How Replaceable Is a Low-Wage Job?

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Abstract

We study the long-run consequences of losing a low-wage job using linked employer-employee wage records and household surveys. For full-time workers earning $15 per hour or less, job loss due to an idiosyncratic firm-wide contraction decreases earnings six years later by 13% and cumulative earnings by over $40,000. Long-run losses stem primarily from reductions in employment and hours rather than wage rates and are concentrated among workers displaced from jobs in industries with higher average wages, tenure, unionization rates, and full-time share. By contrast, workers initially earning $15-$30 per hour see comparable long-run losses driven primarily by reductions in hourly wages.

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The United States labor market is highly dynamic, with about two million layoffs and five million total separations each month. Low-wage workers, such as the many cooks, janitors, drivers, and other employees paid near the minimum wage, are particularly vulnerable to displacement (Farber, 1993). Many economists expect the long-run consequences of job loss for these workers to be minimal because a comparable new position can be easily secured. Jacobson et al. (2011), for example, write that job displacement costs "are usually small for low-wage and low-tenured workers" (pg. 5). With wages low to begin with, the scope for firm-, industry-, or occupation-specific skills (Neal, 1995; Poletaev and Robinson, 2008; Huckfeldt, 2022) or strong firm-worker matches (Lachowska et al., 2020) to directly generate any subsequent wage losses is limited.

Partly as a result, the extensive literature on job displacement typically studies higher-wage workers with lengthy tenure, focusing, "quite deliberately, on the types of job loss events that often involve serious consequences for workers" (Davis and von Wachter, 2011, pg. 7).[1] Whether and how low-wage workers suffer from displacement remains an open question. While their pay rates cannot fall much by construction, displacement may still prove costly if finding a new job that offers the same hours and stability—or any comparable job at all—proves difficult. Constraints on hours and scheduling, (Alexander et al., 2015; Lachowska et al., 2023), job rationing induced by regulation, efficiency wages, or other forces, and skill degradation in unemployment (Mincer and Ofek, 1982; Farber et al., 2016; Dinerstein et al., 2022; Cohen et al., 2023) may all make it more difficult to secure replacement work at an acceptable and legal wage.

This paper studies the consequences of job loss for low-wage workers using a novel combination of administrative earnings records and household surveys. The former come from the U.S. Census Bureau's Longitudinal Employer Household Dynamics (LEHD) program and report quarterly earnings in all unemployment insurance (UI) covered jobs in 21 states, as well as an indicator for any employment nationally. These data are linked to survey responses from the American Community Survey (ACS), which allows us to measure labor force status, weeks and hours worked, and hourly wage rates. We use ACS responses to identify a sample of workers earning \$15 per hour or less in 2020-equivalent dollars between 2001 and 2014. These individuals predominantly hold common and relatively low-skill jobs at the time of job loss, working as cooks, janitors, secretaries, drivers, and retail sales workers, for example. We track them longitudinally for three years prior and six years after job loss using earnings records from the LEHD and any future responses to the ACS, which randomly re-samples a

[1] These studies include Topel (1990); Jacobson et al. (1993); Couch and Placzek (2010); Von Wachter et al. (2009); Davis and von Wachter (2011); Lachowska et al. (2020); Schmieder et al. (2023); and Bertheau et al. (2023), among many others.

meaningful fraction of workers over this follow-up period. While our primary results focus on initially full-time workers, we also study those initially employed part-time.

A key challenge is identifying exogenous and involuntary job separations. Comparing job-leavers to job-stayers is unlikely to yield credible estimates of the causal effects of job loss because low-wage jobs turn over frequently for a plethora of reasons, including poor performance and superior outside offers. To isolate involuntary separations, we exploit firm-specific labor demand shocks proxied by year-over-year employment changes. Our strategy compares workers in firms that experience large employment reductions to workers in similar firms that do not. We condition on granular fixed effects for geography by calendar time by industry, helping ensure that the results capture idiosyncratic shocks instead of local or industry-specific recessions.[2] These shocks are uncorrelated across firms within more narrowly defined markets and orthogonal to workers' characteristics and earnings histories. Our reduced-form results examine their effects on long-run outcomes. We also use them as an instrument for job loss in two-stage least squares (2SLS) estimates.

While similar in spirit to classic analyses of mass layoffs (e.g., Jacobson et al., 1993), this approach has several advantages that make it especially suited to studying low-wage job loss. First, the analysis avoids conditioning directly on job-separation (for treated workers) or job-staying (for controls). Instead, 2SLS estimates of the effect of job loss capture the impacts on workers who leave their jobs because of the shock. This allows us to include a broad set of workers in the analysis instead of focusing on high-tenure workers for whom job separation is most likely to be involuntary, as in much of the previous literature. Second, rather than matching treated workers to controls with similar earnings histories, we control for firm characteristics directly in our regressions. As a result, workers' pre-shock earnings levels are not mechanically balanced and can be used to test the identifying assumptions. Finally, rather than focusing on a single threshold to define firm distress (e.g., decreases in employment greater than 30%), we exploit the full distribution of firm-level shocks, lending additional generality and precision to the analysis. Results change little, however, when focusing only on the most extreme shocks, as in mass layoff studies.

The results show that full-time low-wage workers experience substantial cumulative and long-run earnings losses due to these idiosyncratic shocks to labor demand. Negative shocks sharply increase the probability that workers separate from their job over the next year. Both employment and earnings subsequently decline and recover sluggishly. Six years later,

[2] Demand shocks at the local or industry level have been the focus of a large body of work, including Blanchard and Katz (1992) and Autor et al. (2014), among others. Our estimates target a different causal effect, the impact of an idiosyncratic firm-specific demand shock.

2SLS estimates indicate reductions in quarterly earnings of 13% of the mean and cumulative losses greater than \$40,000, or roughly 130% of pre-shock annual average earnings. Displaced workers are also 3.3 percentage points less likely to have any quarterly earnings (4% of the mean), much of which is explained by a three percentage point increase in the likelihood that workers have zero earnings for two years or more. While these extensive-margin reductions are meaningful, they account for less than half of the long-run effect on earnings, implying a significant earnings reduction among those who find new jobs as well.

Analysis of ACS data shows that most losses stem from reductions in the likelihood and frequency of work, not hourly wage rates. Averaging the four to six years post-shock to maximize power, our estimates show decreases in employment of 5.8 percentage points. This effect reflects a combination of increases in both unemployment (3.2 percentage points) and non-participation (2.6 percentage points). Job loss generates no long-run effect on the likelihood of reporting being on layoff but creates a 4.1 percentage point increase in the likelihood of reporting looking for work. Including zeros, weeks worked last year declines by nearly a month and usual hours worked decreases by three hours. ACS-based outcomes also show that household income responses are comparable to individual responses, suggesting limited intra-household insurance, and that employment responses are not explained by incarceration or cross-state mobility.

The headline earnings and employment effects are consistent with a calibrated model of random search across jobs with heterogeneous wages, hours, and stability. The model combines three simple forces: endogenous search effort, human capital depreciation, and duration dependence in search costs. These forces generate sufficient state-dependence that workers shocked with job loss are less likely to be employed several years later and earn lower wages across fewer hours when they do work. Human capital depreciation accounts for a portion of earnings losses, but the bulk is driven for workers occupying jobs lower down the ladder in terms of both hours and wage rates. Viewed through the model, job loss is costly—a newly unemployed worker would be indifferent between accepting a full-time, \$15 / hour job today and continuing to search with a permanent ~20% boost to unemployment benefits.

Exploring industry heterogeneity provides some clues about which types of low-wage work are most difficult to replace. Workers employed in retail, accommodation and food services, and healthcare experience short-lived earnings reductions that fade to zero after six years. Cumulative losses average \$17,000, or roughly 50% of pre-displacement average annual earnings. By contrast, low-wage workers in manufacturing, education, and other sectors such as construction and natural resource extraction experience substantial long-run losses. In the model, job loss effects depend strongly on the stability of the initial job, providing one way to rationalize these patterns. Looking across all 2-digit North American Industry Classification System (NAICS) industries, we also find larger losses in sectors with higher unionization rates, more full-time workers, longer average tenure, and higher average firm and worker pay premia, suggesting that while some low-wage jobs are relatively easy to replace, those in industries where jobs appear higher quality along several observable dimensions are not.

Finally, since our strategy involves different data and research designs to previous analyses, we compare our effects on low-wage workers to estimates from the same empirical strategy deployed on a sample of workers initially earning \$15-\$30 per hour. This sample has average pre-displacement earnings comparable to many prior studies. Overall, displacement reduces earnings by 17% six years later for this group. ACS responses show that, in contrast to low-wage workers, impacts on employment and participation are small; we cannot reject zero effects on either margin. Instead, hourly wages account for the bulk of earnings declines, consistent with some prior work (e.g., Lachowska et al., 2020). Effects for higher-wage workers vary strongly by tenure, with impacts on high-tenure workers similar to those measured in analyses that use a traditional difference-in-differences design (e.g., Jacobson et al., 1993). There is no evidence of tenure heterogeneity among low-wage workers, however.

Our work builds on a large body of research measuring the consequences of job loss. Much of the literature has focused on understanding the sources of high-tenure workers' long-run losses (Moore and Scott-Clayton, 2019; Jung and Kuhn, 2019; Lachowska et al., 2020; Fackler et al., 2021; Jarosch, 2021; Fallick et al., 2021; Helm et al., 2023), their cyclicality (Davis and von Wachter, 2011; Huckfeldt, 2022; Schmieder et al., 2023), the role of industry- or occupation-specific human capital (e.g., Neal, 1995; Poletaev and Robinson, 2008; Milgrom, 2021), and differences across labor markets (Bender et al., 2002; Bertheau et al., 2023). While some work has explored heterogeneity by skill or experience in the U.S., typically using survey data (e.g., Stevens, 1997; Farber, 2004; Von Wachter et al., 2009), there is limited evidence on the effects of job loss for low-wage workers or workers without substantial tenure. Nevertheless, a common view is that the costs of job displacement for low-wage workers are small, especially because wage rates cannot fall below any legislated minimums.[3]

We make several contributions to this literature. First, our results challenge the view, articulated by Davis and von Wachter (2011), that "many, perhaps most...job loss events involve little financial loss or other hardship for individuals" (pg. 5). The fact that workers in low-skill jobs, paid low wages, and without substantial tenure experience proportionally

[3] Jacobson et al. (2011), for example, write: "Minimum-wage workers, for example, experience little long-term effect from displacement, because they are paid at new jobs about what they were paid at previous jobs. By contrast, middle- and upper-income workers experience large losses over the long term" (pg. 5).

similar costs of job loss to high wage and tenure workers suggests displacement can be disruptive even for workers who have not obviously sorted into or built up advantageous positions in the labor market. Not all low-wage jobs are created equal, however. For instance, our evidence suggests that replacing a full-time, unionized, janitorial job in a public school is likely more challenging than finding new work as a bartender. If workers lucky enough to have held a particularly "good" low-wage job initially drive our results, there must be sufficient job quality heterogeneity in the low-wage labor market to make overall displacement costs quite significant.

More broadly, our results also provide new evidence on the impacts of job loss for a population disproportionately at risk but as yet understudied, complementing related work on the returns to tenure and experience for low-skill workers (Gladden and Taber, 2000; Andersson et al., 2005; Card and Hyslop, 2005; Dustmann and Meghir, 2005). By combining administrative and survey data, we make progress in measuring nonwage and participation responses to job loss. In Jacobson et al. (1993)'s original analysis, the 25% of observations with zero long-run earnings are dropped. While some recent work makes similar restrictions (e.g., Lachowska et al., 2020), others have found that accounting for zero earnings substantially impacts long-run losses (e.g., Von Wachter et al., 2009; Bertheau et al., 2023). Our combined data sets allow us to observe all activity across the U.S., labor force status, weeks and hours worked, and wage rates, all of which are usually only observed in the Displaced Workers Survey (e.g., Farber, 2004). Finally, we develop an alternative methodology for identifying the effects of job loss that accommodates the inclusion of a broader sample of workers, extending the classic mass-layoff design of Jacobson et al. (1993).

Our study also relates to the long-standing literature on labor supply and hours constraints (e.g., Altonji and Paxson (1988)). These constraints might be particularly important in the low-wage labor market. Dube et al. (2022), for example, find that surveyed Walmart workers report a strong preference to work more hours, despite the fact that the survey was conducted when unemployment rates were at historic lows.[4] Lachowska et al. (2023) study hours constraints by combining revealed preference firm rankings with two-way fixed effect decompositions of firm and worker components of hours. They find that low-wage workers' hours are particularly constrained from above. Our results on the importance of reductions in weeks and hours for displaced low-wage workers' earnings losses suggest that these constraints play a key role in the low-wage job ladder.

[4]Other work explores the gap between actual and desired hours more broadly (e.g., Kahn and Lang, 2001; Faberman et al., 2020).

1 Data and sample construction

1.1 Data sources

Our primary source of earnings data is the Census Bureau's LEHD program. The LEHD data consists of quarterly unemployment insurance earnings records shared with the Census by all fifty states and the District of Columbia, covering 96% of private sector jobs (Abowd et al., 2009) and all state and local government workers. Federal employees, self-employed workers, and some agricultural work are excluded. Census-approved projects must seek approval from individual states to access their LEHD data. Twenty-one states (including D.C.), covering 45% of the total U.S. population, approved our request.[5] We also have access to a separate file that indicates whether an individual had earnings in *any* state, including those that did not approve the study, allowing us to construct an indicator for having any LEHD earnings nationally.

Firms in the LEHD data are identified by state employer identification numbers (state-EINs), which correspond to the entities reporting UI taxes to state authorities. A single EIN may encompass multiple establishments within a state, while some firms operate under multiple EINs. As a result, our employer identifier is typically smaller than a firm but larger than an establishment. LEHD data contain a separate quarterly earnings record for each worker-firm pair. We transform this data into a worker-level panel in each state by keeping the top-paying employer in each quarter as well as the sum of earnings from all employers. We inflate all earnings information to 2020 real dollars using the Consumer Price Index (CPI). The vintage of LEHD data we use covers employment from the 1990s through 2014, with exact start dates depending on the state. The records also contain information on several firm characteristics, such as North American Industry Classification System (NAICS) codes.

A key limitation of UI-based earnings records in the U.S. is that they do not include information on hours worked, weeks worked, or hourly wages.[6] Our second data source, individual survey responses to the ACS, helps fill this gap. We have access to full ACS responses from 2001 to 2020. These responses include the date of response, demographic information such as age, sex, race, and education, and information on labor market activity, including employment status, usual hours, weeks worked, and earnings over the last year. The ACS constructs an hourly wage measure defined as total wage earnings divided by the product of usual hours and total weeks worked. We also use the fact that ACS enumerates individuals in Group

[5] These states are Arizona, Arkansas, California, Colorado, Delaware, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Montana, Nebraska, Nevada, New Mexico, Ohio, Oklahoma, Tennessee, Texas, and Wyoming, as well as the District of Columbia.

[6] Washington State is one exception and does collect information on hours (Lachowska et al., 2020), although this data is not part of the LEHD data we have access to.

Quarters, which includes correctional facilities, to construct a measure of incarceration. As in LEHD, all ACS outcomes are CPI-adjusted to 2020 dollars.[7]

Both data sets include de-identified Protected Identity Keys (PIKs) generated by the Census Bureau. PIKs are person identifiers created using social security numbers, names, sex, dates of birth, and address information with reference to the Social Security Administration's Numident file and other administrative sources. We use PIKs to longitudinally track workers over time within LEHD data, to link workers between the LEHD and ACS data, and to link respondents across multiple ACS surveys over time.

1.2 Sample construction

Our primary sample is constructed by linking cohorts of low-wage workers identified in the ACS to the LEHD data. We restrict attention to ACS respondents who are civilian employees, are at work, and whose hourly wage rate falls below $15 per hour in 2020 dollars.[8] To focus on workers who are out of school and unlikely to retire in the near future, we also restrict attention to workers aged 22 to 50. We make no restrictions on job tenure, education, experience, industry, or occupation.

In our primary analysis, we focus on individuals who initially report usually working 40 or more hours per week and working at least 51 weeks in the last year (not necessarily with the same employer, and including paid time off, vacation, and weeks with only a few hours of work). Limiting to full-time, full-year workers ensures the sample consists of attached workers likely to search for new work if displaced and reduces potential measurement error in the constructed ACS hourly wage measure (Baum-Snow and Neal, 2009). The resulting sample includes roughly 80% of full-time low-wage workers and over half of all low-wage workers, as shown in Figure A.1. In our heterogeneity analyses, however, we also examine effects on part-time workers and drop restrictions on weeks worked in the previous year.

This sample of initial ACS respondents is then matched to LEHD records for the state where the respondent reports working and the year and quarter of ACS response. We refer to the matched firm as the worker's "initial" firm.[9] We then construct a panel of LEHD earnings outcomes for three years prior and six years after this date for each worker. In what follows, $t = 0$ refers to the quarter of the initial ACS response in which we identified the worker and matched them to their LEHD records. In the primary analysis, we use initial ACS responses

[7] We winsorize earnings in the LEHD data, and total income, household income, wage earnings, and hourly wages in the ACS data. All winsorization is done at the 99th percentile, excluding zeros, within state.

[8] To reduce measurement error, we drop observations with implausibly low hourly wages (below $2).

[9] Workers who do not match to an initial firm (and thus are potentially working in jobs not covered by UI or mismatched) are dropped.

from 2001 to 2008 to define the sample, ensuring their earnings can be observed in the LEHD for at least three years prior and six years afterward.

Some individuals are randomly re-sampled by future ACS waves as well, allowing us to observe follow-up responses on labor market activity and other outcomes after $t = 0$. Although many workers will not be re-sampled by the ACS, those that do should reflect a random fraction of the full sample. Since 2011, the ACS has interviewed about 2.2-2.3 million housing units each year, or about 1.5-2% of the total stock.[10] All housing units in the U.S. are assigned to one of five representative sampling sub-frames, with units for each survey-year drawn from each frame in rotation. Thus while some individuals who change households may be re-sampled at any point after the initial response, the bulk are re-sampled five years later when the census returns to the sub-frame that contains their housing unit. When studying impacts on outcomes recorded in ACS re-samples, we use initial ACS responses from 2001 to 2014 to maximize the sample size while still ensuring outcomes are observed for at least six years. We then attach any follow-up survey responses through 2020 to the panel. We call this panel the "ACS follow-up sample."

Since our sample construction always begins with an initial set of ACS respondents, many workers in the LEHD are excluded because they were not sampled by the ACS over the sample period. We use the full set of workers not in our analysis as a holdout sample to construct our instrument and the firm-level controls used in the main analysis. These measures include the change in firm-level employment over the next four quarters, total firm size, the share of workers who are new to the firm, average separation rates, separation rates into non-employment, average and median wages, and the 25th, 75th, and 90th percentiles of wages for each firm and quarter. Constructing these measures using the holdout sample ensures that firm characteristics and employment changes are not mechanically related to the labor market activity of workers in the analysis itself.

Finally, to compare effects on low-wage workers to effects on higher-wage workers more similar to those in previous studies, we construct a second sample using an identical process but restricting to initial ACS respondents whose hourly wage falls between \$15 and \$30 per hour instead of below \$15. We construct the same panels of outcomes for these workers, including one sample of initial respondents from 2001 to 2008 that we track in LEHD data and a second sample of respondents from 2001 to 2014 that we track in follow-up ACS responses.

[10] Information on ACS sample sizes can be found here, while Census estimates of total housing unit estimates are available here.

1.3 Summary statistics

Table 1 presents summary statistics for the full analysis sample of 233,000 low-wage workers.[11] The sample is 44% male, 82% white, and 36 years old on average. Consistent with their low wages, levels of education are low relative to population averages, with only 15% holding a bachelor's degree. Information recorded in the initial ACS response shows that workers' total earnings were roughly $26,000 in the year prior to $t = 0$, with the vast majority (96%) comprised of wages. Household earnings are more than twice individual earnings because most workers are married or living with partners who also work.[12] By construction, average weeks worked is about 52 and median hours is 40. The resulting hourly wage averages $11. According to public-use ACS data, 27% of all full-time workers earn an hourly wage of less than $15 per hour, and more than 42% of full-time workers without a high school degree earn below $15 per hour, as shown in Figure A.2.

Earnings recorded in LEHD data show similar levels of labor market activity to self-reported ACS measures. Total earnings at $t = 0$ is about $8,600, with total earnings over the prior four quarters reaching $32,600.[13] The median worker has spent seven quarters with the same firm and 14 quarters in the same two-digit NAICS industry. The largest industries include manufacturing, retail trade, and health care/social assistance, which make up roughly 15% of the sample each. However, many workers are employed in industries beyond the top five listed, such as wholesale trade and freight and logistics. Due to which states approved our LEHD access request, we have no workers from states in the Northeast, but the rest are distributed across the Midwest, South, and West Census regions.

Table 1 also presents summary statistics for the set of low-wage workers who are re-sampled by the ACS four to six years later. There are 45,000 workers in this sample (about 20% of the total). This figure is higher than what is implied by random re-sampling alone due to the extension of the initial ACS response window from 2008 to 2014, which more than doubles the total number of initial respondents.[14] Despite the change in sampling frame, these respondents appear highly similar to the overall sample in terms of demographic character-

[11] The final sample shown here also reflects several further restrictions based on firm characteristics detailed when describing our empirical strategy below.

[12] This difference is likely exacerbated by our sample selection rule, which requires focal individuals to have low wages (and hence low earnings). Among individuals re-sampled by the ACS four to six years later, average individual earnings are almost exactly half of household earnings.

[13] Given that the sample is constructed by conditioning on imputed ACS hourly wages below $15, it is natural that total ACS wage earnings are slightly lower than LEHD earnings. At the average weeks and hours worked reported in initial ACS responses, LEHD earnings imply average wages of $14 per hour.

[14] ACS final interview sample sizes increased substantially in 2005 (from roughly 500,000 interviews to nearly two million) and again in 2011 (up to 2.2-2.3 million). Because the ACS samples about 2% of all households each year but draws them from a rotating set of five sub-frames, a household surveyed in a given year has about a 10% chance of being re-sampled five years later.

istics, income and employment, and LEHD earnings at $t = 0$. Average reported earnings in the initial ACS response remain roughly \$27,000, for example, slightly less than reported earnings in the last four quarters in the LEHD. Measured tenure and industry-experience are slightly longer due to the fact that this sample comprises more records in periods longer after each state's LEHD data begin.

To illustrate the impact of our sample restrictions, Table A.1 presents comparable summary statistics for all ACS workers in our LEHD approving states, full-time workers, full-time workers earning hourly wages less than \$15 per hour, and full-time workers earning hourly wages of \$15 to \$30 per hour. Compared to all workers, low-wage workers have significantly less educational attainment. For example, only 13% have a bachelor's degree compared to 34% among all workers. They are also more concentrated in industries such as retail trade and accommodation and food services. Table A.2 shows that full-time low-wage workers are most likely to work as retail and sales workers, secretaries and administrative assistants, drivers, chefs and cooks, and janitors. These occupations alone cover 16% of all full-time low-wage workers in the ACS. The full population of low-wage workers in the ACS is broadly similar to our ultimate analysis sample in terms of demographic composition, education, earnings, and industry of employment. Restricting to observations that match to the LEHD at $t = 0$ and imposing the sample restrictions necessary for our empirical design described below increases the white and female share of our final analysis sample, however.

2 Empirical strategy

2.1 The instrument

Our empirical strategy uses firm-level year-over-year changes in employment as proxies for idiosyncratic shocks to labor demand. These shocks are defined for firm j in quarter t as total employment in quarter $t+4$ divided by employment in quarter t.[15] Employment is measured in the holdout sample of workers otherwise excluded from our analysis. Using year-over-year changes limits the impact of seasonal fluctuations in employment. To reduce noise and exclude very new firms, we limit the sample to workers whose firms had at least 25 workers observed in the holdout sample and were active for at least four quarters prior to $t = 0$. The median firm-level "shock" in the analysis sample is one, implying most firms experience no change in total employment. The standard deviation is significant, however, at 17%. We do

[15] Using this shock as an instrument is also inspired by Davis et al. (2012), who observe that layoff rates increase smoothly in year-over-year firm growth rates with a sharp kink at zero. The approach is related yet distinct from von Wachter and Bender (2006), who construct a continuous instrument based on firm-level fluctuations in retention rates to study the impacts of job loss on German apprentices. It leverages within-firm variation in shocks, rather than between-firm variation (as we do).

not exclude complete shutdowns, so some firms experience 100% reductions.[16]

For each worker in the analysis sample, we assign the employment shock for the firm-quarter matched to their initial ACS response at $t = 0$. The resulting instrument, which we denote Z_i, is constant over time for each worker observation i. Because treatment varies at the firm rather than the worker level, however, regressing outcomes on Z_i involves implicit comparisons between workers at firms that receive larger versus smaller employment shocks. We analyze outcomes at the worker-level to easily incorporate additional worker-level controls and examine effect heterogeneity, but cluster our inference by firm.

2.2 The controls

Because employment shocks are not randomly assigned across firms, a key threat to our design is that they are correlated with differences in workers' skills, preferences, or labor market conditions. To account for this threat, we control for characteristics of workers' initial firms at $t = 0$ in our regressions. These characteristics consist of measures calculated in the holdout sample, including logs of firm size, average, median, 10th, 25th, and 90th quantiles of wages, average separation rates, average new worker accession rates, and average separations into non-employment averaged over the four quarters prior to $t = 0$. Computing these characteristics in the holdout sample ensures that there is no mechanical link between analysis sample workers' employment history and the controls.

To ensure that the results do not simply reflect local or industry-specific labor demand shocks, we include fixed effects for state interacted with the two-digit NAICS code of the worker's initial firm and with the year and quarter of initial ACS response. This implies our effects are estimated using variation among workers initially employed in the same industry and state at the same calendar time. We also interact a third-degree polynomial of firm characteristics with three levels of worker tenure at $t = 0$ defined (in quarters) as $[1 - 4]$, $[5 - 12]$, and ≥ 13. Finally, we control for the worker's initial hourly wage adjusted to 2020 dollars. Our identifying assumption is that, conditional on these controls, firm-level shocks are independent of workers' potential outcomes. We present several validation tests of this assumption below using workers' observable characteristics. Because our controls do not include information on workers' past labor market outcomes beyond the tenure interaction, none of these characteristics are mechanically balanced by our design.

[16] Because firms are identified only by administrative labels in the LEHD, some large employment reductions may reflect relabeling or mergers. To reduce the influence of any resulting measurement error, we take the maximum of the measured employment change and the fraction of coworkers working in the same firm one year later as our final shock measure. We also exclude year-over-year changes above 200%.

2.3 Empirical specification

Our first empirical specification simply estimates the reduced-form effects of shocks on outcomes measured t quarters after the initial ACS response:

$$Y_{it} = X_i'\alpha_t^0 + \gamma_t Z_i + \psi_{t,n(i),s(i),q(i)} + e_{it} \tag{1}$$

where $\psi_{t,n(i),s(i),q(i)}$ are fixed effects for 2-digit NAICS industry codes ($n(i)$) by state of main employer at $t = 0$ ($s(i)$) by calendar time (year and quarter) of initial response to the ACS ($q(i)$), X_i includes worker i's hourly wage at $t = 0$ and the interaction of initial firm characteristics and worker initial tenure, and Z_i is the shock. We estimate this specification using ordinary least squares separately for each t, with standard errors clustered by firm.

We also present 2SLS estimates of the following system of equations:

$$\begin{aligned} Y_{it} &= X_i'\alpha_t^2 + \beta_t S_i + \psi^1_{t,n(i),s(i),q(i)} + \eta_{it} \\ S_i &= X_i'\alpha^1 + \omega Z_i + \psi^2_{n(i),s(i),q(i)} + \epsilon_i \end{aligned} \tag{2}$$

where S_i is an indicator for worker i's job separation. Our preferred estimates use separation within a year of the initial response (i.e., within $t \in [1, 4]$) since this is the same time window over which firm-level shocks are measured and, as we show below, is the horizon at which effects on separation are largest. Since β_t is simply the reduced form coefficient γ_t rescaled by ω, it is straightforward to see how estimates would change using alternative definitions of S_i.

2.4 Why this strategy?

Since Jacobson et al. (1993)'s pioneering study, "mass-layoff" research designs have been the predominant approach to studying job displacement using administrative data. This approach compares the outcomes of high-tenure job-leavers at distressed firms to a matched sample of job-stayers. Davis and von Wachter (2011), for example, study workers with at least three years of prior job tenure who separate from large firms that experience persistent employment contractions of 30 to 99%. They compare these "treated" workers' outcomes to those of similar "control" workers who do not separate from their jobs.[17] It assumes that, but for the mass layoff, treated workers would have stayed in their jobs.

Low-wage workers, however, often do not remain continuously employed for several years and experience frequent job turnover. Median job tenure in our sample at $t = 0$, for example, is seven quarters. The set of high-tenure low-wage workers may represent a relatively selected

[17] Krolikowski (2018) shows that estimates can be sensitive to whether and for how long control workers are required to remain in their jobs. Couch and Placzek (2010) show that estimates can also be sensitive to whether job losers are restricted to those who claim unemployment insurance (i.e., dropping individuals who find alternative jobs soon after displacement). Our approach avoids both these challenges.

sample that is unlikely to be representative of the broader low-wage workforce. Even among these workers, however, some who separate from their employer while it is distressed may still do so voluntarily (Flaaen et al., 2019). Our approach allows us to include all workers regardless of tenure while accounting for endogenous separations. By using all the variation in firm-level employment changes, we both increase precision and avoid the need to define a specific threshold above which firms qualify as distressed. Thus, we view our approach as the one best suited for the questions that motivate our analysis and the available data.

Our empirical approach can also be used to study job loss in other populations (e.g., among high-tenure workers). Compared to the classic mass-layoff design (Jacobson et al., 1993), the advantage is that it does not require conditioning on pre-layoff earnings, restricting to high-tenure workers, assuming that all separations are involuntary, or using matching to find a comparable control group. The key disadvantage is that identifying the effects of job loss requires an exclusion restriction. This assumption requires firms to respond to shocks by laying off workers rather than adjusting incumbents' wages. The extent to which firms do so is the subject of an active debate in macroeconomics (e.g., Elsby and Solon, 2019; Davis and Krolikowski, 2025). However, if the reduced-form impact of exposure to a firm-specific demand shock is of interest in its own right, then exclusion is not required.

2.5 Validation tests

As noted above, our design requires that firm-level demand shocks are independent of differences in workers' skills or preferences. Taking Equation 1 as a structural relationship, this assumption requires that $Cov(Z_i, e_{it}) = 0$. Figure 1 tests this assumption by regressing various worker characteristics on Z_i. For comparison, we also include regressions of these characteristics on the endogenous variable, S_i, with and without firm-level controls. These estimates are indicated by the hollow circular and diamond markers, respectively, while regressions on the instrument Z_i are indicated with solid square markers.[18] We would expect OLS estimates of the effects of job loss to be severely biased if S_i is strongly correlated with these characteristics, motivating our use of an instrumental variable instead.

The results show that job separation is strongly correlated with workers' prior labor market activity. Workers who separate, for example, have 14% lower earnings, are more likely to have had zero earnings prior to $t = 0$, and have experienced more frequent transitions from employment into non-employment. This pattern is consistent with theoretical models that predict negative selection into non-employment (e.g., Gibbons and Katz, 1991). Including firm-level controls reduces the imbalances somewhat, but meaningful differences remain be-

[18] Table A.3 reports the point estimates used to construct Figure 1. It also reports balance tests for the higher-wage sample of workers that we discuss in Section 6.

tween those who separate and those who do not. For example, separating workers have roughly 5% lower earnings and face a 10% higher likelihood of transitioning from employment into non-employment. Our instrument, by contrast, has no economically meaningful or statistically significant correlation with any of these labor market characteristics, supporting the assumption that it is orthogonal to other unobserved determinants of workers' outcomes.

Workers' demographic characteristics show a similar pattern. Job separators are younger and more likely to be male, white, and less educated. The instrument has no significant correlation with all of these characteristics except for age.[19] To summarize any potential imbalance, we use a covariate index, "Predicted earnings," formed as the fitted values from a regression of earnings prior to $t = 0$ on all available covariates. Though job separation is strongly negatively correlated with this covariate index, the instrument is not, again supporting the identifying assumption.

To demonstrate that our instrument captures idiosyncratic, firm-specific labor demand shocks, we conduct two additional analyses. First, we show that our estimates change little when controlling for county-level unemployment rates or more granular fixed effects, such as commuting zone-by-3-digit NAICS-by-year and quarter of initial ACS response. The results of these sensitivity tests are discussed after presenting our main results. Second, we show that shocks are not correlated across firms in the same local labor market. We do so by randomly permuting firm shocks within a market and examining the effects of these "placebo" shocks on firm's own shocks and workers' outcomes. If the shocks capture common, local factors as opposed to idiosyncratic variation, we would expect other firms' shocks to be correlated with firms' own shocks and their workers' outcomes. Table A.4, however, shows that shocks to other firms in the same market are not predictive of the firm's own shock, its rate of job separation by $t = 4$, or its initial workers' long-run earnings at $t = 24$.

Although orthogonality of the instrument alone is sufficient to consistently estimate the causal effects of firm shocks in Equation 1, the 2SLS model in Equation 2 requires additional assumptions. First, we require that Z_i only affects outcomes through S_i, an exclusion restriction that may be violated. Demand shocks may affect workers who do not separate through reductions in hours and wages, for example. We show below, however, that exclusion may be a reasonable approximation in our setting. Interpreting Equation 2 through the nonparametric local average treatment effect framework (Imbens and Angrist, 1994) requires several additional assumptions. The first is monotonicity, which implies that each worker only becomes weakly more likely to separate as the shock size increases. This assumption

[19] Results change little when controlling for all demographic characteristics.

seems natural in our setting. Because our regression specifications invoke a parametric structure through the additive separability in the controls, we also require that this linear model is a good approximation to the conditional mean of the instrument given the covariates (Blandhol et al., 2022).

3 The causal effects of job loss

3.1 Effects on LEHD outcomes

We start with the reduced-form effects of firm-specific labor demand shocks on low-wage workers. Figure 2 Panel A plots dynamic effects on an indicator for any job separation, defined as having zero earnings in quarter $t+1$ from the primary employer as of quarter-t, as well as an indicator for employment at the worker's $t=0$ firm, which is the employer that was matched to their initial ACS response used to create the sample. Each dot corresponds to the coefficient and 95% confidence interval on Z_i from a separate regression for outcomes measured t quarters from the initial ACS response. Given the scale of the instrument, effect sizes can be interpreted as the impact of a 100% reduction in employment in the leave-out sample (i.e., a firm shut down).

Consistent with the validation tests discussed in Section 2.5, there is no reduced-form relationship between the instrument and any labor market outcomes in the three years prior to $t = 0$. Separations then rise sharply, peaking four quarters later at 18%. They then decline but remain elevated for several further quarters. These later separations may reflect additional job changes as workers find new jobs after separating from their initial employer. After $t = 8$, however, we see no evidence that severely shocked workers experience long-run increases in the likelihood of job separation.

As a result of the spike in separation rates, the likelihood that the worker remains employed at their initial firm declines sharply, falling by 50% by $t = 4$. Over time, the effects of working with the same employer decay as turnover increases for all workers. Six years after the initial ACS response, however, heavily shocked workers are 20 percentage points less likely to remain with their initial employer, indicating that a large share of workers would have enjoyed long employment spells at their firm if not displaced. Consistent with overall high turnover rates, however, remaining employed at the same firm at this horizon is less common; the sample mean is about 33%.

Figure 2 Panel B plots reduced-form effects on an indicator for any earnings and total earnings in the LEHD using the same empirical approach. The patterns mirror those in Panel A. The probability of having any earnings declines sharply, bottoming out at -12 percentage points in $t = 4$. Earnings rates then recover slowly over the next five years, with effects of a 100% shock remaining at about two percentage points in $t = 24$. Because this outcome uses the indicator for any earnings in *any* LEHD state, including those where we cannot observe earnings levels, this persistent gap is unlikely to be due to differential migration-based attrition.[20] The second series in Panel B shows that total quarterly earnings follow a similar pattern to any-employment earnings. Six years after the initial ACS response, heavily shocked workers have \$500 lower earnings per quarter, or about 7% of the mean.

Although these effects are reduced forms, it is straightforward to gauge the magnitude of corresponding 2SLS estimates of the effects of job loss. Panel A, for example, shows that the first-stage effect on job separation by $t = 4$ is roughly 0.5. The 2SLS estimates are thus roughly twice the reduced form estimates. Earnings declines in $t = 4$ would be about \$3,700, or 46% of the mean, and the largest effect on the probability of having any earnings would be roughly 24 percentage points. Since the effect on job separation is largest at $t = 4$, re-scaling by effects on job separation by $t = 2$ or $t = 3$ would imply significantly larger 2SLS effects.

Table 2 presents point estimates for long-run effects on these earnings and employment outcomes, as well as several others. For completeness, the table reports the outcome mean, the reduced form estimate, and the 2SLS estimate taking job separation by $t = 4$ as the relevant endogenous variable. For any separation, having the same employer, any earnings, and earnings levels, these effects correspond to the rightmost points in Figure 2. The point estimate for long-run 2SLS effects on separation from workers' initial employer, for example, is 39 percentage points.

Job loss generates a lasting reduction in earnings. At $t = 24$, quarterly earnings are lower by \$983 (13% relative to the mean), and earnings in the last four quarters are lower by \$4,070, which is also 13% relative to the mean. Moreover, effects on cumulative labor market outcomes summing over $t = -1$ to $t = 24$ are substantial. Workers lose a total of 1.9 quarters of labor market experience and about \$42,000 in earnings on average. These cumulative earnings losses are about 20% of the sample mean and 130% of average earnings over the last four quarters at $t = 0$.[21]

Table 2 also shows that a large share of the estimated effect of job loss on having any earnings is explained by non-employment for at least eight continuous quarters (3 percentage points

[20] Table 2 shows that the reduced-form effect on having any earnings in the 21 states with observed earnings records is about 0.5 percentage points more negative than the national effect, possibly reflecting migration responses. We revisit this issue when analyzing ACS outcomes below.

[21] Figure A.7 reports estimates from the prior literature. Relative to pre-displacement earnings, our effects are similar to those in Lachowska et al. (2020) and Couch and Placzek (2010) but smaller than those reported in Jacobson et al. (1993).

out of 3.3), which represents a 38% increase relative to the mean. This suggests job loss causes a meaningful share of workers to opt out of labor force participation, a result we confirm using ACS questions on labor force status below. This finding is consistent with past findings that low-skilled workers are less attached to the labor market (Juhn et al., 1991). Some workers may also simply have strong outside options that rival the returns to searching for new work. Taking care of family members at home, for example, may be a better option than seeking re-employment. By construction, however, all workers in the sample held full-time jobs as of $t = 0$ and thus at one point found it worthwhile to fully participate in the labor market. Persistent non-employment responses to job loss may therefore reflect either changes in outside options or high costs of renewed search.

Some models of "slippery" job ladders (Krolikowski, 2017; Jarosch, 2021) imply lasting impacts on job stability—i.e., persistent increases in the likelihood of job separation. However, Figure 2 and Table 2 show precise null effects on job stability after six years.[22] Total separations increase by 1.4, indicating that job loss generates an additional 0.4 separations on average. Some of these separations may reflect voluntary job changes as workers navigate finding suitable re-employment opportunities. Figure A.3 decomposes the effects on separations by destination: another job or non-employment. The majority of separations are to other jobs, and effects on job-to-job transitions persist longer than effects on separations to non-employment. However, after two years, neither type of separation is affected, indicating no remaining differences in job stability.

3.2 Extensive- versus intensive-margin effects

Several exercises demonstrate that the majority of the effects cannot be explained solely by reductions in employment and must also reflect intensive-margin reductions in weeks and hours worked, as well as hourly wage rates. Table 2, for example, reports impacts on an indicator for having quarterly earnings below $6,000. The 2SLS estimate of the effect of job loss on this outcome is nearly seven percentage points. This effect is about 2.4 percentage points larger (in absolute terms) than impacts on having any earnings in one of our 21 LEHD states, implying that there must be a meaningful shift in earnings to levels above zero but below $6,000 per quarter as a result of job loss.

We also estimate treated and untreated earnings levels for workers induced to separate by our instrument (Imbens and Rubin, 1997). At $t = 24$, treated and untreated earnings levels are $7,032 and $8,015, respectively, while treated and untreated rates of any earnings in our LEHD states are 76.7% and 81.1%, respectively. If treated workers with any earnings

[22] Stevens (1997), by contrast, used the PSID and found lasting increases in job instability following displacement.

had the same average earnings as control workers with any earnings, then the total effect on earnings would be \$435, or 44% of the actual effect.[23] Put differently, the total effect also reflects sizable differences in mean earnings among those with positive earnings. These means, shown in Figure A.11, indicate a \$724 intensive-margin reduction at $t = 24$.[24]

3.3 Tests of exclusion

Is it reasonable to assume that all effects of labor demand shocks flow through job separation by $t = 4$, as our 2SLS estimates do? Figure 3 provides one assessment. Each panel is constructed by discretizing the instrument into a bin for constant employment growth ($Z_i = 1$) and indicators for increasingly severe shocks. The most severe bin corresponds to year-over-year decreases in employment of 50% or more.[25] We then estimate the effect of a shock in each bin on the likelihood of job separation by $t = 4$ and outcomes measured at various horizons, leaving the least severe category as the omitted group. The resulting "visual instrumental variables" plot shows how reduced-form effects scale with impacts on the first stage (Holzer et al., 1991; Angrist, 1990). In a constant effect model with a valid (i.e., excludable) instrument, we would expect all the dots to fall on a line passing through the origin, up to sampling error. The slope of this line is an estimate of the causal effect of job loss on outcomes.

Panel A plots estimates for an indicator for any earnings at $t = -12$, $t = 12$, and $t = 24$. Consistent with the validation tests reported above showing that the instrument does not predict outcomes prior to the shock, effects at $t = -12$ are close to zero, and the slope is flat. Effects at $t = 12$ increase linearly with effects on job separation. The line of best fit passing through the origin that is plotted has a slope of -0.072, indicating large short-run impacts on employment nearly identical to the 2SLS estimate implied by the reduced-form effect shown in Figure 2. Effects at $t = 24$ show a similar pattern, scaling linearly with effects on job loss at a rate of -0.052, close to the long-run 2SLS effect reported in Table 2.

Panel B shows that results are similar when using quarterly earnings as the outcome. Prior to the shock, there is little evidence that workers' outcomes differ systematically with the level of the coming shock. The implied causal effect on earnings at $t = 12$ and $t = 24$ are -\$1,108 and -\$817, respectively. Both are close to the 2SLS estimates reported earlier. It is possible to test the constant-effects model formally by constructing J-test of the over-

[23] Untreated compliers' quarterly earnings conditional on positive are \$8,015/0.811 = \$9,883. Earnings levels among treated compliers would be \$9,883·0.767 = \$7,580. The resulting effect on earnings would be \$8,015 - \$7,580 = \$435.

[24] If there is positive selection into employment among treated compliers (i.e., because higher skilled workers are more likely to find new work), then this estimate potentially understates the intensive-margin effect.

[25] For simplicity, we exclude the small subset of shocks > 1, which indicate employment growth.

identifying restrictions in the 2SLS model that uses bin indicators as instruments. These tests fail to reject for all outcomes at $t = -12$, $t = 12$ and $t = 24$. We therefore view the evidence as consistent with our view that 2SLS models using job separation by $t = 4$ as the endogenous variable are appropriate.

We also probe the sensitivity of our results to the inclusion of more granular levels of fixed effects and controls for local labor market conditions. Table A.5 reports reduced form effects on long-run earnings in these alternative specifications. The inclusion of county-level unemployment rates does not impact the estimates. Interacting calendar time fixed effects with commuting zones, 3-digit NAICS, or both, all yield similar effects. That is, although the inclusion of commuting zone by 3-digit NAICS increases the R^2 from 0.18 to 0.49 (more than double), the reduced form effects are similar; if anything, the point estimate slightly increases from 492 to 544. The results in Table A.5 reinforce our interpretation of the instrument as capturing only firm-specific shocks that are conditionally unrelated to changes in local labor market conditions.

Finally, we explore the option of using only nonpositive year-over-year employment changes, excluding from the analysis firms that experience employment growth. Table A.6 reports our main effects on LEHD outcomes and finds similar estimates as in Table 2, lending further support to the research design.

3.4 Heterogeneity across demographic groups

Figure A.9 explores effect heterogeneity across various important demographic sub-groups. The results show first that earnings and employment impacts are similar for men and women, though if anything, they are slightly more negative for men both in quarterly earnings as well as in employment. To the extent that men and women have different outside options in home production, this finding suggests our results are not driven primarily by labor force dropout after job loss motivated by substitution to alternative activities like childcare. We also find similar effects on white and non-white workers, although standard errors are large for the relatively small non-white sample.

Overall, we find little evidence of effect heterogeneity across demographic groups. To further support this conclusion, Figure A.10 plots reduced-form effects on earnings and employment outcomes against first-stage effects on job loss by subgroup. If the causal effects of job loss are homogeneous and the exclusion restriction holds, the slope of the fitted line should equal our primary 2SLS estimates. The slope and corresponding 2SLS estimates from Table 2 are shown in the top-right corner of each plot. The results are nearly identical to, and statistically indistinguishable from, our main estimates.

3.5 Effects for part-time and part-year workers

Our primary analysis focuses on individuals who reported usually working at least 40 hours per week for 51 weeks in the previous year as of $t = 0$. While this sample captures most of the full-time low-wage workforce, part-time and part-year workers may experience different consequences of job loss. Both groups may be less attached the labor force overall, for example, and thus show larger participation responses to displacement. Table A.7 examines these questions by estimating impacts on the complete population of full-time low-wage workers and the population of part-time low-wage workers separately.[26] Effects on the former are similar to the baseline estimates in Table 2, indicating that including part-year workers does not materially change our estimates. Part-time workers experience quarterly earning losses of \$600 six years after job loss. These smaller absolute effects of displacement are consistent with part-time jobs being more readily available and higher churn among non-displaced part-time workers. Relative to the mean, earnings effects on part-time workers are also smaller than impacts on full-time workers (11% vs. 15%). Impacts on long-term non-employment account for all of the observed impacts on employment, consistent with the reduction in employment reflecting workers dropping out of the labor force.

4 Effects on follow-up ACS outcomes

To better understand the sources of long-run earnings losses, we next turn to effects on the ACS follow-up sample in Table 3. Since only a fraction of workers are ever re-sampled by the ACS, here we pool quarters 16 to 24 post-layoff to maximize power. Only observations with at least one additional ACS response in this window are included. Despite these differences, the first set of results in Table 3 shows that we find similar earnings impacts as in the LEHD data. 2SLS effects on total income, wages, and household income are -\$5,200, -\$4,700, and -\$6,900, respectively, although standard errors are large enough that we cannot reject that all three effects are the same. The ACS income question asks about earnings over the prior year, so these effects should be compared to impacts on earnings over the last four quarters reported in Table 2. Consistent with the time horizon including periods closer to the initial shock, earnings reductions are slightly larger here than in Table 2. Since ACS earnings outcomes include income from any source—including self-employment—in any location, these results also imply the earnings declines in Table 2 are not attributable to differential attrition from UI-covered jobs in the states where we have LEHD access.

The next set of results shows that job loss leads to a 5.8 percentage point reduction in the likelihood of being employed. This difference reflects an increase of 3.2 percentage points

[26]Part-year full-time workers are too small a population to examine separately.

in the probability of unemployment and an increase of 2.6 percentage points in labor force dropout. Employment in the ACS refers to the previous week, however, among those who did not work, they are also asked about whether they actively searched for work in the previous four weeks. Effects on this outcome stand at 4.1 percentage points, implying that job loss leads to a sizable increase in the probability a worker is still trying to find a job four to six years after the initial shock. At this time horizon, the initial shock of job loss has likely worn off, and workers are likely to have exhausted available unemployment benefits. Very few respondents are likely to still report being on layoff, for example, consistent with the lack of effects on job separation documented in Figure 2.[27]

The next panel of Table 3 estimates effects on weeks worked, usual hours, and hourly wages. To avoid conditioning on endogenous outcomes, all of these outcomes include zeros, with hourly wages for non-workers set to zero. The results show a reduction of 3.2 weeks worked over the last year, or roughly 7% of the mean. Usual hours worked decline significantly as well, dropping by about three hours per week. Finally, hourly wages decline by about \$1.4 per hour, or 9% of the mean. Some of these wage declines may reflect coding non-workers as having zero wages.

Panel A of Figure 4 provides a more complete decomposition of how employment, weeks, hours, and wage rates account for long-run losses. This decomposition uses simple Oaxaca-Blinder-style manipulations of treated and untreated compliers' means for these outcomes. It is based on the observation that average long-run earnings for compliers, denoted $Y(d)$, can be expressed as:

$$\begin{aligned} E[Y(d)] &= E[Y(d)|Y(d)>0]Pr(Y(d)>0) \\ &= E[weeks(d) \cdot hours(d) \cdot wages(d)|Y(d)>0]Pr(Y(d)>0) \\ &= E[weeks(d)|Y(d)>0]E[hours(d)|Y(d)>0]E[wages(d)|Y(d)>0]Pr(Y(d)>0) \\ &\quad + covariance\ terms \end{aligned} \tag{3}$$

where expectations are taken over the complier population, d indicates treatment status, and $weeks(d)$, $hours(d)$, and $wages(d)$ are weeks worked, hours worked and average hourly wages, respectively. The covariance terms appear because the expectation of the product of weeks, hours, and hourly wages is not necessarily equal to the product of their expectations.

The long-run effect of job loss, $E[Y(1)] - E[Y(0)]$, reflects differences in each of the components in the last line of Equation 3. Figure 4 assesses the reduction in treatment effects from

[27]We find no impacts on incarceration or cross-state mobility. Although the estimates in Table 3 are noisy, given the low rates of incarceration overall, it seems unlikely that criminal justice contact explains long-run earnings or employment declines.

changing each component, the means of which are also reported in the text on the figure.[28] The first bar, for example, measures how much smaller long-run impacts would be if treated compliers (i.e., job losers), 86% of whom have any wage earnings, worked at the same rates as untreated compliers (i.e., job stayers), 90% of whom have any wages, but did not change any other component of their total earnings. The next bar measures change in treatment effects if job losers also worked the same number of weeks as stayers (50.1 vs. 48.6). The next bar equalizes usual hours (42.3 vs. 41). The next bar equalizes hourly wages ($16.8 vs. $15.9). The final bar assigns the same covariance terms to make up the residual.

The results show that whether and how much displaced low-wage workers secure new work explains the bulk of the impacts of job loss. Changes in the likelihood of having any earnings explains 28% of the impact, while reductions in weeks worked and usual hours among workers explain about 20% each. Differences in hourly wages account for the rest of the gap, explaining 38% of the total effect. These differences partly reflect growth in hourly wages for non-displaced compliers, who earn 90 cents more per hour at this horizon. The final residual covariance terms indicate that weeks, hours, and wage rates are *more* positively correlated among displaced than non-displaced workers, the opposite of what one might expect if displaced workers were simply more likely to take higher-paid but part-time work as they searched for new opportunities.

5 Explaining long-run losses

Why would job loss lead to long-run earnings and employment losses for low-wage workers? In this section, we use a rich model of job search to discuss potential mechanisms. The model expands the canonical Burdett and Mortensen (1998) framework to include job heterogeneity in wages, hours, and stability, human capital dynamics, endogenous search effort, and duration dependence in search costs. Some of these features have been used in the previous literature to overcome more standard search-and-matching models' inability to generate long-run job loss scars (Davis and von Wachter, 2011), while others are important mechanisms in the search literature more broadly.[29]

These features also provide several mechanisms that could rationalize our reduced-form results. For example, intensive-margin effects of job loss on earnings and hours in the model

[28] Estimates of these quantities are also presented in more detail in Table A.10.

[29] For example, Krolikowski (2017)'s model features a job ladder with fixed and stochastic components of match productivity. Job loss leads workers to accept jobs more susceptible to endogenous destruction, slowing the climb back up the job ladder. Similarly, Jarosch (2021) explains job loss effects through both human capital depreciation and a job ladder in both wages and job stability. Burdett et al. (2020)'s model emphasizes skill accumulation. Few models focus on effects on participation, however. Adding endogenous search with non-convex costs helps us speak to this outcome.

can come from three potential channels: human capital, which depreciates due to increased time in unemployment, labor-leisure tradeoffs, which mute the benefits of seeking out a new full-time job, and an unfavorable offer distribution, which makes the climb back up the job ladder slow. The model can also generate persistent decreases in employment due to job loss in several ways. First, depreciation in human capital due to the initial unemployment shock makes search less attractive, leading some workers to cease searching entirely or to exert less effort when they do search. Likewise, duration dependence in job finding can lead some unemployed workers to search less or, eventually, give up. Workers are also more likely to accept jobs with higher destruction rates when first exiting unemployment, amplifying any deleterious effects of time out of work.

Although these mechanisms may be able to explain our findings in theory, a separate question is whether they can rationalize our estimated effects in practice. To explore this issue, we calibrate the model using consensus values of parameters from the literature where possible and simple benchmarks from the CPS where not. We then simulate the effects of job loss from the model and compare them to our empirical estimates.

5.1 Model overview

The model is set in discrete time. A worker is either employed or unemployed. Employed workers hold jobs defined by the triplet $\omega = (w, h, \delta)'$, denoting the hourly wage, hours, and job destruction rate. At the start of each period, workers receive flow utility from either their job or being unemployed. Their productivity updates and employed workers transition to unemployment with probability δ. If the job is not destroyed or the worker began the period unemployed, they then pick search effort to determine the probability of receiving a job offer from an exogenous distribution $F(\cdot)$. If they receive an offer, they can choose to accept it or remain in their current state.

Wages are paid per efficiency unit μ_t (i.e., productivity), which depreciates at rate ϕ_u while unemployed and appreciates at rate ϕ_e while employed. We assume that utility over consumption and leisure is given by: $u(c, h) = \frac{c^{1-\sigma}}{1-\sigma} - \chi \frac{h^{1+\eta}}{1+\eta}$. These preferences allow workers to value the potential benefits of lower hours while also providing parsimonious parameterizations of consumption and labor supply responses to wage changes. We assume workers have no extra non-labor income and do not save, so that the uncompensated labor supply elasticity is $(1-\sigma)/(\eta+\sigma)$.[30]

Job offer rates increase with effort e_t according to $\lambda(e_t) = e_t/(1+e_t)$. Effort costs depend on

[30] First order conditions in the static problem require $(wh)^{-\sigma}w = h^\eta$, or $log(w)\frac{1-\sigma}{\eta+\sigma} = \log(h)$. In a dynamic problem, which requires that $h^\eta = \lambda w$, the Frisch elasticity is $1/\eta$.

duration unemployed d_t and are given by $c(e_t; d_t) = n\mathbb{1}\{e_t > 0\} + \kappa(d_t)\frac{e_t^{1+\gamma}}{1+\gamma}$. The parameter n captures the size of fixed costs of search. The parameter $\kappa(d_t)$ governs the scale of variable search costs and increases in d_t to reflect the increasing difficulty of finding a job as unemployment duration grows. For simplicity, we chose the parameterization $\kappa(d_t) = \kappa_0 + \kappa_1 d_t$. Duration $d_t = 0$ if the worker is currently employed and resets each unemployment spell. Finally, γ determines the elasticity of search costs with respect to effort.

The worker's problem can be defined by two Bellman equations. The value of holding a job with characteristics ω to a worker with productivity μ_t is defined by:

$$V(\omega;\mu_t) = \underbrace{u(w\mu_t, h)}_{\textit{flow utility}} + \underbrace{\delta\beta U(\mu_{t+1}, 0)}_{\textit{move to unemp.}} + \beta(1-\delta)\max_{e_t}\Bigg\{\underbrace{(1-\lambda(e_t))V(\omega;\mu_{t+1})}_{\textit{no job offer}}$$
$$+ \underbrace{\lambda(e_t)\int \max\{V(\omega';\mu_{t+1}), V(\omega;\mu_{t+1})\}dF(\omega')}_{\textit{consider job offer}} - \underbrace{c(e_t;0)/\beta}_{\textit{effort cost}}\Bigg\}$$

Likewise, the value of being a μ_t worker unemployed for d_t periods is given:

$$U(\mu_t, d_t) = \underbrace{b}_{\textit{flow utility}} + \beta\max_{e_t}\Bigg\{\underbrace{(1-\lambda(e_t))U(\mu_{t+1}, d_t+1)}_{\textit{no job offer}}$$
$$+ \underbrace{\lambda(e_t)\int \max\{V(\omega';\mu_{t+1}), U(\mu_{t+1}, d_t+1)\}dF(\omega')}_{\textit{consider job offer}} - \underbrace{c(e_t;d_t)/\beta}_{\textit{effort cost}}\Bigg\}$$

where b captures the flow utility value of unemployment from both leisure and any consumption out of unemployment benefits. When the worker is both employed and unemployed, optimal search effort is determined by weighing the costs and benefits. If search is too costly or the returns are too low, optimal effort may be zero.

5.2 Calibrating and solving the model

We draw parameter values for our calibration from a variety of sources summarized in Table A.8. Where possible, we use existing parameter values from the literature. Parameters calibrated this way are the coefficient of relative risk aversion σ, the Frisch labor supply elasticity $1/\eta$, the elasticity of search costs to effort γ, and the speed of human capital appreciation / depreciation (ϕ_e and ϕ_u).

We use simple benchmarks from the CPS to calibrate a second set of parameter values. Specifically, we take all individuals aged 22 to 50 in the Current Population Survey from 2010 to 2019. To better reflect the low-wage population studied in this paper, we then reweight this sample to match the occupation distribution of hourly workers earning less than \$15 / hour. The offer distribution $F(\cdot)$ is taken as the empirical wage, hour, and predicted separation rate distribution of workers transitioning from unemployment to employment across survey waves.[31] Likewise, κ_0 and κ_1 are chosen to match the observed re-employment hazard in this reweighted CPS sample, given all other parameters. Figure A.4 plots the relevant estimates from the CPS.

The remaining parameters are calibrated internally. We pick the scale of the disutility of labor χ such that utility is weakly increasing in hours conditional on the wage at all points in the offer distribution. Doing so makes jobs with longer hours always more desirable, but to the weakest degree possible. Likewise, we set b equal to the flow utility of the worst job on the job ladder, making sure that any job is preferred to unemployment. And finally, we set n such that simulated effects of job loss studied below match our long-run causal effects on employment as closely as possible.

We solve the model via value function iteration after making some discrete approximations to state variables. We use 8 points of support in the marginal wage distribution, and 3 points of support each in the marginal hours and destruction rate distributions. Human capital is constrained to a grid of 20 equally-spaced points with per-period probabilities of moving up / down the grid chosen such that the expected increase / decrease in productivity reflects ϕ_e / ϕ_u. We allow for up to $\bar{d} = 12$ periods of duration dependence in the costs of job search in unemployment, after which $d_t = \bar{d}$. We use months as the discrete time unit in the model, although we aggregate to the quarterly level when presenting simulated results.

5.3 Implied job loss effects

Figure 5 summarizes the model's implications for the long-run employment and earnings consequences of job loss. Panels A and B replicate our causal effects alongside the simulated effects from the fully calibrated model and several variations. Simulated effects are the differences in earnings and employment trajectories for a worker beginning in unemployment vs. a worker employed at roughly \$15 / hour full-time, with each worker's level of human capital μ_t initially normalized to one. Because the effects of job loss also depend on the separation rate δ, the employed worker's trajectory is averaged over the distribution of δ among full-time, \$15 / hour jobs in $F(\cdot)$. Finally, to match the timing of job loss events in our reduced-form analysis, model-simulated effects are averaged assuming job loss happens in $t \in \{1, 2, 3, 4\}$ with probabilities proportional to the effect on separations in Figure 2.

Panel A studies effects on employment. The blue line makes clear that the fully calibrated

[31] Predicted separation rates come from a regression of an indicator for transitioning into unemployment across survey waves on hours and occupation dummies.

model comes close to matching our empirical estimates. As in our reduced-form analysis, job loss leads to a persistent, long-run employment decline of roughly 3 p.p., although the initial increase is slightly faster than what we find empirically. Panel B repeats the same exercise for earnings and shows an even closer correspondence between the model-implied effects and our estimates, with both featuring a sluggish increase and a sizable gap of roughly $1,000 per quarter persisting by $t = 24$.

The remaining lines in Panels A and B of Figure 5 demonstrate how several features of the model combine to produce these results. First, the solid red line in both panels shows simulated effects from a "basic" model that shuts down human capital ($\phi_e = \phi_u = 0$), duration dependence ($\kappa_1 = 0$), and fixed costs of search ($n = 0$). This model is close to a textbook, partial equilibrium Burdett and Mortensen (1998) setup, although we still allow for endogenous search. Unsurprisingly, both employment and earnings converge relatively quickly in this case. Without forces that generate state-dependence due to the initial unemployment shock, there is little hope for the basic model to produce long-run effects of job loss.

The dashed purple line shows that adding human capital dynamics (by restoring ϕ_e and ϕ_u to their calibrated values) allows for some persistent effect in earnings, although not in employment. This earnings effect is solely attributable to the fact that job loss reduces total experience, which means productivity at $t = 24$ is lower as well. In this simulation, cumulative experience is about 3.5 months lower as a result of job loss and long-run wages are roughly 2.7% lower, consistent with a $\phi_u + \phi_e = 0.027$ net effect of an extra quarter out of work on productivity.

The dotted gray line shows that adding duration dependence (by restoring κ_1) roughly doubles long-run earnings effects. Duration dependence makes escaping unemployment more costly as spells go on. As a result, employment increases more slowly in this version of the model, and larger impacts of the initial job loss shock on cumulative time unemployed generate larger long-run earnings effects. Despite lower potential wages due to drops in human capital, endogenous search alone does not produce significant long-run differences in search intensities, and any differences in job stability as workers climb back up the job ladder fades away after several years. As a result, long-run employment gaps are near zero.

Relative to this version of the model, the fully calibrated model only adds back in fixed costs of search. Fixed costs cause some unemployed workers to cease searching altogether because the returns are too low, generating a persistent employment effect. Fixed costs of search also means less search activity among the employed because the benefits of climbing further up the job ladder may not be worth the costs. This tradeoff allows differences in job ladder positions to persist for longer as well. As a result, long-run earnings decline further because in addition to having lower productivity, displaced workers tend to hold jobs with lower wage rates and fewer hours. Figure A.5 provides more detail on these findings, including simulated effects on separation rates. As in our reduced-form results, the model produces zero long-run impact on this outcome.

Panel C is helpful for understanding precisely how the model matches our long-run earnings effects. This figure decomposes the simulated gap in annual earnings 5 years after job loss using the same Oaxaca-Blinder manipulations as in Figure 4. About 21% of this gap is explained by non-employment, which is entirely driven by participation (i.e., non-searchers) as opposed to unemployment. The remaining gap is explained by intensive-margin factors, with 19% explained by reductions in hours, 20% by decreases in human capital, and 36% by holding jobs with lower wage rates. These figures are quite similar to our reduced-form decompositions, although the overall role of wages is larger and the model has no notion of within-job variation in weeks worked.

Finally, the model's structure also allows us to provide an alternative measure of the consequences of job loss by comparing the value of holding a full-time, \$15 / hour job vs. being unemployed. At the median value of δ and with productivity equal to 1, the value of this job (i.e., $V(\omega, 1)$) is 12% higher than the value of unemployment at the start of a spell (i.e., $U(1, 0)$) and 14% higher than the value of unemployment 12-months into a spell (i.e., $U(1, 12)$). The consumption equivalents of these differences depend on the reference utility level, but one simple benchmark is this: assuming that b reflects utility purely from consumption out of unemployment benefits, a newly unemployed worker would be indifferent between accepting a full-time \$15 / hour job offer and continuing to search with a permanent, \$57 / month increase in benefits (a 17% increase).

6 Comparing impacts on higher-wage workers

We next examine whether our findings are an artifact of our design and data rather than our focus on low-wage workers by replicating the same analysis on a higher-wage sample. We use the same sample restrictions and specification as in the primary analysis, but condition on initial wages between \$15 and \$30 per hour instead of below \$15. As shown in Figure A.7, higher-tenure workers in this sample tend to have pre-displacement earnings similar to those in previous analyses using the classic mass-layoff design (e.g., Jacobson et al., 1993).

Table 4 presents long-run effects of job loss on higher-wage workers using outcomes measured in the ACS.[32] Point estimates show sizable losses in wages and income, with total wage

[32]Figure A.8 presents reduced-form dynamic effects on separations, job loss, employment, and earnings

earnings declining by more than $7,200. For both low- and higher-wage workers, the reductions in wages in the ACS are about 15% of the overall mean. Unlike for low-wage workers, however, impacts on unemployment and participation are small. Higher-wage workers are not significantly more likely to report being unemployed, not in the labor force, or looking for work four to six years after job loss. Standard errors are relatively large, however, and we cannot reject a reduction in employment of up to four percentage points.

Compared to low-wage workers, reductions in weeks and hours worked for this sample are also small. The overall reduction in weeks worked is less than half that experienced by low-wage workers, for example, and hours worked decreases by less than a third as much. Instead, higher-wage job losers experience significant wage declines of $2.46 per hour, including zeros. Panel B of Figure 4 provides a complete decomposition of how these factors account for the total wage earning impact on higher-wage workers. Overall, the bulk of losses is explained by reductions in wage rates, which decline by nearly $2 per hour, relative to non-displaced workers, and explain 60% of the total.

Table A.9 shows that higher-wage workers experience substantial differences in the impact of job loss by tenure. For example, workers with three or more years of tenure at $t = 0$ have average earnings six years later that are only 10% higher than those of workers with no more than one year of tenure at $t = 0$. However, the effect of job loss is 230% larger ($–3,005 vs. $–1,294) for the high-tenure group. A similar pattern emerges for effects on employment and cumulative earnings. These findings suggest an important role for specific human capital or match-specific effects among higher-wage workers, consistent with prior research (Topel, 1991; Farber, 1993; Neal, 1995; Stevens, 1997; Lachowska et al., 2020). In contrast, among low-wage workers (Columns 1 and 2), there is no evidence of systematic differences by tenure, either in the estimated long-run effects or in their magnitude relative to the subgroup mean outcome. By construction, however, even high-tenure workers in the low-wage sample earn relatively low wages at $t = 0$, indicating that they have not experienced substantial on-the-job wage growth.

7 The importance of industry

While the impacts of job loss on low-wage workers are broadly similar across a range of demographic characteristics, they vary significantly by workers' initial industry. Figure 6

in LEHD data for these workers. Point estimates for long-run effects on LEHD outcomes are presented in Table A.11. At $t = 24$, displaced workers see a reduction of $2,289 in quarterly earnings (17% of the mean) and a reduction of $74,542 in cumulative earnings (21%). These effects include all high-wage workers, but when we restrict to workers with at least three years of tenure, Figure A.7 shows that estimated effects line up closely with the prior literature.

Panel A plots 2SLS effects of job loss on LEHD earnings when splitting the sample into the five most common 2-digit NAICS industries in our data, with all other industries grouped into a residual category. The results show that workers displaced from jobs in Accommodation and Food Services (NAICS 72), Retail (44-45), and Healthcare and Social Assistance (62) experience smaller short-run losses and effectively zero long-run reductions in earnings. Cumulative losses total roughly $15,000, less than half the overall effect reported in Table 2. As shown in Table A.14, the most common low-wage occupations in these industries are cooks and servers, salespersons and cashiers, and nursing and medical assistants.

By contrast, workers displaced from jobs in Manufacturing (31-33), Educational Services (61), and all other industries experience large short-run losses, more sluggish recoveries, and meaningful long-run reductions in earnings. Quarterly earnings remain about $1,500 lower six years later as a result of job loss, with average cumulative losses of roughly $60,000. Low-wage workers in manufacturing are predominately assemblers and fabricators, while those in education are frequently janitors and administrative assistants.[33] The residual category includes a mix of industries that also show large—albeit less precisely estimated—long-run losses when analyzed individually. As shown in Table A.12, for example, point estimates imply large losses in Agriculture, Forestry, Fishing and Hunting (11), Construction (22), Mining, Quarrying, and Oil and Gas Extraction (21), and Wholesale Trade (42).

Taken together, these results highlight that displacement from some types of jobs is more costly than displacement from others. Indeed, for workers in some sectors the traditional perspective that low-wage job loss is relatively inconsequential appears approximately correct. For workers in other sectors, however, job loss entails substantial long-run costs. Analyzing the characteristics of industries with larger long-run losses provides some insight into the potential drivers of these costs. Figure 6 Panel B summarizes these findings by presenting a series of inverse-variance weighted bivariate regressions of long-run earnings effects for workers in each 2-digit NAICS industry on a single industry characteristic.[34]

The results show that one important industry predictor of job loss is jobs stability: losses are larger in industries with lower separation rates, more full- vs. part-time workers, and longer average tenure. This suggests losses are larger if, in the absence of job loss, the worker would have remained in the job for longer.[35] Table A.13 shows that we also find direct

[33]Table A.14 shows that teachers are a small share of low-wage workers in NAICS 61.

[34]These industry characteristics are described further in Table A.12.

[35]Table A.15 reports the share of employed compliers across industries at $t = 24$ by their initial industry. Job loss induces industry switching in all the groupings we study except for healthcare, which may reflect the specialized skills and credentials of even low-wage workers in that sector. For workers in education and manufacturing, who experience some of the largest effects of job loss, counterfactual employment rates in the same industry are high (88% and 82%, respectively) relative to accommodation and food services or retail

evidence of stability heterogeneity when splitting the sample by workers' initial employers' quarterly separation rates. Earnings effects are nearly three times as large when a worker is displaced from a firm in the top tercile of stability vs. the bottom tercile. Interestingly, Figure A.6 shows that initial job stability is also a key driver of effect heterogeneity in our model, providing one simple way to rationalize these differences.

Losses are also larger in industries that appear to be higher quality along several other dimensions, including those with higher unionization rates, higher average wages, and higher firm effects as estimated in Card et al. (2022). Losses are also larger in industries that experienced weaker employment growth over the sample period, consistent with previous work that finds procyclical displacement effects (Schmieder et al., 2023). Losses for higher-wage workers are predicted by many of the same factors.

Beyond the stability heterogeneity captured in our model, multiple factors may make low-wage job loss more consequential in some industries. Some employers might have production requirements that lead them to prefer hiring workers part-time, as in a hedonic model of hours and wages (Lewis, 1969). Although government policies such as hours restrictions and overtime regulations, mandates to provide health care, and minimum wages may also lead to undersupply of low-wage jobs, we do not find large effects in the industries where these factors are typically most binding, such as Retail Trade and Accommodation and Food Services. It is possible, however, that these forces exacerbate short-run losses in these sectors, since even if government policies do not affect equilibrium *levels* of employment (e.g., as found in Cengiz et al., 2019), more competition for the jobs that are offered may prolong job search for displaced workers (Flinn, 2006).

Larger effects in industries with higher average unionization rates and firm premia suggest jobs may also be rationed due to other forces. Union bargained wages and hours, for example, are typically designed to benefit incumbent workers, but may also make these jobs particularly desirable and thus under-supplied. If industry wage differentials reflect payment of efficiency wages (Krueger and Summers, 1988), then equilibrium employment in these sectors may also fail to meet demand and workers may be willing to queue for these jobs. Providing superior within-job earnings stability or a path to higher wages in the future may also be a form of efficiency wage. Displacement from even low-wage jobs in these sectors may thus prove costly. Indeed, Table A.16 shows that workers displaced from industries with larger job loss effects tend to move into industries with lower unionization rates, shorter average tenure, and a higher share of low-wage workers.

(73% and 75%, respectively).

8 Conclusion

This paper studies the effects of job loss on the employment and earnings of low-wage workers. We find that workers initially earning no more than $15 per hour suffer lasting reductions in employment, labor force participation, and earnings as a result of job loss. About 60% of the estimated impact on earnings is due to intensive-margin effects—i.e., reductions in earnings among employed workers driven by decreases in weeks and hours worked. These effects are consistent with a calibrated, partial-equilibrium model of job search where job loss causes long-run reductions in employment and earnings due to a combination of worsened positions on the job ladder and human capital depreciation.

The long-run reductions in earnings we document are comparable to recent estimates of the effects of job loss among workers with substantial tenure and significantly higher wages. For example, Lachowska et al. (2020) document a 15% reduction in earnings (relative to pre-displacement levels) after four to five years, while Moore and Scott-Clayton (2019) find a reduction of 22% after four years. These reductions are typically thought to reflect the fact that over time, workers sort into higher paying firms or better matches and benefit from forces such as the development of firm-specific skills, so starting over can be costly. The influence of these factors in our sample is likely small because all workers at $t = 0$ had low wages and most had limited tenure. That the majority of long-run losses are not explained by decreases in wage rates also suggests these factors play a more limited role for our sample.

An alternative explanation for our results is that there are substantial quality differences among ostensibly similar low-wage jobs. Some jobs offer the promise of more stable, consistent work, as well as potentially other non-wage amenities and future wage growth. These jobs are scarce enough that replacing one can be difficult. However, a sufficiently large fraction of the full-time, low-wage workforce has sorted into one of these positions over time that displacement generates significant costs for the average worker. On the other hand, it is also possible that workers' preferences over specific jobs are also strongly horizontally differentiated. The impacts of job loss would then reflect workers' willingness to wait to find the right job for them. Either perspective calls for a nuanced view of the low-wage labor market in future research.

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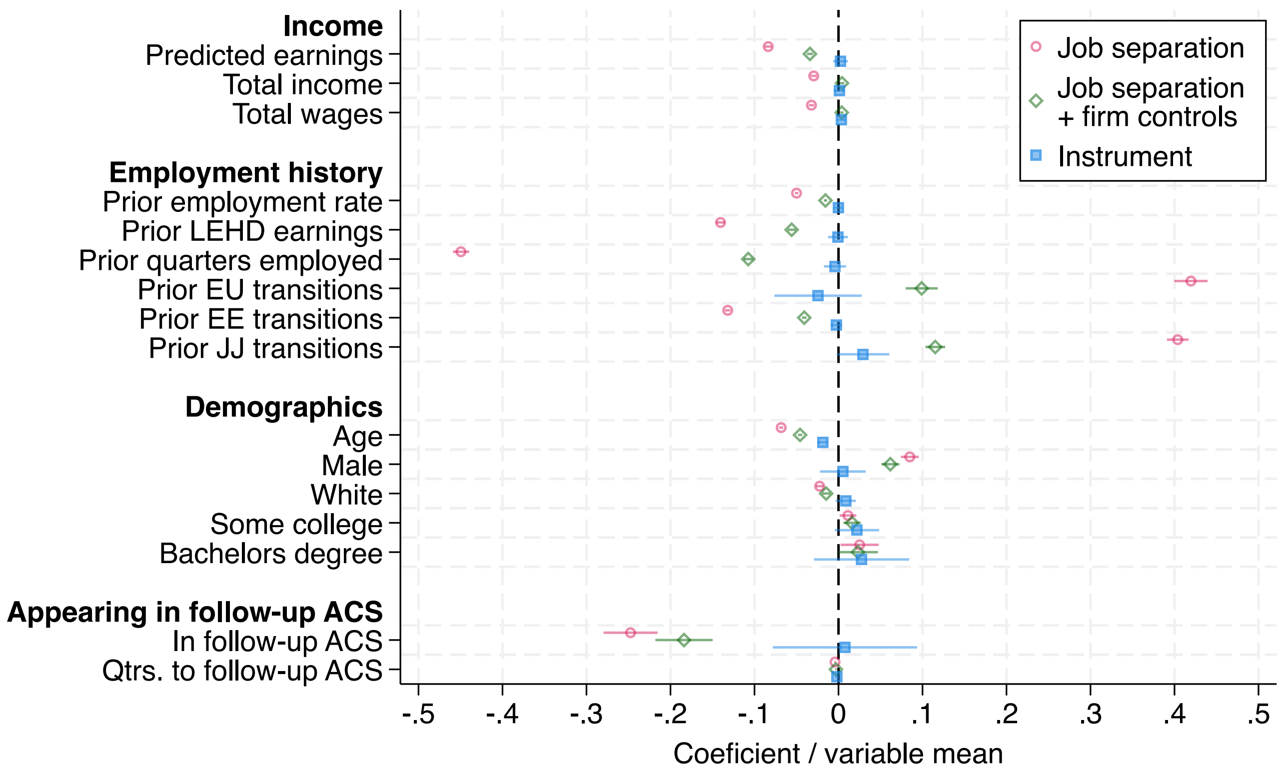
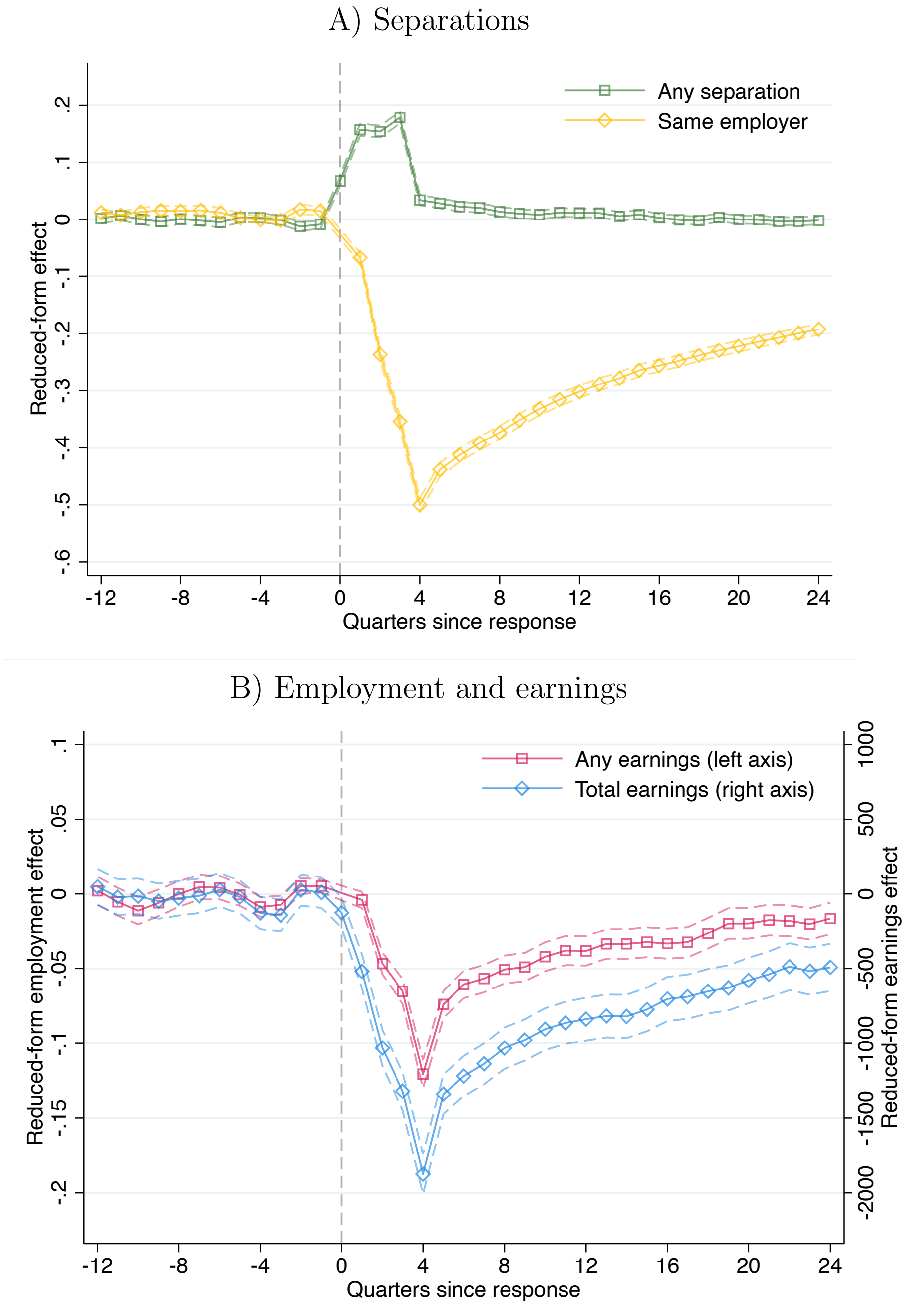
Figure 1: Instrument balance



Notes: This figure shows the association between various worker characteristics and an indicator for separating from workers' $t = 0$ employer within one year (circular and diamond markers) and the instrument (square marker). Each point reports the coefficient on the separation indicator or the instrument from an OLS regression with the variable listed on the y-axis as the outcome. Coefficients are normalized by dividing by the mean of the outcome variable. Income variables are outcomes as of $t = -1$ measured in the ACS. Predicted earnings is a summary covariate index formed using a regression of earnings in $t = -1$ on all available covariates. Employment history variables are averages over $t = -12$ to $t = -1$. Prior quarters employed is the share of quarters with any LEHD earnings. "EU" transitions indicate a quarter with positive LEHD earnings followed by a quarter with zero earnings. "EE" transitions indicate two consecutive quarters with positive LEHD earnings from the same or different employers. "JJ" transitions indicate two consecutive quarters with positive LEHD earnings from different employers. All regressions include the baseline set of fixed effects. The specifications indicated by the square and diamond markers also include controls for firm characteristics interacted with tenure. 95% confidence intervals based on standard errors clustered by employer at $t = 0$ are indicated by the horizontal bars.

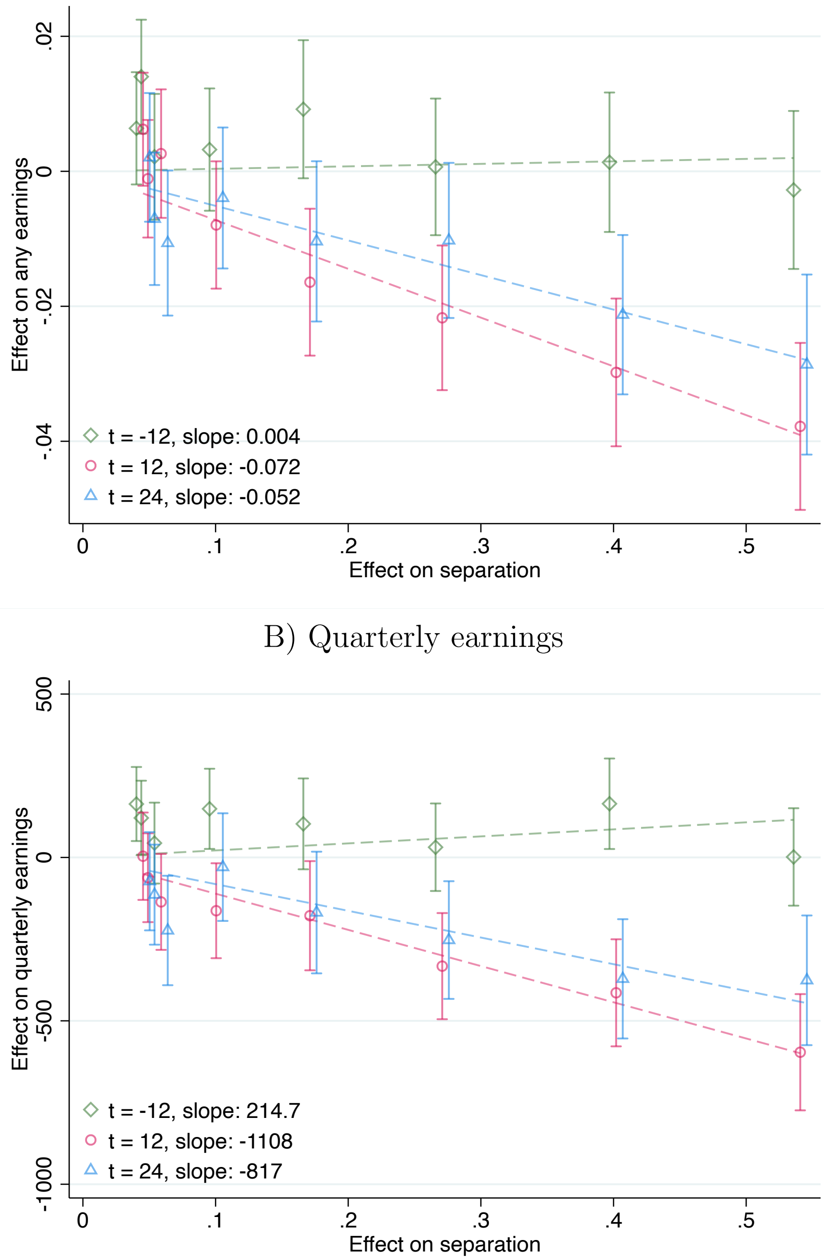
Figure 2: Reduced-form effects on job separations, earnings, and employment



Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations (Panel A) and earnings and employment (Panel B) in the three years prior to and six years after initial ACS response. Each coefficient and standard error comes from a separate regression using outcomes measured in the quarter indicated on the x-axis. The scale of the instrument implies the coefficients can be interpreted as the impact of 100% leave-out decrease in employment shock. Separation is an indicator for having zero earnings from your top-paying employer in the prior quarter. Same employer is an indicator for having the same top-paying employer as at $t = 0$. Any earnings is an indicator for any earnings in LEHD nationally. Total earnings is the sum of quarterly earnings from all employers in the 21 LEHD states included in the study, inflated to 2020 equivalents using the CPI. Standard errors are clustered by employer at $t = 0$.

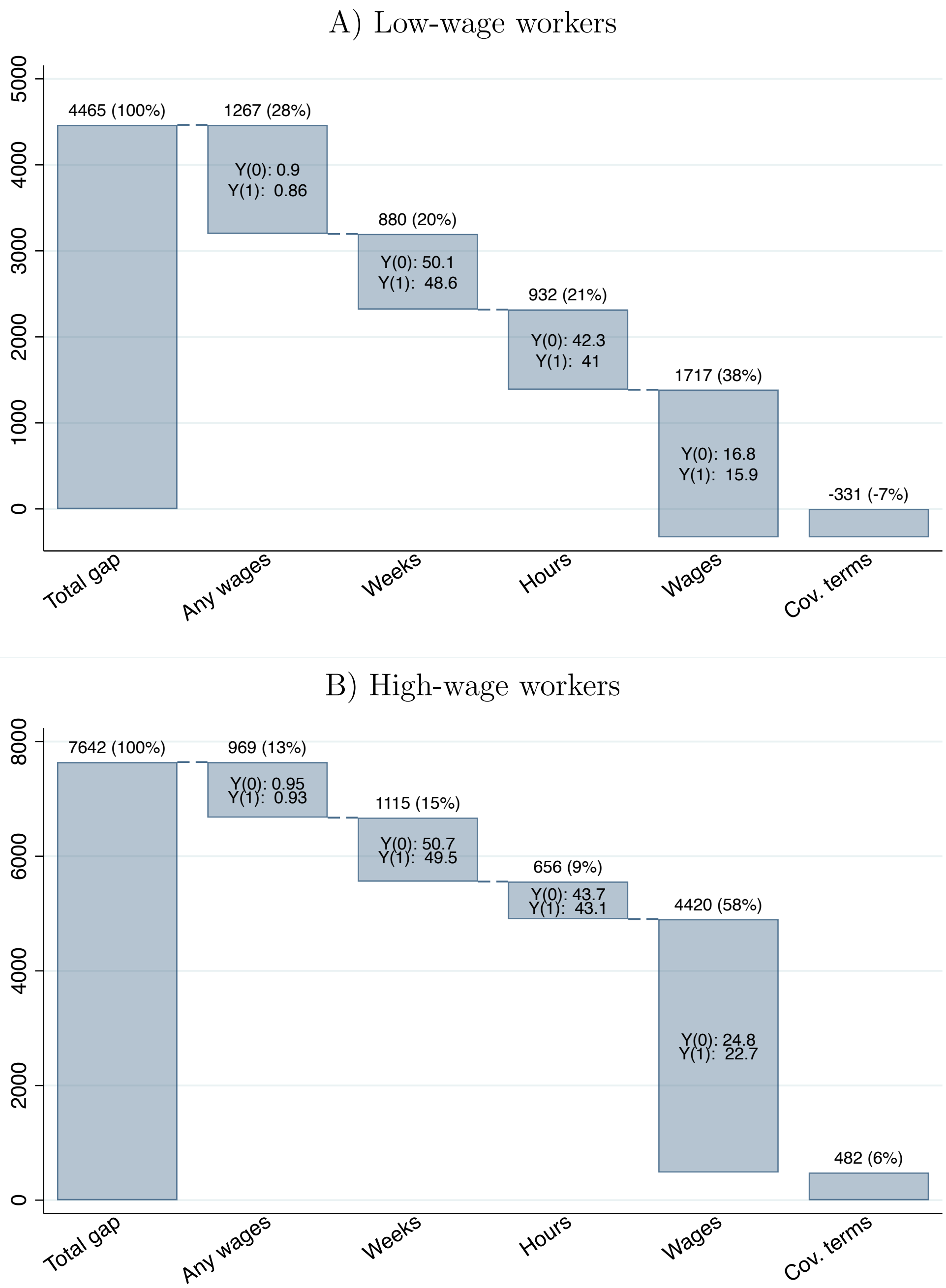
Figure 3: Visual IV estimates of effects of job loss using discretized instrument

A) Any earnings



Notes: This figure plots first-stage effects on job separation by $t = 4$ against reduced form effects on employment (Panel A) and earnings (Panel B) when the instrument is discretized by severity. The highest bin, corresponding to constant leave-out levels of employment, serves as the omitted category. The rightmost quantile corresponds to leave-out decreases in employment of 50% or more. The slopes reported in the legend are taken from unweighted regressions of reduced-form on first-stage effects omitting a constant. The lines plot these regression fits. A constant effects model with job separation serving as the sole causal channel implies the regression lines plotted should fit all points, up to sampling error, and pass through the origin. All dollar values are inflated to 2020 equivalents using the CPI.

Figure 4: Decomposition of job loss effects

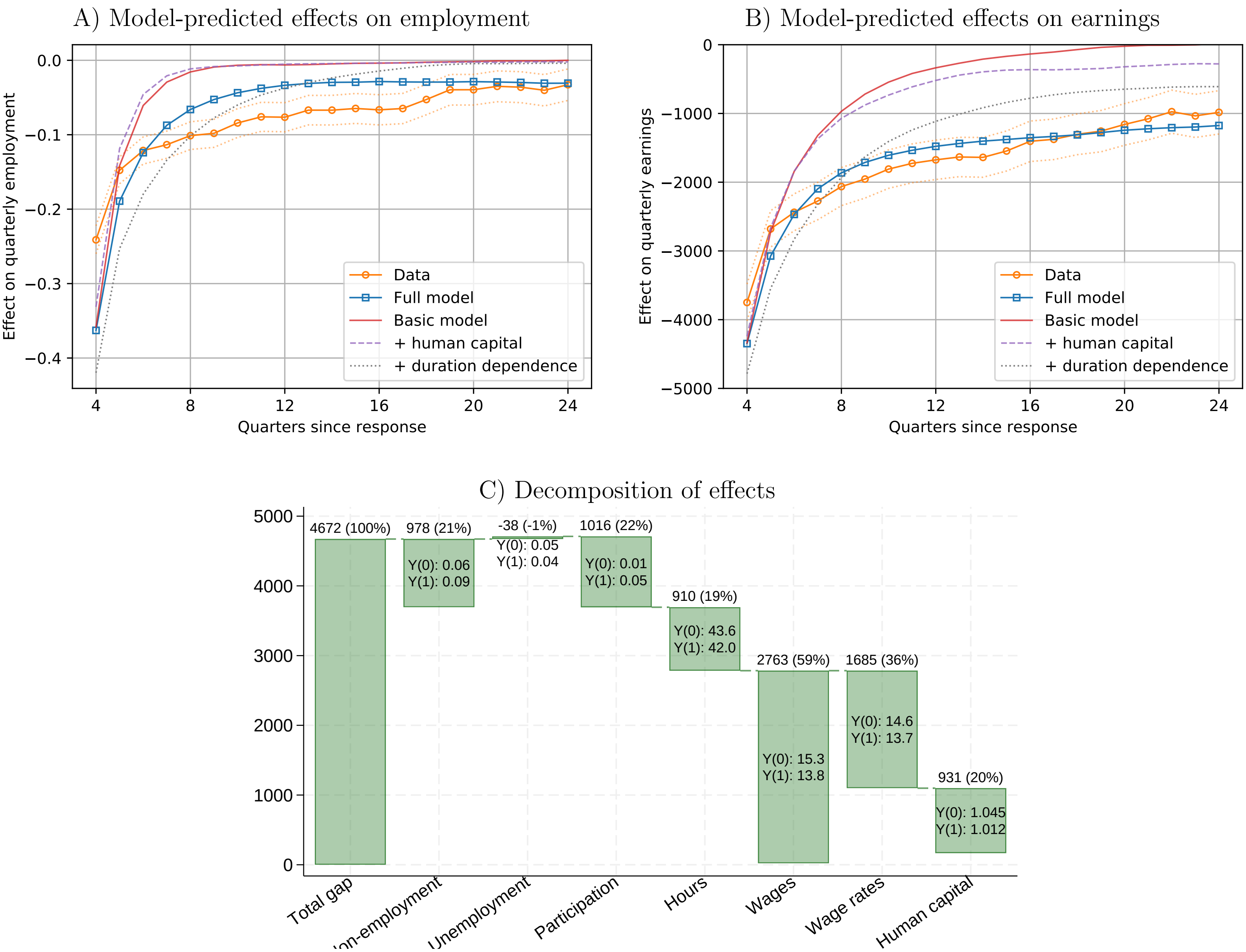


Notes: This figure presents decompositions of the long-run earnings effects of job loss into components explained by employment, weeks, hours, and wages. The decomposition is based on the observation that complier mean earnings can be expressed as:

$$
\begin{aligned}
E[Y(d)] &= E[Y(d)|Y(d)>0]Pr(Y(d)>0) = E[weeks(d)\cdot hours(d)\cdot wages(d)|Y(d)>0]Pr(Y(d)>0) \\
&= E[weeks(d)|Y(d)>0]E[hours(d)|Y(d)>0]E[wages(d)|Y(d)>0]Pr(Y(d)>0) + covariances
\end{aligned}
$$

where expectations are taken over the complier population and d indicates treatment status. Each step in the graph successively assigns treated workers (i.e., job losers) the mean outcome for untreated workers (i.e., jobs stayers) for each component and measures the reduction in the total treatment effect. Treated and untreated means for each component are also denoted using $Y(0), Y(1)$ notation on the figure, and presented in further detail in Table A.10. All dollar values are inflated to 2020 equivalents using the CPI.

Figure 5: Model-predicted effects of job loss

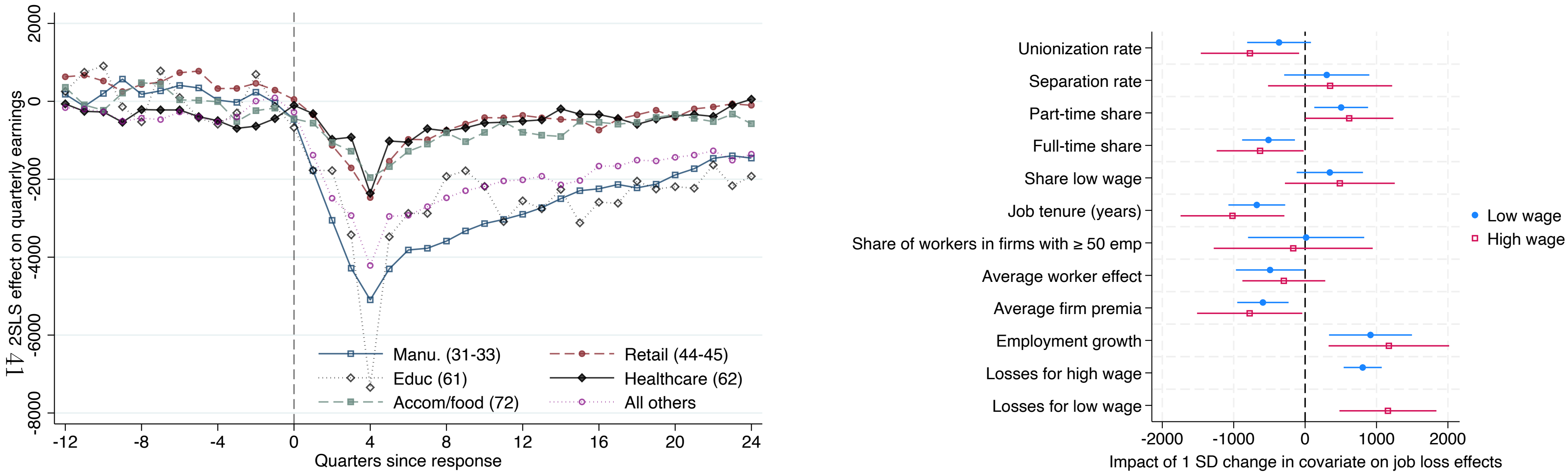


Notes: Panels A and B plot simulated effects of job loss from the calibrated search model versus 2SLS estimates of the effects on employment (Panel A) and earnings (Panel B). Simulated effects are obtained by differencing the average employment and earnings trajectories for a newly unemployed worker and a worker in a \$15/hour, full-time job, with both workers' initial human capital normalized to one. The employed worker's trajectory is averaged over the distribution of δ among \$15/hour, full-time jobs in the offer distribution $F(\cdot)$. Simulated effects reflect the average assuming job loss occurs in $t \in \{1, 2, 3, 4\}$, with probabilities proportional to the effect on separations in Figure 2. The red "basic model" line shuts down human-capital dynamics, duration dependence, and fixed costs by setting ϕ_e, ϕ_u, κ_1, and n equal to zero. The dashed purple "+ human capital" line restores ϕ_u and ϕ_e to their calibrated values. The dotted gray "+ duration dependence" line also restores κ_1. The solid black "full model" restores fixed search costs n. Panel C then decomposes simulated effects of job loss from the fully calibrated search model using the same techniques as in Figure 4. Because the model includes no notion of weeks worked (beyond unemployment), the decomposition is based on expressing annual earnings $Y(d)$ for displaced ($d = 1$) and non-displaced ($d = 0$) workers as: $E[hours(d)wages(d)|Y(d) > 0]Pr(Y(d) > 0)$. Any residual is explained by covariance terms omitted from the figure.

Figure 6: Impacts of job loss for low-wage workers in common NAICS-2 industries and their predictors

A) Industry-specific job loss effects

B) Predictors of industry-specific job loss effects



Notes: Panel A plots 2SLS estimates of the effects of job loss for low-wage workers when splitting the sample into the five most common NAICS 2-digit industries in our sample and using the conventional grouping of manufacturing (31-33) and retail trade (44-45) codes. All others includes all industries not in the top five, such as utilities, construction, wholesale trade, and arts and entertainment. Sample shares are shown in Table 1. The outcome is total quarterly earnings in a given quarter from all employers in the 21 LEHD states included in the study, inflated to 2020 equivalents using the CPI. Panel B presents estimates of bivariate regressions of the impacts of job loss for workers in each 2-digit NAICS industry on the industry characteristic listed on the y-axis. Regressions are weighted by the inverse of squared standard error of the industry-specific job loss effect. The blue squares use effects for low-wage workers, while the pink hollow squares use effects for higher-wage workers. Effects are scaled by the standard deviation of the relevant characteristic, which are drawn from the Current Population Survey over 2001-2014. Characteristics are estimated using employed workers aged 22 to 50 and in one of our LEHD approving states. Unionization rate is the share of workers represented by a union. Share low-wage is the share of workers with hourly wages between \$2 and \$15. Both variables are computed by restricting the sample to the Outgoing Rotation Groups. Job tenure (years) is instead restricted to individuals belonging to the Job Tenure Supplement and Occupational Mobility Supplement. Employment shares in firms with > 50 employees is computed using the Annual Social and Economic Supplement. Average workers effects and firm premia are taken from Card et al. (2022)

Table 1: Summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Primary sample			ACS follow-up sample		
	Mean	S.D.	p50	Mean	S.D.	p50
Demographics						
Male	0.44			0.43		
White	0.82			0.86		
Age	35.6	(8.77)	36	37.0	(8.74)	38
Some college	0.47			0.48		
Bachelor's degree	0.15			0.14		
Income and employment at $t = 0$						
Household earnings	66,330	(42,510)	57,400	66,470	(40,840)	58,660
Total individual earnings	26,470	(9,502)	25,950	26,550	(9,515)	26,210
Wage and salary earnings	25,490	(8,125)	25,500	25,570	(8,083)	25,770
Weeks worked last year	51.95	(0.13)	52	51.90	(0.09)	52
Usual hours worked	44.62	(9.34)	40	44.37	(8.99)	40
Hourly wage	11.19	(2.78)	11.8	11.24	(2.81)	11.8
LEHD activity at $t = 0$						
Quarterly earnings	8,572	(4,702)	7,660	8,528	(4,598)	7,632
Last four quarters	32,570	(17,510)	29,500	32,750	(17,100)	29,500
Quarters with same firm	11.68	(11.6)	7	15.13	(14.7)	9
Quarters in same industry	18.04	(14.9)	14	23.27	(18.6)	19
Industry (NAICS)						
Manufacturing (31-33)	0.16			0.17		
Retail trade (44-45)	0.15			0.14		
Health care / social assistance (62)	0.15			0.17		
Education (61)	0.08			0.10		
Accommodation / food (72)	0.07			0.06		
All others	0.39			0.36		
Census region						
Midwest	0.40			0.50		
South	0.34			0.30		
West	0.26			0.20		
Total observations	234,000			46,000		
Total individuals	233,000			45,000		
Total firms	96,000			29,500		

Notes: This table presents summary statistics for the primary sample of low-wage ACS respondents linked to LEHD data (Columns 1-3) and the subset of the primary sample linked to a second ACS response four to six years later (Columns 4-6). Demographics and income and employment information come from the initial ACS response. LEHD activity and industry information come from LEHD records for the highest-paying firm linked to in the quarter of ACS response. All dollar values are inflated to 2020 equivalents using the CPI.

Table 2: Long-run effects on LEHD outcomes

	(1) Mean	(2) Reduced form	(3) 2SLS
Earnings and employment			
Any employment	0.82	-0.016	-0.033
		(0.0054)	(0.0105)
Any employment (LEHD states)	0.79	-0.022	-0.043
		(0.0057)	(0.0110)
Quarterly earnings	7,654	-492	-983
		(80)	(156)
Earnings last four quarters	30,540	-2,036	-4,070
		(301)	(582)
Non-employed for 8+ quarters	0.079	0.015	0.030
		(0.0039)	(0.0075)
Consecutive quarters with zero earnings	1.40	0.29	0.58
		(0.06)	(0.12)
Earnings $<$ \$6,000	0.40	0.034	0.067
		(0.0062)	(0.0124)
Job separation			
Same employer	0.34	-0.19	-0.39
		(0.0047)	(0.0086)
Any separation	0.07	-0.002	-0.0041
		(0.0035)	(0.0069)
Cumulative outcomes			
Quarters with any earnings	22.9	-0.94	-1.89
		(0.08)	(0.15)
Earnings	203,900	-20,870	-41,720
		(1,424)	(2,740)
Separations	2.15	0.72	1.44
		(0.03)	(0.06)
Job separation by $t = 4$ (first stage)		0.50	
		(0.01)	

Notes: This table presents estimates of the long-run effects of labor demand shocks for the primary sample. All outcomes are measured as of 24 quarters after initial ACS response. Column 1 reports overall sample means, Column 2 reports reduced form effects, and Column 3 rescales effects by the first-stage effects on job separation by $t = 4$ reported at the bottom of the table. Standard errors clustered by firm at $t = 0$ are reported in parentheses. Same employer is an indicator for working for the same firm as at $t = 0$. All dollar values are inflated to 2020 equivalents using the CPI.

Table 3: Long-run effects on ACS outcomes

	(1) Mean	(2) Reduced form	(3) 2SLS
Income			
Total income	33,880	-2,763	-5,243
		(865)	(1,399)
Wages	31,710	-2,486	-4,717
		(867)	(1,398)
Household income	76,550	-3,647	-6,919
		(2034)	(3,281)
Employment			
Employed	0.88	-0.031	-0.058
		(0.015)	(0.024)
Unemployed	0.034	0.017	0.032
		(0.009)	(0.014)
Not in labor force	0.082	0.013	0.026
		(0.012)	(0.020)
Not in labor force & non-emp 8+	0.041	0.013 0.024	
		(0.009) (0.015)	
Looking for work	0.043	0.021	0.041
		(0.010)	(0.016)
On layoff	0.016	-0.001	0.001
		(0.006)	(0.009)
Weeks, hours, and wages			
Weeks worked last year	45.4	-1.71	-3.24
		(0.71)	(1.13)
Usual hours worked	38.2	-1.59	-3.01
		(0.65)	(1.04)
Hourly wage	15.5	-0.76	-1.43
		(0.38)	(0.61)
Implied extensive-margin wage effect	10.2	-0.21	-0.41
		(0.15)	(0.25)
Other			
Enumerated in Group Quarters	0.003	-0.003	-0.005
		(0.003)	(0.005)
Moved to new state	0.064	-0.002	-0.003
		(0.010)	(0.016)
Job separation by $t = 4$ (first stage)		0.53	
		(0.02)	

Notes: This table presents estimates of the long-run effects of labor demand shocks for the subset of the primary sample linked to a second ACS response four to six years later. All outcomes are averages of any ACS response in the 16-24 quarters after initial ACS response. Column 1 reports overall sample means, Column 2 reports reduced form effects, and Column 3 rescales effects by the first-stage effects on job separation by $t = 4$ reported at the bottom of the table. Standard errors clustered by firm at $t = 0$ are reported in parentheses. Weeks worked, usual hours, and hourly wage outcomes all include zeros. All dollar values are inflated to 2020 equivalents using the CPI. The employment variables all refer to the week before responding to the ACS except for the "Looking for work" variables, which refer to the previous four weeks.

Table 4: Long-run effects on ACS outcomes for higher-wage workers

	(1) Mean	(2) Reduced form	(3) 2SLS
Income			
Total income	51,630	-3,792	-6,688
		(643)	(1,036)
Wages	49,120	-4,111	-7,251
		(660)	(1,064)
Household income	99,110	-4,205	-7,417
		(1378)	(2,216)
Employment			
Employed	0.93	-0.009	-0.015
		(0.008)	(0.012)
Unemployed	0.021	0.008	0.015
		(0.005)	(0.008)
Not in labor force	0.047	0.0002	0.0003
		(0.006)	(0.010)
Not in labor force & non-emp 8+	0.022 0.003 0.005		
	(0.004) (0.007)		
Looking for work	0.028	0.008	0.013
		(0.006)	(0.009)
On layoff	0.012	-0.008	0.014
		(0.004)	(0.006)
Weeks, hours, and wages			
Weeks worked last year	47.9	-0.86	-1.51
		(0.36)	(0.58)
Usual hours worked	41.3	-0.55	-0.98
		(0.36)	(0.58)
Hourly wage	22.6	-1.39	-2.46
		(0.27)	(0.43)
Implied extensive-margin wage effect	20.5	-0.30	-0.52
		(0.15)	(0.24)
Other			
Enumerated in Group Quarters	0.002	0.003	0.005
		(0.002)	(0.002)
Moved to new state	0.071	0.023	0.040
		(0.007)	(0.012)
Job separation by $t = 4$ (first stage)		0.57	
		(0.01)	

Notes: This table presents estimates of the long-run effects of labor demand shocks for the subset of the primary sample of higher-wage workers linked to a second ACS response four to six years later and earning wages $\in (\$15, \$30)$ at $t = 0$. See also the notes of Table 3.

A Online appendix

Table A.1: Impact of sample restrictions on sample composition in the ACS

	Public data			Public data, full-time			Public data, full-time, wage $\leq$ \$15			Public data, full-time, wage $\in$ (\$15,\$30]		
	Mean	S.D.	p50	Mean	S.D.	p50	Mean	S.D.	p50	Mean	S.D.	p50
Demographics												
Male	0.53			0.60			0.52			0.57		
White	0.77			0.78			0.72			0.78		
Age	37.19	(8.26)	38	37.75	(8.00)	38	35.43	(8.64)	35	37.36	(7.97)	38
Some college	0.65			0.66			0.44			0.64		
Bachelors degree	0.34			0.34			0.13			0.28		
Income and employment												
Household earnings	107,395	(83,288)	88,512	113,949	(82,223)	94,960	68,259	(50,387)	57,189	96,501	(52,132)	87,004
Total individual income	56,998	(53,846)	43,647	67,205	(54,325)	52,594	26,811	(14,351)	25,742	51,737	(16,802)	49,677
Wage and salary earnings	51,364	(48,180)	40,869	64,607	(49,102)	51,311	25,203	(8,240)	25,242	50,220	(12,740)	48,746
Weeks worked last year	48.12	(9.47)	52	51.95	(0.13)	52	51.95	(0.14)	52	51.95	(0.13)	52
Usual hours worked	41.32	(10.93)	40	44.79	(7.73)	40	44.93	(8.87)	40	44.40	(7.32)	40
Hourly wage	26.68	(196.52)	20.06	27.60	(19.55)	22.50	10.85	(2.95)	11.42	21.77	(4.20)	21.43
Industry (NAICS)												
Manufacturing (31-33)	0.13			0.16			0.15			0.16		
Retail trade (44-45)	0.10			0.09			0.13			0.09		
Health care / social assistance (62)	0.12			0.10			0.12			0.10		
Education (61)	0.09			0.06			0.05			0.08		
Accommodation / food (72)	0.05			0.04			0.09			0.03		
All others	0.52			0.54			0.46			0.53		
Census region												
Midwest	0.30			0.31			0.29			0.34		
South	0.31			0.32			0.35			0.32		
West	0.38			0.36			0.34			0.34		
Total observations	2,104,801			1,276,139			308,282			570,322		

Notes: This table presents summary statistics for three samples. All the estimates are based on authors' calculations from the public use files of the American Community Survey from 2001 to 2008 maintained by IPUMS. All dollar values are adjusted to reflect 2020 dollars. Columns 1 to 3 include all employed workers without any restrictions on hours, weeks of work, or hourly wages. Columns 4 to 6 include workers who worked for at least 51 weeks in the last year and whose usual hours worked are at least 40 and no restrictions on hourly wages. Columns 7 to 9 include full-time workers who earn an hourly wage of no more than \$15 . The difference between the sample in Columns 7 to 9 and our primary analysis sample (Table 1 Columns 1 to 3) stems from the sample restrictions based on matching to the LEHD data. Finally, Columns 10 to 12 include full-time workers who earn an hourly wages between \$15 to \$30.

Table A.2: Occupational distribution of full-time low-wage workers in the ACS

	Among workers with wage $\leq$ \$15	
Occupation	Percent	Cumulative percent
First-Line Supervisors of Sales Workers	3.74%	3.74%
Secretaries and Administrative Assistants	3.33%	7.07%
Driver/Sales Workers and Truck Drivers	3.19%	10.25%
Chefs and Cooks	2.90%	13.15%
Janitors and Building Cleaners	2.69%	15.84%
Laborers and Freight, Stock, and Material Movers, Hand	2.22%	18.06%
Nursing, Psychiatric, and Home Health Aides	2.16%	20.23%
Retail Salespersons	2.09%	22.32%
Cashiers	2.00%	24.32%
Customer Service Representatives	1.92%	26.23%
Agricultural workers	1.90%	28.14%
Construction Laborers	1.65%	29.78%
Stock Clerks and Order Fillers	1.62%	31.40%
Other production workers	1.57%	32.97%
Assemblers and Fabricators	1.54%	34.51%
Grounds Maintenance Workers	1.45%	35.95%
Maids and Housekeeping Cleaners	1.42%	37.37%
Bookkeeping, Accounting, and Auditing Clerks	1.29%	38.66%
Receptionists and Information Clerks	1.21%	39.87%
Waiters and Waitresses	1.20%	41.07%

Notes: This table shows estimated occupational distribution of workers based on authors' calculations from the public use files of the 2001-2008 American Community Survey maintained by IPUMS. This table presents the top 20 most common occupations among full-time workers in the last year who earn an hourly wage of no more than \$15 (see Columns 7 to 9 of Table A.1 for summary statistics of this sample). Full-time is defined as working for at least 51 weeks in the last year and having usual hours worked of at least 40. Although occupation codes changed several times (link), IPUMS provides harmonized occupation codes based on 2010 occupation classification. We used the harmonized 2010 occupation code for the calculations reported in the table.

Table A.3: Instrument balance

	Low-wage workers				Higher-wage workers	
	(1) Outcome mean	(2) Left job (OLS)	(3) Left job (OLS)	(4) Instrument (2SLS)	(5) Outcome mean	(6) Instrument (2SLS)
Labor market activity						
Average prior employment	0.89	-0.045	-0.014	-0.0001	0.935	0.00021
		(0.0010)	(0.0009)	(0.0023)		(0.001)
Average prior earnings	6,997	-983	-390	-4.52	11,740	-39.14
		(18.49)	(16.44)	(42.13)		(33.330)
Prior quarters employed	12.9	-5.78	-1.38	-0.053	18.04	0.121
		(0.06)	(0.03)	(0.09)		(0.086)
Prior emp to non-emp transitions	0.02	0.009	0.002	-0.0005	0.0122	-0.000127
		(0.0002)	(0.0002)	(0.0005)		(0.000)
Prior continuous employment	0.70	-0.092	-0.028	-0.0018	0.777	-0.00066
		(0.0011)	(0.0009)	(0.0024)		(0.001)
Prior employer changes	0.08	0.0313	0.0089	0.0023	0.0492	0.00108
		(0.0005)	(0.0005)	(0.0012)		(0.001)
Demographics						
Age	35.6	-2.43	-1.62	-0.67	37.29	-0.581
		(0.04)	(0.05)	(0.11)		(0.074)
Male	0.44	0.0377	0.027	0.0023	0.542	-0.00135
		(0.002)	(0.002)	(0.006)		(0.004)
White	0.82	-0.0185	-0.012	0.0071	0.854	0.0000209
		(0.002)	(0.002)	(0.005)		(0.003)
Some college	0.47	0.005	0.0076	0.010	0.608	0.0123
		(0.00)	(0.002)	(0.01)		(0.004)
Bachelor's degree	0.15	0.004	0.0034	0.004	0.248	0.00817
		(0.002)	(0.002)	(0.004)		(0.004)
Appearing in follow-up ACS						
In follow-up ACS	0.071	-0.0175	-0.0130	0.0005	0.0761	-0.00119
		(0.0012)	(0.0012)	(0.0031)		(0.002)
Qrts. to follow-up ACS	17.83	-0.0727	-0.0545	-0.0351	18.31	0.00788
		(0.0071)	(0.0076)	(0.0186)		(0.012)
Summary index						
Predicted earnings	8,282	-692.7	-281.2	20.48	13,170	-4.724
		(16.84)	(14.94)	(37.13)		(26.580)
Total observations	234,000					
Total individuals	233,000					
Total firms	96,000					
Controls						
State-by-industry-by-year-by-quarter fixed effects		✓	✓	✓		✓
Firm characteristics			✓	✓		✓

Notes: This table shows the association between various worker characteristics and an indicator for separating from workers' $t = 0$ employer within one year (Columns 2 and 3) and the leave-out-mean instrument (Column 4). The mean of the outcome variable is shown for reference in Column 1. The final outcome is a summary covariate index formed using a regression of earnings on all available covariates. All regressions use the baseline set of fixed effects, including state-by-industry-by-year-by-quarter fixed effects. Columns 3 and 4 also include controls for firm characteristics interacted with tenure. "Average prior employment" is the share of periods employed in the four years prior to $t = 0$, and "Prior quarters employed" is the number of quarters employed prior to $t = 0$. Columns 1-4 refer to the sample of low-wage workers and Columns 5 and 6 to the sample of higher wage workers' earnings between \$15 to \$30 in 2020 equivalent dollars. Column 6 reports the association between the instrument and pre-displacement worker characteristics in the higher-wage sample, and Column 5 reports the mean of each outcome in this sample for benchmarking.

Table A.4: Effects of placebo shocks

	(1) Commuting zone	(2) NAICS 3	(3) Commuting zone-by-NAICS 3
Dependent variable			
Instrument	0.0023	0.0060	0.0059
	(0.0044)	(0.0041)	(0.0052)
Job separation by $t = 4$	-0.0028	-0.0039	-0.0072
	(0.0095)	(0.0087)	(0.0109)
Earnings at $t = 24$	54.0	9.5	48.6
	(137.2)	(122.4)	(155.1)

Notes: This table reports the results of regressing a "placebo" shock on key outcomes. The first row uses the firm's realized shock as the outcome (i.e., the instrument used in the main analysis). The second row uses job separation by $t = 4$ (i.e., the endogenous variable used in the main analysis). The third row uses quarterly earnings at $t = 24$ for workers in the firm at $t = 0$ (i.e., a key long-run outcome). The placebo shock is defined by randomly assigning each firm the shock of another firm in the same local labor market. We examine three definitions of a local labor market, each of which is more granular than the fixed effects used in our primary specification. Column 1 uses commuting zone (rather than state)-by-2 digit NAICS-by-year and quarter of initial ACS response. Column 2 uses state-by-3 (rather than 2) digit NAICS-by-year and quarter of initial ACS response. Column 3 uses commuting zone (rather than state)-by-3 (rather than 2) digit NAICS-by-year and quarter of initial ACS response. Each permutation assigns each firm a placebo shock and then regress the outcome listed in the row on the placebo shock and our baseline set of fixed effects and firm-level controls from Equation 1. Each cell reports the average value of the regression coefficient on the placebo shock and the average standard error across 1,000 permutations. Appendix B provides further details on the procedure.

Table A.5: Robustness of job loss effects to local labor market shocks

	(1)	(2)	(3)	(4)	(5)
Reduced-form estimate	491.9	491.2	489.5	512.0	543.9
	(80.5)	(80.5)	(86.7)	(105.2)	(132.4)
R^2	0.18	0.18	0.26	0.36	0.49
Outcome mean	7654	7654	7654	7654	7654
Controls					
Base	✓	✓	✓	✓	✓
County level unemployment rate at $t = 0$		✓			
State-by-NAICS3-by-year-quarter FEs			✓		
Commuting zone-by-NAICS2-by-year-quarter FEs				✓	
Commuting zone-by-NAICS3-by-year-quarter FEs					✓

Notes: This table examines the robustness of the reduced-form effect of firm-specific shocks on total quarterly earnings six years after initial ACS response. Column 1, indicated with "Base," corresponds to our primary specification. The remaining columns add additional controls or increase the granularity of the fixed effects, as indicated by the check marks at the bottom of the table. The scale of the instrument implies the coefficients can be interpreted as the impact of 100% leave-out-mean decrease in employment. Total quarterly earnings is the sum of quarterly earnings from all employers in the 21 LEHD states included in the study, inflated to constant 2020 dollars using the CPI. Standard errors are clustered by employer at $t = 0$.

Table A.6: Long-run effects on LEHD outcomes using non-positive firm-level employment shocks

	(1) Mean	(2) Reduced form	(3) 2SLS
Earnings and employment			
Any employment	0.82	-0.046	-0.055
		(0.0078)	(0.0089)
Any employment (LEHD states)	0.79	-0.055	-0.065
		(0.0082)	(0.0094)
Quarterly earnings	7,692	-627	-746
		(115)	(132)
Earnings last four quarters	30,690	-2,506	-2,979
		(431)	(493)
Non-employed for 8+ quarters	0.078	0.038	0.045
		(0.0056)	(0.0065)
Job separation			
Same employer	0.33	-0.31	-0.36
		(0.0065)	(0.0067)
Any separation	0.07	0.005	0.0056
		(0.0052)	(0.0059)
Cumulative outcomes			
Earnings	204,200	-25,320	-30,100
		(2,048)	(2,331)
Separations	2.16	1.21	1.44
		(0.05)	(0.05)
Job separation by $t = 4$ (first stage)		0.84	
		(0.01)	

Notes: This table presents estimates of the long-run effects of labor demand shocks for a sub-sample of the primary sample that drops firms with employment growth between $t = 0$ and $t = 4$. The table is analogous to Table 2.

Table A.7: Effects on full- vs. part-time low wage workers

	All full time ($\geq$ 40 hours)		Part-time (< 40 hrs)	
	(1)	(2)	(2)	(2)
	Mean	2SLS	Mean	2SLS
Earnings and employment				
Any employment	0.80	-0.045	0.75	-0.020
		(0.0097)		(0.0174)
Any employment (LEHD states)	0.77	-0.054	0.72	-0.023
		(0.0101)		(0.0179)
Quarterly earnings	7,404	-1121	5,403	-600
		(141)		(210)
Earnings last four quarters	29,520	-4,193	21,410	-2,495
		(526)		(783)
Non-employed for 8+ quarters	0.086	0.032	0.123	0.023
		(0.0070)		(0.0136)
Cumulative outcomes				
Earnings	196,200	-40,460	129,600	-22,350
		(2,462)		(3,458)
Separations	2.34	1.40	2.59	1.15
		(0.05)		(0.09)

Notes: This table presents 2SLS estimates of the effects of job loss for variations on the primary low-wage sample. Columns 1 and 2 include all workers who as of $t = 0$ usually worked full-time and had hourly wages below \$15 / hour, regardless of weeks worked in the last year. Columns 3 and 4 limit to low-wage workers with usual hours below 40 per week as of $t = 0$. All outcomes are measured as of 24 quarters after initial ACS response. Standard errors clustered by firm at $t = 0$ are reported in parentheses. All dollar values are inflated to 2020 equivalents using the CPI.

Table A.8: Parameter calibration

(1) Param.	(2) Value	(3) Source	(3) Notes
Externally calibrated			
β	0.988	-	Annual 5% discount factor.
σ	1.26	Elminejad et al. (2022)	Table 1, Panel A, median in economics studies.
η	0.54	Chetty et al. (2011)	Table 1, "micro" intensive-margin elasticity.
γ	3.974	Hyman et al. (2024)	Based on estimates from DellaVigna et al. (2017).
ϕ_e	0.003	Dustmann and Meghir (2005)	Annual return to tenure for unskilled workers from Table 6, column 1, converted to quarterly.
ϕ_u	0.024	Schmieder et al. (2016)	Converted to quarterly from Table 4, column 1.
Calibrated with auxiliary data / benchmarked			
$F()$	-	CPS	Wage, hour, and separation probabilities for newly employed workers in same occupation distribution as analysis sample.
κ_0, κ_1	5.09,63.8	CPS	Calibrated to unemployment exit rates in same occupation distribution as analysis sample.
χ	$4.8 \cdot 10^{-5}$	-	Set to make utility weakly increasing in hours conditional on wage at all points in offer distribution.
b	-0.84	-	Set to flow utility of lowest point on job ladder.
Calibrated internally			
n	0.62	-	Calibrated to match long-run causal effect on employment.

Notes: This table reports the source and values of model parameters used in the baseline calibration in Section 5.

Table A.9: Tenure heterogeneity for low- and higher-wage workers

	(1)	(2)	(3)	(4)
	Low-wage		Higher-wage	
	Mean	β	Mean	β
Quarterly earnings				
1-4 quarters	[7,199]	-1019	[12,540]	-1294
		(322)		(406)
5-12 quarters	[7,461]	-1016	[13,240]	-2307
		(244)		(323)
13+ quarters	[8,302]	-923.9	[13,740]	-3005
		(244)		(239)
Any earnings				
1-4 quarters	[0.783]	-0.052	[0.842]	-0.017
		(0.0214)		(0.0169)
5-12 quarters	[0.816]	-0.028	[0.870]	-0.051
		(0.0166)		(0.0128)
13+ quarters	[0.861]	-0.034	[0.903]	-0.064
		(0.0160)		(0.0098)
Cumulative earnings				
1-4 quarters	[187,500]	-41,330	[332,400]	-52,370
		(5,527)		(6,966)
5-12 quarters	[198,800]	-41,380	[353,600]	-72,540
		(4,254)		(5,455)
13+ quarters	[225,300]	-44,350	[370,600]	-90,200
		(4,576)		(4,264)

Notes: This table shows 2SLS effects on quarterly earnings, employment, and cumulative earnings at $t = 24$, splitting the sample quarters of tenure at $t = 0$. Columns 1-2 present estimates for the primary low-wage sample initially earning an hourly wage of \$15 or less, while Columns 3-4 present estimates for workers initially earning \$15-\$30 per hour. Columns 1 and 3 show the outcome mean, Columns 2 and 4 show point estimates, with standard errors reported in parenthesis below. Quarterly earnings is the sum of quarterly earnings from all employers in the 21 LEHD states included in the study, inflated to 2020 equivalents using the CPI. Standard errors are clustered by employer at $t = 0$.

Table A.10: Decomposition of the long-run effects of job loss on wage earnings

	Low wage		High wage	
	Y(0)	Y(1)	Y(0)	Y(1)
Any wage earnings	0.90	0.86	0.95	0.93
Wage earnings	32,320	27,855	52,580	44,938
Earnings if > 0	35,804	32,295	55,534	48,467
Reduction		13.8%		14.5%
Intensive share		70.9%		87.6%
Extensive share		29.1%		12.4%
Weeks worked	45.2	41.9	48.0	45.9
Weeks if > 0	50.1	48.6	50.7	49.5
Reduction		7.2%		4.3%
Intensive share		40.1%		53.1%
Extensive share		59.9%		46.9%
Usual weekly hours	38.1	35.4	41.4	40.0
Hours if > 0	42.3	41.0	43.7	43.1
Reduction		7.3%		3.5%
Intensive share		40.6%		41.0%
Extensive share		59.4%		59.0%
Hourly wage	15.1	13.7	23.5	21.1
Wages if > 0	16.8	15.9	24.8	22.7
Reduction		9.5%		10.5%
Intensive share		55.5%		81.9%
Extensive share		44.5%		18.1%

Notes: This table reports complier means of employment, total wage earnings, weeks worked, usual weekly hours, and average hourly wage both unconditionally and conditional on positive. Columns (1) and (2) report results for our primary sample of low-wage workers, who earn \$15 or less per hour at $t = 0$. Columns (3) and (4) report results for the high wage comparison sample of workers earning between \$15 to \$30 per hour at $t = 0$. Since workers with no earnings have weeks, hours, and hourly wages coded as zeros, estimates conditional on positive are simply the unconditional estimate divided by the share with any earnings. For consistency, total wage earnings are coded here as the product of weeks worked, usual weekly hours worked, and the hourly wage. This definition differs slightly from the wage earnings variable used in prior tables, which is reported by respondents directly. Estimated effects are similar to those in Table 3, however. Note also that employment status in the ACS is not the same as an indicator for any wage earnings. The former relates to employment in the previous week, while the latter captures wage earnings over the previous year. All models include the baseline set of controls and pool quarters 16 to 24. All dollar values are inflated to constant 2020 dollars using the CPI.

Table A.11: Long-run effects on LEHD outcomes for higher-wage workers

	(1) Mean	(2) Reduced form	(3) 2SLS
Earnings and employment			
Any employment	0.88	-0.029	-0.049
		(0.0043)	(0.0072)
Any employment (LEHD states)	0.85	-0.037	-0.063
		(0.0046)	(0.0078)
Quarterly earnings	13,370	-1337	-2289
		(105)	(177)
Earnings last four quarters	53,530	-5,607	-9,600
		(392)	(660)
Non-employed for 8+ quarters	0.051	0.018	0.031
		(0.0030)	(0.0051)
Earnings < \$6,000	0.40	0.046	0.092
		(0.0088)	(0.0044)
Implied extensive margin effect	12,400	-521	-893
		(82)	(137)
Job separation			
Same employer	0.46	-0.27	-0.46
		(0.0052)	(0.0082)
Any separation	0.05	0.000	-0.0002
		(0.0029)	(0.0050)
Cumulative outcomes			
Quarters with any earnings	24.05	-1.13	-1.93
		(0.06)	(0.10)
Earnings	358,500	-43,540	-74,550
		(1,833)	(3,073)
Separations	1.44	0.94	1.61
		(0.02)	(0.04)
Quarters with zero earnings	0.90	0.37	0.63
		(0.05)	(0.08)
Job separation by $t = 4$ (first stage)		0.58	
		(0.01)	

Notes: This table presents estimates of the long-run effects of labor demand shocks for the sample of workers with initial wages $\in (\$15, \$30)$ at $t = 0$. A. All outcomes are measured as of 24 quarters after initial ACS response. Column 1 reports overall sample means, Column 2 reports reduced form effects, and Column 3 rescales effects by the first-stage effects on job separation by $t = 4$ reported at the bottom of the table. Standard errors clustered by firm at $t = 0$ are reported in parentheses. "Implied extensive margin effect" is the impact on an indicator for having any LEHD earnings in quarter t times average earnings over -4 to -1. Same employer is an indicator for working for the same firm as at $t = 0$. All dollar values are inflated to 2020 equivalents using the CPI.

Table A.12: Industry-level job loss effects and characteristics

	Low-wage effect	High-wage effect	Unionization rate	Separation rate	Part-time share	Low-wage share	Job tenure (years)	Share in firms $\geq$ 50 emp	Avg firm premia	Avg worker effect	Emp growth (01-14)
Accommodation and Food Services (72)	-576.000	-1843.000	0.031	0.037	0.283	0.843	4.028	0.532	0.039	-0.263	0.273
	(387.900)	(783.100)	(0.173)	(0.189)	(0.450)	(0.363)	(4.710)		-	-	-
Administrative and Support and Waste Management (56)	143.700	-742.500	0.048	0.046	0.181	0.611	4.702	0.449	0.180	-0.141	0.297
	(540.800)	(711.300)	(0.213)	(0.209)	(0.385)	(0.488)	(5.201)		-	-	-
Agriculture, Forestry, Fishing and Hunting (11)	-3079.000	-4332.000	0.022	0.052	0.129	0.804	8.674	0.250	0.158	-0.240	-0.172
	(1399.000)	(4412.000)	(0.147)	(0.222)	(0.335)	(0.397)	(8.119)		-	-	-
Arts, Entertainment, and Recreation (71)	1576.000	-671.100	0.076	0.037	0.242	0.597	5.333	0.535	0.125	-0.059	0.443
	(1527.000)	(1668.000)	(0.265)	(0.190)	(0.428)	(0.491)	(5.604)		-	-	-
Construction (23)	-3453.000	-4828.000	0.126	0.041	0.123	0.322	5.758	0.298	0.252	0.019	-0.065
	(957.600)	(704.300)	(0.332)	(0.199)	(0.328)	(0.467)	(5.973)		-	-	-
Educational Services (61)	-1923.000	-2277.000	0.359	0.026	0.184	0.474	6.112	0.832	0.133	0.069	0.106
	(933.900)	(864.300)	(0.480)	(0.158)	(0.387)	(0.499)	(5.966)		-	-	-
Finance and Insurance (52)	-851.800	-1134.000	0.020	0.015	0.064	0.371	5.720	0.733	0.318	0.176	0.018
	(799.800)	(619.000)	(0.140)	(0.121)	(0.244)	(0.483)	(5.806)		-	-	-
Health Care and Social Assistance (62)	53.470	-1147.000	0.086	0.024	0.186	0.423	5.318	0.641	0.192	-0.059	0.121
	(365.100)	(522.000)	(0.281)	(0.154)	(0.389)	(0.494)	(5.610)		-	-	-
Information (51)	-1028.000	-1461.000	0.119	0.022	0.096	0.349	5.944	0.689	0.391	0.190	-0.392
	(1220.000)	(915.100)	(0.324)	(0.147)	(0.295)	(0.477)	(6.242)		-	-	-
Management of Companies and Enterprises (55)	-1340.000	-1723.000	0.021	0.014	0.068	0.295	5.736	0.653	0.337	0.185	1.191
	(2252.000)	(1288.000)	(0.143)	(0.117)	(0.252)	(0.456)	(5.517)		-	-	-
Manufacturing (31-33)	-1457.000	-3297.000	0.110	0.021	0.052	0.400	7.016	0.707	0.351	-0.053	-0.281
	(300.400)	(349.500)	(0.313)	(0.143)	(0.222)	(0.490)	(6.771)		-	-	-
Mining, Quarrying and Oil and Gas Extraction (21)	-2893.000	-1584.000	0.053	0.019	0.028	0.199	5.128	0.711	0.621	0.079	1.180
	(2680.000)	(1931.000)	(0.223)	(0.137)	(0.164)	(0.400)	(5.810)		-	-	-
Other Services (81)	-1052.000	-2055.000	0.027	0.037	0.225	0.594	5.355	0.256	0.145	-0.077	0.051
	(1064.000)	(1708.000)	(0.163)	(0.188)	(0.418)	(0.491)	(5.628)		-	-	-
Professional, Scientific, and Technical Services (54)	-1498.000	-2205.000	0.020	0.020	0.101	0.301	5.148	0.512	0.328	0.270	0.135
	(1391.000)	(886.600)	(0.139)	(0.138)	(0.301)	(0.459)	(5.420)		-	-	-
Public Administration (92)	-1737.000	-3078.000	0.369	0.013	0.039	0.252	8.425	0.880	0.279	-0.010	-0.052
	(1265.000)	(983.600)	(0.482)	(0.115)	(0.194)	(0.434)	(7.216)		-	-	-
Real Estate and Rental and Leasing (53)	-631.900	-882.800	0.026	0.026	0.132	0.491	4.845	0.445	0.223	-0.027	-0.134
	(1344.000)	(1232.000)	(0.159)	(0.160)	(0.339)	(0.500)	(4.967)		-	-	-
Retail Trade (44-45)	-100.800	-685.000	0.056	0.029	0.192	0.642	5.028	0.635	0.108	-0.157	0.042
	(368.100)	(499.000)	(0.230)	(0.169)	(0.394)	(0.480)	(5.569)		-	-	-
Transportation and Warehousing (48-49)	-295.800	-3350.000	0.264	0.024	0.104	0.351	6.895	0.676	0.245	-0.045	-0.128
	(807.900)	(809.900)	(0.441)	(0.154)	(0.306)	(0.477)	(6.692)		-	-	-
Utilities (22)	-556.000	62.840	0.283	0.012	0.025	0.178	9.494	0.787	0.487	0.195	-0.060
	(5572.000)	(2652.000)	(0.450)	(0.111)	(0.157)	(0.383)	(8.099)		-	-	-
Wholesale Trade (42)	-3146.000	-2052.000	0.049	0.020	0.060	0.449	6.279	0.562	0.295	0.081	-0.387
	(804.600)	(742.800)	(0.216)	(0.139)	(0.238)	(0.497)	(6.208)		-	-	-

Notes: This table presents estimates of the effects job loss 24 quarters after initial ACS response at $t = 0$ for low- and high-wage workers in each 2-digit NAICS industry along with average industry characteristics, which are drawn from the Current Population Survey over 2001-2014. Standard errors for the effects / means are included in parentheses where appropriate. Characteristics are estimated using employed workers aged 22 to 50 and in one of our LEHD approving states. Unionization rate is the share of workers represented by a union. Low-wage shares is the share of workers with hourly wages below $15 and above $2. Both variables are computed by restricting the sample to the Outgoing Rotation Groups. Job tenure (years) is instead restricted to individuals belonging to the Job Tenure Supplement and Occupational Mobility Supplement. Employment shares in firms with > 50 employees is computed using the Annual Social & Economic Supplement. Average workers effects and firm premia are taken from Card et al. (2022). All dollar values are inflated to constant 2020 dollars using the CPI.

Table A.13: Heterogeneous effects by initial employer stability

	(1)	(2)	(3)
	Initial employer's quarterly sep rate		
	$\leq 0.8\%$	(0.8%,1.6%)	$\geq 1.6\%$
Earnings	-1,651	-1,117	-687
	(363)	(281)	(229)
National employment	-0.026	-0.039	-0.027
	(0.021)	(0.018)	(0.016)
Same employer	-0.507	-0.438	-0.317
	(0.023)	(0.017)	(0.011)
Cumulative earnings	-67,160	-46,420	-32,970
	(6646)	(4971)	(3970)

Notes: This table presents long-run 2SLS estimates of the effects of job loss for low-wage workers split by the quarterly separation rate of their initial employer. All outcomes are measured as of 24 quarters after initial ACS response. Standard errors clustered by firm at $t = 0$ are reported in parentheses. Same employer is an indicator for working for the same firm as at $t = 0$. All dollar values are inflated to 2020 equivalents using the CPI.

Table A.14: Low-wage workers' top occupations by industry

NAICS2	Occupation (2010)	Share
Agriculture, Forestry, Fishing and Hunting (11)	agricultural workers, nec	0.653
	graders and sorters, agricultural products	0.056
	driver/sales workers and truck drivers	0.030
	heavy vehicle and mobile equipment service technicians and mechanics	0.026
	farmers, ranchers, and other agricultural managers	0.023
	other	0.212
Mining, Quarrying and Oil and Gas Extraction (21)	construction equipment operators except paving, surfacing, and tamping equipment operators	0.137
	extraction workers, nec	0.125
	driver/sales workers and truck drivers	0.120
	laborers and freight, stock, and material movers, hand	0.087
	derrick, rotary drill, and service unit operators, and roustabouts, oil, gas, and mining	0.068
	other	0.463
Utilities (22)	meter readers, utilities	0.147
	water wastewater treatment plant and system operators	0.078
	first-line supervisors of production and operating workers	0.063
	sales representatives, services, all other	0.062
	electrical power-line installers and repairers	0.042
	other	0.608
Construction (23)	construction laborers	0.303
	carpenters	0.141
	painters, construction and maintenance	0.089
	pipelayers, plumbers, pipefitters, and steamfitters	0.051
	roofers	0.032
	other	0.384
Manufacturing (31-33)	assemblers and fabricators, nec	0.112
	other production workers including semiconductor processors and cooling and freezing equipment operators	0.067
	laborers and freight, stock, and material movers, hand	0.048
	sewing machine operators	0.041
	electrical, electronics, and electromechanical assemblers	0.041
	other	0.690
Wholesale Trade(42)	driver/sales workers and truck drivers	0.128
	laborers and freight, stock, and material movers, hand	0.127
	sales representatives, wholesale and manufacturing	0.095
	packers and packagers, hand	0.064
	shipping, receiving, and traffic clerks	0.063
	other	0.523
Retail Trade (44-45)	first-line supervisors of sales workers	0.172
	retail salespersons	0.166
	cashiers	0.125
	stock clerks and order fillers	0.075
	laborers and freight, stock, and material movers, hand	0.050
	other	0.412
Transportation and Warehousing (48-49)	driver/sales workers and truck drivers	0.184
	laborers and freight, stock, and material movers, hand	0.124
	bus and ambulance drivers and attendants	0.054
	dispatchers	0.047
	postal service mail carriers	0.045
	other	0.545
Information (51)	customer service representatives	0.122
	receptionists and information clerks	0.075
	bookbinders, printing machine operators, and job printers	0.067
	advertising sales agents	0.057
	correspondent clerks and order clerks	0.038
	other	0.641
Finance and Insurance (52)	bank tellers	0.155
	financial managers	0.080
	insurance claims and policy processing clerks	0.064
	credit counselors and loan officers	0.059
	insurance sales agents	0.056

Continued on next page

Table A.14: Low-wage workers occupations by industry

NAICS2	Occupation (2010)	Share
	other	0.586
Real Estate and Rental and Leasing (53)	janitors and building cleaners	0.192
	real estate brokers and sales agents	0.103
	property, real estate, and community association managers	0.055
	counter and rental clerks	0.049
	customer service representatives	0.042
	other	0.559
Professional, Scientific, and Technical Services (54)	secretaries and administrative assistants	0.097
	bookkeeping, accounting, and auditing clerks	0.063
	receptionists and information clerks	0.053
	billing and posting clerks	0.045
	office clerks, general	0.040
	other	0.704
Management of Companies and Enterprises (55)	secretaries and administrative assistants	0.084
	receptionists and information clerks	0.083
	heavy vehicle and mobile equipment service technicians and mechanics	0.073
	cashiers	0.073
	securities, commodities, and financial services sales agents	0.064
	other	0.622
Administrative and Support and Waste Management (56)	grounds maintenance workers	0.192
	janitors and building cleaners	0.184
	security guards and gaming surveillance officers	0.118
	maids and housekeeping cleaners	0.044
	laborers and freight, stock, and material movers, hand	0.031
	other	0.432
Educational Services (61)	janitors and building cleaners	0.231
	secretaries and administrative assistants	0.081
	teacher assistants	0.076
	elementary and middle school teachers	0.066
	other teachers and instructors	0.039
	other	0.508
Health Care and Social Assitance (62)	nursing, psychiatric, and home health aides	0.192
	medical assistants and other healthcare support occupations, nec	0.106
	preschool and kindergarten teachers	0.065
	personal care aides	0.062
	receptionists and information clerks	0.051
	other	0.524
Arts, Entertainment, and Recreation (71)	grounds maintenance workers	0.071
	recreation and fitness workers	0.065
	janitors and building cleaners	0.064
	security guards and gaming surveillance officers	0.056
	waiters and waitresses	0.054
	other	0.691
Accomodation and Food Services (72)	chefs and cooks	0.278
	waiters and waitresses	0.171
	food service and lodging managers	0.073
	food preparation and serving related workers, nec	0.055
	first-line supervisors of food preparation and serving workers	0.053
	other	0.369
Other Services (81)	automotive service technicians and mechanics	0.115
	hairdressers, hairstylists, and cosmetologists	0.075
	maids and housekeeping cleaners	0.066
	cleaners of vehicles and equipment	0.063
	janitors and building cleaners	0.054
	other	0.626
Public Administration (92)	police officers and detectives	0.230
	sheriffs, bailiffs, correctional officers, and jailers	0.139
	receptionists and information clerks	0.042
	secretaries and administrative assistants	0.040

Continued on next page

Table A.14: Low-wage workers occupations by industry

NAICS2	Occupation (2010)	Share
	office clerks, general	0.030
	other	0.519

Notes: The table reports the top 5 occupations (using 2010 census classification) for each 2-digit NAICS industry. Employment estimates are drawn from the Current Population Survey over 2001-2014. The sample includes all low-wage ($2-$15 per hour), full-time workers aged 22 to 50 in one of the 21 LEHD approving states.

Table A.15: Industry composition and switching after job loss

	Educ (61)		Manu. (31-33)		All others		Accom/food (72)		Healthcare (62)		Retail (44-45)	
Outcome	$Y(1)$	$Y(0)$	$Y(1)$	$Y(0)$	$Y(1)$	$Y(0)$	$Y(1)$	$Y(0)$	$Y(1)$	$Y(0)$	$Y(1)$	$Y(0)$
Earnings conditional on working	9447	12194	9017	10073	10605	11602	7848	8528	8816	8944	14728	14748
Share in initial industry group	.67	.88	.49	.82	.73	.83	.61	.73	.84	.83	.59	.75
Share in industry (NAICS2):												
11	.011	.0022	.002	.0071	.02	.018	-.0011	.0037	.0014	-.00019	.0016	-.0071
21	.00087	-.00046	.0038	.0032	.016	.016	-.00031	-.00012	-.0027	-.0013	.0048	.0057
22	.0091	.0048	.0046	.0013	.0076	.0019	.0044	-.00086	-.00014	.000031	.0044	.0061
23	-.00042	-.0044	.015	.014	.097	.12	.0055	.008	.003	.0083	.027	.029
31-33	.038	.011	.49	.82	.078	.044	.016	.021	.0065	.0054	.053	.039
42	-.0043	-.0022	.065	.0025	.052	.11	.015	.0073	.026	-.0097	.072	.061
44-45	.039	-.011	.092	.034	.072	.048	.088	.072	.031	.033	.59	.75
48-49	.000082	.019	.026	.0089	.082	.09	.018	.0063	-.0017	.01	.023	.0045
51	.012	-.004	.011	.0083	.041	.047	.0056	.0056	-.0064	.0043	.012	.0058
52	.023	-.018	.012	.0033	.081	.085	.012	.00095	.0045	.014	.019	.0016
53	.012	-.008	.0021	.0062	.026	.027	.028	.00086	.0069	.0056	.0079	-.0049
54	.0087	.045	.015	.017	.057	.047	.032	.013	.0046	.0077	.039	.031
55	-.0032	-.004	.011	.0069	.012	.0088	-.0073	.0035	.004	.0051	.015	.021
56	-.0036	-.017	.085	.028	.14	.15	.068	.038	.039	.024	.026	.019
61	.67	.88	.018	.0089	.014	.017	.017	.017	.012	.007	.013	.0035
62	.095	.038	.08	.0068	.058	.037	.067	.045	.84	.83	.039	.023
71	.0091	.011	.0075	.0052	.027	.032	.0079	.0043	.0019	.0072	.0041	.0067
72	.014	-.0088	.039	.016	.046	.022	.61	.73	.02	.012	.015	-.0027
81	-.0021	.0065	.019	.011	.037	.039	.0029	.014	.0084	.03	.021	.0068
92	.071	.06	.0066	-.0072	.039	.038	.012	.007	.0042	.0082	.0099	.0033

Notes: This table shows the effects of job loss on the industry distribution of low-wage workers after six years ($t = 24$). The sample is split according to the five most common initial two-digit industries among low-wage workers. "All others" includes all remaining industries not in the top five, such as utilities, construction, wholesale trade, and arts and entertainment. Columns labeled $Y(1)$ and $Y(0)$ report the industry distribution among job losers and stayers, respectively. To estimate complier means conditional on working (i.e., $E[Y(d) \mid Y(d) > 0]$), we use the equality $E[Y(d) \mid Y(d) > 0] = E[Y(d)]/P(Y(d) > 0)$. Unconditional complier means are estimated using standard methods (e.g., Imbens and Rubin, 1997).

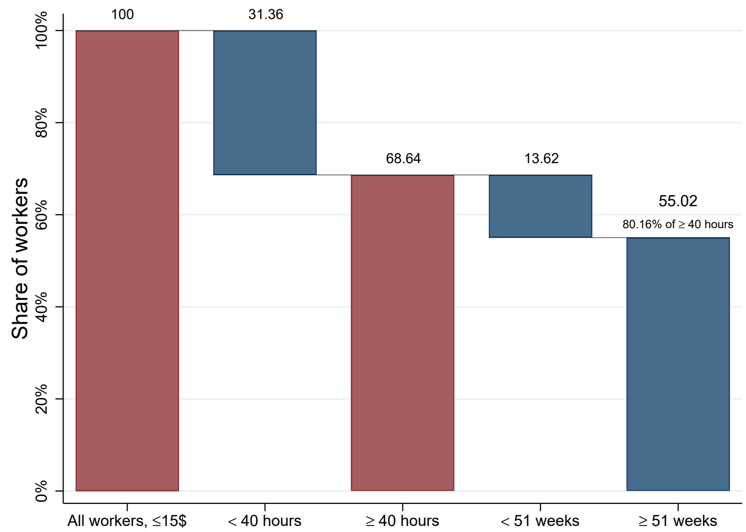
Table A.16: Changes in industry characteristics

	(1) Educ (61)	(2) Manu. (31-33)	(3) All other	(4) Accom/food (72)	(5) Healthcare (62)	(6) Retail (44-45)
Union share	-.061	-.0058	-.0014	.0069	-.0027	.0075
	[.35]	[.1]	[.098]	[.048]	[.087]	[.063]
Separations	.00023	.0024	-.00031	-.0013	.000077	-.00031
	[.025]	[.023]	[.029]	[.035]	[.026]	[.028]
Part-time	-.0072	.034	.0056	-.018	-.00052	-.008
	[.17]	[.073]	[.13]	[.25]	[.18]	[.17]
Full-time share	.0098	-.036	-.0057	.018	.00053	.0082
	[.81]	[.92]	[.87]	[.74]	[.81]	[.82]
Share low wage	.013	.037	.01	-.048	.0076	-.022
	[.45]	[.42]	[.45]	[.75]	[.44]	[.58]
Job tenure (years)	-.1	-.5	-.024	.16	-.02	.13
	[6.3]	[6.7]	[5.8]	[4.4]	[5.3]	[5.3]
Share in firms with $>$ 50 workers	-.037	-.035	.015	.0086	.0016	-.0083
	[.82]	[.67]	[.55]	[.55]	[.62]	[.62]
Average firm premia	.015	-.051	-.0052	.023	-.0023	.017
	[.15]	[.32]	[.25]	[.08]	[.19]	[.16]
Average worker effect	-.035	-.0015	-.013	.032	-.0042	.019
	[.07]	[-.051]	[-.0018]	[-.21]	[-.058]	[-.1]
Employment growth	-.022	.11	.025	-.037	-.0071	-.02
	[.09]	[-.21]	[.011]	[.22]	[.12]	[.041]
Average hours	.28	-.72	-.16	.26	.015	.22
	[39]	[42]	[41]	[39]	[39]	[40]

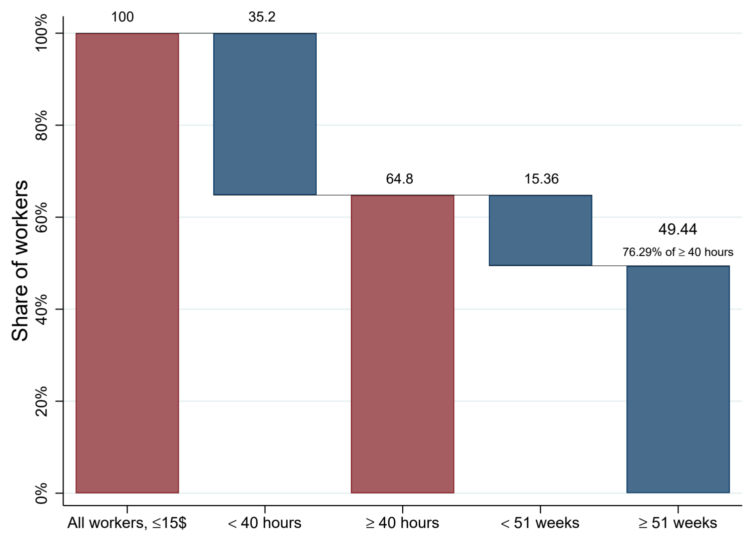
Notes: This table reports the estimated effects of job loss on the characteristics of the industry in which a worker is employed six years later, conditional on working. The industry characteristics are the same as in Figure 6, and the estimated changes in industry characteristics are based on the destination industry distribution reported in Table A.15. The square brackets in each cell report the mean of each industry characteristic among control (job stayers) compliers.

Figure A.1: Hours and weeks worked for workers with wage $\leq$ \$15

A) American Community Survey

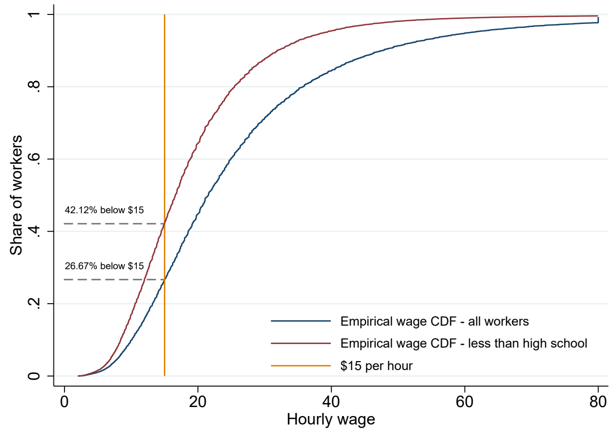


B) Current Population Survey



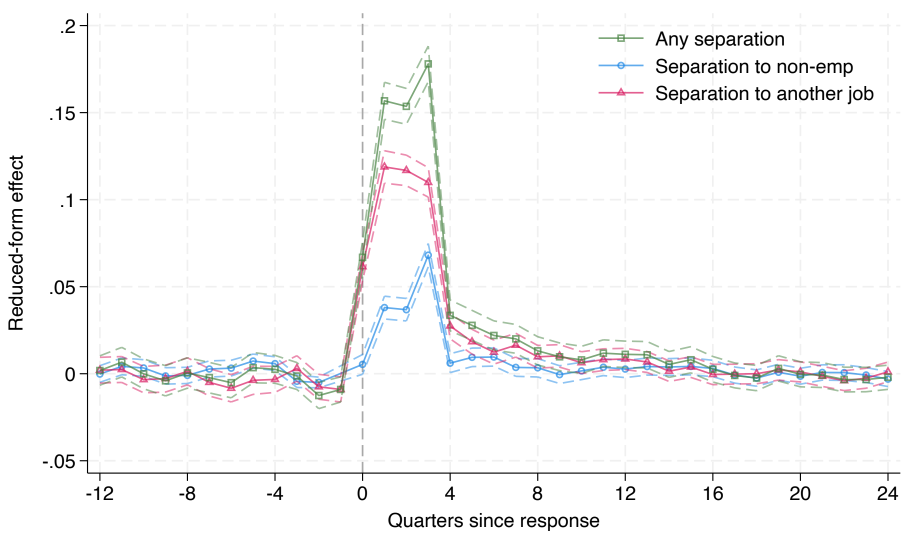
Notes: This figure shows the distribution of usual hours and weeks worked last year among low-wage workers. Panel A shows the results from American Community Survey data, while Panel B uses Current Population Survey data, restricting to participants in the Annual Social and Economic Supplement within either wave 4 or 8 (the Outgoing Rotation Groups, or "Earners study"). The samples cover 2001-2014 and respondents between the ages of 22 to 50, employed in an hourly job, and in one of our LEHD approving states. Both samples include only workers reporting hourly wages below \$15 and above \$2. For the ACS data, we impute hourly wages as total annual income from wages divided by number of weeks worked times usual hours worked per week, while for CPS data we used the reported hourly wage last week.

Figure A.2: Distribution of hourly wages among employed workers in the American Community Survey



Notes: This figure shows the distribution of hourly wages among employed workers. The figure is based on the authors' calculations using the publicly available American Community Survey, 2001-2008. We restrict attention to ACS respondents between the ages of 22 to 50 who are civilian employees, at work, who report usually working at least 40 or more hours per week and 51 weeks in the last year. To be consistent with the sample restrictions imposed in the analysis and to reduce measurement error, we also drop observations with implausibly low hourly wages (below $2 per hour). The plots contains two data series. The first is for all workers satisfying the above restrictions. The second is for workers with a high-school diploma or less (i.e., no more than 12 years of education).

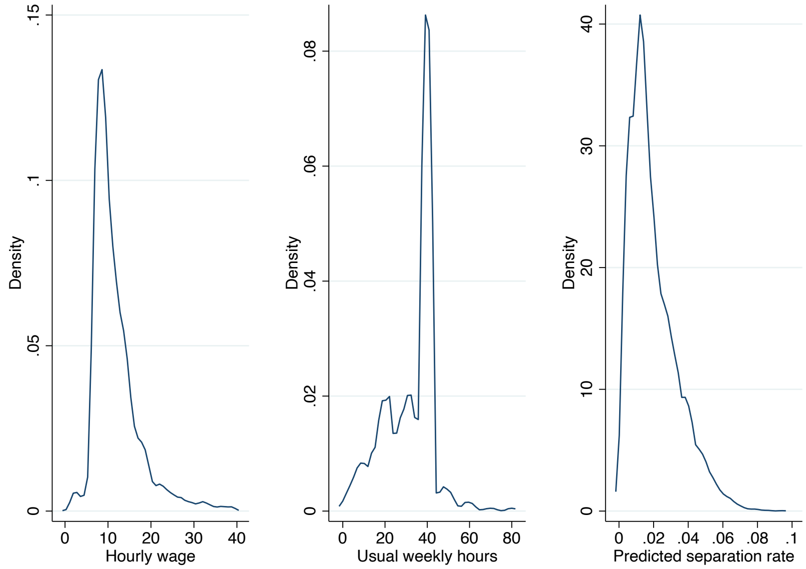
Figure A.3: Reduced-form effects on job separations by destination



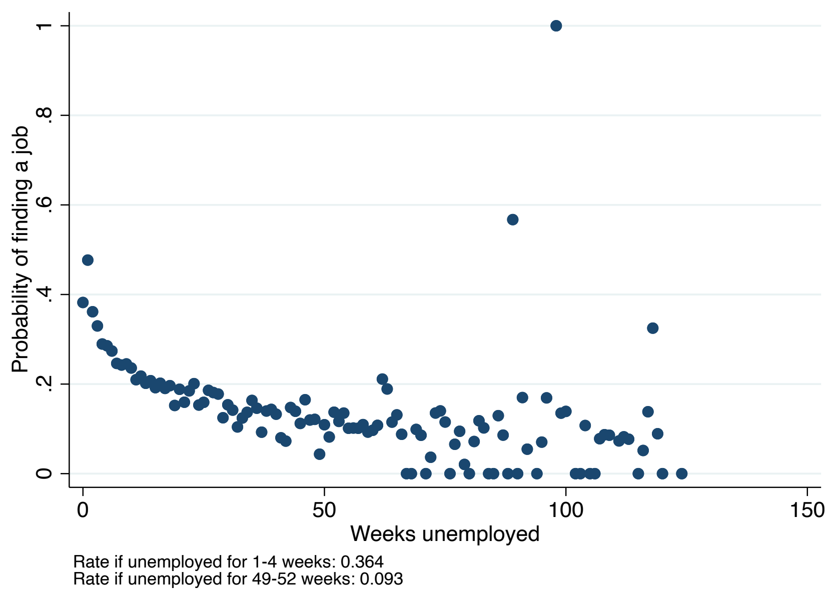
Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations, separations to non-employment, and separations to another job. Each coefficient and standard error comes from a separate regression using outcomes measured in the quarter indicated on the x-axis. The scale of the instrument implies the coefficients can be interpreted as the impact of 100% leave-out decrease in employment shock. Separation is an indicator for having zero earnings from your top-paying employer in the prior quarter. Standard errors are clustered by employer at $t = 0$.

Figure A.4: Offer distribution and unemployment exit hazards in the CPS

A) Wage, hour, and separation rate distributions

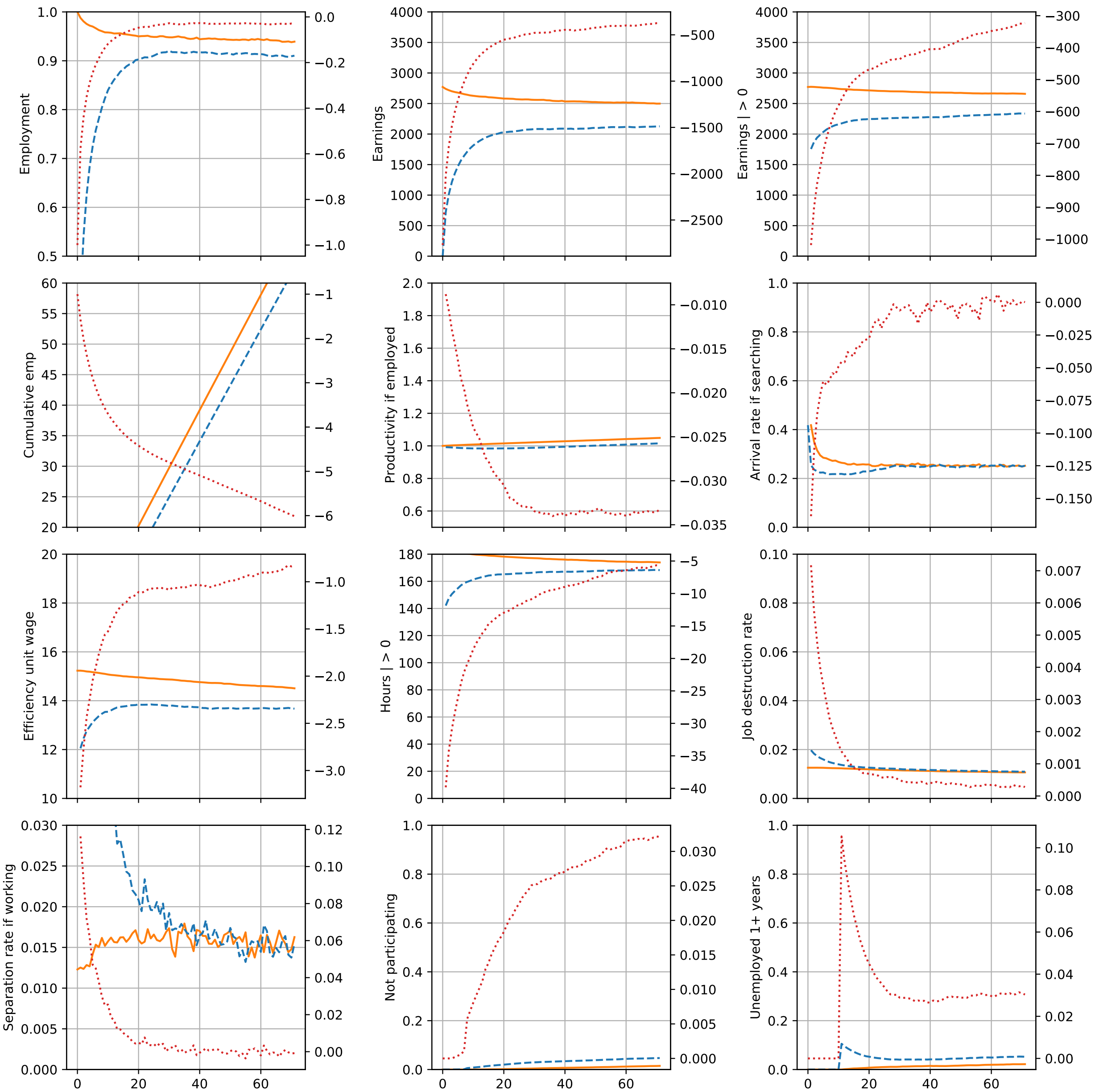


B) Unemployment exit hazards



Notes: This figure shows two analyses of CPS data used to calibrate the model in Section 5. Both panels use all individuals aged 22 to 50 in the Current Population Survey from 2010 to 2019, inclusive. Panel A plots the distribution of wages, hours, and predicted separation rates among workers transitioning from unemployment to employment across waves and for whom an hourly wage is reported, which requires the respondent to be in an outgoing rotation group. Predicated separation rates come from a regression of an indicator for transitioning from employment to unemployment across waves on a third-order polynomial in usual hours and three-digit occupation fixed effects. Panel B plots the probability of transitioning from unemployment to employment as a function of weeks unemployed. In both panels, observations are weighted to match the occupation distribution of all workers with hourly wages $\leq$ \$15 / hour.

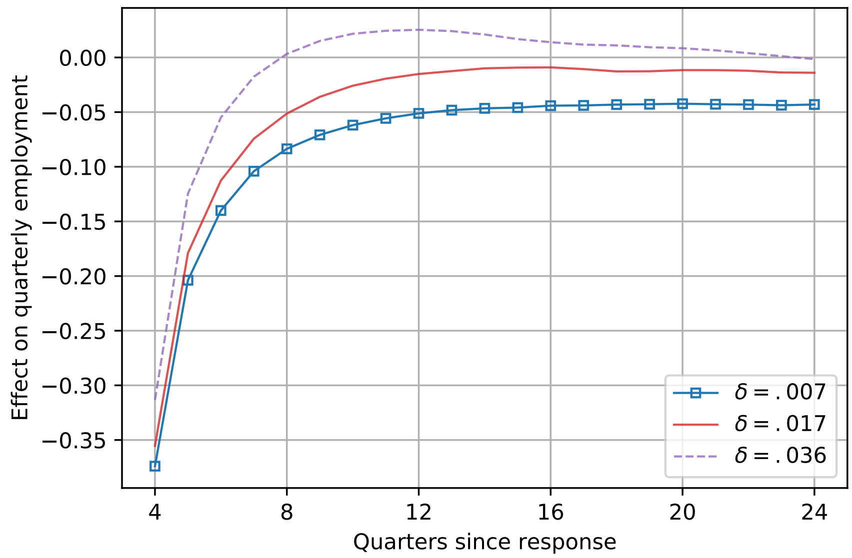
Figure A.5: Model-predicted effects of job loss on additional outcomes



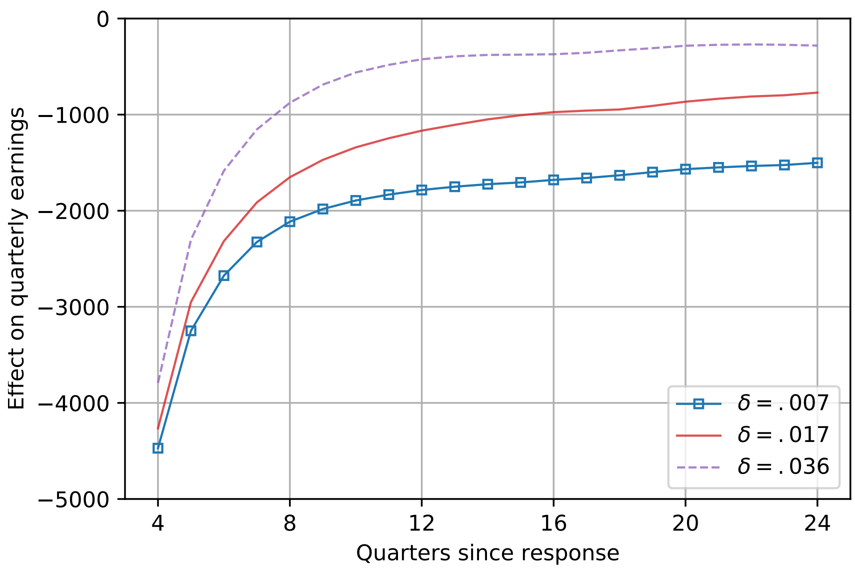
Notes: This figure presents simulated effects of job loss from the fully calibrated search model on several additional outcomes. Simulated effects are calculated by differencing employment and earnings six years later for a newly unemployed worker vs. worker in a \$15 / hour, full-time job, with both workers' initial human capital normalized to one. The employed worker's outcomes are averaged over the distribution of δ among \$15 / hour, full-time jobs in the offer distribution $F(\cdot)$. Outcomes are indicated by the y-axis labels. The unit of the x-axis is months. The solid orange line shows trajectories for the unemployed worker, the dashed blue line plots outcomes for the employed worker, and the dotted red line on the second y-axis reports the difference.

Figure A.6: Effects of job loss by initial job stability

A) Effects on employment

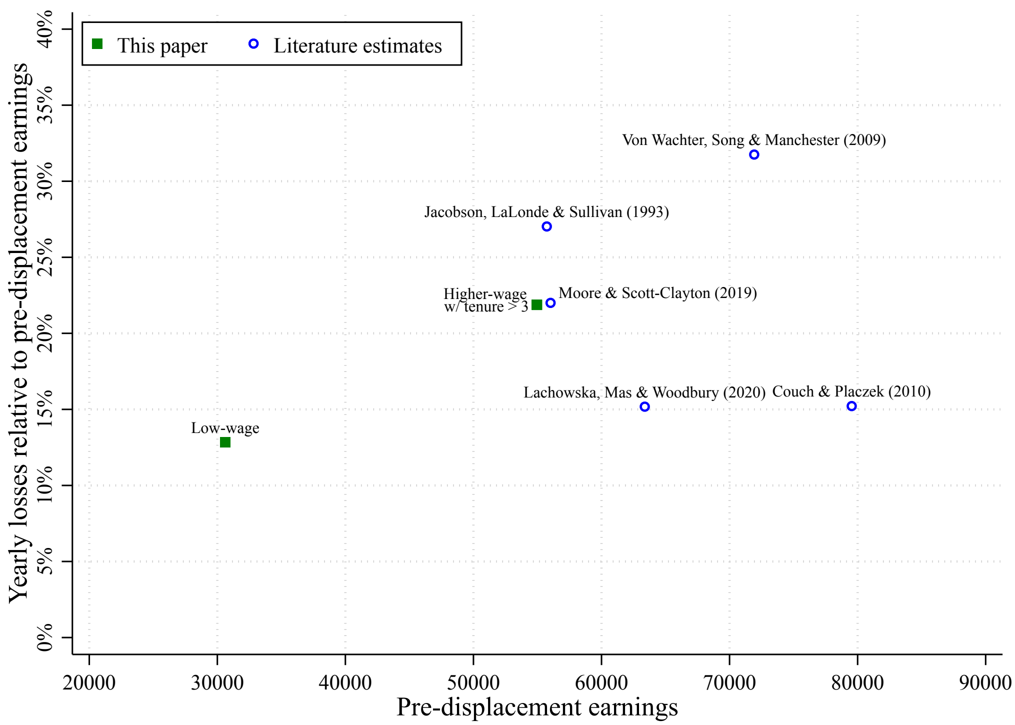


B) Effects on earnings



Notes: This figure presents simulated effects of job loss from the fully calibrated search model on earnings and employment. Simulated effects are calculated by differencing employment and earnings six years later for a newly unemployed worker vs. worker in a \$15 / hour, full-time job, with both workers' initial human capital normalized to one. Simulated effects reflect the average assuming job loss occurs in $t \in \{1, 2, 3, 4\}$ with probabilities proportional to the effect on separations in Figure 2. Effects relative to an employed initially worker holding a job with three different values of δ are plotted.

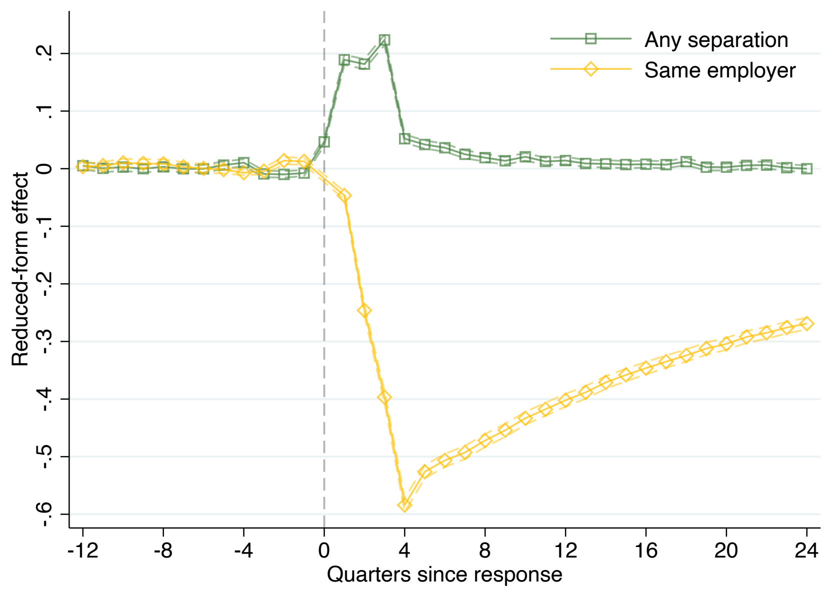
Figure A.7: Comparison to prior studies



Notes: This figure shows estimates of the effect of job loss among workers with at least three years of tenure from the literature using U.S. state-level administrative UI earnings records and a difference-in-differences design. Estimates from prior studies are marked by a blue circle. The estimates from this paper are marked by the green square. We report two estimates from this paper. We report two estimates from this paper. First, the effect of job loss on earnings from Table 2, which is titled "low-wage." Second, the estimate is for the sample of higher-wage workers with at least three years of tenure as of $t = 0$ and is titled "Higher-wage w/ tenure > 3."

Figure A.8: Reduced-form effects for higher-wage workers

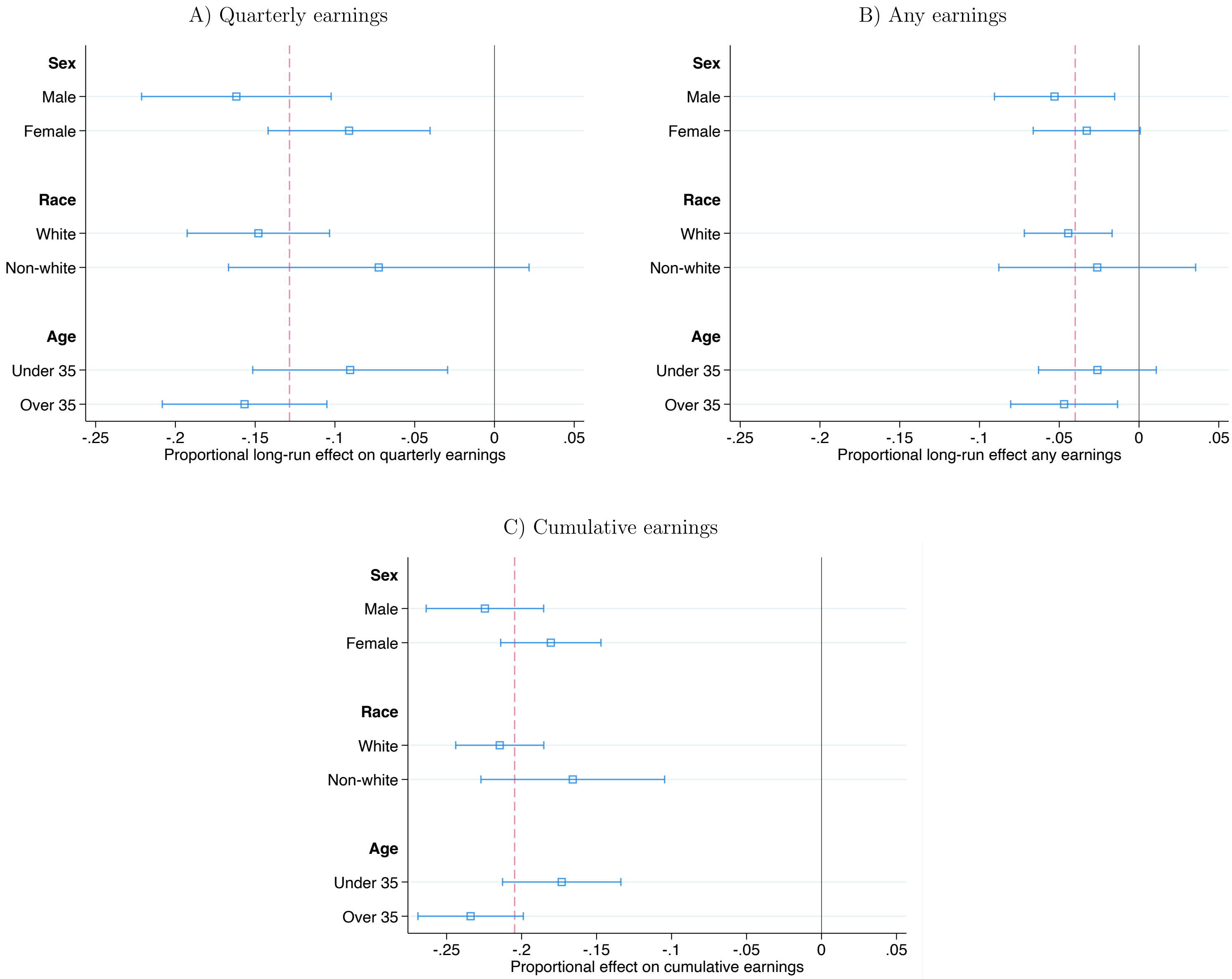
A) Separations



B) Employment and earnings

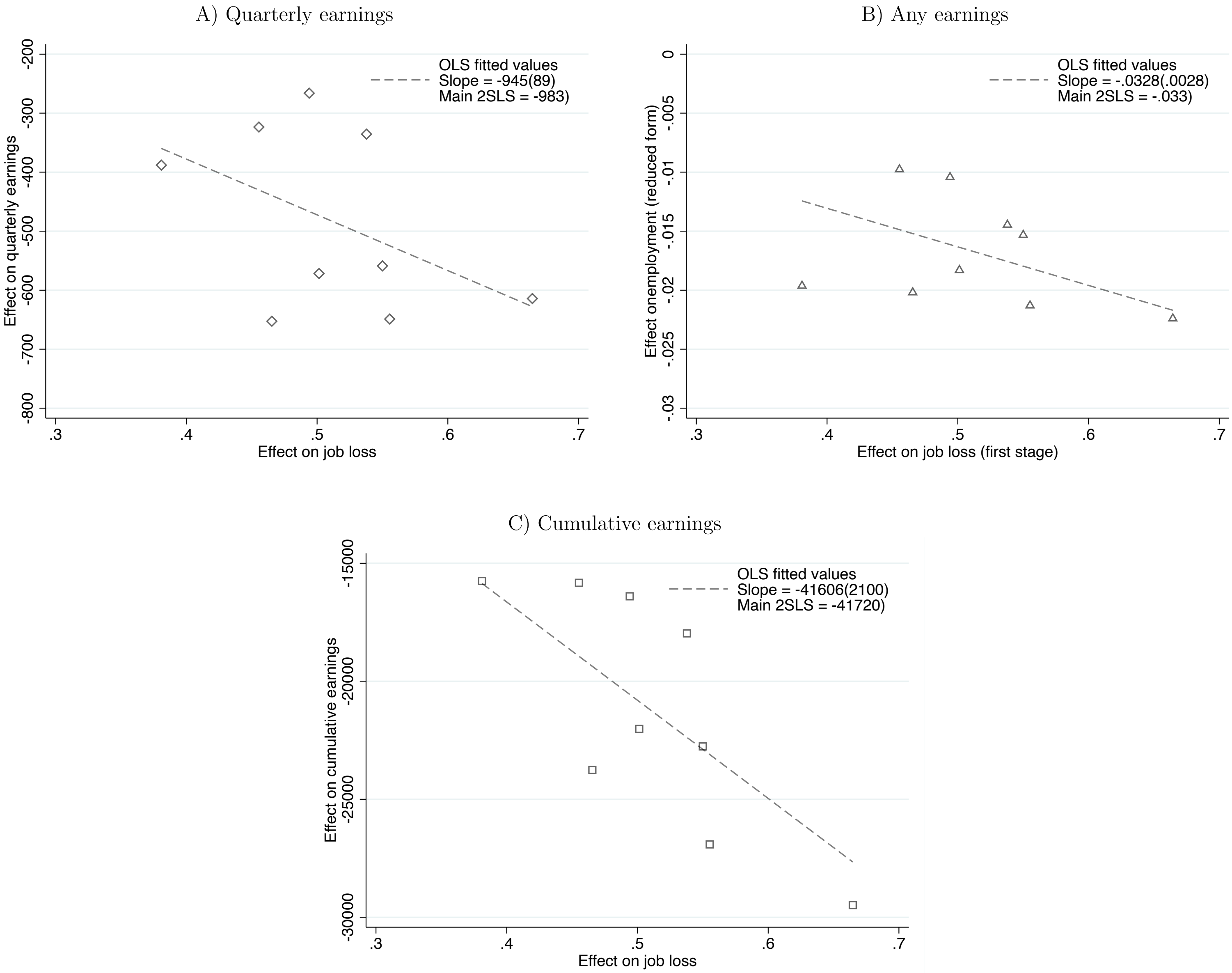
Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations (Panel A) and earnings and employment (Panel B) in the three years prior to and six years after initial ACS response for workers initially earning between \$15 and \$30 per hour. Each coefficient and standard error comes from a separate regression using outcomes measured in the quarter indicated on the x-axis. The scale of the instrument implies the coefficients can be interpreted as the impact of 100% leave-out decrease in employment shock. Separation is an indicator for having zero earnings from your top-paying employer in the prior quarter. Same employer is an indicator for having the same top-paying employer as at $t = 0$. Any earnings is an indicator for any earnings in LEHD nationally. Total earnings is the sum of quarterly earnings from all employers in the 21 LEHD states included in the study, inflated to 2020 equivalents using the CPI. Standard errors are clustered by employer at $t = 0$.

Figure A.9: Demographic heterogeneity in long-run effects of job loss



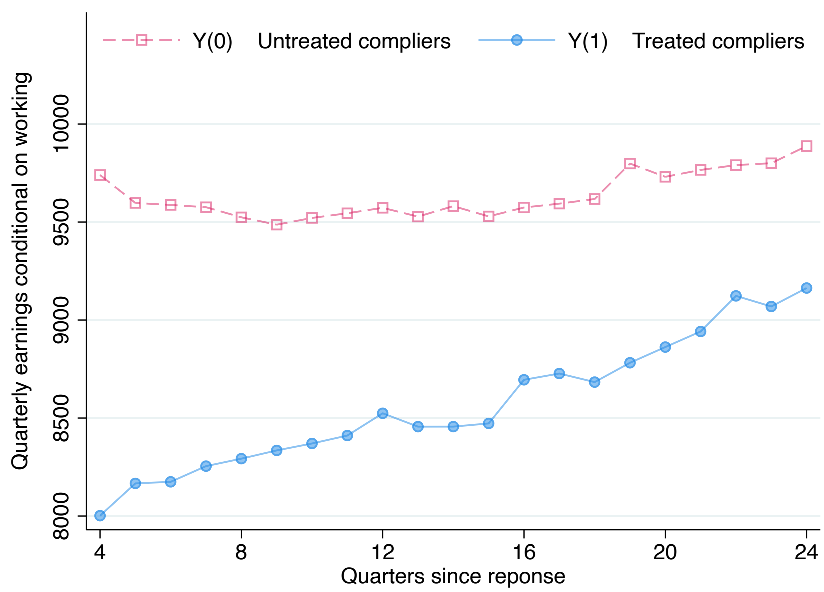
Notes: This figure plots 2SLS effects on long-run quarterly earnings (Panel A), employment (Panel B), and cumulative earnings (Panel C), splitting the sample by the observable characteristic listed. Each effect is divided by the relevant outcome mean for each sub-group to adjust for scale. Any earnings is an indicator for any earnings in the LEHD nationally. Total earnings is the sum of quarterly earnings from all employers in the 21 LEHD states included in the study, inflated to 2020 equivalents using the CPI. Standard errors are clustered by employer at $t = 0$. All models include the baseline set of controls and report effects at $t = 24$.

Figure A.10: First-stage vs. reduced-form effects across demographic groups



Notes: This figure plots first-stage effects on job loss and reduced-form effects on long-run quarterly earnings (Panel A), employment (Panel B), and cumulative earnings (Panel C). Each point corresponds to the estimated effect on job loss (x-axis) and the estimated effect on a long-run outcome (y-axis) in a different sample split by race, sex, or age. Any earnings is an indicator for any earnings in the LEHD nationally. Total earnings is the sum of quarterly earnings from all employers in the 21 LEHD states included in the study, inflated to constant 2020 dollars using the CPI. Standard errors are clustered by employer at $t = 0$. The line represents the OLS fit and the slope and standard error are reported in the top corner. The regression specification does not include an intercept. The intercept is not statistically significant when it is included. The 2SLS estimates reported at the top-right corner are from Table 2.

Figure A.11: Average earnings among working treated and untreated compliers



Notes: This figure shows estimates of average quarterly earnings in the LEHD data among the treated ($Y(1)$) and untreated ($Y(0)$) compliers conditional on working (i.e., observing some positive earnings in the LEHD data) using the standard formulas from Imbens and Rubin (1997). Each coefficient comes from a separate regression using outcomes measured in the quarter indicated on the x-axis. Quarterly earnings are measured using all employers in the 21 LEHD states included in the study, inflated to constant 2020 dollars using the CPI.

B Within-labor market placebo shocks

This appendix describes the permutation procedure employed to construct the estimates presented in Table A.4. We are interested in testing whether our instrument is correlated across firms in the same local labor market and therefore may capture local labor market shocks as opposed to idiosyncratic, firm-specific shocks.

Since our main specification includes state-by-NAICS2-by-year and quarter fixed effects, any common shocks to firms at this level would be absorbed. To explore whether shocks may be correlated within more narrowly defined markets, we construct "placebo" shocks by randomly permuting the instrument among firms in the same cell. Cells are defined as more granular variations on the groups defined by our baseline fixed effects. In one option, we replace states with commuting zones. Another option replaces NAICS 2 with NAICS 3 codes. A final option replaces both state and NAICS 2 codes with commuting zones and NAICS 3 codes, respectively.

To implement the test, we use the following procedure:

1. We begin by collapsing the data to the firm-by-cell level. Denote by Y_{jc} the average outcome for firm j in cell c.

2. To account for mechanical correlations explained in the next sub-section, we use a split sample technique when permuting shocks. Within a cell c, we randomly split the firms into two equally sized groups. We then assign each firm in the first group the shock of a random firm in the second group (without replacement). Denote each firm's assigned placebo shock $Z_{jc}^{placebo}$.

3. Using only the first group,[36] we then regress Y_{jc} on $Z_{jc}^{placebo}$ and the same controls as in our primary specification, Equation 1:

$$Y_{jc} = X_{jc}'\alpha^0 + \gamma Z_{jc}^{placebo} + \psi_{n(j,c),s(j,c),q(j,c)} + e_{jc} \tag{B.1}$$

   where $\psi_{n(j,c),s(j,c),q(j,c)}$ are our primary set of fixed effects for 2-digit NAICS ($n(j,c)$) by state ($s(j,c)$) by year and quarter ($q(j,c)$), and X_{jc} are the firm-level controls in Equation 1.

We repeat the above permutation procedure for 1,000 times and record estimates of γ and a standard error. Each cell in Table A.4 reports the average value of $\hat{\gamma}$ across these simulations and the average standard error. We conduct the procedure using as outcomes: the instrument—i.e., the firm's own shock, Z_{jc}; job separation by $t = 4$—i.e., the endogenous variable; and average earnings at $t = 24$ for the firm's $t = 0$ workers—i.e., a long-run outcome. The results show no significant correlation between placebo shocks and these outcomes.

B.1 Accounting for mechanical correlations

Care must be taken to ensure there is no mechanical correlation between $Z_{jc}^{placebo}$ and Y_{jc}. To understand the issue, consider the following simplified specification that omits the firm-level

[36]Cells with only one firm are excluded.

controls:

$$Y_{jc} = \gamma Z_{jc} + \psi_{n(j,c),s(j,c),q(j,c)} + e_{ic} \tag{B.2}$$

Assume that Z_{jc} is uncorrelated across all firms, so $Cov(Z_{jc}, Z_{j'c}) = 0\ \forall j \neq j'$. Then γ is given by:

$$\gamma = \frac{Cov(Y_{jc}, Z_{jc} - \bar{Z}_{j,c})}{Var(Z_{jc} - \bar{Z}_{j,c})} = \frac{Cov(Y_{jc}, Z_{jc}) - \frac{1}{N_{j,c}} Cov(Y_{jc}, Z_{jc})}{Var(Z_{jc} - \bar{Z}_{j,c})} \tag{B.3}$$

where $\bar{Z}_{j,c}$ is the mean of Z_{jc} within a state, NAICS 2 and time group $(n(j,c), s(j,c), q(j,c))$ and $N_{j,c}$ is the number of firms in this group. The second equality follows from the assumption that firm shocks are uncorrelated (both overall and within a fixed effect group).

If shocks are permuted within a cell c, then the specification becomes:

$$Y_{jc} = \gamma^p Z_{j'c} + \psi_{n(j,c),s(j,c),q(j,c)} + \zeta_{ic} \tag{B.4}$$

where $Z_{j'c}$ is the shock of another firm $j' \neq j$ in the same group c. Because these groups are nested by the groups that define the fixed effects $\psi_{n(j,c),s(j,c),q(j,c)}$, however, $\bar{Z}_{j,c}$ is unchanged. The coefficient γ^p will therefore be:

$$\gamma^p = \frac{Cov(Y_{jc}, Z_{j'c}) - \frac{1}{N_{j,c}} Cov(Y_{jc}, Z_{jc})}{Var(Z_{j'c} - \bar{Z}_{j,c})} = \frac{-\frac{1}{N_{j,c}} Cov(Y_{jc}, Z_{jc})}{Var(Z_{j'c} - \bar{Z}_{j,c})} \tag{B.5}$$

Thus, even if all shocks are completely uncorrelated, γ will not be equal to zero. Bias is larger when groups are small. The fundamental issue is that when shocks are permuted but all the data is retained, the firm's own shock Z_{jc} contributes to the demeaning step. A simple solution, however, is to use a split sample technique so that Z_{jc} is excluded from $\bar{Z}_{j,c}$. We do so by drawing placebo shocks from half the observations within each cell, assigning them to the other half, and estimating γ^p using observations from the first half only.

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