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 as opposed to 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, LaLonde and Sullivan (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), strong firm-worker matches (Lachowska, Mas and Woodbury, 2020), or firm and industry wage differentials (Krueger and Summers, 1988; Card, Rothstein and Yi, 2022; Schmieder, von Wachter and Heining, 2022) 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, (Maher, 2007; Alexander, Haley and Ruan, 2015; Lachowska et al., 2023*a*), job rationing induced by regulation, efficiency wages, or other forces, and skill degradation in unemployment (Mincer and Ofek, 1982; Kroft, Lange and Notowidigdo, 2013; Farber, Silverman and von Wachter, 2016; Dinerstein, Megalokonomou and Yannelis, 2022; Cohen, Johnston and Lindner, 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. This data is 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 workers earning $15 per hour or less in 2020-equivalent dollars between 2001 and 2014.

[1] These studies include Topel (1990); Jacobson, LaLonde and Sullivan (1993); Couch and Placzek (2010); Hijzen, Upward and Wright (2010); Von Wachter, Song and Manchester (2009); Davis and von Wachter (2011); Lachowska, Mas and Woodbury (2020); Schmieder, von Wachter and Heining (2022); and Bertheau et al. (2022), among many others.

These individuals predominately 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 meaningful fraction of workers over this follow-up period. The analysis sample includes over 230,000 workers at nearly 100,000 firms. While our primary results focus on initially full-time workers, we also study those initially employed part time.

A key empirical 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 turnover frequently for a plethora of reasons, including poor performance and superior outside offers. To isolate involuntary separations, we build on von Wachter and Bender (2006) and 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, LaLonde and Sullivan, 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

[2]Demand shocks at the local or industry level have been the focus of a large body of work, including Blanchard and Katz (1992), David, Dorn and Hanson (2013), Autor et al. (2014), Yagan (2019), Costinot, Sarvimaki and Vogel (2022), among others. Our estimates target a different causal effect, the impact of a idiosyncratic firm-specific demand shock.

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, 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 outcomes in the ACS provides further insight into the sources of these losses. The majority stems 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 increases in incarceration or cross-state mobility.

To translate these estimates into a summary metric of the value of a full-time low-wage job, we calibrate a simple Burdett and Mortensen (1998)-style job ladder model to our causal effects. Our estimates show quick initial rebounds but persistent intensive-margin losses, so the calibrated model requires a high job arrival rate and an offer distribution where the best jobs are relatively rare. A novel bound we derive implies an unemployed worker should be willing to pay at least three times monthly earnings to trade places with a worker holding a full-time $15 per hour job. While these rents are the result of search frictions in the model, we view the offer distribution as a reduced-form way to quantify displacement costs. Other forces that make full-time work difficult to obtain, such as skill depreciation in unemployment, would have similar implications for the value of the full-time job so long as they generate similar recovery profiles.

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. Looking across all 2-digit NAICS industries, we 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 of job displacement, 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 smaller; we cannot reject zero effects on either margin. Instead, reductions in hourly wages account for the bulk of earnings declines, consistent with prior work (e.g., Lachowska, Mas and Woodbury, 2020). Effects for high-wage workers vary strongly by tenure, with impacts on high-tenure workers broadly similar to those measured in analyses that use a traditional difference-in-differences design (e.g., Jacobson, LaLonde and Sullivan, 1993). There is no evidence of tenure heterogeneity among low-wage workers, however.

Our work builds on a large body of research measuring and interpreting 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, Mas and Woodbury, 2020; Fackler, Mueller and Stegmaier, 2021; Jarosch, 2021; Fallick et al., 2021; Gregory, Menzio and Wiczer, 2021; Helm, Kügler and Schönberg, 2023), their cyclicality (Davis and von Wachter, 2011; Huckfeldt, 2022; Schmieder, von Wachter and Heining, 2022), the role of industry- or occupation-specific human capital (e.g., Neal, 1995; Poletaev and Robinson, 2008; Milgrom, 2021), and differences across labor markets (Bertheau et al., 2022). 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 and Handwerker, 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 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. However, this finding does not mean that all displacements entail lasting adverse effects. Long-run losses are concentrated among workers in particular industries, implying that not all low-wage jobs are created equal. For instance, replacing a full-time, unionized, janitorial job in a public school is likely more challenging than finding new work as a bartender. If our results are driven by workers lucky enough to have held a particularly "good" low-wage job initially, they demonstrate that there is 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, Holzer and Lane, 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, LaLonde and Sullivan (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, Mas and Woodbury, 2020), others have found that accounting for zero earnings substantially impacts long-run losses (e.g., Von Wachter, Song and Manchester, 2009; Bertheau et al., 2022). 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, 1999, 2004, 2017). 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 approaches of Jacobson, LaLonde and Sullivan (1993) and von Wachter and Bender (2006).

Our study also relates to the long-standing literature on labor supply and hours constraints

[3] Jacobson, LaLonde and Sullivan (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).

(e.g., Rosen (1969); Altonji and Paxson (1986, 1988)). These constraints might be particularly important in the low-wage labor market. Dube, Naidu and Reich (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*b*) study hours constraints by combining revealed preference firm rankings with two-way fixed effect decompositions of firm and worker components of hours (Abowd, Kramarz and Margolis, 1999). They find broad evidence of hours constraints and 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 an important role in the low-wage job ladder.

1 Data and sample construction

This section describes the data sources from the U.S. Census Bureau used in the analysis. We detail the construction of the primary analysis sample of low-wage workers. We then present and discuss summary statistics.

1.1 Data sources

Our primary source of earnings data is the Census Bureau's Longitudinal Employer Household Dynamics (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, which typically reflect the entity reporting UI taxes to state authorities and may comprise multiple establishments. LEHD data contain a separate quarterly earnings record for each worker-firm

[4]Other work explores the gap between actual and desired hours more broadly (e.g., Kahn and Lang, 2001; Johnson, 2011; Alexander and Haley-Lock, 2015; Faberman et al., 2020; Schneider, 2021).

[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.

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 earning 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 American Community Survey (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 Quarters, which includes correctional facilities, to construct a measure of incarceration. As in the LEHD data, we inflate all nominal outcomes in the ACS to 2020 equivalents using the CPI.[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

[6] Washington State is one exception and does collect information on hours (Lachowska, Mas and Woodbury, 2022), although this data is not part of the LEHD data we have access to.

[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 99 percentile, excluding zeros, within each state.

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

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 B.1. In our heterogeneity analyses, however, we also examine effects on part-time workers and drop any 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 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 responses to surveys through 2020 to the panel. We

[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.

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

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 percentile 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, a sample with pre-displacement earnings levels more comparable to that of workers studied in the prior literature. As we show below, we find similar impacts as in previous work for this higher-wage sample. 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.

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

[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.

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 B.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 characteristics, 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. The implications of restricting to wages below \$15 per hour are especially useful for understanding the composition of our sample. 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

[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 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.

industries such as retail trade and accommodation and food services.[15] 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 15.84% of 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 and imposing the sample restrictions necessary for our empirical design described below increase the white and female share of our final analysis sample, however.

Lastly, our sample of low-wage workers would also be typically considered "low-skilled" based on their occupations. Figure B.5 plots the distribution of workers across occupations by the occupation's average wages of full-time workers. Low-wage workers are employed in similar or lower average-wage occupations than workers with no more than a high-school degree. Moreover, low-wage workers are employed in occupations with substantially lower average wages than workers earning $15 to $30 per hour or manufacturing workers.

2 Empirical strategy

This section develops our empirical strategy, which isolates exogenous changes in labor demand using coworkers' separation rates and compares it to traditional approaches for studying the consequences of job loss, including mass layoffs (e.g., Jacobson, LaLonde and Sullivan, 1993) and analyses of the Displaced Worker Survey supplement to the Current Population Survey (e.g., Farber, 1993). We then present and discuss tests of our identifying assumptions.

2.1 The instrument

Our empirical strategy requires idiosyncratic shocks to labor demand. We use the change in firm-level employment over the next year measured in the holdout sample of workers otherwise excluded from our analysis. This measure is defined for firm j in quarter t as total employment in quarter $t+4$ divided by employment in quarter t. Employment includes all workers at the firm not included in our analysis sample. Using this shock as an instrument for job loss builds on von Wachter and Bender (2006), who construct a continuous instrument based on firm-level fluctuations in retention rates to study the impacts of early career job loss

[15]Figure B.3 provides a more complete breakdown of the relative industry distribution. Figure B.4 shows that workers with hourly wages of $15 to $30 are distributed more similarly to the average full-time worker.

for German workers in apprenticeships. The approach is also inspired by Davis, Faberman and Haltiwanger (2012), who observe that layoff rates increase smoothly in year-over-year firm growth rates with a sharp kink at zero.

Using year-over-year changes limits the impact of seasonal fluctuations in employment. To reduce noise and exclude very new firms just starting up, 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 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.

A key feature of this design is that treatment varies at the firm rather than the worker level. Regressing outcomes on Z_i involves implicit comparisons between workers at firms that receive larger versus smaller employment shocks. There is no need to specifically condition on job separation among treated workers or job-staying among controls. As a result, we could conduct the analysis on firm-level outcome means. We instead opt to analyze worker-level outcomes, allowing us to easily incorporate additional individual-level controls and examine effect heterogeneity by individual characteristics such as tenure, and cluster standard errors by firm.

2.2 The controls

Because employment shocks are not randomly assigned across firms (e.g., Hilger, 2016), a key threat to our design is that they are correlated with differences in workers' skills or preferences. To compare individuals working in similar firms, we use 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.

[16]Because firms are identified only with anonymous administrative labels in the LEHD data, some large reductions in employment and shutdowns 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%.

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 interacted with the year and quarter of initial ACS response. This implies our effects are estimated using variation among workers working 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, such as prior earnings and job separation rates. 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.

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 tenure at $t = 0$, and Z_i is the firm-level 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 the 2SLS estimates would change using alternative definitions of S_i.

2.4 Why this strategy?

Since Jacobson, LaLonde and Sullivan (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] The implicit assumption is that, absent the mass-layoff, all 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, for example, is seven quarters. The set of high-tenure low-wage workers may represent a relatively selected 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, Shapiro and Sorkin, 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.

A large literature also uses the Displaced Worker Survey (DWS) to study displacement. While the DWS includes information on the cause of separation (e.g., plant closure), it also has several known drawbacks, including a lack of earning history, potential bias due to changes in the recall period, and potentially undercounting of job displacement events (Von Wachter, Handwerker and Hildreth, 2009; Farber, 2010). Moreover, because the DWS is restricted to displaced workers, the researcher must also construct a comparison group of workers who were not displaced. Thus, we view our approach as the one best suited for the questions that motivate our analysis and the available data.

[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.

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., Greenwald, 1986; Gibbons and Katz, 1991). Including firm-level controls reduces the imbalances somewhat, but meaningful differences between those who separate and do not remain. 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

[18] Table A.3 reports the point estimates used to construct Figure 1.

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

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 level 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 presents the results. Markets are defined as more granular variations on our baseline state-by-NAICS2-by-year-quarter fixed effects. Column 1 replaces states with commuting zones, Column 2 replaces NAICS 2 with NAICS 3 codes, and Column 3 replaces state and NAICS 2 codes with commuting zones and NAICS 3 codes, respectively. We conduct 1,000 permutations. In each permutation, we assign a firm a placebo shock from another firm in the same market 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 entry in the table reports the average value of the regression coefficient and the average standard error. The results show 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 $t = 24$. These estimates reinforce our interpretation that the instrument indeed identifies firm-specific shocks that are unrelated to local labor market conditions.

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 an exclusion restriction that Z_i only affects outcomes through S_i. It is possible that exclusion is 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 to reality in our setting. Interpreting Equation 2 through the nonparametric local average treatment effect (LATE) 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 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 would be suggested by some models of "slippery" job ladders (Krolikowski, 2017; Jarosch, 2021) and as was found by some work using the PSID (Stevens, 1997).

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%.

Panel B of Figure 2 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 follows a similar pattern to the indicator for any earnings. Six years after the initial ACS response, heavily shocked workers have \$500 lower earnings per quarter, or about 7% of the sample 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 at $t = 24$ 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 $t = 0$. 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.

Table 2 also shows that a large share of the estimated effect of job loss on having any

[20] Table 2 shows that the reduced-form effect on having any earnings in one of the 21 states where we observe earnings records is about 0.5 percentage points more negative than the effect on any earnings nationally, which may reflect some migration responses. We return to this question when analyzing ACS outcomes below.

earnings is explained by non-employment for at least eight quarters (3 percentage points out of 3.3), which increases by 38% 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; Juhn, Murphy and Topel, 2002). 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.

3.2 Extensive versus intensive margin effects

A natural question is what share of these long-run earnings impacts are explained by extensive versus intensive margin reductions in labor market activity. 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.

The "implied extensive-margin effect" reported in Table 2 provides another assessment of intensive-margin responses by estimating effects on the constructed outcome $1\{y_{i,t} > 0\} \cdot y_{i,-1}$, where $y_{i,t}$ is earnings t quarters since initial ACS response. If earnings levels were unaffected by the shock except through whether workers had any earnings at all, we would expect impacts on this outcome to match those on overall earnings. Effects on this outcome are only 38% of the total effect, however, implying substantial intensive margin reductions as well.[21] We show below using ACS that these reductions in earnings come primarily from changes in hours and weeks worked.

In the final part of the paper, we estimate treated and untreated earnings levels for workers

[21] This exercise is most credible when earnings trajectories are relatively flat in the absence of job loss, so that earnings at $t = 0$ are a good approximation to full-time earnings several years later. Figure B.9 shows that this is approximately true.

whose job loss is affected by our instrument (Imbens and Rubin, 1997; Abadie, 2002). 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 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.[22] Put another way, the actual effect must also include substantial differences in mean earnings conditional on having any earnings. These means are plotted directly in Figure B.9, which shows a \$724 intensive-margin reduction as of $t = 24$.[23]

3.3 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 3 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.[24] 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 from the labor force.

3.4 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 ($Z_i =$

[22] 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.

[23] 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.

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

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, Katz and Krueger, 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-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 $t = 24$. In addition, in Section 6, we show that visual instrumental variables plots based on estimating effects within sub-groups (e.g., sex or age) also support the exclusion restriction. 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.

Lastly, we 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

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

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.

3.5 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 4. 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 4 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. Most of this difference is accounted for by a 3.2 percentage point increase in the probability of unemployment, although there is also a large increase of 2.6 percentage points in labor force dropout. Since many individuals who report not participating may still be searching for jobs, effects on looking for work may be a more reliable measure of participation. 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.

The next panel of Table 4 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. To provide a simple and transparent assessment of intensive-margin wage adjustments, Table 4 also estimates effects on $1\{\text{Hourly wage}_{i,t} > 0\} \cdot (\text{Hourly wage})_{i,0}$, mirroring our tests of intensive margin earnings adjustments above. These effects are roughly a third of the total effect, indicating that both intensive and extensive-margin wage reductions play a role.

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 assess the reduction in treatment effects from changing each component, means of which are also reported in the text on the figure.[26] 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 earn-

[26] Estimates of these quantities are also presented in more detail in Table A.6.

ings 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.

3.6 Other ACS outcomes

Migration. Job losers may insure themselves by moving in with friends and family (Huttunen, Møen and Salvanes, 2018). We can examine migration responses in two ways. First, using the ACS follow-up sample, we find no impact on whether the ACS response occurs in a different state than where the ACS respondent was initially surveyed at $t = 0$. However, the estimates are noisy, and we cannot reject migration effects of up to two percentage points. Of course, it is also possible that sub-state migration still plays an important insurance role. Second, in the LEHD data, we can compare long-run effects on employment using only earnings in the states in our data set (4.3 percentage point decrease), including the state in which job loss occurred, to estimates using the indicator for some earnings in *any* LEHD state, regardless of whether it is in our data set or not (3.3 percentage points). Absent any effect of job loss on migration, effects on employment estimated in our data set should be attenuated toward zero relative to effects on any employment nationally, which is the opposite of what we find.[27] Thus, there is some indication of migration responses; however, given the standard errors, we cannot reject the null of no effects on migration.

Criminal justice. Job losers may resort to crime, leading to entanglements with the justice system that in turn reduce labor market activity. The final row of Table 4 shows that we find no statistically significant effects on being enumerated in Group Quarters, which is predominately comprised of carceral institutions for this sample, although standard errors are relatively large. Given the low rates of incarceration overall, however, it seems unlikely that criminal justice contact explains long-run earnings declines.

[27] To see this, consider a simple example. Assume a migration rate of M towards states uncovered by our earning records and a baseline employment rate of 80% among treated (job losers) and control workers. Let the effect of the treatment be τ. The estimated effect using only approving states is $(0.8 - \tau) \cdot (1 - M) - 0.8 \cdot (1 - M) = \tau * (1 - M)$. Hence, as migration rates increase, the effects in the restricted data should be attenuated toward zero.

4 Quantifying losses

The sizable impacts of job loss we estimate imply the value of keeping a low-wage job may be large. The estimates in Table 2, for example, show cumulative losses over a six year horizon of roughly \$42,000. At a 5% annual discount rate, the implied present value of losses is roughly \$36,000. As noted in the previous section, however, these impacts reflect a combination of participation responses and decreases in weeks, hours, and wage rates among displaced workers. Non-participants may have chosen to substitute to activities they value more than seeking work, including home production, and unemployed workers may collect benefits while they are jobless. Both forces imply simple present-value calculations likely overstate the value of a low-wage job.

An alternative approach to quantifying the value of a full-time low-wage job is to interpret our estimates through a simple model. We do so by considering a discrete-time job ladder model in the style of Burdett and Mortensen (1998). Jobs are characterized by a bundle of wages and hours that yield earnings e. Offers arrive each period with probability λ from distribution F both on and off the job.[28] Existing jobs are destroyed at exogenous probability δ, while unemployed workers enjoy utility b, which captures both the value of leisure and any consumption funded by non-work income sources such as social insurance. Given a discount rate β, the value of being unemployed can be written as:

$$V_u = b + \beta \left(\lambda \int_{e*}^{\infty} V(x)dF(x) + (1-\lambda)V_u \right) \tag{4}$$

where e^* is the reservation level of earnings.[29] The value of holding a job with earnings e can be written as:

$$V(e) = e + \beta \left(\lambda \int_{e}^{\infty} V(x)dF(x) + \delta V_u + (1-\delta-(1-F(e))\lambda)V(e) \right) \tag{5}$$

Since the flow utility associated with holding a job that pays earnings e is e itself, this model features linear utility over earnings. To account for non-participation, we assume workers who are unemployed draw values of non-participation V_n from an exogenous distribution G and drop out of the labor market (i.e., stop searching entirely) if $V_n > V_u$.

Let $\omega(e) = (V(e) - V_u)/e$ capture the rents as a fraction of earnings associated with holding

[28]While some studies find lower job arrival rates on vs. off the job, Appendix E shows that they are more comparable for low-wage workers.

[29]That is, the earnings level that satisfies $V_u = V(e^*)$. We assume that $F(e^*) = 0$. If unacceptable job offers are made, this assumption is equivalent to re-normalizing $\tilde{\lambda} = \lambda(1 - F(e^*))$ as the arrival rate of minimally acceptable job offers.

a job that pays e relative to being unemployed. This metric has the natural interpretation as the multiple of e that an unemployed worker would be willing to pay to trade places with a worker in a job paying e. While computing $\omega(e)$ requires full knowledge of the model parameters, the following proposition, proved in Appendix D, establishes a simple bound that provides some intuition for when it may be large:

Proposition 1. *The proportional rents associated with holding a job with earnings e are bounded below by:*

$$\omega(e) \geq \frac{2(1+r)}{2(r+\delta)+\lambda(2-F(e))}(1-\rho_e) \geq \frac{1+r}{r+\delta+\lambda}(1-\rho_e)$$

where $r = (1-\beta)/\beta$ *and* $\rho_e = b/e$.

The rightmost inequality provides the weakest bound on rents. It is effectively an appropriately discounted difference in flow utility between unemployment and holding an e-level job. Existing calibrations imply this bound should be small because jobs turnover frequently enough that holding one at any given point in time is not very valuable. Shimer (2012), for example, reports monthly job-finding rates of 43% and separation rates of 3% using Current Population Survey Data. Using an annual interest rate of 5% and setting $\rho_e = 0.5$ implies rents are worth roughly one month's earnings. The second bound tightens this inequality by taking account of how *rare* an e-level job is. A job at the median of the offer distribution offers rents at least 30% higher. A job at 90th percentile features 70% higher rents.

We estimate this model by picking parameters $\lambda, \delta, Pr(V_n > V_u)$ and F, which is assumed to be discrete, to match our causal effects. The procedure, detailed further in Appendix D, targets the following moments for both treated and untreated compliers via diagonally weighted minimum distance: the probabilities of observing zero earnings and earning less than \$4,000, \$5,000, \$6,000, \$7,000, and \$8,000 over $t = 4$ to $t = 24$. We assume eight points of support in F, with one point at each of these quarterly earnings levels and one final level treated as an additional parameter. The model is set at the monthly level.

Table 5 shows the results. Column 1 reports estimates of the core parameters and the cumulative mass function of the discrete monthly earnings distribution. Estimated λ and δ are 0.29 and 0.016, respectively, while the likelihood of transitioning to non-participation is 0.18.[30] The estimated distribution of job offers exhibits heavy right skew. Nearly 70% of offers entail earnings below \$1,333 per month, or approximately 22 hours per week at \$15

[30]As shown in Figure B.10, these estimates are similar to those from other studies using U.S. data. In Appendix E, we show that low-wage workers have comparable monthly transition rates in a panel of CPS respondents.

per hour. Less than 10% of job offers are estimated to pay more than \$2,333 per month.[31] These parameters provide a tight fit to the targeted moments, as shown in Figure B.8.

Columns 2 through 4 of Table 5 report the implied bounds on $\omega(e)$ at each level of earnings. Utility in unemployment b is set at \$1,333, implying an approximately 50% replacement rate for a full-time \$15 per hour job. Because λ is large relative to r and δ, the lower bound implies relatively small rents of approximately $3.2 \cdot \rho_e$. A \$2,666 per month job, for example, is worth about two months worth's pay. Column 3 shows that accounting for rarity of good job offers significantly increases theirs rents. Rents in a job paying \$2,666 per month, for example, are at least 317% of earnings. The final column uses the full structure of the model, including the discrete offer distribution, to compute rents exactly. These values are higher than both bounds and sometimes significantly so. They are, however, meaningfully lower than the present value of cumulative earning losses.

In the model, these rents reflect the fact that some workers are "lucky" enough to land a high-earning job, while others are not. We view the random job arrival process in the model as a reduced-form representation of a more general set of frictions than just luck. Rents may reflect the rewards of costly search effort, information acquisition, or other investments, for example, implying that the rents are only non-zero ex-post. More fundamentally, our estimates imply job loss entails a) relatively short unemployment spells for participants and b) large intensive-margin earnings losses. If F accurately describes transition probabilities to the better but rare opportunities that drive these intensive-margin losses, then the bounds in Column 2 of Table 5 illustrate that the costs of job loss for low-wage workers are likely to be substantial.

5 Comparing impacts on higher-wage workers

We next test 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 B.6, higher tenure workers in this sample tend to have similar pre-displacement earnings to workers in other analyses of displacement that use administrative records and a mass-layoff

[31] Despite the concentrated offer distribution, Figure B.11 shows that the implied accepted offer distribution still has a relatively thick right tail compared to benchmarks from the CPS. Intuitively, relatively low δ and high on-the-job job arrival rates still allow individuals to concentrate in the best job offers over time, even if offers are rare. The model, therefore, does not appear to imply an implausibly skewed observed earnings distribution.

research design.

Figure 5 presents the reduced-form effects of firm-specific labor demand shocks on separations, job loss, employment, and earnings for these workers. The pattern of effects Panel A is remarkably similar to that in Figure 2. Consistent with the validity of the design, the shocks are uncorrelated with all outcomes prior to $t = 0$. The increase in separations over $t \in [0, 4]$ results in large reductions in the probability of remaining with the same employer, which falls by 58% at $t = 4$. The dynamic effects on employment and earnings show sharp and immediate drops that recover sluggishly and stabilize at lower levels after six years. 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%).[32] These effects include all high-wage workers, but when we restrict to workers with at least three years of tenure, Figure B.6 shows that estimated effects line up closely with the prior literature.[33]

Table 6 presents long-run effects measured in ACS outcomes and documents sizable losses in wages and income, with total wage earnings declining by more than $7,200. Losses here are slightly smaller than what is reported in LEHD data, suggesting some substitution to activity potentially not covered in the administrative data sources. 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 lower-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.

[32] Point estimates for long-run effects on LEHD outcomes for higher-wage workers are presented in Table A.7.

[33] Table 7 explores tenure heterogeneity explicitly and is discussed further in Section 6.

6 Worker-level heterogeneity

Table 7 reports 2SLS effects on long-run earnings outcomes when splitting the sample by workers' tenure with their initial employer at $t = 0$. Among low-wage workers (Columns 1 and 2), there is no evidence of differences by tenure in either the estimated long-run effects or their size relative to the sub-group mean outcome. Point estimates for quarterly earnings at $t = 24$ suggest somewhat larger earning losses for workers with shorter tenure, if anything. Importantly, even high-tenure workers in our sample have relatively low wages as of $t = 0$. These are therefore workers who have not experienced substantial on-the-job earnings growth. It is therefore possible that *all* workers in our sample possess limited firm-specific human capital or valuable matches with their initial employer regardless of tenure. Instead, job availability and hours constraints may be better explanations for the long-run earnings losses suffered by these workers.

A different pattern of effects emerges among higher-wage workers (Columns 3 and 4), who show evidence of significant 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 10% greater than 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 workers. A similar pattern also emerges for effects on employment and cumulative earnings. In fact, impacts on low-tenure high-wage workers are not statistically distinguishable from zero. These findings suggest a more important role for specific human capital or match effects among higher-wage workers, consistent with some prior research (Topel, 1991; Farber, 1993; Neal, 1995; Stevens, 1997; Lachowska, Mas and Woodbury, 2020).[34]

Figure 6 explores effect heterogeneity across various important demographic sub-groups. Each estimate and confidence interval corresponds to an estimated effect on total quarterly earnings (Panel A), employment (Panel B), or cumulative earnings (Panel C) when splitting by the group characteristic indicated in the row. To facilitate comparisons across groups, we divide each effect by the group's outcome mean as of $t = 24$. The red dotted line in the background shows the estimated proportional effect in the full sample.

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

[34] Some work, however, finds insignificant tenure effects among workers with at least three years on the job (Von Wachter, Song and Manchester, 2009).

after job loss motivated by substitution to alternative activities like childcare. Overall labor force participation rates for prime-age men over our sample period was roughly 90%; we expect this number to understate the degree of labor force participation for the men in our sample given that all workers were employed full-time as of $t = 0$. Most studies, from both the U.S. and Europe, find that women suffer larger earning losses from job loss than men (e.g., Maxwell and D'Amico, 1986; Crossley, Jones and Kuhn, 1994; Illing, Schmieder and Trenkle, 2022; Aloni and Avivi, 2024); however, the previous literature has not focused on low-wage workers.

We also find similar effects on white and non-white workers, although standard errors are large for the relatively small non-white sample. There is little prior work on differences in the cost of job loss by race. Most recently, Sorkin (2023) found larger losses in LEHD earnings data among displaced Black workers with at least 11 years of tenure. This sample is meaningfully different than the one we study, in which most workers have less than two years of tenure at the time of displacement. There is also some evidence from the Displaced Worker Survey that young Black males are more at risk of and suffer more considerable costs from job loss (Fairlie and Kletzer, 1998).

Finally, Figure 6 also shows that we find similar results for workers under versus over 35. Consistent with prior work such as von Wachter and Bender (2006), however, point estimates suggest smaller losses for younger workers than older workers. In our case, some of this difference may be attributable to older workers being more likely to drop out of the labor force after job loss, consistent with impacts on having any LEHD earnings reported in Panel B. It is also possible, however, that older workers have acquired more specialized skills and experience that make it more difficult to find suitable re-employment opportunities (Neal, 1998).

Overall our findings indicate that job loss effects are comparable across sex, race, and age splits of the sample. To further support this conclusion, Figure B.7 presents a visual instrumental variables test that plots reduced-form effects on earnings and employment outcomes against first-stage effects on job loss by sub-group (Holzer, Katz and Krueger, 1991; Angrist, 1990). The slope of the fitted line should match our primary 2SLS estimates in Table 2 if the causal effects of job loss are homogeneous across demographic groups and the exclusion restriction holds. The slope and 2SLS estimates from Table 2 are reported in the top-right corner of each plot. The estimates are remarkably similar to and statistically indistinguishable from our primary estimates.

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 7 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 in these industries, less than half the overall effect reported in Table 2. As shown in Table A.9, 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 recovery, 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.[35] 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.8, 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 8 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.[36]

The results show that losses are on average larger in industries that appear to be higher quality along several dimensions, including those with high unionization rates, more full-time workers, higher average wage and tenure, and higher firm and worker effects as estimated

[35] Table A.9 shows that teachers are a small share of low-wage workers in NAICS 61.

[36] These industry characteristics are described further in Table A.8. With only 21 industry groupings, multivariate regression is infeasible.

in Card, Rothstein and Yi (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, von Wachter and Heining, 2022). Losses for high-wage workers are predicted by many of the same factors. These findings suggest that not all low-wage jobs provide the same long-run opportunities; workers who hold onto a "good" job fair significantly better in the long-run than if they been displaced.

Multiple factors may make full-time, consistent work difficult to replace in these industries. Some employers might have production requirements that lead them to prefer hiring workers part-time, as in an hedonic model of hours and wages (Lewis, 1969; Rosen, 1974; Lachowska et al., 2023*b*). Rationed jobs may include roles with predictable and consistent schedules, especially as many large employers adopt scheduling technologies that emphasize part-time, variable hours. 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 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, worker tenure, and firm premia suggest jobs may also be rationed due to other forces. Union bargained wages and hours policies, 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 efficiency wage. Displacement from even low-wage jobs in these sectors may thus prove costly due to the time it takes to find a new opportunity.

8 Alternative channels

This section discusses several other mechanisms frequently cited as potential drivers of the earning losses following job loss for higher-wage workers and examines whether they may also play a role for low-wage workers.

Match effects. It is possible that displaced workers in our sample had particularly strong matches initially that are difficult to replace. While job-specific match effects have been shown to be an important driver of the costs of job loss among long-tenured and higher-wage workers (e.g., Lachowska, Mas and Woodbury, 2020), by virtue of their low-wage at $t = 0$ our workers mechanically cannot have enjoyed large match effects in wages in their initial job. Some workers may have been enjoyed important matches in non-wage amenities that make replacing the job more difficult. Low-wage workers with longer tenure would presumably, by revealed preference, enjoy stronger matches on these dimensions and therefore suffer more significant losses from job displacement. However, Table 7 shows negligible heterogeneity in the effects of job loss when splitting the sample by workers' tenure with their initial employer. In fact, the point estimates indicate slightly larger, though not statistically different, losses for workers with only a year of tenure or less.

Firm premia. An alternative explanation is that workers were displaced from firms that paid particularly high wages. This explanation is also unlikely to directly explain our findings again by virtue of workers' low wages as of $t = 0$. Thus, while the results in the previous subsection show that losses are higher for workers displaced from industries with higher average firm premia, loss of these premia themselves cannot mechanically account for long-run earning losses.

Labor-leisure trade-offs. Displaced workers may be indifferent between part-time (or no) work and finding a full-time replacement job and thus have limited incentives to increase their earnings. We view this explanation as less compelling for several reasons as well. First, all workers in our sample were displaced after working for at least a year, indicating a preference for working full-time. Our ACS estimates show large increases in long-run unemployment and reports of looking for work, implying that workers at least profess to want to work. Second, Figure 6 shows that the effects on labor market outcomes are similar for male and female workers, who may have different outside options and preferences for part-time work. Third, while workers may benefit from higher levels of leisure post-displacement, our estimates show they would have continued to work more had they not initially lost their jobs. Unless preferences respond to job loss directly, one would expect workers not initially displaced to also seek to reduce their labor supply.

Skill degradation. At $t = 24$, displaced workers have 1.9 fewer quarters of work experience and their earnings conditional on working are reduced by \$724. Attributing this intensive margin reduction in wages to changes in work experience imply that a year of experience increases wages by \$1,524, which is 20% of average earnings at $t = 24$. This rate of return to experience is implausibly large. Thus, while our results are not inconsistent with at least some human depreciation during unemployment, this channel is unlikely to explain most of the observed long-run reductions in earnings following job loss. These arguments are also consistent with some prior work that finds limited returns to experience for low-skill workers (Card and Hyslop, 2005) and the relatively flat earnings trajectory of non-displaced compliers documented in Figure B.9.

9 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. Interpreted through the lens of a dynamic job ladder model, our estimates imply sizable benefits of holding a full-time $15 per hour job relative to unemployment: rents are at least three times monthly earnings.

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, Mas and Woodbury (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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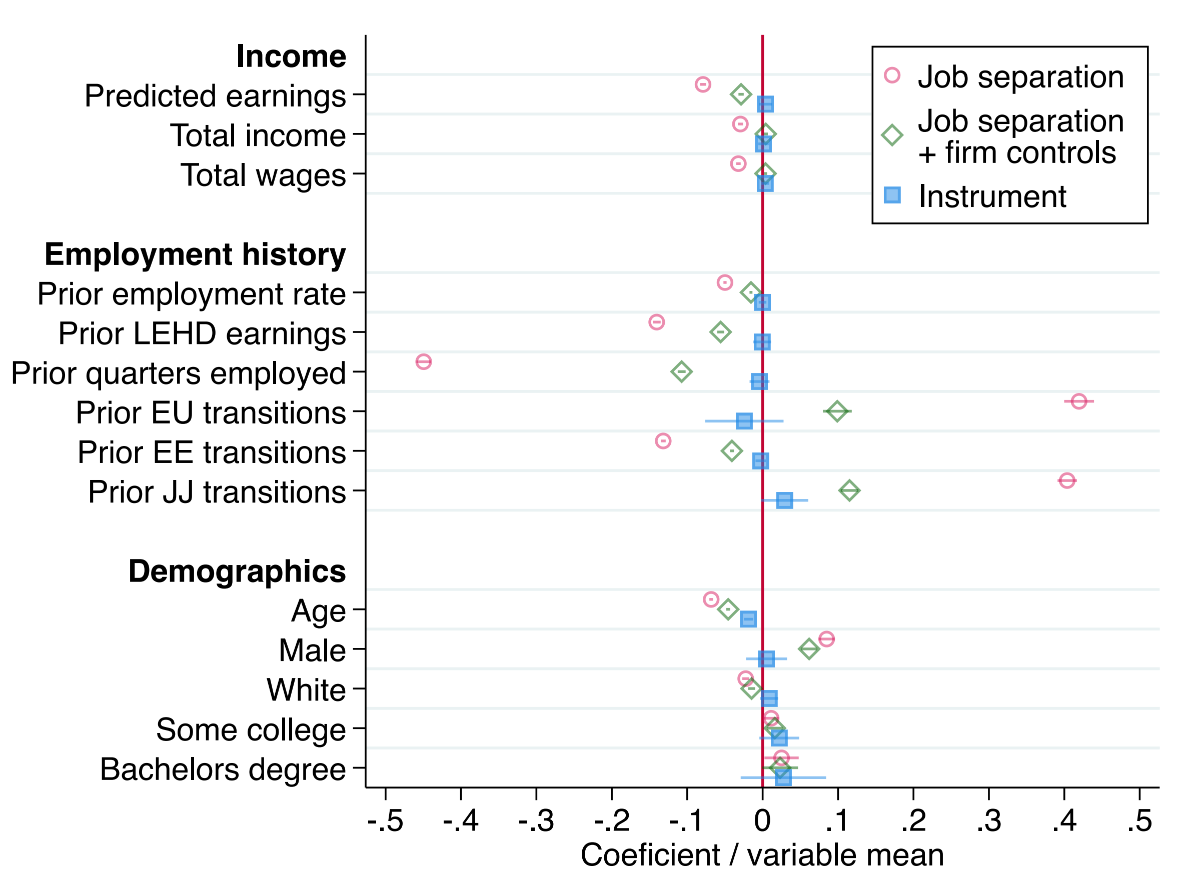
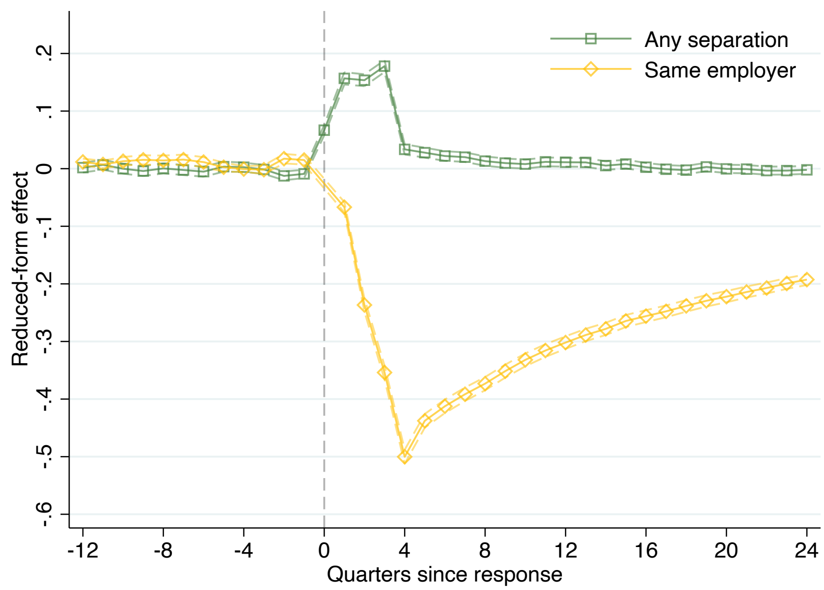
Figure 1: Instrument balance



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 ares 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. "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

A) Separations

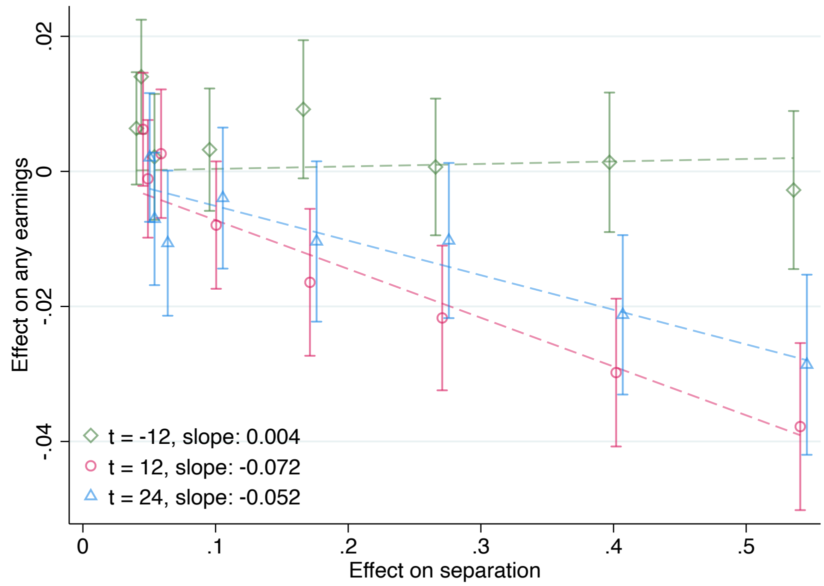


B) Employment and earnings

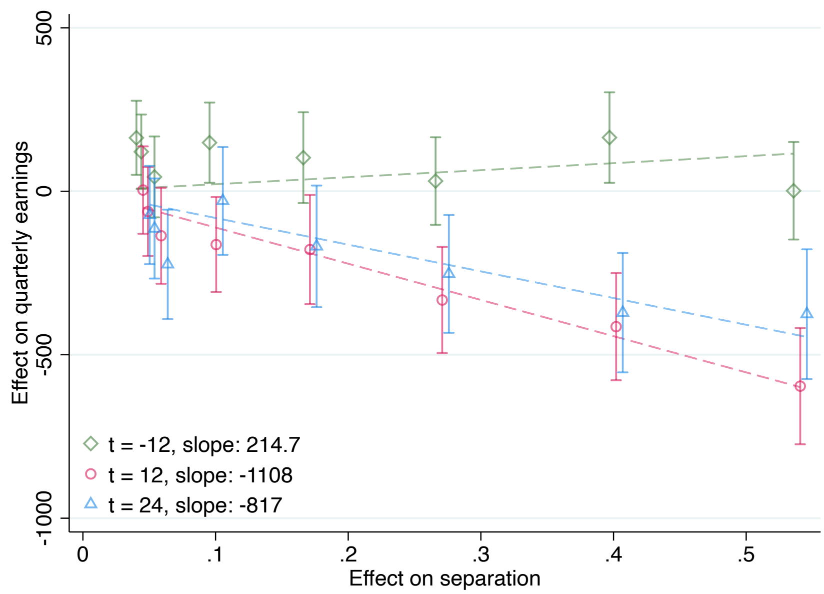
Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations (Panel A) and earnings and employment (Panel B) in the three years prior to and six years after initial ACS response. Each coefficient and standard error comes from a separate regression using 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

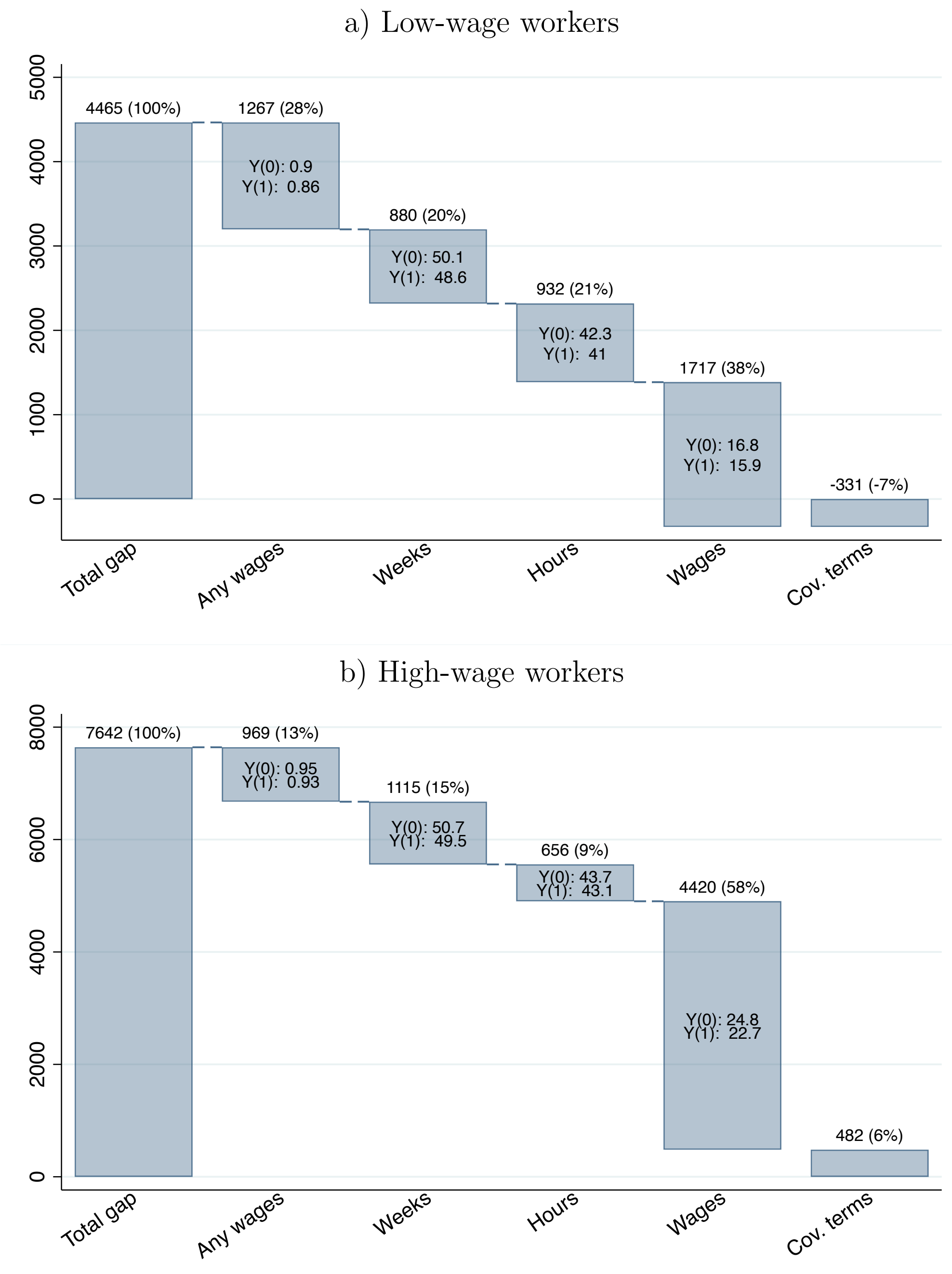


B) Quarterly earnings



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



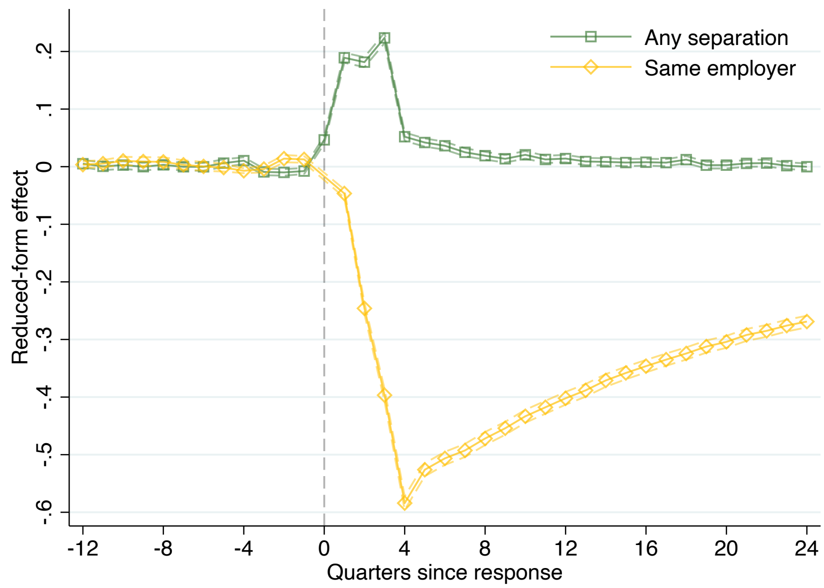
Notes: This figure presents decompositions of the long-run earnings effects of job loss into components explained by employment, weeks, hours, and wages. The decomposition is based on the observation that complier mean earnings can be expressed as:

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

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.6. All dollar values are inflated to 2020 equivalents using the CPI.

Figure 5: Reduced-form effects for higher-wage workers

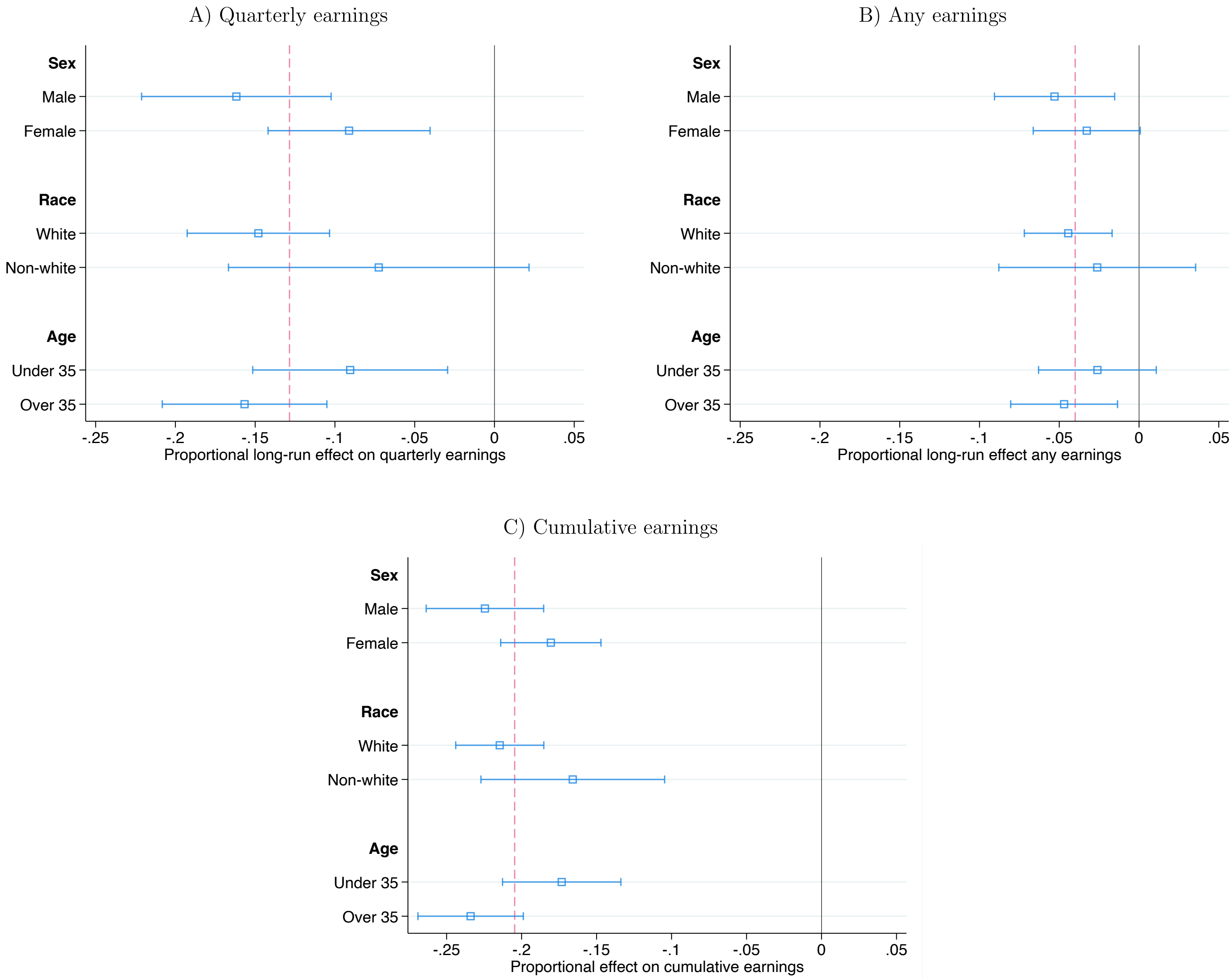
A) Separations



B) Employment and earnings

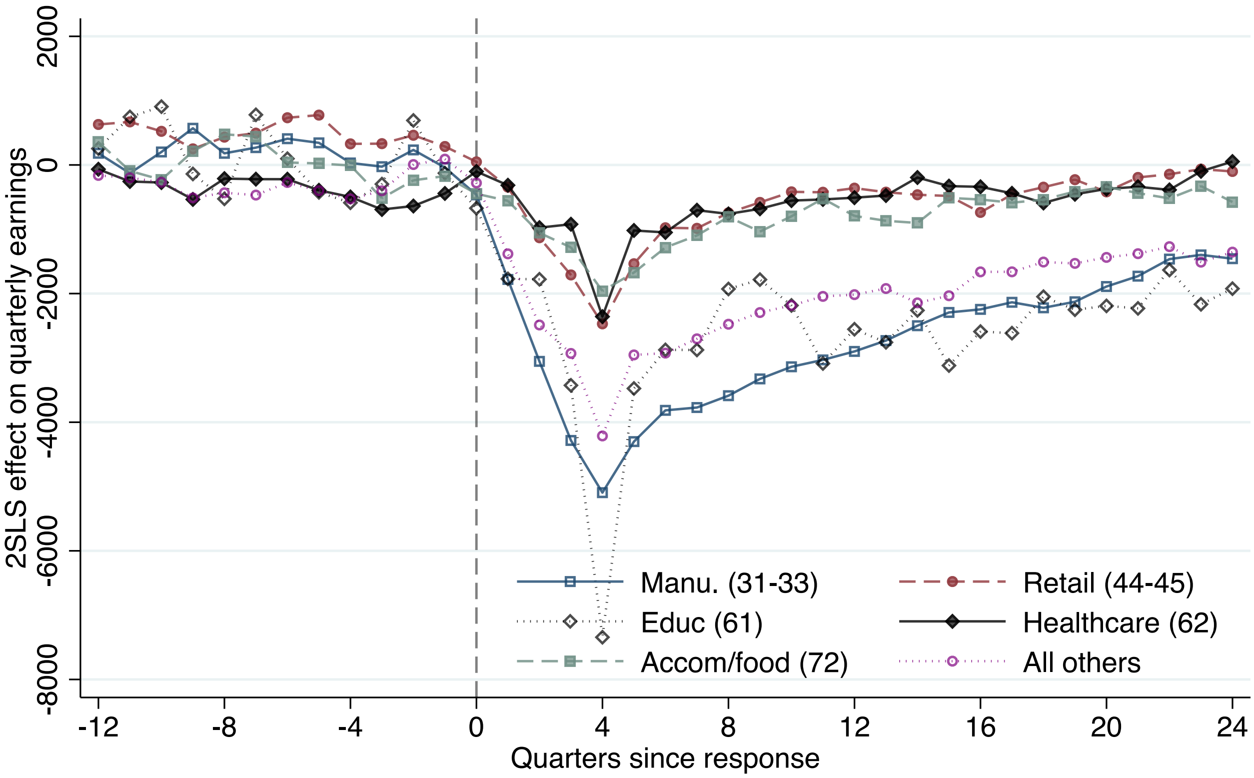
Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations (Panel A) and earnings and employment (Panel B) in the three years prior to and six years after initial ACS response for workers initially earning between \$15 and \$30 per hour. Each coefficient 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 6: Demographic heterogeneity in long-run effects of job loss



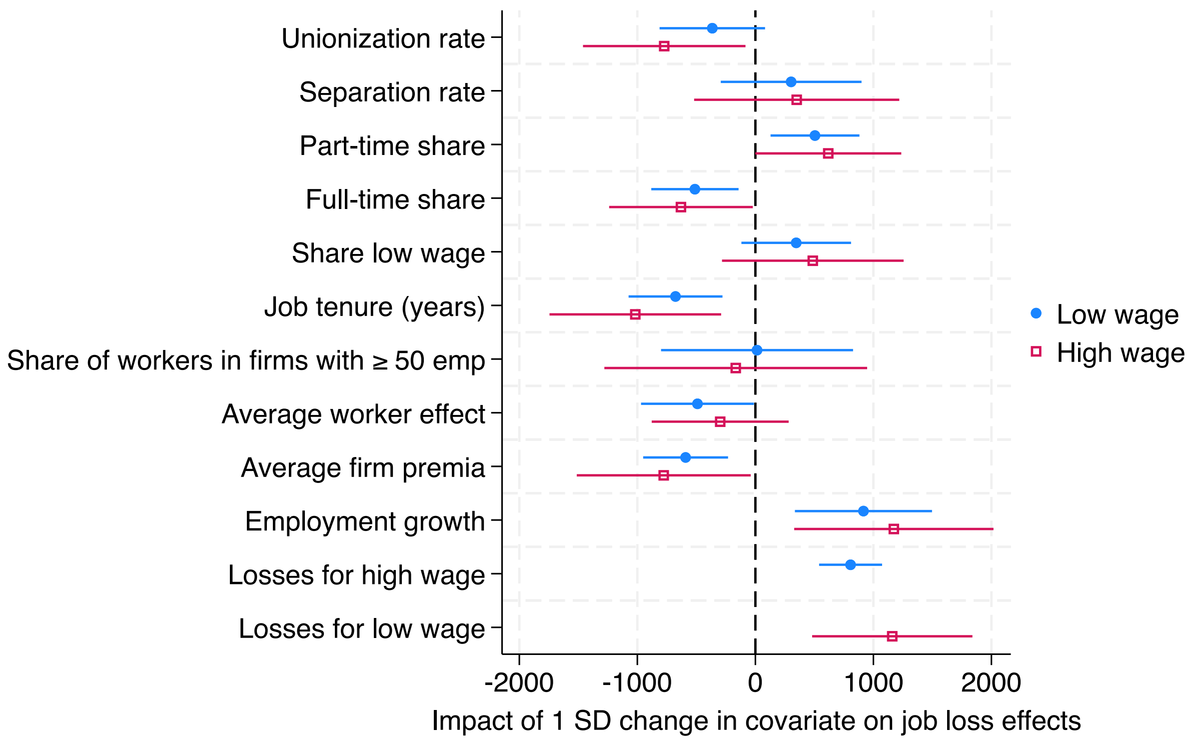
Notes: This figure plots 2SLS effects on long-run quarterly earnings (Panel A), employment (Panel B), and cumulative earnings (Panel C), splitting the sample by the observable characteristic listed. Each effect is divided by the relevant outcome mean for each sub-group to adjust for scale. Any earnings 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 7: Impacts of job loss for low-wage workers in common NAICS-2 industries



Notes: This figure 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.

Figure 8: Predictors of industry-specific job loss effects



Notes: This figure 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, Rothstein and Yi (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)
Implied extensive margin effect	6,630	-190	-381
		(61)	(118)
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. "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 3: 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 limits 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 4: 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)
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. “Implied extensive margin wage effect” is the impact on in indicator for having any wage in quarter t times the ACS wage recorded at $t = 0$. All dollar values are inflated to 2020 equivalents using the CPI.

Table 5: Model-based quantification of job loss effects

		Rent bounds		
	Est	Weak bound	Better bound	Exact rents
	(1)	(2)	(3)	(4)
Parameter				
λ	0.26			
	(0.02)			
δ	0.016			
	(0.000)			
$Pr(V_n > V_u)$	0.18			
	(0.01)			
Monthly earnings CDF / rents				
$1,333	0.673	0%	0%	0%
	(0.014)			
$1,666	0.778	71%	112%	191%
	(0.010)			
$2,000	0.860	119%	198%	373%
	(0.006)			
$2,333	0.911	153%	265%	574%
	(0.004)			
$2,666	0.942	178%	317%	794%
	(0.003)			
$3,333	0.971	214%	390%	1209%
	(0.002)			
$4,000.	0.983	238%	438%	1618%
	(0.001)			
Top earnings	$4,751	257%	479%	2020%
	(4.16)			

Notes: This table shows estimates of parameters from the model used to quantify the impacts of job loss and described in Section 4. Column one shows estimates of the core parameters of the model, including monthly job arrival (λ) and destruction rates (δ), the probability of labor force dropout ($Pr(V_n > V_u)$), and the CMF of the discrete wage distribution. The final row shows the estimated earnings level for the top mass point in the earnings distribution. Columns 2 through 4 present bounds and estimates of proportional rents for holding a job at each point in the wage distribution, as well as exact computation of rents using the discrete distribution of job offers. Rents are differences in the present value of utility relative to unemployment as a fraction of monthly earnings. All rent calculations assume a 5% annual interest rate and set b equal to the lowest earnings mass point, $1,333 per month, which implies a 50% replacement rate for a full-time $15 per hour jobs. Standard errors reported assume a diagonal variance-covariance matrix for the targeted moments. All dollar values are inflated to 2020 equivalents using the CPI.

Table 6: 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)
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 linked to a second ACS response four to six years later and earning wages $\in (\$15, \$30)$ at $t = 0$. 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. "Implied extensive margin wage effect" is the impact on an indicator for having any wage in quarter t times the ACS wage recorded at $t = 0$. All dollar values are inflated to 2020 equivalents using the CPI.

Table 7: 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 report 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 $t = 0$.

A Appendix tables

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 work are at least 40 and no restrictions on hourly wages. Columns 7 to 9 includes 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

	(1) Outcome mean	(2) Left job	(3) Left job	(4) Instrument
Labor market activity				
Average prior employment	0.89	-0.045	-0.014	-0.0001
		(0.0010)	(0.0009)	(0.0023)
Average prior earnings	6,997	-983	-390	-4.52
		(18.49)	(16.44)	(42.13)
Prior quarters employed	12.9	-5.78	-1.38	-0.053
		(0.06)	(0.03)	(0.09)
Prior emp to non-emp transitions	0.02	0.009	0.002	-0.0005
		(0.0002)	(0.0002)	(0.0005)
Prior continuous employment	0.70	-0.092	-0.028	-0.0018
		(0.0011)	(0.0009)	(0.0024)
Prior employer changes	0.08	0.0313	0.0089	0.0023
		(0.0005)	(0.0005)	(0.0012)
Demographics				
Age	35.6	-2.43	-1.62	-0.67
		(0.04)	(0.05)	(0.11)
Male	0.44	0.0377	0.027	0.0023
		(0.002)	(0.002)	(0.006)
White	0.82	-0.0185	-0.012	0.0071
		(0.002)	(0.002)	(0.005)
Some college	0.47	0.005	0.0076	0.010
		(0.00)	(0.002)	(0.01)
Bachelors degree	0.15	0.004	0.0034	0.004
		(0.002)	(0.002)	(0.004)
Summary index				
Predicted earnings	8,302	-656.3	-237.2	31.81
State-by-NAICS2-by-time FE		✓	✓	✓
Firm characteristics			✓	✓
Total observations	234,000			
Total individuals	233,000			
Total firms	96,000			

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 $t = 0$.

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 C 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: 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 4, 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.7: 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.8: 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)
Accomodation 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 Assitance (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, Rothstein and Yi (2022). All Dollar values are inflated to constant 2020 dollars using the CPI.

Table A.9: 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

Continued on next page

Table A.9: Low-wage workers occupations by industry

NAICS2	Occupation (2010)	Share
	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
	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

Continued on next page

Table A.9: Low-wage workers occupations by industry

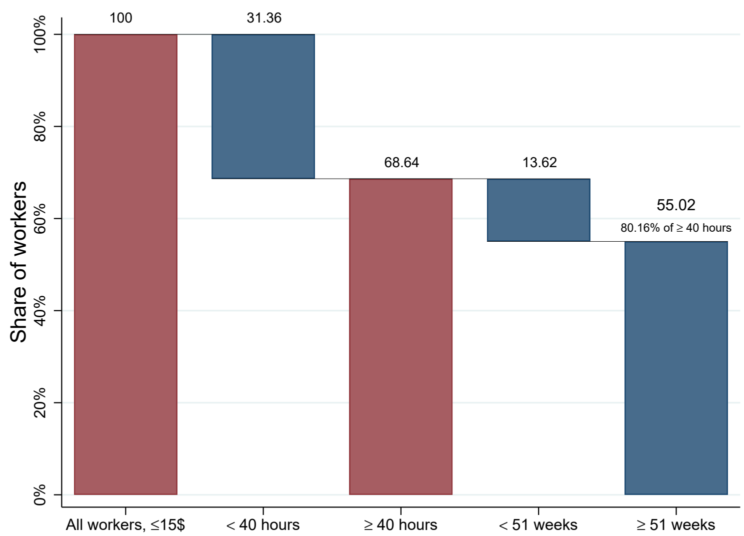
NAICS2	Occupation (2010)	Share
	receptionists and information clerks	0.051
	other	0.524
Arts, Entertainment, and Recreation (71)	grounds maintenance workers	0.071
	recreation and fitness workers	0.065
	janitors and building cleaners	0.064
	security guards and gaming surveillance officers	0.056
	waiters 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
	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.

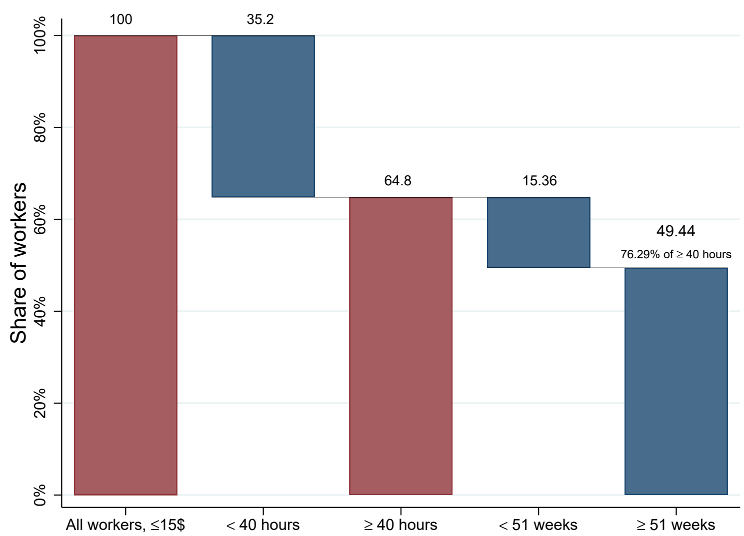
B Appendix figures

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

A) American Community Survey

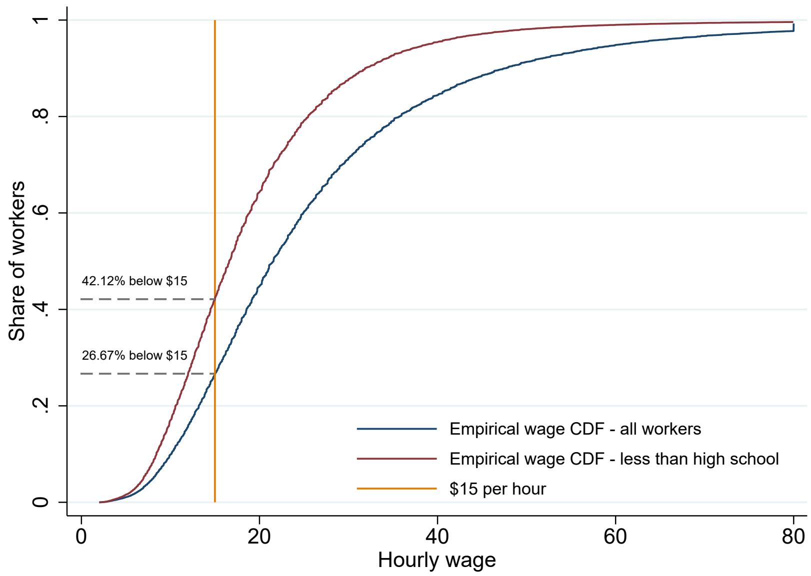


B) Current Population Survey



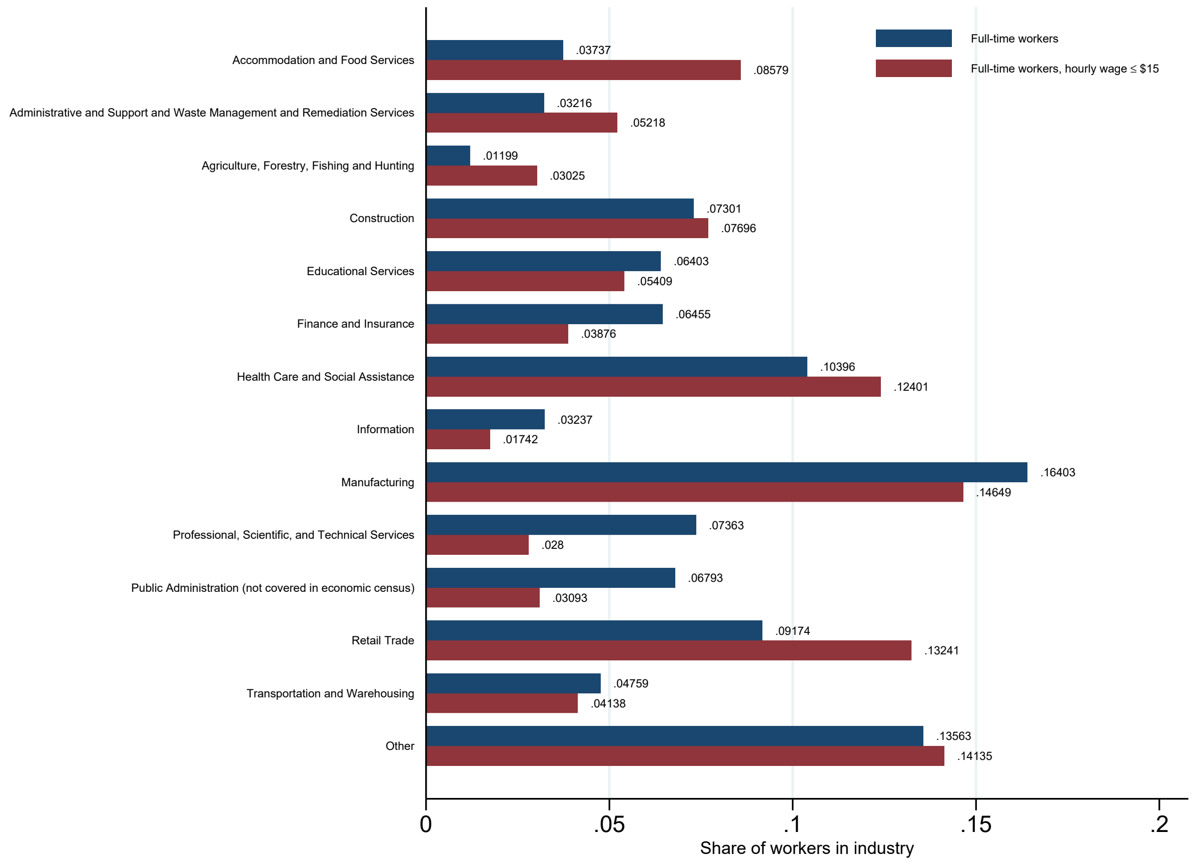
Notes: This figure shows the distribution of usual hours and weeks worked last year among low-wage workers. Panel A shows the results from American Community Survey data, while Panel B uses Current Population Survey data, restricting to participants in the Annual Social and Economical Supplement within 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 a 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 B.2: Distribution of hourly wages among employed workers in the American Community Survey



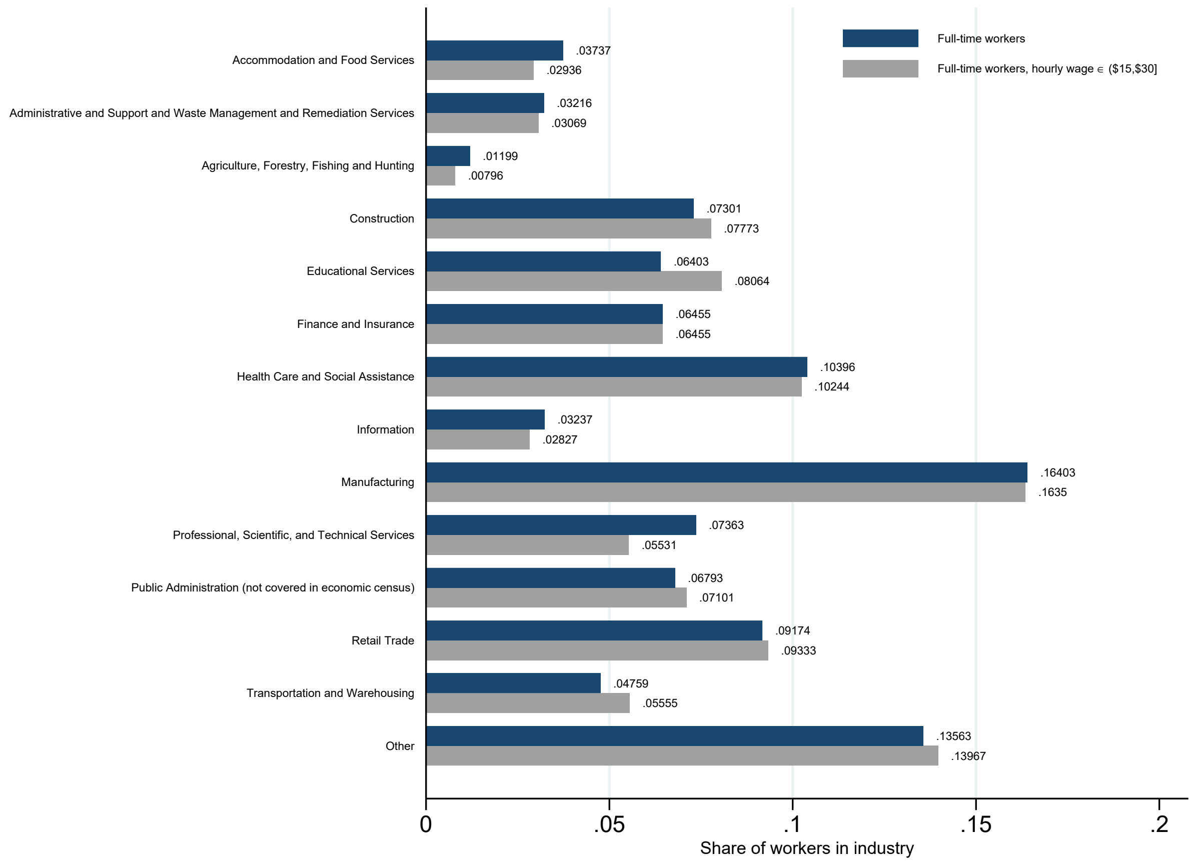
Notes: This figure shows the distribution of hourly wages among employed workers. The figure is based on the authors' calculations using the publicly available American Community Survey, 2001-2008. We restrict attention to ACS respondents between the ages of 22 to 50 who are civilian employees, at work, 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 B.3: Distribution of all full-time workers and low-wage workers across industries



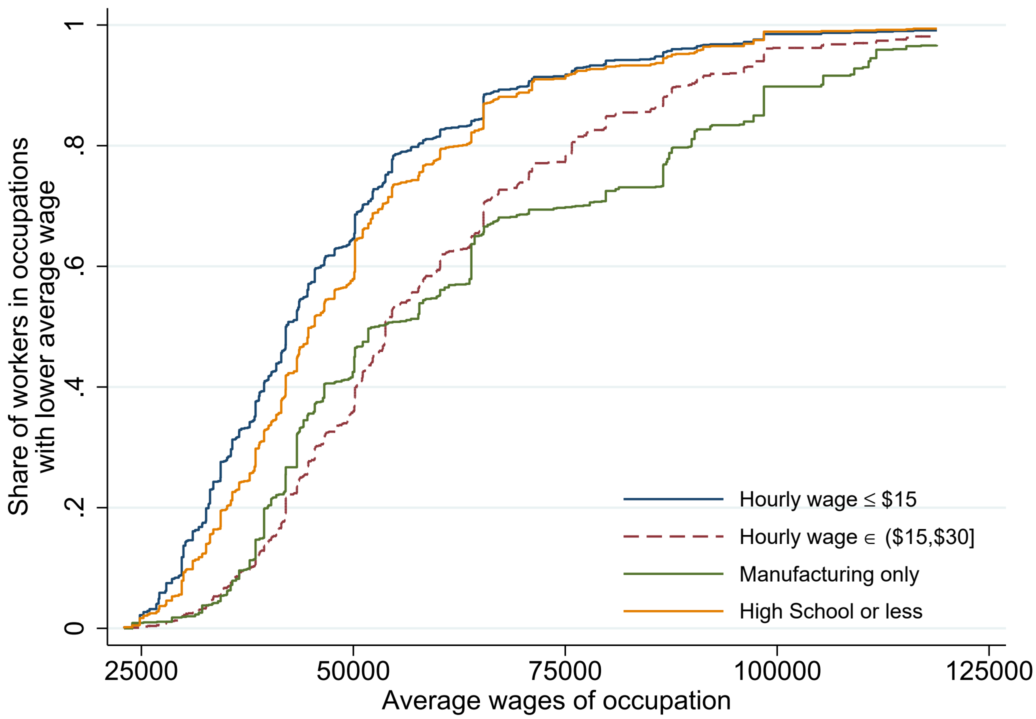
Notes: This figure shows the distribution of employed workers across industries based on 2-digit North American Industry Classification System (NAICS) codes. The figure includes two samples. All workers who are employed full-time in the last year are defined as individuals who worked for at least 51 weeks with usual hours of at least 40. The second sample further imposes that the hourly wage rate is no more than $15 inflation adjusted to 2020 values. The figure is based on the authors' calculations using the publicly available American Community Survey, 2001-2008. We further restrict attention to ACS respondents between the ages of 22 to 50 who are civilian employees, at work. The "Other" category includes the following industry codes: "Management of Companies and Enterprises", "Utilities", "Mining, Quarrying, and Oil and Gas Extraction", "Real Estate and Rental and Leasing", "Wholesale Trade", "Arts, Entertainment, and Recreation", "Other Services (except Public Administration)."

Figure B.4: Distribution of all full-time workers and workers earnings wages of $15 to $30 across industries in the ACS



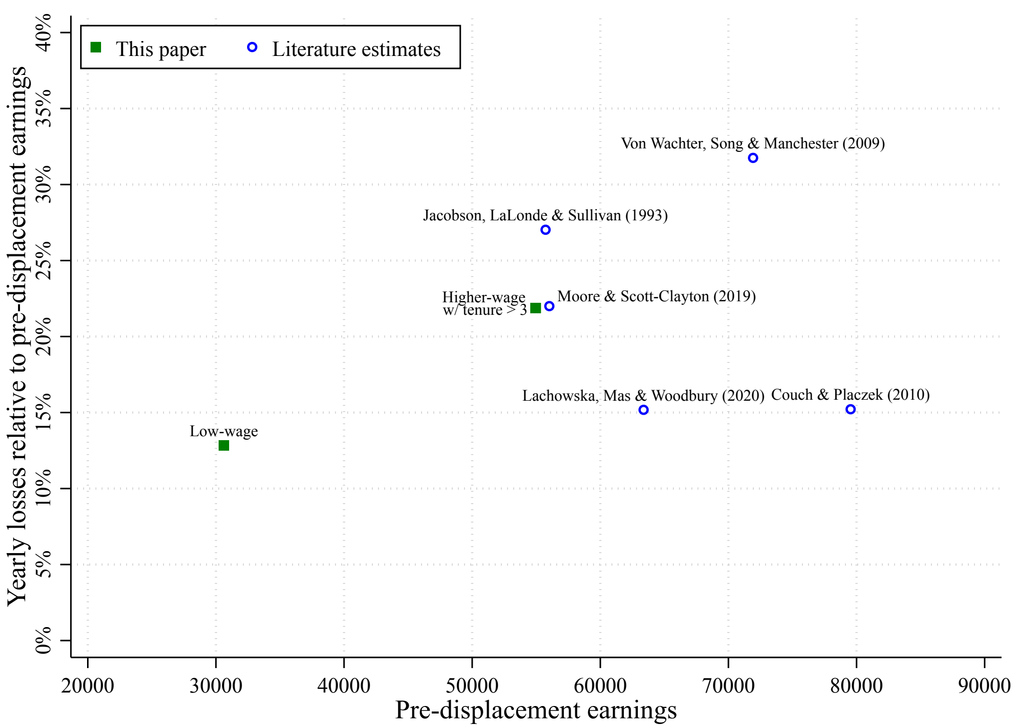
Notes: This figure shows the distribution of employed workers across industries based on 2-digit North American Industry Classification System (NAICS) codes. The figure includes two samples. All workers who are employed full-time in the last year are defined as individuals who worked for at least 51 weeks with usual hours of at least 40. The second sample further imposes that the hourly wage rate is between $15 to $30 inflation adjusted to 2020 values. The figure is based on the authors' calculations using the publicly available American Community Survey, 2001-2008. We further restrict attention to ACS respondents between the ages of 22 to 50 who are civilian employees, at work. The "Other" category includes the following industry codes: "Management of Companies and Enterprises", "Utilities", "Mining, Quarrying, and Oil and Gas Extraction", "Real Estate and Rental and Leasing", "Wholesale Trade", "Arts, Entertainment, and Recreation", "Other Services (except Public Administration)."

Figure B.5: Distribution of full-time workers by occupation average wage



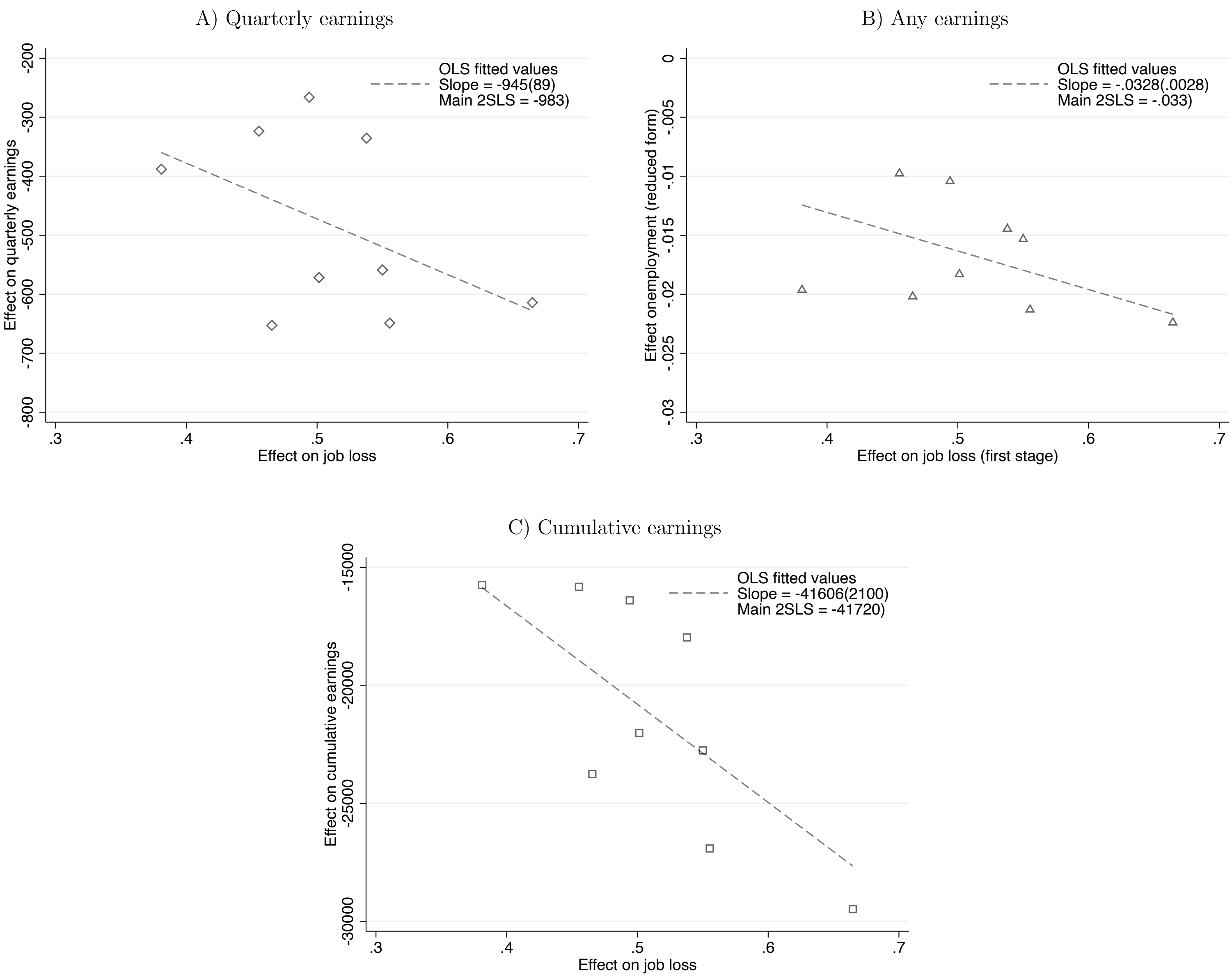
Notes: This figure shows the distribution of full-time employed workers across occupations (based on 2010 occupation codes). The x-axis reports the average wage of full-time workers in each occupation using ACS surveys from 2001 to 2020. The y-axis reports the share of workers working in occupations with average wages of equal or less the value on the x-axis (i.e., the cumulative distribution function). The figure includes four samples of workers who are employed full-time in the last year defined as individuals who worked for at least 51 weeks with usual hours of at least 40. The blue line represent low-wage workers defined as individuals earning an hourly wage of $15 or less, the dashed red line higher-wage workers defined as earning hourly wages between $15 to $30, the green line includes only workers in manufacturing industries, and the dashed yellow line workers with 12 or less years of education (i.e., high-school graduates or less). We also further restrict attention to ACS respondents between the ages of 22 to 50 who are civilian employees, at work, and work in one of our 21 LEHD approving states.

Figure B.6: Comparison to prior studies



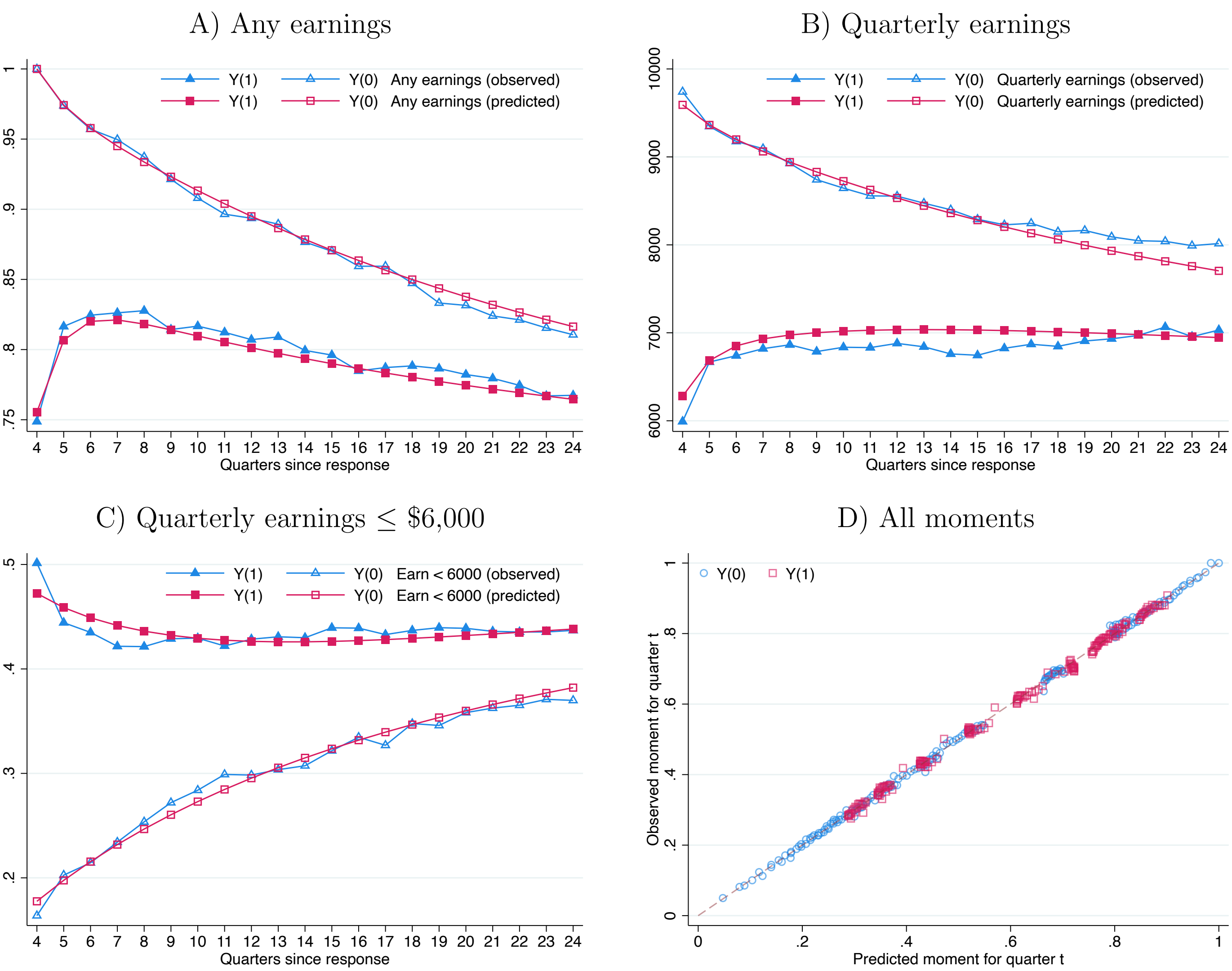
Notes: This figure shows estimates of the effect of job loss among workers with at least three years of tenure from the literature using U.S. state-level administrative UI earning records and a difference-in-differences design. Estimates from prior studies are marked by a blue circle. The estimate in this paper for the sample of higher-wage workers with at least three years of tenure as of $t = 0$ is marked by the green square.

Figure B.7: First-stage vs. reduced-form effects across demographic groups



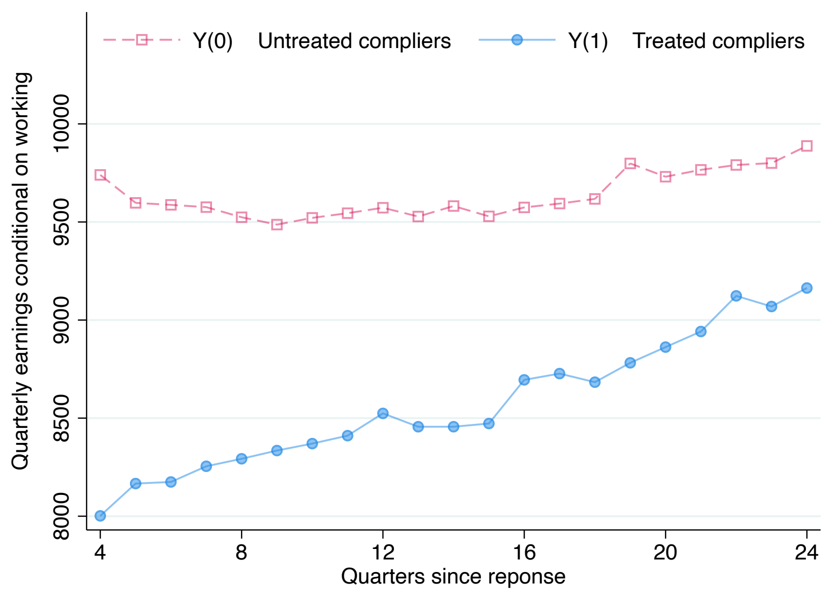
Notes: This figure plots first-stage effects on job loss and reduced-form effects on long-run quarterly earnings (Panel A), employment (Panel B), and cumulative earnings (Panel C). Each point corresponds to the estimated effect on job loss (x-axis) and the estimated effect on a long-run outcome (y-axis) 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 B.8: Model fit



Notes: This figure plots the predicted earnings outcomes from the job ladder model against observed outcomes. Panel A shows the fit for an indicator for any quarterly earnings. Panel B plots the fit of total quarterly earnings. Panel C plots the fit for an indicator for quarterly earnings below \$6,000. And Panel D plots the fit of all moments, with quarterly earnings rescaled by its maximum observed value so that all moments fall in $[0, 1]$.

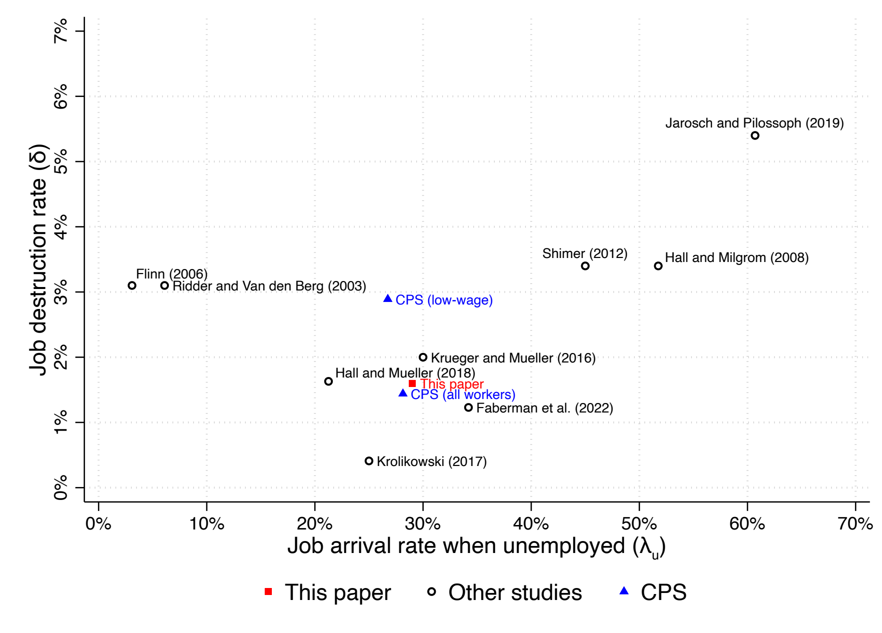
Figure B.9: Average earnings among working treated and untreated compliers



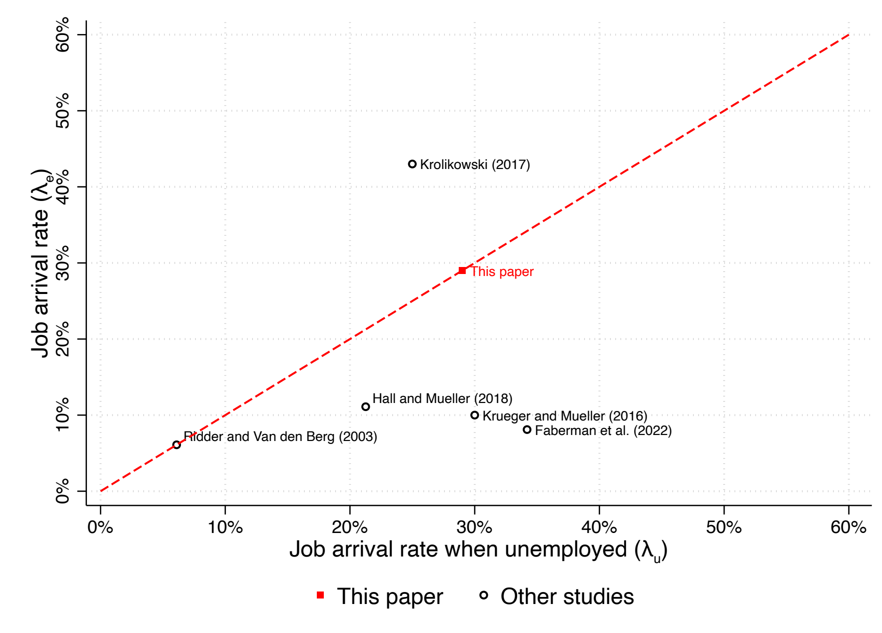
Notes: This figure shows estimates of average quarterly earnings in the LEHD data among the treated ($Y(1)$) and untreated ($Y(0)$) compliers conditional on working (i.e., observing some positive earnings in the LEHD data) using the standard formulas from Imbens and Rubin (1997) and Abadie (2002). Each 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.

Figure B.10: Search model parameters in the literature

A) Job arrival rates from unemployment (λ_u) and destruction rates (δ)

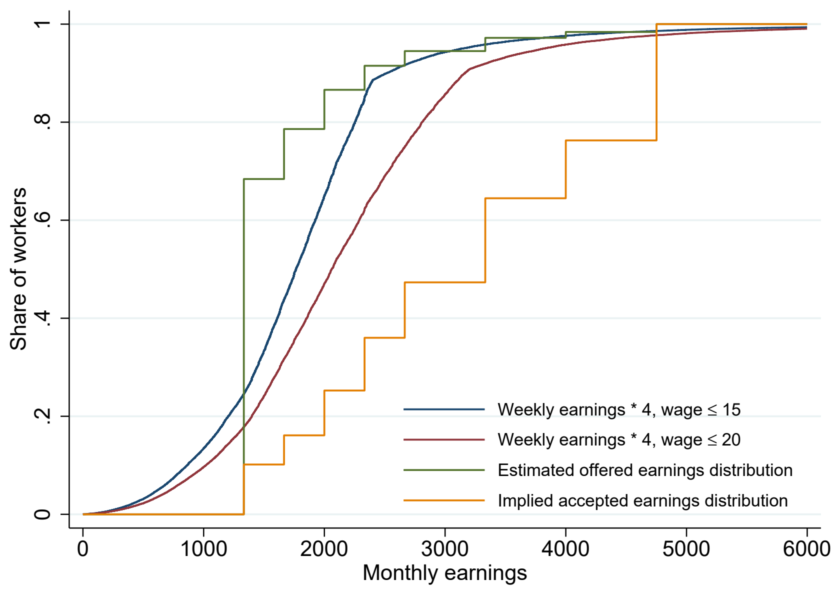


B) Job arrival rates from unemployment (λ_u) and employment (λ_e)



Notes: This figure shows estimates of key parameters of job search models in other studies. Panel A reports the job arrival rate among unemployed workers (λ_u) and the job destruction rate (δ) in other studies as well as the CPS data described in Appendix E. The CPS estimates are based on the transition probabilities in Table E.1. The job arrival rate among unemployed workers (λ_u) is defined as the likelihood of moving from a state of unemployment to full-time work or part-time work due to economic reasons. The job destruction rate (δ) is defined as the likelihood of moving from full-time employment to unemployment or part-time work due to economic reasons. Panel B reports the job arrival rate among unemployed workers (λ_u) and employed workers (λ_e). All rates are normalized to the monthly level.

Figure B.11: Offered and accepted wage distribution vs. the CPS



Notes: This figure plots offered and accepted wages from the estimated offer distribution described in Section 4. The figure also plots two benchmarks from the CPS outgoing rotation groups. The blue line plots the cumulative distribution of implied monthly earnings for all workers with wage last week of ≤ 15 per hour. The red line does the same for workers with a wage last week of ≤ 20 per hour. CPS sample restrictions are described in Appendix E.

C 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,[37] we then regress Y_{jc} on $Z_{jc}^{placebo}$ and the same controls as in our primary specification, Equation 1:

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

   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

[37]Cells with only one firm are excluded.

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.

C.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 controls:

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

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:

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

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:

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

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:

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

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, 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.

D Earnings-ladder model

D.1 Derivation of baseline model

We begin with a simple transformation of the value functions that facilitates manipulation and builds a connection to continuous time versions of the same model. The value functions for unemployed and workers employed at earnings e can be written respectively as:

$$V_u = b + \beta \left(\lambda \int_0^\infty max\{V(x), V_u\} dF(x) + (1 - \lambda) V_u \right)$$
$$V(e) = e + \beta \left(\lambda \int_0^\infty max\{V(x), V(e)\} dF(x) + \delta V_u + (1 - \delta - \lambda) V(e) \right)$$

Because search is equally productive on- and off-the-job by assumption, it can be shown that reservation earnings e^* are equal to b.

Re-arranging these expressions slightly yields:

$$(1 - \beta) V_u = b + \beta \left(\lambda \int_{e^*}^\infty [V(x) - V_u] dF(x) \right)$$
$$(1 - \beta) V(e) = e + \beta \left(\lambda \int_e^\infty [V(x) - V(e)] dF(x) + \delta (V(u) - V(e)) \right)$$

Letting $\frac{1}{1+r} = \beta$, $\bar{V}_u = V_u/(1 + r)$, and $\bar{V}(e) = V(e)/(1 + r)$ yields:

$$r\bar{V}_u = b + \lambda \int_{e^*}^\infty [\bar{V}(x) - \bar{V}_u] dF(x)$$
$$r\bar{V}(e) = e + \lambda \int_e^\infty [\bar{V}(x) - \bar{V}(e)] dF(x) + \delta (\bar{V}(u) - \bar{V}(e))$$

Notice that these expressions also describe the flow utility from unemployment and employment at earnings e in an equivalent model set in continuous time (i.e., with instantaneous discount factor r and Poisson arrival rates λ). A similar discussion of the connection between the continuous- and discrete-time versions of the model appears in the supplemental material to Hornstein, Krusell and Violante (2011), Section 3.1.1.

In this transformed model, the "flow" difference in utility from holding a job at earnings level e relative to unemployment can be expressed as:

$$\begin{aligned} r\bar{V}(e) - r\bar{V}_u &= \lambda \left[\int_e^\infty [\bar{V}(x) - \bar{V}(e)] dF(x) - \int_{e^*}^\infty [\bar{V}(x) - \bar{V}_u] dF(x) \right] \\ &\quad + e - b + \delta(\bar{V}_u - \bar{V}(e)) \\ &= \lambda \left[\bar{V}_u - \int_{e^*}^e \bar{V}(x) dF(x) - (1 - F(e))\bar{V}(e) \right] + e - b + \delta(\bar{V}_u - \bar{V}(e)) \end{aligned}$$

where the second line uses the assumption that $F(e^*) = 0$.

Some further simple algebra shows that these flow rents can be expressed as:

$$r\bar{V}(e) - r\bar{V}_u = \frac{r}{r+\delta+\lambda} \left[e - b + \lambda \int_{e^*}^e [\bar{V}(e) - \bar{V}(x)] dF(x) \right]$$

Because $\bar{V}'(e) = 1/(r+\delta+\lambda(1-F(e))) > 0$, $\bar{V}(\cdot)$ is an increasing function of e. Thus $\int_{e^*}^e [\bar{V}(e) - \bar{V}(x)] dF(x)$ must be positive. It follows that:

$$\frac{r\bar{V}(e) - r\bar{V}_u}{e} \geq \frac{r}{r+\delta+\lambda}(1-\rho_e)$$

where $\rho_e = b/e$. Moreover, because $F(e)$ is non-decreasing, $V(\cdot)$ must also be convex, which implies that:

$$\int_{e^*}^e [\bar{V}(e) - \bar{V}(x)] dF(x) \geq \frac{(F(e) - F(e^*))(\bar{V}(e) - \bar{V}(e^*))}{2}$$

which is the triangular approximation to this integral. Because $\bar{V}(e^*) = \bar{V}_u$ by definition, a tighter bound can obtained by substituting this inequality. After some algebraic rearrangement and using the assumption that $F(e^*) = 0$ again, the previous inequality can be written as:

$$\frac{r\bar{V}(e) - r\bar{V}_u}{e} \geq \frac{2r}{2(r+\delta) + \lambda(2 - F(e))}(1-\rho_e)$$

In a continuous time version of the model, we therefore have that rents are bounded by:

$$\frac{\bar{V}(e) - \bar{V}_u}{e} \geq \frac{2}{2(r+\delta) + \lambda(2 - F(e))}(1-\rho_e) \geq \frac{1}{r+\delta+\lambda}(1-\rho_e)$$

Converting back to the original discrete time value functions produces the result in Proposition 1:

$$\frac{V(e)-V_u}{e} \geq \frac{2(1+r)}{2(r+\delta)+\lambda(2-F(e))}(1-\rho_e) \geq \frac{1+r}{r+\delta+\lambda}(1-\rho_e)$$

Notice that as r becomes small the continuous time and discrete time version of the bounds converge, as one would expect taking the limit of the discrete time model as time periods shrink to zero.

D.2 Discrete earnings distributions

When the earnings distribution is known, it is possible to compute rents exactly. We do so assuming a discrete distribution of earnings offers at M mass points $\{e_1, \ldots, e_M\}$. A discrete distribution of earnings offers implies that value functions can be written as the linear system:

$$r\bar{V}_u = b + \lambda \sum_{x=1}^{M} [\bar{V}_x - \bar{V}_u] f_x$$
$$r\bar{V}_m = e_m + \lambda \sum_{x=m}^{M} [\bar{V}_x - \bar{V}_m] f_x + \delta(\bar{V}_u - \bar{V}_m), \quad m \in \{1, ..., M\}$$

where $\bar{V}_m$ is the value of holding a job at earnings level e_m (divided by $1+r$) and f_m is the mass of job offers at e_m. Because we have assumed no job offers are made below the reservation earnings level, optimal search behavior requires that if e_1 is the reservation earnings level $\bar{V}_u = \bar{V}_1$. The model features equally productive search on and off the job, which implies $b = e^* = e_1$. One can also treat the value of b as another parameter to be estimated. The set of unknowns thus consists of $\{b, \bar{V}_1, \ldots, \bar{V}_m\}$ in the general case.

The entire system can be written in matrix form as:

$$\mathbf{e} = \mathbf{W}\mathbf{V}$$

where

$$
\begin{aligned}
\mathbf{W} &= r\mathbf{I}_{M+1} - \lambda\mathbf{P} - \delta(\mathbf{I}_{1,M+1} - \mathbf{I}_{M+1}) \\
\mathbf{e} &= \{b, e_1, \ldots, e_M\}' \\
\mathbf{V} &= \{\bar{V}_u, \bar{V}_1, \ldots, \bar{V}_M\}' \\
\mathbf{P} &= \begin{pmatrix} -1 & f_1 & f_2 & \ldots & f_M \\ 0 & -\sum_{m=1}^{M} f_m & f_2 & \ldots & f_M \\ 0 & 0 & -\sum_{m=2}^{M} f_m & \ldots & f_M \\ 0 & 0 & 0 & \ldots & 0 \end{pmatrix}
\end{aligned}
$$

and where $\mathbf{I}_n$ is the n-by-n identity matrix, and $\mathbf{I}_{1,n}$ is an n-by-n matrix with ones in the first column and zeros elsewhere.

If b is known, as in the case where $\lambda_u = \lambda_e$, an exact solution for $\mathbf{V}$ can be found as $\mathbf{V} = \mathbf{W}^{-1}\mathbf{e}$. Otherwise, one can solve for the values of b and $\{\bar{V}_1, \ldots, \bar{V}_m\}$ that solve this system exactly. Exact rents can then be computed substituting the integral for summation over the discrete distribution of earnings offers.

D.3 Estimation

We fit the model via diagonally weighted minimum distance matching the following moments for both treated and untreated compliers: the probabilities of observing zero earnings, earning less than \$4,000, \$5,000, \$6,000, \$7,000, \$8,000, \$10,000, and \$12,000, and average earnings. We assume eight points of support in F, with one point at each of these quarterly earnings levels and one final level treated as an additional parameter. Since the exact timing of the layoff event within $t \in [1, 4]$ is unmodeled, we use the post-layoff observations from $t = 4$ to $t = 24$ only.

Model-based moments are derived by computing quarterly outcomes from the monthly employment and earnings outcomes implied by the model for a cohort of treated and untreated compliers from $t = 4$ to $t = 24$. The core model parameters $\{\lambda, \delta, Pr(V_n > V_u), F\}$ are the same for both groups. We also estimate the initial distribution of employment across earnings levels supported by F, the share of non-employed workers as of $t = 4$, and the share of nonparticipants as of $t = 4$. These shares are allowed to differ for the two groups to capture the fact that treated compliers lose their jobs at some point between $t = 1$ and $t = 4$. Figure B.8 demonstrates that the model closely fits the observed moments.

E CPS analysis

To compare our estimates to patterns in publicly available data, this section constructs estimates of employment dynamics using panel data from the Current Population Survey. We use CPS extracts covering 1996 to 2019 from IPUMS, which provides linked individual-level responses across survey waves. As in our main analysis, we restrict to individuals aged 22 to 50 and not in school. We also drop individuals not successfully linked across all eight survey waves. Recent research has found that CPS responses can be linked across waves with minimal error but some attrition due to cross-state migration and survey drop out. In 2009, linkage rates across survey waves one year apart was estimated to be 79% (Rivera Drew, Flood and Warren, 2014).

We then restrict to the sample of full-time hourly workers with a valid hourly wage observation recorded in the first outgoing rotation, or wave 4, and track their monthly transitions between waves five through eight. Employment states are classified using EMPSTAT, which defines whether the worker is consider employed, unemployed (U), or inactive (I) / out of the labor force. We further break down employment status using WRKSTAT as follows:

- **Full-Time (EF):** Full-time schedules (10); Full-time hours (35+), usually full-time (11) Part-time for non-economic reasons, usually full-time (12); Not at work, usually full-time (13).
- **Part-Time Economic Reasons (EPbus):** Full-time hours, usually part-time for economic reasons (14); Part-time for economic reasons (20); Part-time for economic reasons, usually full-time (21); Part-time hours, usually part-time for economic reasons (22).
- **Part-Time Non Economic (EPvol):** Full-time hours, usually part-time for non-economic reasons (15); Part-time for non-economic reasons, usually part-time (40); Part-time hours, usually part-time for non-economic reasons (41).

To construct standard errors, we estimate multinomial logistic regressions for appearing in each state in wave $t+1$ with indicators for each state at time t as covariates, with observations weighted by WTFNL. Standard errors are clustered by respondent.

Table E.1 reports transition rates splitting the sample by the observed hourly wage in wave 4.

Table E.1: Monthly transitions rates for CPS workers

A) All workers

	EF_{t+1}	$EPbus_{t+1}$	$EPvol_{t+1}$	U_{t+1}	I_{t+1}
EF_t	0.9679	0.0099	0.0096	0.0054	0.0071
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
$EPbus_t$	0.5208	0.3144	0.0982	0.0463	0.0204
	(0.0035)	(0.0034)	(0.0019)	(0.0013)	(0.0009)
$EPvol_t$	0.3525	0.0661	0.5340	0.0139	0.0335
	(0.0027)	(0.0013)	(0.0029)	(0.0006)	(0.0009)
U_t	0.2330	0.0484	0.0224	0.5789	0.1173
	(0.0026)	(0.0013)	(0.0009)	(0.0032)	(0.0020)
I_t	0.2108	0.0149	0.0348	0.0850	0.6545
	(0.0023)	(0.0006)	(0.0009)	(0.0015)	(0.0028)
Observations	1,909,410				
N. of individuals	670,543				

B) Wage < \$15 / hour

	EF_{t+1}	$EPbus_{t+1}$	$EPvol_{t+1}$	U_{t+1}	I_{t+1}
EF_t	0.9410	0.0195	0.0179	0.0094	0.0121
	(0.0005)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
$EPbus_t$	0.4705	0.3472	0.1129	0.0472	0.0222
	(0.0057)	(0.0057)	(0.0034)	(0.0023)	(0.0015)
$EPvol_t$	0.3250	0.0872	0.5324	0.0177	0.0378
	(0.0046)	(0.0026)	(0.0051)	(0.0012)	(0.0017)
U_t	0.2082	0.0592	0.0303	0.5512	0.1511
	(0.0046)	(0.0026)	(0.0018)	(0.0059)	(0.0039)
I_t	0.1759	0.0185	0.0384	0.0930	0.6742
	(0.0036)	(0.0012)	(0.0017)	(0.0027)	(0.0046)
Observations	336,610				
N. of individuals	118,429				

C) Wage $\in [15, 30)$ / hour

	EF_{t+1}	$EPbus_{t+1}$	$EPvol_{t+1}$	U_{t+1}	I_{t+1}
EF_t	0.9703	0.0092	0.0074	0.0061	0.0069
	(0.0003)	(0.0002)	(0.0002)	(0.0001)	(0.0002)
$EPbus_t$	0.5429	0.3004	0.0867	0.0523	0.0176
	(0.0075)	(0.0073)	(0.0038)	(0.0030)	(0.0018)
$EPvol_t$	0.3410	0.0624	0.5529	0.0157	0.0279
	(0.0061)	(0.0030)	(0.0066)	(0.0015)	(0.0019)
U_t	0.2413	0.0442	0.0191	0.5934	0.1021
	(0.0051)	(0.0023)	(0.0015)	(0.0061)	(0.0035)
I_t	0.2235	0.0136	0.0248	0.0946	0.6435
	(0.0049)	(0.0013)	(0.0017)	(0.0033)	(0.0060)
Observations	465,274				
N. of individuals	163,190				

Notes: This table reports transition rates between employment states for a matched panel of CPS respondents over their fifth through eighth survey waves. EF stands for full-time employment, EPbus stands for part time for economic reasons, EPvol stands for part time for voluntary reasons, U stands for unemployed, and I stands for inactive / out of the labor force. Standard errors are clustered at the respondent level and are calculated by fitting a multinomial logistic regression with the employment state at $t + 1$ as the dependent variable and as independent variables indicators for the state at t. A separate regression was estimated for each wage level. The sample includes all individuals working full-time during wave four. Wages are adjusted to January 2020 equivalents using the CPI.