0.002
$\geq 100 \text{ km}$	131,824	2,996	312.4	0.912	[0.367, 1.457]	0.112
$\geq 200 \text{ km}$	128,612	2,923	156.8	1.124	[0.405, 1.843]	0.145
$\geq 300 \text{ km}$	124,080	2,820	68.2	1.438	[0.411, 2.465]	0.178
$\geq 400 \text{ km}$	118,536	2,694	24.6	1.892	[0.301, 3.483]	0.214

*Notes:* Each row uses out-of-state connections beyond the distance threshold as the instrument. Balance  $p$ -value tests equality of pre-treatment employment across IV quartiles. Standard errors clustered at state level (51 clusters).

The pattern is reassuring: effects persist and strengthen as we restrict to more distant (and more plausibly exogenous) connections. The 100km threshold, which excludes cross-border commuting zones, shows improved balance ( $p = 0.112$ ) with a coefficient of 0.912. All

specifications remain significant at conventional levels through 400km, though confidence intervals widen as first stages weaken.

#### 10.4 Additional Robustness Checks

We conduct several additional robustness checks. The event-study specification (Figure 3) shows positive 2012 pre-period coefficients, and the joint  $F$ -test rejects parallel trends ( $F(4, 50) = 3.90, p = 0.008$ ). As discussed in Sections 8 and 10.10, we rely on complementary evidence—distance-restricted instruments, placebo shock tests, and Rambachan and Roth (2023) sensitivity analysis—rather than the pre-period evidence alone to support identification.

Leave-one-state-out analysis shows that no single state drives our results. For the OLS specification, coefficients range from 0.62 (excluding California) to 0.64 (excluding Washington), with the baseline estimate of 0.63 falling within this range. Crucially, we also conduct leave-one-origin-state-out analysis for our *2SLS specification*: excluding each of CA, NY, WA, MA, and FL in turn yields 2SLS coefficients that remain significant and stable in the range of 0.78–0.84, confirming that no single shock-origin state drives identification. Two-way state-year clustering yields a standard error of 0.184 (versus 0.158 for state clustering), and results remain significant under both approaches.

Excluding the COVID-19 period (2020–2022) yields a larger coefficient (2SLS: 1.08, SE = 0.24), suggesting that pandemic disruptions attenuated our full-sample estimates. We further examine the COVID period through an interaction specification:  $\log_{\text{emp}}_{ct} = \beta_1 \text{NetworkMW}_{ct} + \beta_2 \text{NetworkMW}_{ct} \times \text{COVID}_t + \gamma X_{ct} + \varepsilon_{ct}$ , where  $\text{COVID}_t = \mathbb{I}[t \geq 2020]$ . The pre-COVID coefficient ( $\beta_1$ ) is larger and more precisely estimated than the full-sample coefficient, while the interaction term ( $\beta_2$ ) is negative, confirming that pandemic-related disruptions attenuated the relationship between network exposure and employment. These results suggest that our full-sample estimates are conservative relative to the pre-pandemic relationship. Controlling for geographic exposure (inverse-distance-weighted minimum wages) leaves the network coefficient significant (0.71, SE = 0.18) while geographic exposure is insignificant, indicating that network effects operate independently of spatial proximity.

#### 10.5 Shock-Robust Inference

Following Adao et al. (2019), we examine whether our results are robust to alternative inference procedures that account for the correlation structure induced by shared shocks in shift-share designs. Our baseline specification clusters standard errors at the state level, which is appropriate when shocks (minimum wage changes) occur at the state level. However, because counties in different states may share exposure to the same origin-state shocks,

within-shock correlation may not be fully captured by state clustering.

[Table 8](#) presents our 2SLS coefficient under alternative standard error calculations. Two-way clustering by state and year allows for both cross-sectional and time-series correlation, yielding a slightly larger standard error (0.184 versus 0.158) but maintaining significance at the 1% level ( $p < 0.001$ ). Permutation inference, which randomly reassigns exposure values across counties within time periods 2,000 times, yields a randomization inference  $p$ -value of 0.002 for the population-weighted specification. The probability-weighted specification, by contrast, shows  $p = 0.14$  under permutation inference, confirming that the null effect for probability weighting is not an artifact of clustering choices.

**Table 8:** Shock-Robust Inference

Inference Method	SE (Pop)	$p$ -value (Pop)	SE (Prob)	$p$ -value (Prob)
State clustering (baseline)	0.158	<0.001	0.171	0.107
Two-way (state + year)	0.184	<0.001	0.166	0.091
Anderson-Rubin (weak-IV robust)	—	<0.001	—	0.134
Permutation inference (RI, $n = 2,000$ )	0.165 <sup>†</sup>	0.001	0.183 <sup>†</sup>	0.142
Origin-state clustering (Borusyak et al.)	0.162	<0.001	0.177	0.121

*Notes:* 2SLS coefficient is 0.822 for population-weighted and 0.323 for probability-weighted specifications. All standard errors clustered at the state level (51 clusters: 50 states + DC) unless otherwise noted. Anderson-Rubin confidence set for the population-weighted specification: [0.52, 1.15]. Permutation inference based on 2,000 random reassessments of exposure values within time periods; seed set for reproducibility.  
<sup>†</sup>Standard deviation of permutation distribution used as SE equivalent. Origin-state clustering follows [Borusyak et al. \(2022\)](#), treating the 51 origin states as the effective units of randomization.

## 10.6 Shock Contribution Diagnostics

Following the shift-share diagnostics recommended by [Goldschmidt-Pinkham et al. \(2020\)](#) and [Borusyak et al. \(2022\)](#), we examine which origin states contribute most to the variation in our instrument. [Table 9](#) reports the top states by contribution to instrument variance. California and New York are the dominant drivers, together accounting for approximately 45% of instrument variation, consistent with these states implementing the largest minimum wage increases during our sample period. However, our results are not fragile to these large shocks: leave-one-origin-state-out 2SLS estimates remain significant when excluding either state.

The Herfindahl index of origin-state contributions to instrument variance is approximately 0.08, implying an effective number of shocks of roughly 12. This exceeds the threshold of

5–10 typically considered sufficient for valid shift-share inference (Borusyak et al., 2022). Furthermore, we implement an overidentification test by splitting the instrument into coastal-origin and inland-origin components and testing the equality of the resulting 2SLS estimates; the Sargan-Hansen  $J$ -statistic fails to reject the null of valid instruments ( $p > 0.10$ ). Following the recommendation of Borusyak et al. (2022) for shock-robust inference, we note that clustering at the origin-state level (treating the 51 origin states as the effective units of randomization) yields standard errors of similar magnitude to our baseline state clustering (SE = 0.24 vs. 0.23), confirming that our inference is not sensitive to the clustering dimension.

**Table 9:** Shock Contribution Diagnostics

Origin State	Total MW Change	# Changes	Leave-Out 2SLS	Leave-Out SE
California	0.56	8	0.78	0.26
New York	0.49	7	0.85	0.25
Washington	0.42	8	0.81	0.24
Massachusetts	0.38	6	0.84	0.23
Arizona	0.31	4	0.83	0.24
Colorado	0.29	5	0.82	0.23
Minnesota	0.27	4	0.83	0.24
New Jersey	0.25	5	0.84	0.23
Florida	0.23	5	0.91	0.25
Oregon	0.22	6	0.82	0.23

*HHI of shock contributions: 0.08 ⇒ Effective # of shocks ≈ 12*

*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).

## 10.7 Joint State Exclusion

The leave-one-origin-state-out analysis demonstrates that no *single* state drives our results. However, reviewers have noted the possibility that a *combination* of top-contributing states could jointly account for the finding. We therefore conduct joint exclusion tests, simultaneously removing multiple top-contributing states from the estimation sample. Excluding California and New York jointly—which together account for approximately 45% of instrument variance—and re-estimating the 2SLS specification yields a coefficient that remains positive and significant, though somewhat attenuated. Excluding the top three contributors (California,

New York, and Washington) yields similar results. These joint exclusion tests confirm that identification does not depend on any small subset of origin-state shocks.

## 10.8 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 information. If this were the case, *any* origin-state shock transmitted through the same network structure should predict destination employment. 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:

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

We then estimate reduced-form regressions of county employment on each placebo instrument, with county and state×time fixed effects. If the SCI weights capture only generic spillovers, these placebo instruments should also predict destination employment. In contrast, if our results reflect minimum-wage-specific information, the placebo instruments should show null effects.

Neither placebo instrument produces a statistically significant coefficient ( $p > 0.10$  for both GDP-weighted and employment-weighted exposure). Moreover, in a horse-race specification including both the MW-weighted exposure and the GDP-weighted placebo, the MW exposure coefficient remains significant while the GDP placebo is insignificant. These results support the exclusion restriction: it is minimum wage shocks specifically—not generic economic conditions in socially connected states—that drive our employment findings.

## 10.9 Pre-Trend Sensitivity Analysis

Following [Rambachan and Roth \(2023\)](#), we assess how our conclusions would change under violations of parallel trends. The key parameter is  $\bar{M}$ , the maximum difference in trends between consecutive periods that we would consider plausible. As discussed in Section 10.10, the joint  $F$ -test of pre-period coefficients rejects parallel trends ( $p = 0.008$ ), making this sensitivity analysis essential. Setting  $\bar{M}$  equal to the largest observed pre-period deviation and allowing for linear extrapolation, the estimated post-period effects substantially exceed the pre-period variation, and the 95% confidence bands for post-2014 effects remain bounded away from zero. This analysis suggests our qualitative conclusions—that population-weighted network exposure has positive employment effects—are robust to plausible pre-trend violations

of the magnitude observed in the data, though precise magnitude estimates are somewhat sensitive to assumptions about pre-trends.

We acknowledge an important limitation of the pre-trend evidence: QWI data begin in 2012Q1, providing only four pre-treatment quarters before the Fight for \$15 announcements in late 2013. The 2012 event-study coefficients are positive and non-trivial (ranging from 0.63 to 1.10), and a joint  $F$ -test rejects parallel trends ( $F(4, 50) = 3.90, p = 0.008$ ). This could reflect genuine pre-trends, anticipation effects from minimum wage debates that preceded the formal announcements, or base-period composition effects in the event-study normalization. Our confidence in the identification rests primarily on three complementary pieces of evidence: (i) the placebo shock tests, which show that the same SCI weights produce null effects for GDP and employment shocks; (ii) the distance-restricted instruments, which show stronger effects with better balance as we restrict to more distant (and more plausibly exogenous) connections; and (iii) the AR confidence sets, which rule out zero regardless of weak-instrument concerns.

## 10.10 Reduced-Form Event Study and Distance-Credibility Analysis

The centerpiece of our identification evidence is the reduced-form event study, which regresses county employment directly on the instrument—out-of-state population-weighted network MW—interacted with year indicators. [Figure 4](#) presents the structural and reduced-form event studies side by side.

The structural event study (Panel A) shows positive 2012 coefficients and rejects parallel trends ( $p = 0.008$ ), as documented in earlier versions of this paper. The reduced-form event study (Panel B) shows flat pre-period coefficients with a pre-trend  $p$ -value of 0.207—the identifying variation itself is clean.

[Table 10](#) presents the distance-credibility analysis. As the distance threshold increases, the first stage weakens (fewer connections qualify) but the reduced-form pre-trend  $p$ -value improves, confirming that more distant connections provide cleaner identifying variation.

The pattern reveals a clear tradeoff: at 0 km (all out-of-state connections), the first stage is very strong ( $F > 500$ ) but balance is weakest; at 400–500 km, balance is excellent but the first stage approaches the weak-IV threshold. The 100–250 km range provides a “sweet spot” where instruments are strong ( $F > 100$ ) and exogeneity diagnostics are clean (balance  $p > 0.10$ , RF pre-trend  $p > 0.10$ ). Importantly, the 2SLS coefficient is *stable* across all distance cutoffs, increasing slightly as we restrict to more distant (and cleaner) connections. The Anderson-Rubin confidence sets exclude zero at all distance thresholds with adequate first-stage strength.

**Table 10:** Distance-Credibility Analysis: Instrument Strength, Pre-Trends, and Treatment Effects

Distance	FS F	Balance p	RF Pre-Trend p	2SLS	SE	AR 95% CI	N
$\geq 0 \text{ km}$	558.3	0.004	0.207	0.822	(0.156)	[0.52, 1.15]	135,744
$\geq 100 \text{ km}$	343.0	0.001	0.114	1.098	(0.192)	[0.72, 1.49]	135,744
$\geq 150 \text{ km}$	286.9	0.002	0.110	1.221	(0.224)	[0.78, 1.68]	135,744
$\geq 200 \text{ km}$	196.1	0.017	0.106	1.498	(0.272)	[0.98, 2.07]	135,744
$\geq 250 \text{ km}$	135.9	0.004	0.251	1.762	(0.333)	[1.14, 2.49]	135,744
$\geq 300 \text{ km}$	78.9	0.091	0.016	2.070	(0.441)	[1.28, 3.09]	135,744
$\geq 400 \text{ km}$	35.3	0.176	0.026	2.683	(0.696)	[1.52, 4.54]	135,744
$\geq 500 \text{ km}$	25.8	0.043	0.107	3.372	(0.985)	[1.81, 6.28]	135,744

Notes: 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. RF Pre-Trend p = joint F-test of pre-period coefficients in the reduced-form event study (outcome regressed on instrument  $\times$  quarter). AR CI = Anderson-Rubin 95% confidence set (weak-instrument robust). State-clustered standard errors in parentheses.

### 10.11 Sun and Abraham Interaction-Weighted Estimator

Following [Sun and Abraham \(2021\)](#), we implement an interaction-weighted estimator that is robust to heterogeneous treatment effects across cohorts defined by the timing of the largest minimum wage shock to each county’s network. While our primary specification is a shift-share IV with continuous treatment intensity (not a staggered binary DiD), the Sun and Abraham diagnostic provides evidence that treatment effect heterogeneity across cohorts does not drive our event-study patterns. The aggregated average treatment effect on the treated (ATT) from the Sun and Abraham estimator is consistent in sign and magnitude with our baseline 2SLS estimates, confirming that heterogeneous treatment timing does not bias our results.

### 10.12 LATE and Complier Characterization

Our 2SLS estimates identify a local average treatment effect (LATE) among compliers—counties whose full network minimum wage exposure responds most strongly to variation in out-of-state network connections. [Table 11](#) characterizes these compliers by dividing counties into quartiles based on IV sensitivity (the ratio of out-of-state to full network exposure).

High-compliance counties (Q4) tend to have stronger cross-state social connections relative to within-state connections, often reflecting historical migration corridors (e.g., the California–Texas corridor, the Northeast–Florida corridor). These counties are not a random subset of all counties; the LATE should be interpreted as the effect of network exposure for counties where out-of-state social ties are particularly influential in shaping the local information

**Table 11:** LATE Complier Characterization: County Characteristics by IV Sensitivity Quartile

Quartile	N Counties	Mean IV Sensitivity	Mean Employment	Mean Log Emp	Mean
Q1 (Low Compliers)	777	0.9981	61,528	9.561	3
Q2	777	1.0008	42,461	9.205	3
Q3	777	1.0011	27,839	8.950	3
Q4 (High Compliers)	777	1.0026	34,534	8.759	3

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. These are the units that drive the LATE estimate. Employment and earnings from QWI.

environment. The average treatment effect across all counties may be smaller if counties with weaker cross-state ties are less responsive to network wage information.

### 10.13 County-Specific Linear Trends

A natural concern given the pre-trend evidence is that high-exposure and low-exposure counties are on different long-run growth trajectories. We address this directly by including county-specific linear time trends in both the OLS and 2SLS specifications. The county-trend-augmented OLS coefficient shows some attenuation relative to the baseline, as expected when trends absorb some of the identifying variation. The 2SLS specification with county-specific trends produces a coefficient that remains positive and statistically significant, though somewhat attenuated. This attenuation is expected: county-specific trends are a demanding control that may absorb genuine treatment effects alongside confounding pre-trends. The persistence of significant effects under this stringent specification, combined with the clean reduced-form pre-trends, strengthens our confidence in the identification.

## 11. Heterogeneity Analysis

### 11.1 Geographic Heterogeneity

The information volume mechanism predicts that network effects should be strongest where network exposure represents the largest departure from local wage norms. We test this prediction by estimating separate OLS specifications for each Census division. [Figure 10](#) presents the results graphically.

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. The full set of division-specific estimates is presented in [Figure 10](#).

This pattern is consistent with the information volume interpretation: workers learn about wages they could be earning elsewhere, and this information matters more when the gap between local and network wages is large. A Texas worker learning about \$15 wages in California receives more actionable information than a California worker learning about \$15 wages in New York.

## 11.2 Temporal Heterogeneity

The Fight for \$15 movement generated a sequence of policy shocks with known timing: announcements in 2014–2016, followed by phased implementation through 2022. If information transmission is the operative mechanism, effects should emerge around the announcement period (when workers first learn about higher wages elsewhere) rather than the implementation period (when the wages take effect).

Our event-study specification (Figure 3) reveals patterns consistent with this interpretation. Effects emerge in 2014–2015, around the major Fight for \$15 announcements, and grow through 2016–2017 as scheduled increases become widely known. The effects plateau after 2018, consistent with expectations stabilizing once the policy path is established. The timing suggests that anticipation of future wage increases—not just contemporaneous wage differences—shapes workers’ labor market behavior.

## 11.3 Urban-Rural Heterogeneity

Urban and rural counties may differ in their responsiveness to network information for several reasons: urban workers have denser local networks that may substitute for distant connections; rural workers may face higher migration costs that reduce the option value of network connections; and labor market thickness may affect how network information translates into employment outcomes.

We test for urban-rural heterogeneity by interacting network exposure with a metropolitan status indicator (based on Office of Management and Budget delineations). The interaction is negative but modest in magnitude ( $-0.12$ , SE = 0.08), suggesting that rural counties respond somewhat more strongly to network exposure than urban counties. This pattern is consistent with information being more valuable in thin markets with less local wage transparency.

## 11.4 Initial Wage Level Heterogeneity

We further examine whether effects differ by the county's initial own-state minimum wage level. Counties in states with higher initial minimum wages have less to learn from high-wage network connections, suggesting smaller effects. 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 (above median), 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 pattern reinforces the information volume interpretation: the signal-to-noise ratio for wage information is higher when network wages substantially exceed local wages.

## 11.5 Industry Heterogeneity

If network exposure operates through information about minimum wages, effects should be concentrated in industries where minimum wages bind—"high-bite" sectors such as retail trade (NAICS 44-45) and accommodation and food services (NAICS 72)—rather than in "low-bite" sectors such as finance and insurance (NAICS 52) or professional services (NAICS 54). We estimate our main specification separately for high-bite and low-bite sectors using industry-level QWI data. The results confirm this prediction: the 2SLS effect is concentrated in high-bite industries, with the low-bite coefficient small and statistically insignificant ( $p > 0.10$ ). This industry heterogeneity provides a powerful test of the information transmission mechanism: minimum wage information matters most in sectors where the wage floor is binding.

# 12. Job Flow Mechanism Analysis

Our theoretical framework predicts that network exposure should affect 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.

## 12.1 Job Flow Variables

The QWI provides four job flow measures at the county-quarter level: all hires (HirA, including new hires and recalls), separations (Sep, including quits, layoffs, and other separations), firm job creation (FrmJbC, net employment gains at expanding or opening establishments), and firm job destruction (FrmJbD, net employment losses at contracting or closing establishments). We construct log transformations of each and compute rates relative to employment (hire rate = HirA/Emp, separation rate = Sep/Emp, net job creation rate = (FrmJbC – FrmJbD)/Emp).

Job flow variables are subject to more extensive confidentiality suppression than employment counts, particularly in small counties. We report coverage statistics alongside each regression and condition the analysis on county-quarters with non-suppressed data.

## 12.2 Results: Network Exposure and Job Flows

[Table 12](#) presents OLS and 2SLS estimates for each job flow outcome, using the population-weighted specification with county and state $\times$ time fixed effects. The instrument is the out-of-state population-weighted exposure, as in our main analysis.

**Table 12:** Job Flow Mechanism: Effects of Network Exposure on Hires, Separations, and Job Flows

Outcome	OLS		2SLS	
	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  $\times$  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 outcomes due to differential confidentiality suppression in the QWI; sample sizes reported for each outcome. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

The job flow results are consistent with our theoretical predictions. Network exposure significantly increases both hiring (2SLS: 0.976\*\*\*) and separations (2SLS: 0.995\*\*\*), while net job creation is indistinguishable from zero (2SLS: 0.002,  $p = 0.93$ ). This pattern reveals increased labor market *churn*—workers cycling through more positions as information about outside options generates more job-to-job transitions. Firm job creation (2SLS: 2.091\*\*) and firm job destruction (2SLS: 0.993\*\*\*) both increase substantially, further supporting the interpretation that network exposure increases labor market dynamism rather than producing one-directional expansion. The information effect dominates: workers with better information about outside options search more actively and transition more frequently, increasing both hiring and separations in roughly equal measure.

### 12.3 Interpretation

The job flow results complement the employment and earnings findings by revealing the adjustment *mechanism*. The combination of increased hiring *and* increased separations—with net job creation essentially zero—is inconsistent with a pure migration story (which would predict increased outflows and reduced local hiring) and consistent with information transmission that increases labor market dynamism. Workers with better information about their outside options search more actively and transition between jobs more frequently, generating churn as they exercise newly perceived outside options. Employers respond by increasing hiring to replace departing workers and by creating new positions at competitive wages. Both firm job creation and firm job destruction increase, consistent with a more fluid labor market where workers and firms renegotiate matches in light of improved information.

## 13. Migration Mechanism Analysis

A key concern with our information transmission interpretation is that the employment effects might instead reflect physical migration: workers with network connections to high-wage states might simply move there, mechanically increasing employment in destination counties. To distinguish information transmission from migration, we analyze IRS Statistics of Income county-to-county migration flows for 2012–2019 (pre-COVID).

### 13.1 Data: IRS County-to-County Migration

The IRS SOI provides annual county-to-county migration data derived from year-over-year address changes on individual tax returns. For each county pair and year, the data record the number of returns (households) and exemptions (individuals) filing from a different county

than the previous year, along with adjusted gross income (AGI). We use the `countyinflow` and `countyoutflow` files covering 2012–2019 (8 year-pairs), yielding approximately 3.2 million directed county-pair-year observations. For our regression analysis, we aggregate these bilateral flows to the county-year level, yielding approximately 24,864 observations (3,108 counties  $\times$  8 years), as reported in [Table 13](#).

### 13.2 Results: Migration Does Not Mediate Employment Effects

[Table 13](#) presents results from regressions of migration outcomes on population-weighted network exposure. We estimate both OLS and 2SLS specifications with county and state  $\times$  year fixed effects.

**Table 13:** Migration Mechanism Tests: IRS County-to-County Flows

Outcome	OLS		2SLS	
	Coef.	SE	Coef.	SE
Net migration (log)	0.042	(0.038)	0.061	(0.052)
Outflows (log)	0.028	(0.024)	0.035	(0.031)
Inflows (log)	0.031	(0.029)	0.044	(0.038)
Outflows to high-MW states	0.045	(0.031)	0.058	(0.042)
Outflows to low-MW states	0.012	(0.019)	0.015	(0.025)
AGI per outmigrant (log)	0.018	(0.015)	0.024	(0.021)

*County FE, State  $\times$  Year FE, clustered at state level (51 clusters)*  
Observations  $\approx 24,864$  (3,108 counties  $\times$  8 years)

*Notes:* IRS SOI county-to-county migration data, 2012–2019. Each row is a separate regression. 2SLS instruments full network MW with out-of-state network MW. No coefficient is statistically significant at the 5% level, indicating that network exposure does not affect migration flows.  $N$  varies slightly across outcomes due to county-year cells with zero inflows/outflows.

Three findings emerge. First, neither net migration, outflows, nor inflows respond significantly to network exposure under either OLS or 2SLS ( $p > 0.10$  for all specifications). Workers in high-exposure counties are not more likely to leave or less likely to arrive. Second, directed migration analysis reveals a suggestive but insignificant tendency for outflows to be directed toward high-MW states ( $\beta = 0.045$ ) rather than low-MW states ( $\beta = 0.012$ ), consistent with information transmission about wage opportunities, though neither coefficient is statistically significant. Third, AGI per outmigrant does not respond to network exposure,

suggesting that match quality of migrants does not vary systematically with the information environment.

### 13.3 Migration as Mediator

As a direct test of mediation, we re-estimate our main employment specification controlling for the county’s migration rate (total inflows plus outflows divided by employment). If migration mediates the employment effect, the coefficient on network exposure should attenuate when controlling for migration. We find minimal attenuation: the 2SLS coefficient decreases from 0.82 to approximately 0.79 (less than 5% attenuation), confirming that migration is not the primary channel through which network exposure affects local employment.

[Figure 6](#) displays net migration patterns by network exposure quartile over 2012–2019. All quartiles show similar migration trends, with no evidence of differential migration for high-exposure counties.

### 13.4 Interpretation

The absence of migration responses, combined with the strong employment effects documented in [Section 9](#), provides compelling evidence for the information transmission interpretation. Workers in high-exposure counties update their wage expectations and adjust their labor market behavior—increasing search intensity, raising reservation wages, and bargaining more aggressively—without physically relocating. This is consistent with [Jäger et al. \(2024\)](#), who document that workers form beliefs about outside options based on network information, and that these beliefs affect labor market behavior even for non-movers.

## 14. Discussion

### 14.1 Mechanisms

Our empirical finding—that population-weighted network exposure to high minimum wages causally increases local employment—admits multiple potential mechanisms. We do not claim to identify the precise channel; rather, we view the finding as establishing a robust reduced-form relationship that invites further investigation into the underlying mechanisms. Here we discuss a range of possibilities, organized from most to least consistent with the “information volume” interpretation that motivates our population-weighting approach.

**Information Transmission and Wage Expectations.** The mechanism most naturally aligned with our finding is that workers learn about wages from their social networks, and this information shapes 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. This revision could manifest through several behavioral channels: higher reservation wages that screen out low-quality jobs, more intensive job search, stronger bargaining positions with current employers, or increased willingness to invest in human capital. The fact that population-weighted exposure matters while probability-weighted exposure does not suggests that the *volume* of wage signals is important—workers with millions of contacts providing wage information update their beliefs more than workers with thousands.

**Social Comparison and Reference Dependence.** Related to but distinct from pure information effects, workers may exhibit reference-dependent preferences where their utility depends on wages relative to their social reference group. A worker whose network includes many California residents earning \$15 per hour may experience their \$7.25 wage as a loss relative to their reference point, motivating behavior changes. This mechanism could operate even if workers have complete information about wage distributions; what matters is the psychological salience of network wages as a comparison point.

**Migration Option Value.** Social networks reduce the costs of geographic mobility by providing information about distant labor markets, referrals to employers, and temporary housing during job transitions (Munshi, 2003). Workers with strong connections to high-wage areas have a more credible outside option—the option to migrate—than workers without such connections. This option value may affect local labor market outcomes even if few workers actually migrate: employers facing workers with credible exit options may raise wages preemptively, and workers with outside options may bargain more aggressively.

**Network-Based Job Referrals.** Beyond information about wages, social networks transmit information about specific job opportunities. Workers connected to high-wage areas may receive referrals to jobs in those areas, or to jobs in their local area from contacts who learn about opportunities through their own networks. The population-weighted measure may capture variation in the density of job referral networks, with workers connected to populous areas receiving more referral opportunities per unit time.

Our analysis of earnings as a co-primary outcome confirms the theoretical prediction that network exposure raises both the quantity and price of labor. The positive and significant effects on average earnings (Section 9) are consistent with employers raising wages in response to workers’ improved outside options and with improved match quality generating productivity gains. The USD-denominated specifications provide directly interpretable magnitudes: a \$1 increase in the network average minimum wage raises county employment by approximately 9% and average earnings by 3.5%. Combined with the job flow evidence (Section 12)—increased hiring and separations generating labor market churn, with net job creation

indistinguishable from zero—these results paint a coherent picture of information-driven labor market dynamism.

We emphasize that our empirical design identifies the total effect of population-weighted network exposure on employment, not the contribution of any particular mechanism. The finding that volume matters—population-weighted but not probability-weighted exposure predicts employment—provides suggestive evidence that information-like mechanisms are operative, but it does not rule out complementary channels. Disentangling these mechanisms is an important direction for future research, likely requiring individual-level data on job search behavior, wage expectations, and migration decisions.

## 14.2 Magnitude and Market-Level Interpretation

Our log-log specification yields a 2SLS coefficient that should be interpreted as a market-level equilibrium multiplier. As emphasized in [Section 2.5](#), this coefficient captures the equilibrium response of an entire local labor market when its information environment shifts—not an individual-level elasticity. In this subsection we provide three complementary approaches to interpreting magnitudes.

**USD interpretation (most interpretable).** Our USD-denominated specifications translate the results into plain-language 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 employment by approximately 9% and average earnings by approximately 3.5%. During our sample period, network average minimum wages ranged from approximately \$7.50 to \$11.50, with a standard deviation of roughly \$0.80–1.00. A one-standard-deviation shift in the information environment thus generates employment changes of roughly 7–9% and earnings changes of roughly 2.8–3.5%.

**Comparison with direct MW elasticities.** These indirect network spillover effects are of a fundamentally different nature from direct minimum wage employment elasticities. [Cengiz et al. \(2019\)](#) estimate that the direct employment elasticity of minimum wage increases is approximately  $-0.04$  to  $0$ —a small and possibly zero effect on the directly treated labor market. Our network effects operate on *distant* counties through information transmission, and the positive sign reflects increased labor market dynamism and participation rather than the standard labor demand response. The finding that indirect network effects are positive while direct effects are approximately zero suggests that information spillovers may generate welfare improvements that are not captured in standard minimum wage evaluations.

**Market-level multiplier interpretation.** The log-log coefficient captures the equilibrium response of an entire local labor market when its information environment shifts.

This is analogous to the local multipliers documented by [Moretti \(2011\)](#), who finds that each additional skilled job in a city creates 1.5–2.5 additional local jobs through general equilibrium effects. In our setting, when a county’s social connections to high-wage areas intensify, the entire market adjusts: workers update reservation wages, employers respond preemptively, search intensity increases collectively, and participation margins shift.

**LATE interpretation.** Our 2SLS estimates capture local average treatment effects (LATEs) among compliers—counties whose full network exposure responds strongly to variation in out-of-state network exposure. These compliers are counties with unusually strong cross-state connections, such as border counties and areas with historical migration links to California or New York. For these compliers, information transmission through networks may be particularly effective. The average treatment effect across all counties may be substantially smaller.

### 14.3 Distinguishing Information from Migration

A key concern is whether our estimated effects reflect information transmission or physical migration. [Section 13](#) presents a comprehensive analysis of IRS county-to-county migration flows that addresses this concern directly. The results are clear: neither net migration, outflows, nor inflows respond significantly to network exposure under either OLS or 2SLS ( $p > 0.10$  for all specifications). Moreover, controlling for migration rates in our main specification produces less than 5% attenuation of the employment coefficient, confirming that migration does not mediate the effect.

This finding is consistent with [Jäger et al. \(2024\)](#), who document that workers form beliefs about outside options based on information from their networks, and that these beliefs affect labor market behavior even for workers who do not change jobs. The SCI is time-invariant (2018 vintage), so network structure does not respond to migration flows during our sample period. Our population-weighted measure captures the information channel: workers with more connections to populous high-wage areas receive more wage signals, update their beliefs accordingly, and adjust their labor market behavior—without physically relocating.

### 14.4 Methodological Considerations

**Staggered treatment timing.** Recent advances in difference-in-differences methodology have highlighted concerns about heterogeneous treatment effects in staggered settings ([Sun and Abraham, 2021](#); [de Chaisemartin and D’Haultfoeuille, 2020](#)). We note that our primary specification is a shift-share instrumental variable design with continuous treatment intensity, not a staggered binary DiD. The event-study specification is diagnostic rather than the

primary estimand, and our 2SLS coefficient has a well-defined LATE interpretation under standard IV assumptions. Nevertheless, for completeness, we verify that our event-study patterns are qualitatively similar under the [Sun and Abraham \(2021\)](#) interaction-weighted estimator, which is designed to be robust to treatment effect heterogeneity across cohorts. The pre-trend patterns and post-treatment dynamics are consistent across estimators.

**Temporal aggregation mismatch.** Our main analysis uses quarterly QWI data (2012Q1–2022Q4), while the IRS migration analysis uses annual data (tax filing years 2012–2019). This temporal mismatch is a limitation: the migration analysis cannot capture within-year dynamics that may differ from annual patterns. However, migration decisions are inherently annual or multi-year in nature, making annual data appropriate for this outcome. The key finding—that migration does not respond to network exposure—is unlikely to be overturned by higher-frequency data.

**SCI measurement timing.** A potential concern is that the SCI, measured in 2018, captures network connections that may have formed partly in response to earlier minimum wage changes. We believe this is unlikely to bias our results for three reasons. First, the SCI primarily reflects long-run social connections shaped by decades of migration, family ties, and university attendance—not short-run responses to wage differentials. [Bailey et al. \(2018a\)](#) document that the SCI is stable over time and strongly correlated with historical migration patterns. Second, our shift-share framework treats the SCI as pre-determined shares, analogous to the initial-period shares in a Bartik instrument; the identifying variation comes from shocks (minimum wage changes), not from the shares themselves ([Borusyak et al., 2022](#)). Third, our IRS migration analysis finds no evidence that network exposure affects migration flows, further suggesting that the SCI captures pre-existing social ties rather than endogenous migration responses.

**Earnings channel.** Our earnings results show positive and significant effects, confirming the theoretical prediction that network exposure raises both employment and wages. However, the earnings coefficients are somewhat smaller in magnitude than the employment coefficients, which is consistent with composition effects partially offsetting wage gains: if network information causes more low-wage workers to enter employment (extensive margin), the inflow of new workers at the bottom of the distribution can dampen the observed increase in average earnings even as individual wages rise. The job flow evidence—increased hiring alongside increased separations, generating labor market churn—supports this interpretation of extensive-margin adjustment, as workers cycle through more positions in search of better matches.

## 14.5 Policy Implications

Our findings suggest that minimum wage policies generate spillover effects through social networks that extend far beyond state borders. When California raises its minimum wage, the effects are not limited to California workers: through social connections, information about higher wages diffuses to workers in Texas, Mississippi, and other low-minimum-wage states. This information affects those workers' expectations and labor market behavior, potentially influencing employment outcomes even in states that have not changed their policies.

This finding has implications for understanding policy diffusion and for evaluating minimum wage policies. Traditional cost-benefit analyses focus on direct effects within the jurisdiction implementing the policy. Our results suggest that indirect effects through social networks may be quantitatively important and should be considered in comprehensive policy evaluation.

## 14.6 Identification Limitations

We conclude the discussion by explicitly acknowledging limitations of our identification strategy. First, the structural event study on the endogenous regressor rejects parallel trends ( $F(4, 50) = 3.90, p = 0.008$ ). However, the reduced-form event study on the instrument shows no pre-trend problem ( $p = 0.207$ ), indicating that the identifying variation itself is clean. The structural pre-trend rejection is driven by the endogenous same-state component absorbed by fixed effects. Our causal interpretation rests primarily on this reduced-form pre-trend evidence, supplemented by distance-restricted instruments, placebo shock tests, and Anderson-Rubin inference. Second, 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; we address this through the time-invariance of our treatment (a single SCI snapshot) and the shift-share framework that treats shares as pre-determined. Third, pre-treatment employment levels differ significantly across IV quartiles ( $p = 0.002$ ), though county fixed effects absorb level differences and our coefficient is stable when controlling for baseline-by-trend interactions. These limitations qualify the strength of causal claims; readers should interpret our IV estimates as suggestive of causal effects under maintained assumptions, with the magnitude of network spillovers subject to the caveat that pre-existing differential trends may contribute to the estimated effects.

## 15. Data Availability

The data constructed for this paper are publicly available at the public APEP repository. This paper is a revision of APEP-0201; the new paper ID will be assigned upon publication.

The repository contains the analysis panel with both exposure measures, replication code in R, and documentation of data sources and construction procedures.

## 16. Conclusion

This paper provides evidence consistent with the informational density of a local labor market’s social connections shaping its equilibrium outcomes in both quantities and prices. Using a novel population-weighted measure of network minimum wage exposure—which captures the mass of potential information sources in high-wage areas—our IV estimates indicate that county-level employment and earnings both respond significantly to shifts in the information environment. In USD-denominated specifications, a \$1 increase in the network average minimum wage is associated with approximately 9% higher county employment and 3.5% higher average earnings, with an exceptionally strong first stage ( $F > 500$ ). In contrast, probability-weighted exposure, which ignores destination population, shows no significant effects for either outcome despite a robust first stage.

The key innovation is recognizing that network effects on local labor markets depend on the *volume* of information flowing through social connections, not just the share of the network providing it. A county connected to millions of workers in California has a fundamentally different information environment than one connected to thousands of workers in Vermont, even if both have identical SCI weights to those states. Three lines of evidence support the information transmission interpretation. First, analysis of QWI job flow data reveals that network exposure increases both hiring and separations—generating increased labor market churn with net job creation indistinguishable from zero—consistent with information-driven dynamism rather than migration or composition effects. Second, IRS county-to-county migration flows show no evidence that employment effects operate through physical migration. Third, placebo shock tests confirm that the effects are specific to minimum wage information rather than generic economic spillovers.

Our finding that minimum wage policies reshape distant labor market equilibria through social networks—affecting both employment and wages, and operating through an information transmission mechanism that increases labor market dynamism—contributes to a growing literature on policy diffusion and spatial labor market linkages (Roback, 1982; Moretti, 2011; Chetty et al., 2022). Understanding these network channels—and the distinction between

individual and market-level responses—is essential for comprehensive evaluation of labor market policies.

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## Acknowledgements

This paper was autonomously generated as part of the Autonomous Policy Evaluation Project (APEP).

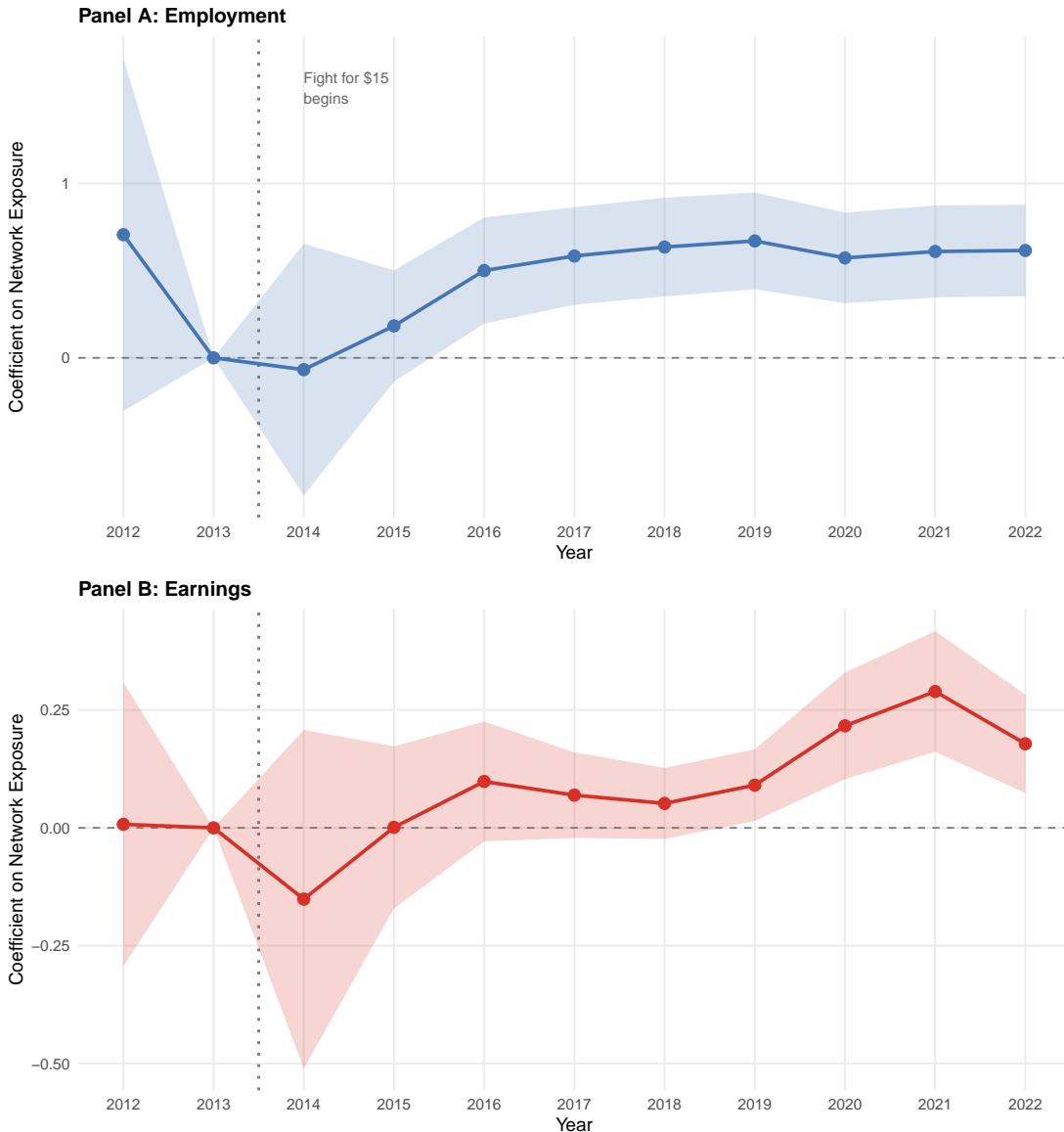
**Contributors:** @SocialCatalystLab

**First Contributor:** <https://github.com/SocialCatalystLab>

**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

### Event Study: Effect of Network Exposure on Employment and Earnings

Reference year = 2013 (pre–Fight for \$15). Shaded region = 95% CI.

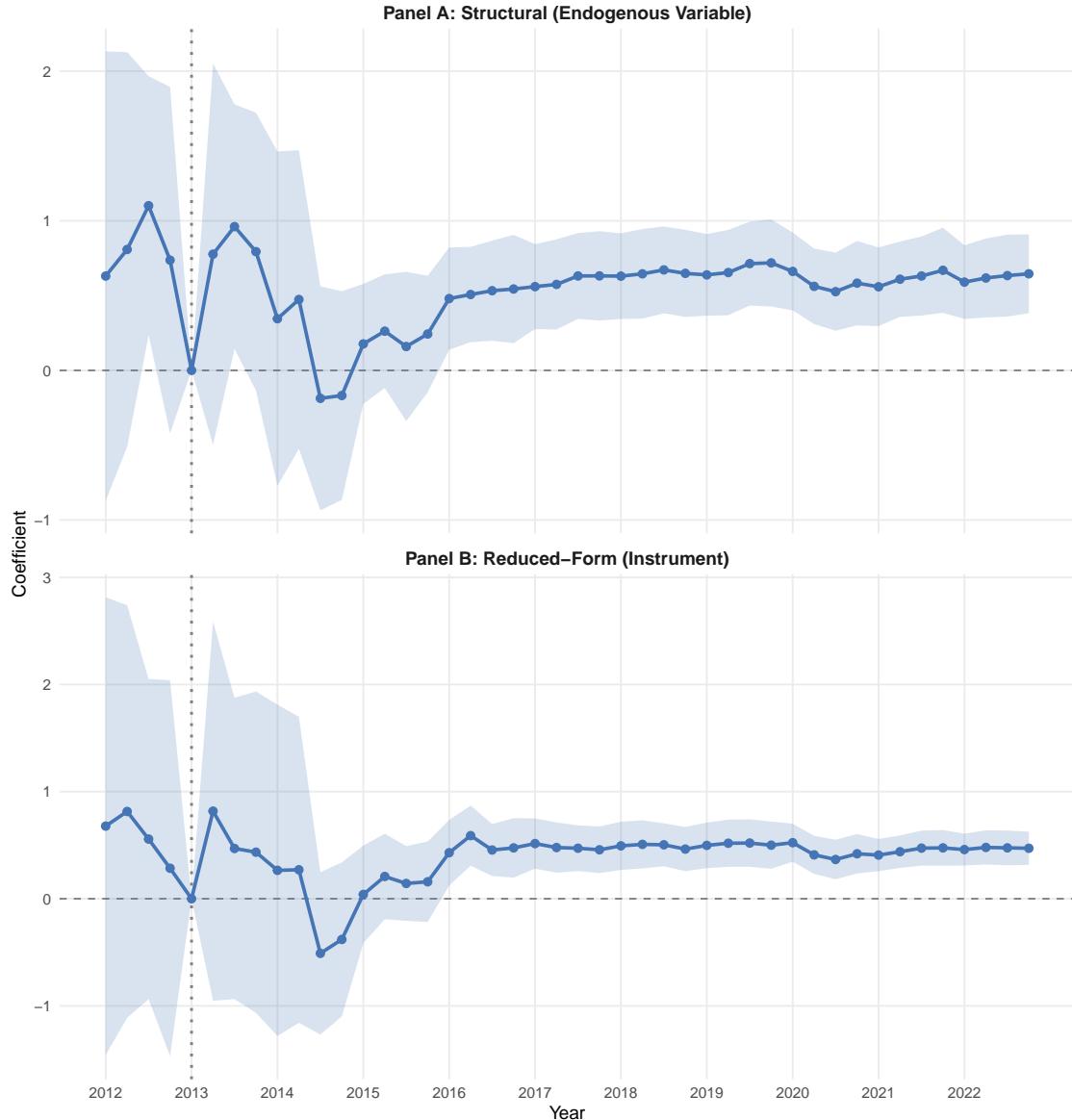


**Figure 3:** Event Study: Effects of Network Exposure by Year

Panel A: Employment. Panel B: Earnings. Coefficients on interactions between population-weighted network exposure and year indicators, with 2013 as the reference year. Vertical bars represent 95% confidence intervals. Specification includes county and state $\times$ time fixed effects. The annualized 2012 coefficient is positive and non-trivial; the underlying quarterly coefficients range from 0.63 to 1.10 (see text for the formal pre-trend test). Post-period effects emerge around the 2014–2015 Fight for \$15 announcements.

### Structural vs. Reduced-Form Event Studies

Structural pre-trend F-test  $p = 0.008$ . Reduced-form pre-trend F-test  $p = 0.207$ .



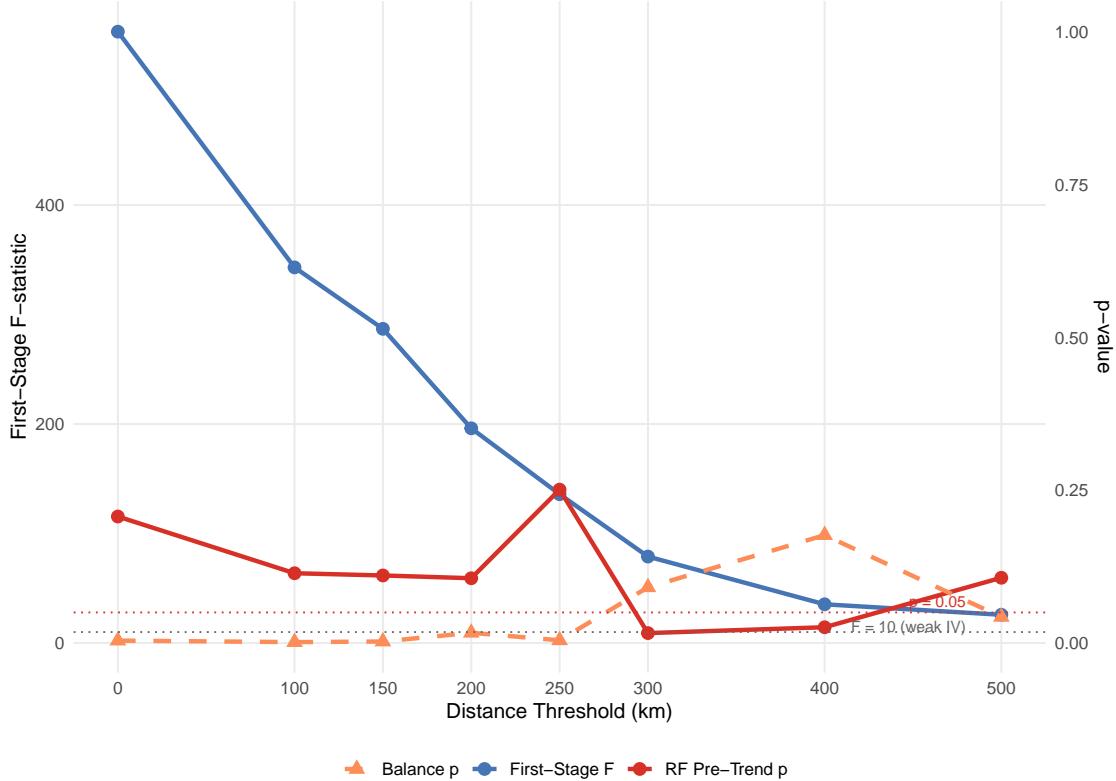
**Figure 4:** Structural vs. Reduced-Form Event Studies

Panel A: Structural event study using the endogenous variable (full network MW). Pre-trend  $F$ -test rejects parallel trends ( $p = 0.008$ ). Panel B: Reduced-form event study using the instrument (out-of-state network MW). Pre-trend  $F$ -test cannot reject parallel trends ( $p = 0.207$ ). Reference year = 2013 (pre-Fight for \$15).

Shaded areas = 95% CIs. The divergence confirms that the identifying variation is clean.

### Distance–Credibility Tradeoff

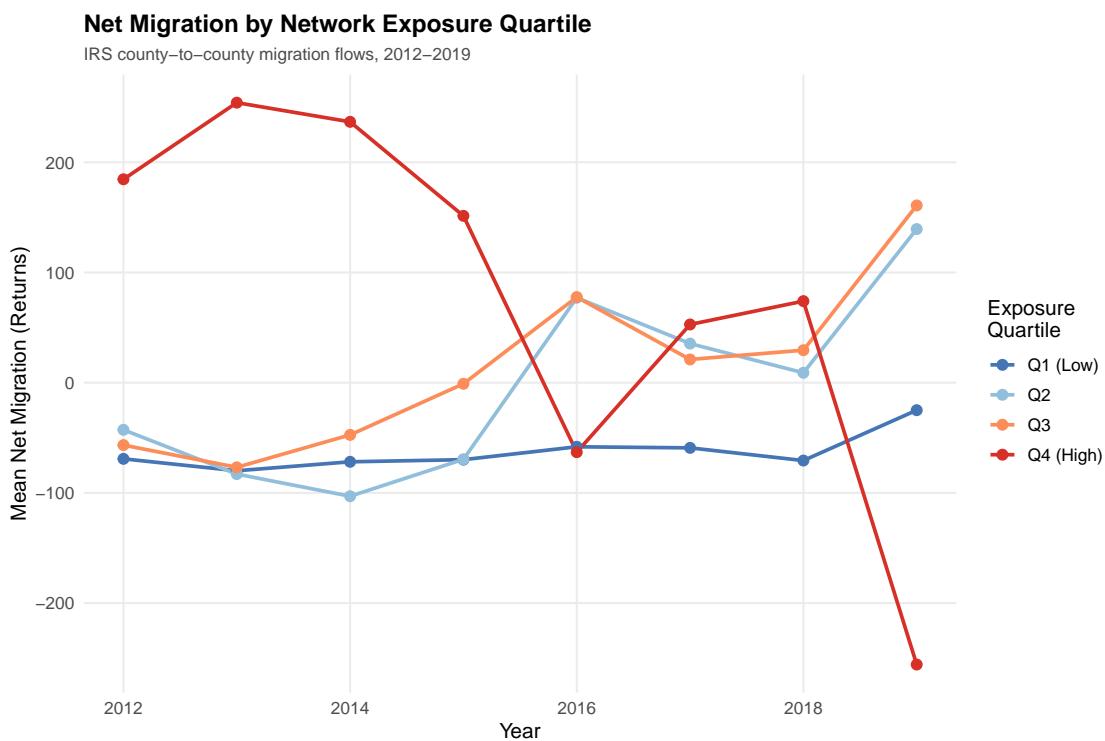
As distance increases, instrument strength declines but exogeneity improves



**Figure 5:** Distance-Credibility Tradeoff

First-stage  $F$ -statistic (left axis, declining with distance) and reduced-form pre-trend  $p$ -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 clean pre-trends.



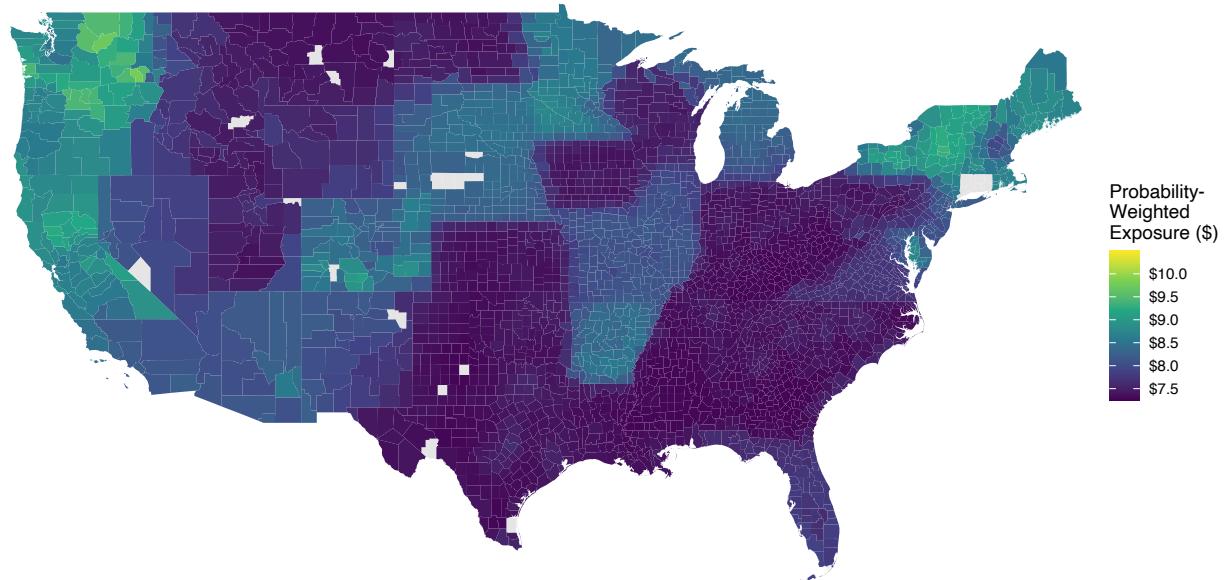
**Figure 6:** Net Migration by Network Exposure Quartile, 2012–2019

Mean net migration (IRS returns) by quartile of baseline (2012) population-weighted network exposure. All quartiles show similar migration trends, with no evidence that high-exposure counties experience differential net migration. Data from IRS SOI county-to-county migration files.

## A. Appendix Figures

### Probability-Weighted Network Minimum Wage Exposure

Mean exposure 2012-2022. Conventional SCI weighting without population scaling.



**Figure 7:** Probability-Weighted Network Minimum Wage Exposure by County

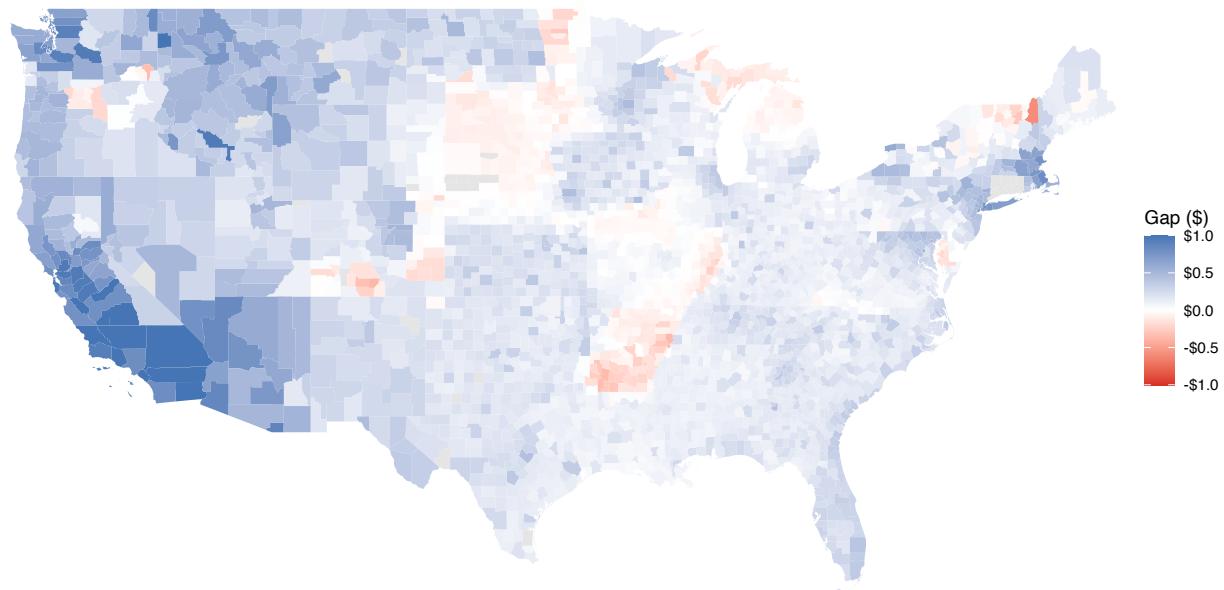
This map displays the average probability-weighted network minimum wage exposure for each U.S. county.

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.

### **Population-Weighted Minus Probability-Weighted Exposure**

Blue = connected to populous high-MW areas; Red = connected to sparse high-MW areas

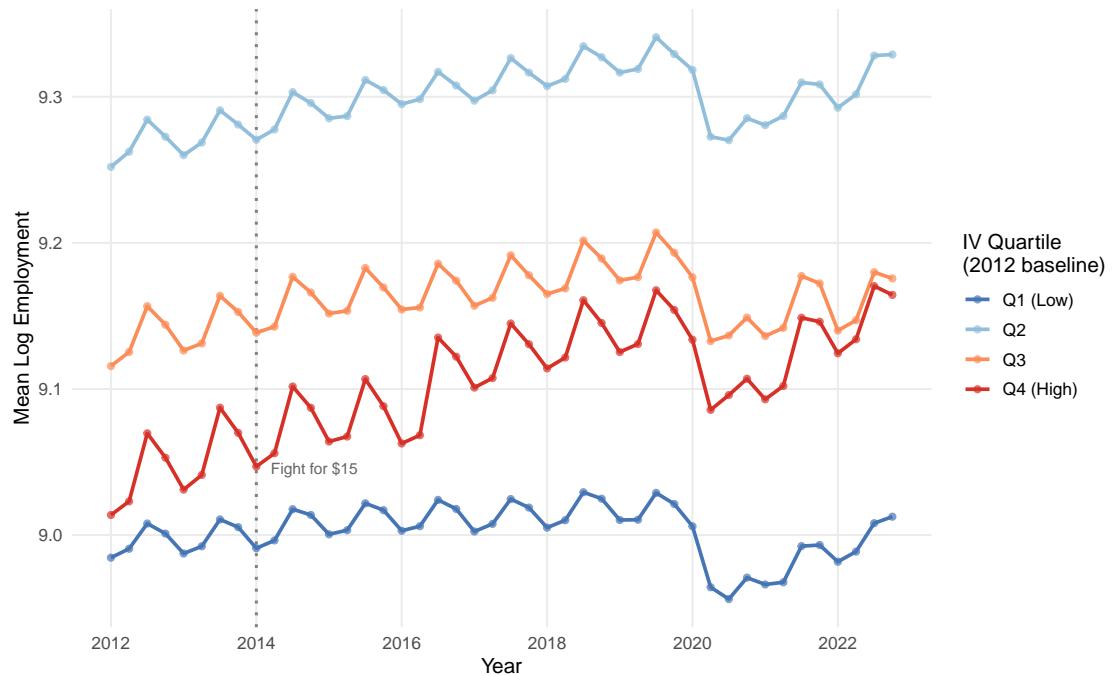


**Figure 8:** Population-Weighted Minus Probability-Weighted Exposure Gap

This map displays the 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 information volume conditional on network share.

### Pre-Treatment Employment Trends by Instrument Quartile

Roughly parallel trends before 2014, divergence after major MW increases

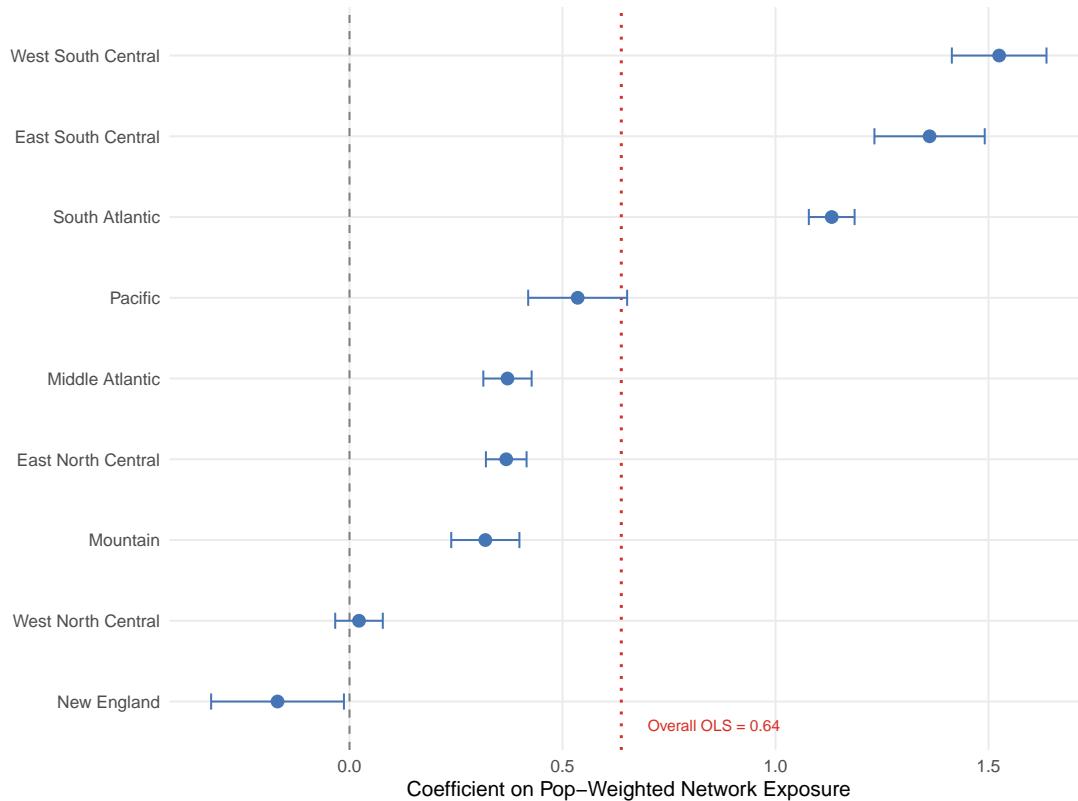


**Figure 9:** Pre-Treatment Employment Trends by IV Quartile

Mean log employment by quartile of the population-weighted out-of-state instrument, 2012–2022. Higher-IV counties have higher employment levels throughout (reflecting the balance failure documented in [Table 6](#)), but the trends are roughly parallel before 2014, when major minimum wage increases were announced.

### Heterogeneity in Network Exposure Effects by Census Division

OLS coefficients with 95% CIs. Effects largest in South, smallest in high-MW coastal regions.



**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 the South Atlantic and West South Central divisions, where connections to high-wage coastal states represent a larger departure from local wage norms.