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 generates a 13% reduction in earnings six years later and over $40,000 cumulative lost earnings. Most of the long-run decrease stems from reductions in employment and hours as opposed to wage rates: job losers are twice as likely to report being unemployed and looking for work. By contrast, workers initially earning $15-$30 per hour see comparable long-run earnings losses driven primarily by reductions in hourly wages. Calibrating a dynamic job ladder model to the estimates implies that the rents from holding a full-time $15 per hour job relative to unemployment are worth about $20,000, more than seven times monthly earnings.

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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 replacement 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), while Davis and von Wachter (2011) note that "many, perhaps most...job loss events involve little financial loss or other hardship for individuals" (pg. 5).

Yet evidence on the impacts of low-wage job loss is limited. 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] Long-run wage losses are interpreted as reflecting the destruction of valuable firm- and industry-specific matches (Oi, 1962; Jovanovic, 1979; Topel and Ward, 1992; Gibbons and Katz, 1992; Burdett and Mortensen, 1998). But low-wage workers' hourly pay cannot fall much by construction. Instead, reductions in employment and hours may drive losses for these workers if finding a suitable new job proves costlier than the literature presumes. For example, scheduling technologies that emphasize part-time, variable hours (Maher, 2007; Alexander, Haley and Ruan, 2015), job rationing due to regulation, and potential skill degradation in unemployment (Mincer and Ofek, 1982; Kroft, Lange and Notowidigdo, 2013; Farber, Silverman and von Wachter, 2016; 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 unique 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 respondents to identify a sample of usual full-time workers earning $15 per hour or less in 2020-equivalent dollars between 2001 to 2008. These individuals predominately hold common and relatively low-

[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.

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.

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 worker performance and the arrival of 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] We also show that these shocks are uncorrelated across firms in more narrowly defined markets and are 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 forced to leave their jobs due to 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 provide a useful diagnostic on the identifying assumptions. Finally, rather than focusing on a single threshold to define firm distress (e.g., decreases in employment greater than 30%), we exploit the full distribution of firm-level shocks, lending additional generality and precision to the analysis. Results change little, however, when focusing only on the most extreme shocks, as in mass layoff

[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.

studies.

The results show that 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 suggest 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 be employed (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 reductions are meaningful, they account for less than half of the long-run effect on earnings, implying a significant reduction in earnings along the intensive margin (e.g., in weeks and hours worked).

Analysis of outcomes in the ACS reveals that the majority of long-term earning losses stem from reductions in the likelihood and frequency of work, not hourly wage rates. Averaging the four to six years post-shock to maximize power, our estimates show decreases in employment of 5.8 percentage points. This effect reflects a combination of increases in both unemployment (3.2 percentage points) and non-participation (2.6 percentage points). Job loss generates no long-run effect on the likelihood of reporting being on layoff but creates a 4.1 percentage point increase in the likelihood of reporting looking for work. 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 insurance from added-worker effects (Lundberg, 1985; Blundell, Pistaferri and Saporta-Eksten, 2016; Halla, Schmieder and Weber, 2020) and that employment responses are not explained by increases in incarceration or differential cross-state mobility.

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 on a sample of workers initially earning $15-$30 per hour. For this group, job loss generates substantial long-run losses similar to those documented in prior research. Displacement reduces earnings by 17% in LEHD data six years later. ACS responses show similar but slightly smaller long-run reductions in total and household income, suggesting higher-wage workers may shift to work not covered by unemployment insurance after displacement. 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). In line with past work arguing for a role for firm-specific human capital (e.g., Neal, 1995), we find significant heterogeneity by tenure for higher-wage workers. However, there is no evidence of tenure heterogeneity among low-wage workers.

We conclude by interpreting these results through a Burdett and Mortensen (1998)-style job ladder model calibrated to match the causal estimates. The results show that low-wage workers receive job offers relatively frequently—at an average of 0.29 per month, for example—but that most offers are concentrated at the bottom of the earnings distribution. Nearly 95% of job offers would result in earnings below $32,000 per year. As a result, workers who hold a "good" low-wage job that offers relatively high earnings are meaningfully better off than workers who do not. We estimate that the flow rents from a continuing $32,000-per-year employment relationship are large: an unemployed worker would be willing to pay approximately seven times the monthly salary to trade places with the employed worker. These figures are significantly larger than what is typically used in some existing calibrations (Hagedorn and Manovskii, 2008; Ljungqvist and Sargent, 2017).

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), 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, typically using survey data in the U.S. (e.g., Stevens, 1997; Farber, 2004; von Wachter and Handwerker, 2009) and using administrative records in Europe (e.g., von Wachter and Bender, 2006; Seim, 2019; Helm, Kügler and Schönberg, 2022; Schmieder, von Wachter and Heining, 2022), 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, if only because wage rates cannot fall below any legislated minimums.[3]

We make several contributions to this literature. First, we provide new evidence on the consequences 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 work-

[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).

ers (Gladden and Taber, 2000; Andersson, Holzer and Lane, 2005; Card and Hyslop, 2005; Dustmann and Meghir, 2005). Second, 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 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). This makes it possible to distinguish between participation and unemployment responses to job loss, to account for substitution to other activities such as self-employment (Von Greiff, 2009) or incarceration (Britto, Pinotti and Sampaio, 2022; Khanna et al., 2021), and to examine intra-household insurance. 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 (e.g., Rosen (1969); Altonji and Paxson (1986, 1988)). These constraints are a particularly important feature of 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) study hours constraints by combining revealed preference firm rankings with two-way fixed effect decompositions of firm and worker components of hours (Abowd and Kramarz, 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 provide further evidence of the importance of labor supply constraints for this population.

Finally, our work connects to canonical models of labor market frictions, unemployment, and wage inequality (Rogerson and Shimer, 2011). While some prior work has found that models built on the ideas from Mortensen and Pissarides (1994) fail to capture the income effects of job loss or their cyclicality (Davis and von Wachter, 2011), we find that a straightforward extension of Burdett and Mortensen (1998)-style job ladder models closely replicates the estimated causal effects of job loss.[5] Moreover, we show how a measure of frictional rents

[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]Following Davis and von Wachter (2011), various extensions to the Mortensen and Pissarides (1994)

can be assessed with only partial knowledge of the model parameters, complementing prior work on calibrating the degree of frictional inequality in labor markets (Hornstein, Krusell and Violante, 2011). Our results thus also provide a new connection between the reduced-form effects of job displacement and search-theoretic models of the labor market.

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 state and local government workers. Federal employees, self-employed workers, and some agricultural work are excluded, however. 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.[6] 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 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. 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,

search and matching model have been proposed to generate the costs of job loss that have been documented empirically (Krolikowski, 2017; Jung and Kuhn, 2019; Burdett, Carrillo-Tudela and Coles, 2020; Jarosch, 2021).

[6] 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.

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.[7] 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 develop a measure of incarceration. As in the LEHD data, we inflate all nominal outcomes in the ACS to 2020 equivalents using the CPI.[8]

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 full-time low-wage workers identified in the ACS to the LEHD data. We restrict attention to ACS respondents who are civilian employees, are at work, report usually working at least 40 or more hours per week, and whose hourly wage rate falls below $15 per hour in 2020 dollars.[9] We also restrict to individuals who report working 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, however, includes

[7] 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.

[8] 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.

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

roughly 80% of low-wage workers who usually work 40+ hours per week, as shown in Figure B.1. To focus on workers out of school and unlikely to retire in the near future, we also restrict attention to workers aged 22 to 50. We make no further restrictions on job tenure, education, experience, or industry and occupation.

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.[10] 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.[11] 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 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 samples to construct our instrument and the firm-level controls used in the main analysis. These measures include the change in employment over the next four quarters, where employment is defined as the number of workers for whom the firm is the top-paying employer in each quarter. We also measure total firm size, the share of workers who are new to the firm, average separation

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

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

rates, separation rates into non-employment, average and median wages, and 25th, 75th, and 90th percentile of wages for each firm and quarter in this holdout sample. 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 sample.

Finally, to compare effects on low-wage workers to effects on higher-wage workers more similar to workers 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. While higher wage than our primary analysis sample, these workers remain low-wage relative to most previous studies. The sample of displaced workers in Lachowska, Mas and Woodbury (2020), for example, has average hourly wages of $58 in 2010 dollars (see their Table 1). We use $15 to $30 to attempt to isolate workers who are still likely to be hourly workers but for whom sources of frictions such as minimum wages are unlikely to be binding. As we show below, however, we find similar impacts as in previous work for this sample, despite lower wage levels. 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.[12] 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.[13] By construction, average weeks worked is about 52 and median hours is 40. The resulting hourly wage averages $11. According to public-use ACS data, 27% of all full-time workers earn an hourly wage of less than $15 per hour, and more than 42% of full-time workers without a high school degree earn below $15 per hour, as shown in Figure B.2.

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

[13]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.

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.[14] 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.[15] 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 industries such as retail trade and accommodation and food services.[16] Table A.2 shows that full-time low-wage workers are most likely to work as retail and sales workers, secretaries

[14]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.

[15]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.

[16]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.

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. Roughly 60% of the sample of higher-wage workers earning \$15 to \$30 per hour are employed in occupations with similar average wages as manufacturing workers and the rest in occupations with lower average wages.

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. 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 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.[17] 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 (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 simple regression adjustment and 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.

[17] 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 below several validation tests of this assumption 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 demand 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.[18] The implicit assumption is that, absent the mass-layoff, all treated workers would have followed the controls and 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, bias due to changes in the job displacement recall period, and potentially undercounting job displacement events (Von Wachter, Handwerker and Hildreth, 2009; Farber, 2010). Moreover, the DWS is restricted to workers who have been displaced and does not include 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.

[18]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 unobserved 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.[19] 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 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.[20] 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

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

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

commuting zone-by-3-digit NAICS-by-year and quarter of initial ACS response. The results of these sensitivity tests are discussed after presenting our main results. Second, we show that shocks are not correlated across firms in the same local labor market. We do so by randomly permuting firm shocks within a market and examining the effects of these "placebo" shocks on firm's own shocks and workers' outcomes. If the shocks capture common, local level factors as opposed to idiosyncratic variation, we would expect other firms' shocks to have similar effects as firms' own shocks.

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 re-enforce 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 Causal effects on low-wage workers

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.[21] 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

[21] 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.[22] 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

[22] 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.7 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.[23] 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.7, which shows a \$724 intensive-margin reduction as of $t = 24$.[24]

3.3 Tests of exclusion

Is it reasonable to assume that all effects of labor demand shocks flow through job separation by $t = 4$, as our 2SLS estimates do? Figure 3 provides one assessment. Each panel is constructed by discretizing the instrument into a bin for constant employment growth ($Z_i = 1$) and indicators for increasingly severe shocks. The most severe bin corresponds to year-over-year decreases in employment of 50% or more.[25] We then estimate the effect of a shock in each bin on the likelihood of job separation by $t = 4$ and outcomes measured at various horizons, leaving the least severe category as the omitted group. The resulting "visual instrumental variables" plot shows how reduced-form effects scale with impacts on the first stage (Holzer, 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.

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

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

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

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 7, 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 double), the reduced form effects are similar; if anything, the point estimate slightly increases from 492 to 544. The results in Table A.5 re-enforce our interpretation of the instrument as capturing only firm-specific shocks unrelated to changes in local labor market conditions.

3.4 Effects on follow-up ACS outcomes

To better understand the sources of long-run earnings losses, we next turn to effects on the ACS follow-up sample in Table 3. Since only a fraction of workers are ever re-sampled by the ACS, here we pool quarters 16 to 24 post-layoff to maximize power. Only observations with at least one additional ACS response in this window are included. Despite these differences, the first set of results in Table 3 shows that we find similar earnings impacts as in the LEHD data. 2SLS effects on total income, wages, and household income are -\$5,200, -\$4,700, and -\$6,900, respectively, although standard errors are large enough that we cannot reject that all three effects are the same.[26] The ACS income question asks about earnings over the prior year, so

[26]The fact that household income decreases by a similar or larger magnitude than individual income suggests that low-wage workers do not insure themselves from adverse employment shocks through their spouses. The added worker theory would imply that in response to a negative income shock, other household members would increase their labor supply, and therefore, total household income will decrease by less (Mincer, 1962; Blundell, Pistaferri and Saporta-Eksten, 2016). In our setting, we do not find any evidence

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.

Lastly, we estimate effects on weeks and usual hours worked 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 3 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.

3.5 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

for such an insurance channel.

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 involvement. 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 3 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.

4 Causal effects of job loss on higher-wage workers

The results so far show that reductions in employment and hours play a large role in the long-run earnings loss for low-wage job losers. In this section, we study the effects of job loss for workers initially earning between \$15 and \$30 per hour. This analysis complements our previous results in two ways. First, it tests whether our findings are an artifact of our new research design and data rather than focusing on low-wage workers. Second, it allows us to compare the impacts of job loss for low- and higher-wage workers using the same design and data and to examine whether the drivers of the long-run costs of job loss are different between these two groups.

4.1 Effects on LEHD outcomes

Figure 4 presents the reduced-form effects of firm-specific labor demand shocks on separations, job loss, employment, and earnings. Interestingly, the pattern of effects on separations

[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.

in Panel A is remarkably similar to that in Figure 2, indicating that also for higher-wage workers we find no evidence of long-run increases in job separations. 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 in employment and earnings that recover sluggishly and stabilize at permanently lower levels after six years. At $t = 24$, displaced workers see a reduction of $2,289 in quarterly earnings (17% relative to the mean) and a reduction of $74,542 in cumulative earnings (21% relative to the mean).

Table 4 presents point estimates for long-run effects on LEHD outcomes. Displaced workers see a lasting reduction in employment in the LEHD. At $t = 24$, national employment rates are five percentage points lower, or 5.6% of the mean. Moreover, there is a three percentage points increase in non-employment for at least eight quarters, which is a 61% increase relative to the mean. Interestingly, the "implied extensive-margin effect" estimate is 39% of the total impact on earnings, the same as for low-wage workers. As we discuss in the next section, some of the reductions in employment due to job loss may reflect transitions to jobs not covered by the LEHD; we find smaller income and employment reductions in the ACS.

4.2 Effects on follow-up ACS outcomes

As we did with low-wage workers, we turn to the higher-wage ACS follow-up sample to diagnose the sources of long-run earnings losses. Table 5 shows sizable losses in wages and income as measured in the ACS, 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. The difference is especially surprising because we expect the reductions in ACS to be larger than in the LEHD (as is the case for low-wage workers) since ACS outcomes are measured closer to the job loss event (between $t = 16$ and $t = 24$ instead of at $t = 24$). 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. Little of this decline is explained by workers reporting being non-employed. Most higher-wage job losers thus appear to go back to work but at lower wages than pre-displacement. Wage losses for this group may therefore reflect the destruction of firm-specific human capital or firm pay rents.

5 Decomposing earnings losses

To summarize the results so far, we next examine how much of ACS earnings reductions can be explained by changes in the likelihood of having any wage earnings, weeks worked, usual hours worked, and hourly wages. Table 6 presents estimates of complier means for displaced and non-displaced workers for these outcomes for both our main sample of low-wage workers as well as the sample of higher-wage workers initially earning between \$15 and \$30 per hour. By dividing by the share of workers reporting positive values of each outcome, we compute implied means conditional on positive for each complier group, sample, and outcome.

While the effect of job loss on wage earnings is larger for higher-wage workers, impacts are similar relative to the mean among non-displaced compliers (about 14%). Among low-wage workers, however, a larger share of the decline in wage earnings is due to the extensive margin—29.1% relative to 12.4% among higher-wage workers. Consistent with previous results, higher-wage workers also experience smaller absolute and proportional reductions in weeks and hours worked. Although both low- and high-wage workers experience a reduction of roughly 10% in hourly wages, this partly reflects the fact that workers with no earnings are coded as having a wage of zero. Table 6 shows that nearly half of this reduction for low-wage workers can be attributed to non-work. Among high-wage workers, however, 82% of the change reflects reductions in hourly wages, which fall from \$24.8 to \$22.7 on average as a result of job loss.

To summarize the relative importance of changes in wage rates, the last line of Table 6 reports an estimate of the share of total earnings decline that can be explained by changes in the hourly wage. This reduction is the implied difference in mean earnings if displaced compliers experienced the reported reductions in hourly wages but continued to work the same average weeks and hours as non-displaced complier workers (see Appendix G for details). Among low-wage workers, most of the reductions in earnings are due to difficulties finding employment at a sufficient level of hours and maintaining it. However, even among low-wage workers, 38% of the wage losses of job displacement are due to reductions in the hourly wage. Among higher-wage workers, 58% of the reductions in wages can be explained by decreases in the hourly compensation.

6 The role of tenure

Heterogeneity in the effects of job loss by initial tenure is especially interesting as several theoretical models predict more significant losses for longer tenure workers (Carrington and Fallick, 2017). Table 7 reports 2SLS estimates of the effect of job loss on long-run earnings outcomes when splitting the sample by workers' tenure with their initial employer. Among low-wage workers (Columns 1 and 2), there is no evidence of differences in the effects of job loss by tenure in the estimated effect and relative to the overall mean. Point estimates on 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 $t = 0$. It is, therefore, possible that *all* workers in our sample possess limited firm-specific human capital or valuable matches with their initial employer. 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 (either firm or industry) or match effects among higher-wage workers, consistent with some prior research (Topel, 1991; Farber, 1993; Neal, 1995; Stevens, 1997).[28]

7 Effects by sex, race, and age

There is little work on how the effects of job loss vary by worker demographics in the U.S., largely due to data limitations. Administrative earnings data based on UI records do not typically include demographic information such as sex and race, except for the selected sample of individuals who claimed UI. We overcome this challenge using the LEHD's Individual Characteristics File, which includes demographic information from the Decennial, ACS, Social Security Administration, and other sources (Abowd et al., 2009).

Figure 5 explores heterogeneity in the long-run effects of job loss for low-wage workers across

[28]Some work, however, finds insignificant tenure effects among workers with relatively high tenure of at least three years (Von Wachter, Song and Manchester, 2009; Lachowska, Mas and Woodbury, 2020).

various important 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 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); 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 past work on differences in the cost of job loss by race. However, there is some evidence from the Displaced Worker Survey that young black males are more at risk of job loss and suffer more considerable losses from it (Fairlie and Kletzer, 1998).

Lastly, Figure 5 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 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.6 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.

8 Discussion of potential mechanisms

Our results so far show that a full-time low-wage job is not easily replaced. In this section, we discuss the consistency of several potential mechanisms with our findings as well as with the empirical evidence from the existing literature.

Job specific 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), we view match effects as likely to be less important in our setting. Workers with longer tenure would presumably, by revealed preference, enjoy stronger matches 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.

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 likely for several reasons as well. First, all workers in our sample were displaced after working a full-time job consistently 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 5 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 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 during non-employment. At $t = 24$, displaced workers have 1.9 lower 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.7.

Job rationing. Our findings are most consistent with full-time, consistent jobs for low-wage workers being relatively scarce. The fact that most of our effects are driven by intensive margin reductions in earnings due to weeks and hours changes implies that workers accept part-time or inconsistent work instead of remaining unemployed. They also keep looking for better opportunities: Table 2 shows that job displacement leads to a cumulative increase of 1.44 job separations, indicating some amplification of the initial shock as displaced workers climb back up the job ladder. In Appendix F, we also show that low-wage workers in the CPS are twice as likely to transition to part-time work for economic reasons than voluntary part-time work out of unemployment, with the former accounting for 20% of all unemployment to employment transitions (versus 15% for higher-wage workers). Moreover, Figure B.8 shows the the distribution of hourly workers working part-time involuntarily in the CPS is heavily concentrated among low wage earners.

Multiple potential factors may generate job rationing in the low-wage labor market. 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). Rationed jobs may include roles with predictable and consistent schedules, especially as many large employers adopt scheduling technologies that emphasize part-time, variable hours. 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. 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 low-skill workers (Flinn, 2006).

9 An earnings ladder model

To conclude, we interpret these results using a simple discrete-time search model. We do so for two reasons. First, our results so far suggest that frictions in the low-wage labor market are non-negligible. Indeed, as discussed in Manning (2011), job loss is often viewed as an ideal experiment for gauging the extent of frictional labor market rents.[29] However, it is not obvious how to map the 2SLS results to frictions and rents directly without the additional structure we introduce in this section. As an added benefit, this structure also allows us to assess whether our causal effects are consistent with search behavior measured in other, observational, studies of worker transitions in data such as the CPS.

Second, the surplus associated with a typical job is a key parameter in a large literature in macroeconomics that uses search and matching models (Pissarides, 1985; Mortensen and Pissarides, 1994) to study fluctuations in U.S. unemployment rates and vacancies. Since Shimer (2005) showed that standard models cannot match the volatility in unemployment rates and vacancies, various extensions and solutions have been proposed. As discussed in Hagedorn and Manovskii (2008) and Ljungqvist and Sargent (2017), among others, one resolution to the "Shimer puzzle" entails low average surplus so that small fluctuations in productivity can generate larger variation in vacancy creation. Our results suggest that at least for full-time low wage jobs, surplus may be relatively high. The model in this section allows us to quantify how high surplus may be.

9.1 Model and bounds on rents

Consider a discrete-time "earnings ladder" model of the low-wage labor market where job offers 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.[30] 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:

$$V_u = b + \beta \left(\lambda \int_{e*}^{\infty} V(x) dF(x) + (1 - \lambda) V_u \right) \tag{3}$$

[29] "To estimate rents for the employed, the experiment one would like to run is to consider what happens when workers are randomly separated from jobs" (pg. 989).

[30] As Davis and von Wachter (2011) showed, some models without on-the-job search perform poorly in predicting the effects of job loss on earnings. We explore the impact of allowing for different offer arrival rates on and off the job further below.

where e^* is the reservation level of earnings.[31] The value of holding a job with earnings e can be written as:

$$V(e) = e + \beta \left(\lambda \int_e^\infty V(x)dF(x) + \delta V_u + (1 - \delta - (1 - F(e))\lambda)V(e) \right) \tag{4}$$

Since the flow utility associated with holding a job that pays earnings e is e itself, this model features linear utility over earnings. This common assumption in the search literature can be viewed as local approximation to a richer nonlinear utility function (Hagedorn and Manovskii, 2008). In Appendix E, we explore sensitivity to choices for utility from earnings $u(e)$ that allow for diminishing marginal utility from consumption and leisure.

Let $\omega(e) = (V(e) - V_u)/e$ capture the rents as a fraction of earnings associated with holding 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. When jobs offering earnings e arrive rarely, we expect $\omega(e)$ to be large. If job mobility is sufficiently high, however, the value of holding a job that pays e relative to being unemployed will be small because unemployed workers can expect to find a comparable job quickly.

Calculating $\omega(e)$ directly requires evaluating the value functions. The value of $V(\cdot)$ depends on all the parameters of the model, including the distribution of job offers F. The following proposition, however, establishes that rents can be bounded with only partial knowledge of the parameters.

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 $r = (1 - \beta)/\beta$.

We leave the proof for Appendix D, which also establishes that a similar bound can be obtained in the continuous-time equivalent of the same model. The first inequality provides the tightest lower bound possible without additional knowledge of F. It is highest when δ and λ are small relative to r, indicating that a worker can expect to reap the discounted rewards of a high-paying job for longer, and when $F(e) \to 1$, indicating that jobs at this

[31] 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.

earnings level are rare. The second inequality provides an additional bound that requires no knowledge of F. It can be obtained from the first inequality by simply setting $F(e) = 0$.

In models without a job ladder, such as the canonical version of the Diamond-Mortensen-Piassarides framework, rents are exactly equal to the weaker bound. Existing estimates and calibrations imply this bound should be small. 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. If the value of non-employment is close to a worker's marginal product (i.e., ρ_e is close to one), as argued in Hagedorn and Manovskii (2008), rents would be just a small fraction of earnings. Intuitively, because λ is an order of magnitude larger than r, even though earnings are significantly higher in a job than when unemployed, rents are low because unemployed workers find new jobs quickly. The presence of a skewed job ladder, however, can make holding a rare job more valuable even when λ is relatively large. Our tighter bound captures some of those effects.

In our application, we make one final modification to this model. The results in Tables 2 and 3 show that non-participation is an important response to job loss. To account for this channel, we assume that there are two types of workers: active (with share π) and inactive (share $1-\pi$). Active workers seek new jobs when they become unemployed. Inactive workers do not seek new work after they become unemployed, effectively facing $\lambda = 0$. We calculate rents only for active workers. Pinning down rents for inactive workers is more difficult without taking a stance on the reason for non-employment, which is not observed in our data.[32]

9.2 Identification

We model treated compliers as workers who recently experienced job destruction (i.e., $\delta = 1$ at some point in $t \in [1, 4]$). Untreated compliers are individuals whose job was not initially destroyed (i.e., $\delta = 0$ in $t \in [1, 4]$). Both groups' average outcomes can be identified under the same assumptions that justify our 2SLS analysis (Imbens and Rubin, 1997; Abadie, 2002). Consider first active workers. Assuming that both groups subsequently converge back to the stationary equilibrium implied by the model, the speed of convergence of earnings and employment outcomes for these two groups is informative about key parameters. To see why,

[32] If inactive workers would be indifferent between their current activity, whatever that may be, and holding a job, then clearly their rents would be zero. If inactive workers are simply discouraged job seekers receiving unemployment benefits b in perpetuity, however, rents for active workers represent a lower bound on rents for inactive workers.

denote the share of treated compliers employed at time t as y_t^1 (and likewise y_t^0 for untreated compliers).

$$\triangle y_t^d = \lambda - y_t^d(\lambda + \delta) \tag{5}$$

Likewise, the change in the share of treatment-status d individuals with earnings below e at time t, $\triangle Q_t^d(e)$, can be written as:

$$\triangle Q_t^d(e) = \delta - [\lambda(1 - F(e)) + \delta]Q_t^d(e) \tag{6}$$

Thus, if one is willing to set the model in quarterly time, implying that individuals gain and lose jobs only in between each quarter, one could estimate quarterly λ, δ, as well as the CDF of earnings offers F, by regressing changes in employment on employment levels and changes in the share of workers earning below e on its level for active workers. In Appendix D.4, we describe how the same intuition applies to identification of δ, λ and F, as well as the share of active workers π, when allowing for non-participation.

The likely sub-quarterly frequency of job mobility makes setting the model in quarterly time less attractive. In addition, our outcomes measure the probability of observing any earnings and the probability of earning less than particular levels over the course of a quarter, not employment itself or earnings levels in a particular job. Appendix D.5 shows how the search model parameters can still be identified allowing for K discrete periods within a quarter if the support of F is discrete.

9.3 Estimation

Allowing for subquarterly mobility makes the dynamics of employment and earning quantiles nonlinear in the model parameters, so 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. Our preferred estimates use $K = 3$, implying monthly job arrival and destruction, a frequency similar to discrete-time transitions measured in studies using the CPS. Calibrating the model by matching these moments can be viewed as asking what structural parameters best match the causal effects recovered by our instrumental variable design. A similar style exercise is performed by Harasztosi and Lindner (2019), who investigate the effect of minimum wage changes.

9.4 Results

Table 8 shows the primary estimates. Column 1 reports parameter estimates for the core search model parameters, the cumulative mass function of the discrete earnings distribution, and the location of the top earnings level. The earnings levels shown in the rows correspond to monthly totals to match the discrete-time assumption of the model. The remaining columns report bounds and estimates of rents associated with each earnings level.

Estimated λ and δ are 0.29 and 0.016, respectively. As shown in Figure B.9, these estimates are similar to those from other studies using U.S. data.[33] The estimated share of active workers, π, is 74%. Because inactive workers remain non-employed once they lose their jobs, over time they account for a larger fraction of the non-employed population and depress the average job-finding rate out of non-employment. In a steady state, the unemployment rate for active workers is given by $\delta/(\lambda+\delta) = 0.016/(0.016+0.29) = 0.052$. If all inactive workers are also non-employed in the steady state, total non-employment rates would be 30%, and inactive workers would comprise 87% of the non-employed population. The estimates in Table 2 point to eventual convergence to this steady state: average zero-earnings rates are 18%, and individuals with no earnings for at least two years comprise about 44% of this group. As shown in Figure 2, however, even at this horizon significant gaps remain between treated and control workers, indicating that the steady state has yet to be reached.

The estimated distribution of job offers reported in Table 8 exhibits heavy right skew. Nearly 70% of offers entail earnings below \$1,333 per month, or approximately 22 hours per week at \$15 per hour. Less than 10% of job offers are estimated to pay more than \$2,333 per month. Despite the concentrated offer distribution, Figure B.10 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.

Figure 6 shows that the estimated parameters provide a close fit to the targeted moments. Panel A shows that model-predicted rates of having any earnings closely track the estimated

[33] In Appendix F, we show that low-wage workers have comparable monthly transition rates from unemployment to employment and job destruction rates in a panel of CPS respondents.

rates, including the observed non-monotonic pattern for treated compliers. This pattern is driven by the presence of inactive workers. Initially, a large share of the non-employed are active workers who very recently lost their jobs and average job-finding rates are correspondingly high. As a result, employment rates jump up quickly before beginning a slow convergence to the steady state described in the previous paragraph.[34]

Panel B of Figure 6 shows that the model also closely matches estimated effects on total quarterly earnings, which follow a similar time profile to effects on any earnings in Panel A. By $t = 24$, a significant earnings gap remains between both groups—roughly \$980 dollars. Re-scaling by implied rates of any earnings from Panel A suggests most of this gap comes from differences in positions on the earnings ladder, rather than the role of non-employment: a roughly \$710 gap remains among individuals with any earnings.[35] This lingering intensive-margin gap reflects the fact that even five years after the job loss event, treated compliers are still catching up to untreated compliers' more advantageous position on the job ladder.

Panel C shows that the model also provides a good fit to one example earnings level: an indicator for quarterly earnings below \$6,000. Fits for other levels of earnings targeted are similar. To summarize the overall fit of the model, Panel D plots normalized predicted versus observed moments.[36] A perfect fit would require all dots to fall on the 45-degree line. The deviation from perfect fit, as measured by the diagonally weighted minimum distance at the minimizing solution, is roughly 390.1. Since we match 378 total moments, a χ^2 goodness of fit test would not reject at the 5% level if the true variance-covariance matrix of the targeted moments were diagonal.[37]

Columns 2 through 4 of Table 8 report bounds on rents for active workers implied by the estimated model parameters. All rent calculations assume a 5% annual interest and set $b =$ \$1,333 (or \$4,000 per quarter) since, in a model with equal search productivity on and off the jo, the reservation earnings levels must equal the value of non-employment, and the lowest earnings level in our discrete distribution is \$1,333. The weak rent bounds in Column 2 are simply $(1 - e/1333) \cdot (1 + r)/(r + \delta + \lambda)$, where e depends on the row. Rents are thus zero

[34] Table A.6 presents estimates of employment rates and earnings levels for both groups at $t = 4$—i.e., initial conditions. All treated compliers need not be unemployed at the start of quarter four because the treatment is job separation *by* $t = 4$. Hence some workers may have separated earlier and already found new work. One hundred percent of untreated compliers are employed at the start of quarter four by definition.

[35] This calculation is performed by dividing unconditional earnings levels by rates of having any earnings and subtracting them: $8015/(1 - .189) - 7032/(1 - .233) = 714$.

[36] Quarterly earnings is normalized here by the observed maximum so that it falls on the same scale as other outcomes, which are probabilities.

[37] Since the true variance-covariance matrix has non-zero off-diagonal elements, this test provides only a heuristic assessment of model fit. Unfortunately, disclosing the full variance-covariance matrix for targeted moments is infeasible due to Census policies capping the total number of estimates disclosed per project.

for earnings level \$1,333 and highest for the top earnings level. Rents are generally small because λ is large relative to r and δ, implying that the rent bound is approximately $3.2 \cdot \rho_e$. The 163% bound for earnings of \$2,666 per month (or \$8,000 per quarter), for example, implies the job is worth only about a month and half worth's pay.

Column 3 shows that accounting for the distribution of job offers significantly increases rents. Rents in a job earning \$2,666 per month, for example, are 290% of earnings. The difference between the bounds in Columns 2 and 3 reflects the rarity of high-earning job offers. Since most of the mass in the job offer distribution is concentrated in the left tail, rent bounds are larger in higher-earning and more rare jobs. Holding a job at the top-level of earnings, for example, entails rents that are at least 440% of earnings. Although we imposed a discrete distribution of earnings offers, these rent bounds do not require this assumption. They remain valid so long as λ and δ are consistently estimated and the CDF of the true offer distribution evaluated at each level of earnings is close to the estimated CMF.

The rent calculations in Columns 2 and 3, however, are only lower bounds. If we are willing to make full use of the discrete approximation to the distribution of earnings offers, it is also possible to calculate rents exactly, as is shown in Appendix D.3. These estimates are reported in Column 4. As expected, exact rents are higher than both bounds and sometimes significantly so. A job that pays \$2,666 per month yields rents that are 750% of earnings, for example. This earnings level is relatively close to average levels of quarterly earnings reported in the sample prior to job loss and roughly what a full-time worker earning \$15 per hour could expect to make.[38]

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. Nevertheless, we view these rent metrics as a useful way to assess the extent to which workers who do hold relatively higher-earning jobs are better off than in a counterfactual where they do not.

[38]Figure B.11 presents the implied treatment effects of job loss on total wage earnings, total income (including benefits b), and the average value of jobs ($V(e)$) held using estimates from the model. Job loss also generates large and long-lasting reductions in total income (including unemployment benefits). Due to shifts into unemployment and lower-earning jobs, average job values decline both overall and among employed workers in a pattern that mirror effects on earnings losses.

9.5 Further extensions

In Appendix E, we explore various extensions to the model, such as allowing on- and off-the-job search productivity to differ, incorporating nonlinear utility over earnings, and estimating the model for the higher-wage sample, and discuss the connection of our rent measures to other measures of frictional inequality such as the "mean-min ratio" proposed by Hornstein, Krusell and Violante (2011).

10 Conclusion

This paper studies the effects of job loss on the employment and earnings of low-wage workers such as secretaries, drivers, and cashiers. 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 to holding a full-time \$15 per hour job relative to unemployment: rents are at least 290% of monthly earnings and possibly as much as 750%.

Why are low-wage jobs difficult to replace? A large existing literature has argued that the long-run effects of job loss on wages for *high-tenure* workers reflect the loss of valuable firm- or industry-specific human capital or matches. These explanations may play some role for lower-wage workers as well. However, given that a large share of the earnings effects is accounted for by the apparent difficulty of simply returning to work full-time at all, other frictions specific to the low-wage labor market, such as job rationing and hours restrictions, may also be important. Assessing the importance of these and other factors is an important task for future research.

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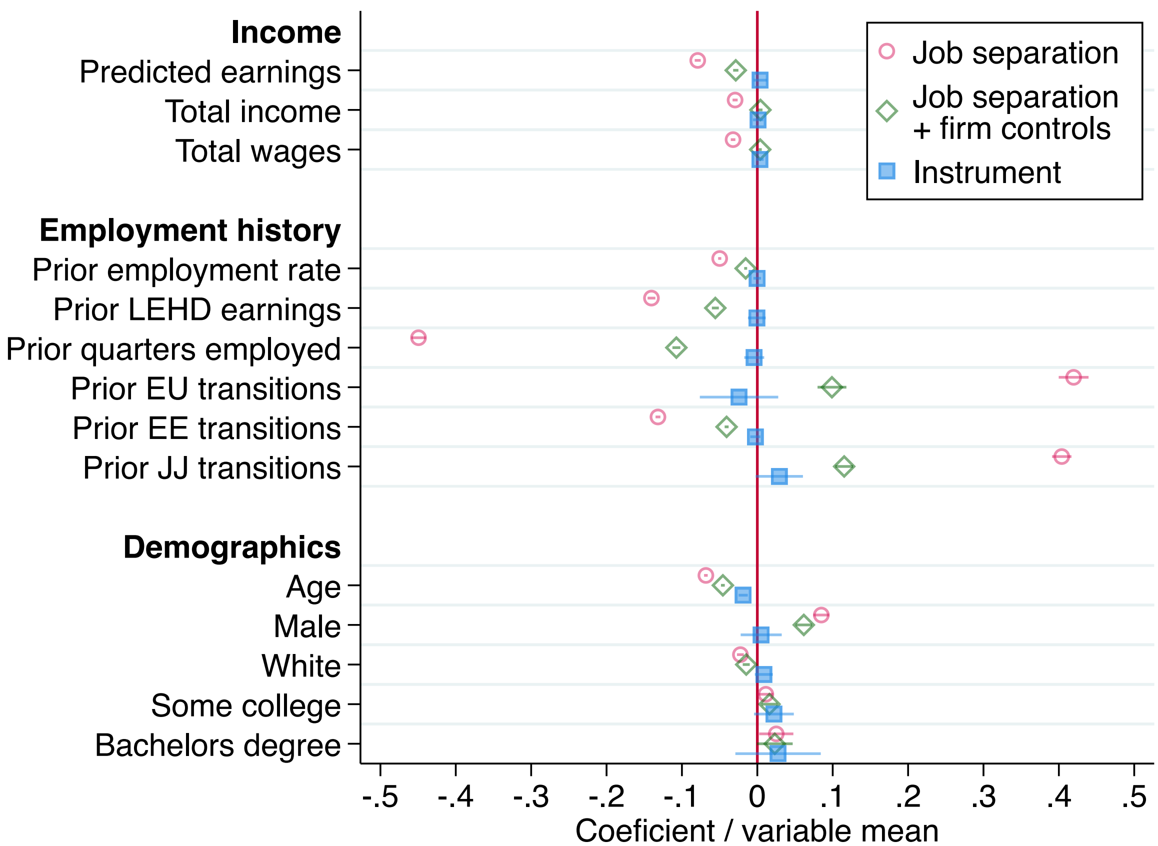
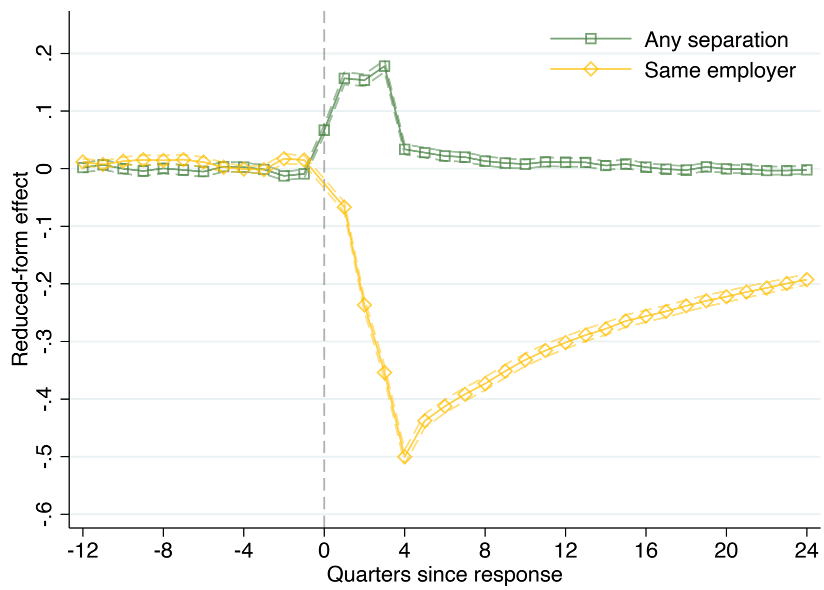
Figure 1: Instrument balance



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. Predicted earnings is a summary covariate index formed using a regression of earnings (or employment) on all available covariates. All regressions use the baseline set of fixed effects. The specifications indicated by the square and diamond markers 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 $t = 0$. 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

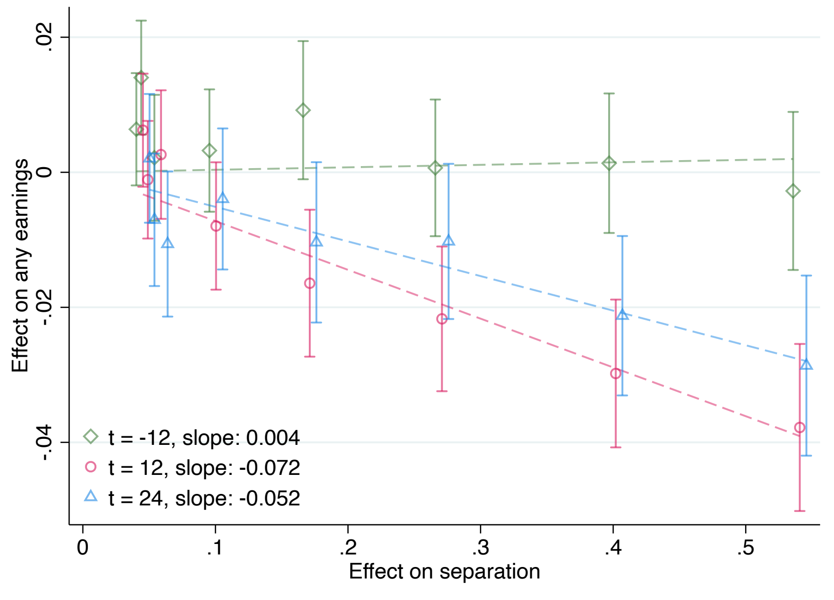


B) Employment and earnings

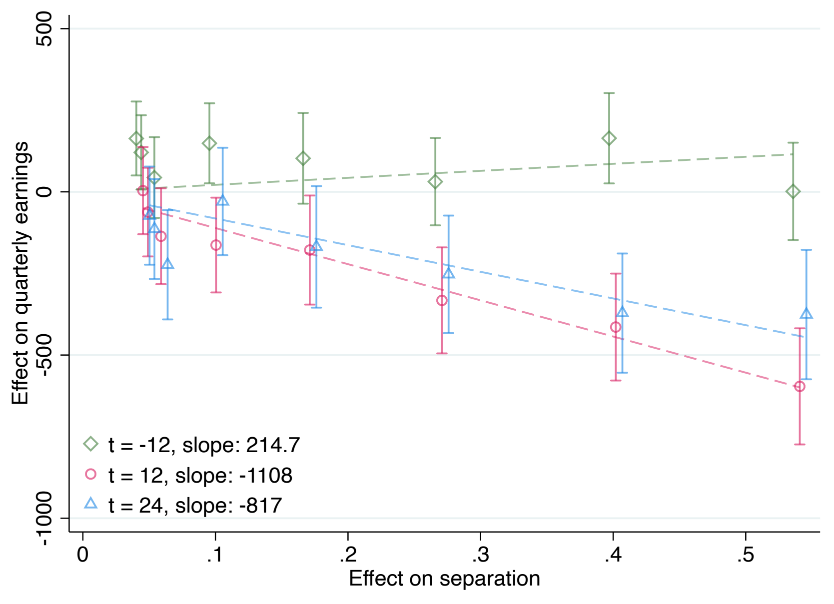
Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations (Panel A) and earnings and employment (Panel B) in the three years prior to and six years after initial ACS response. Each coefficient and standard error comes from a separate regression using 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 constant 2020 dollars 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



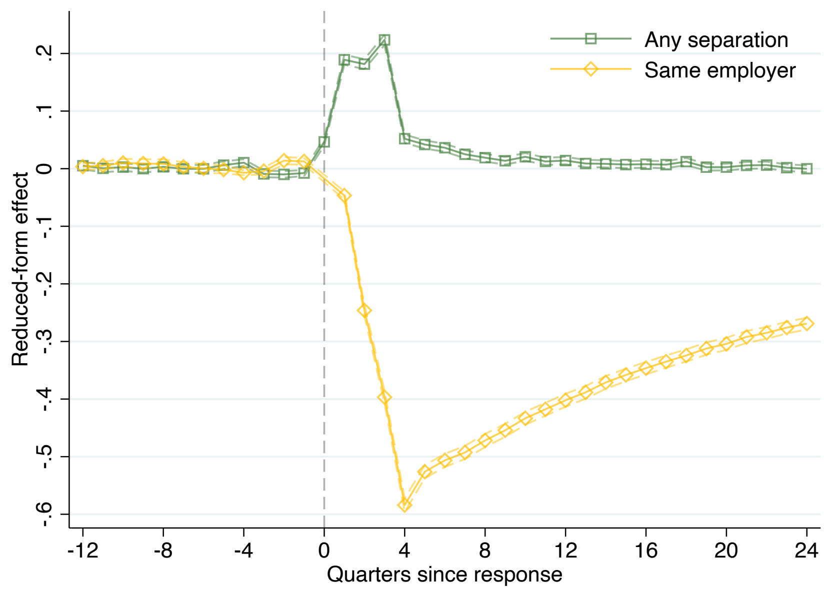
B) Quarterly earnings



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.

Figure 4: Reduced-form effects for higher-wage workers

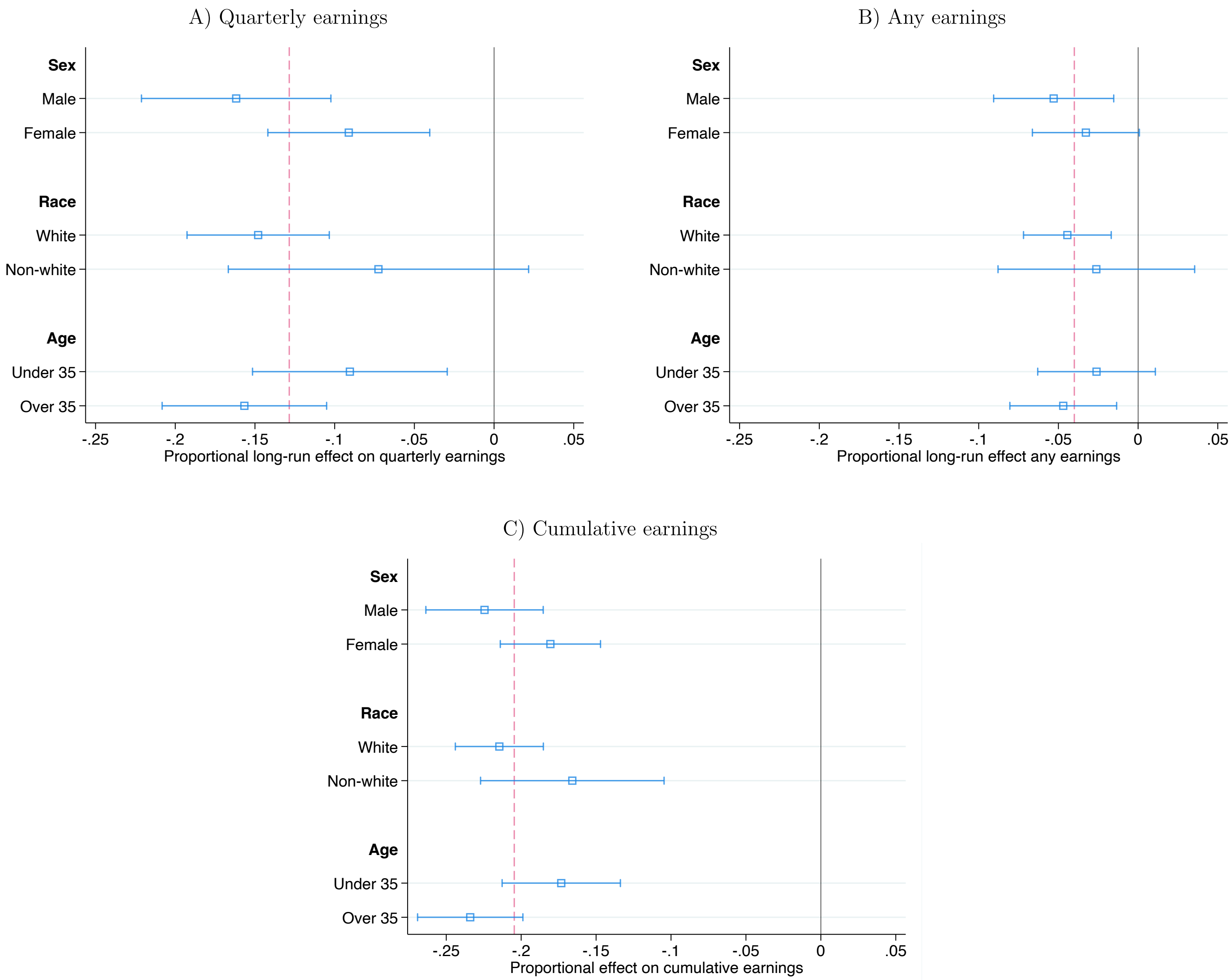
A) Separations



B) Employment and earnings

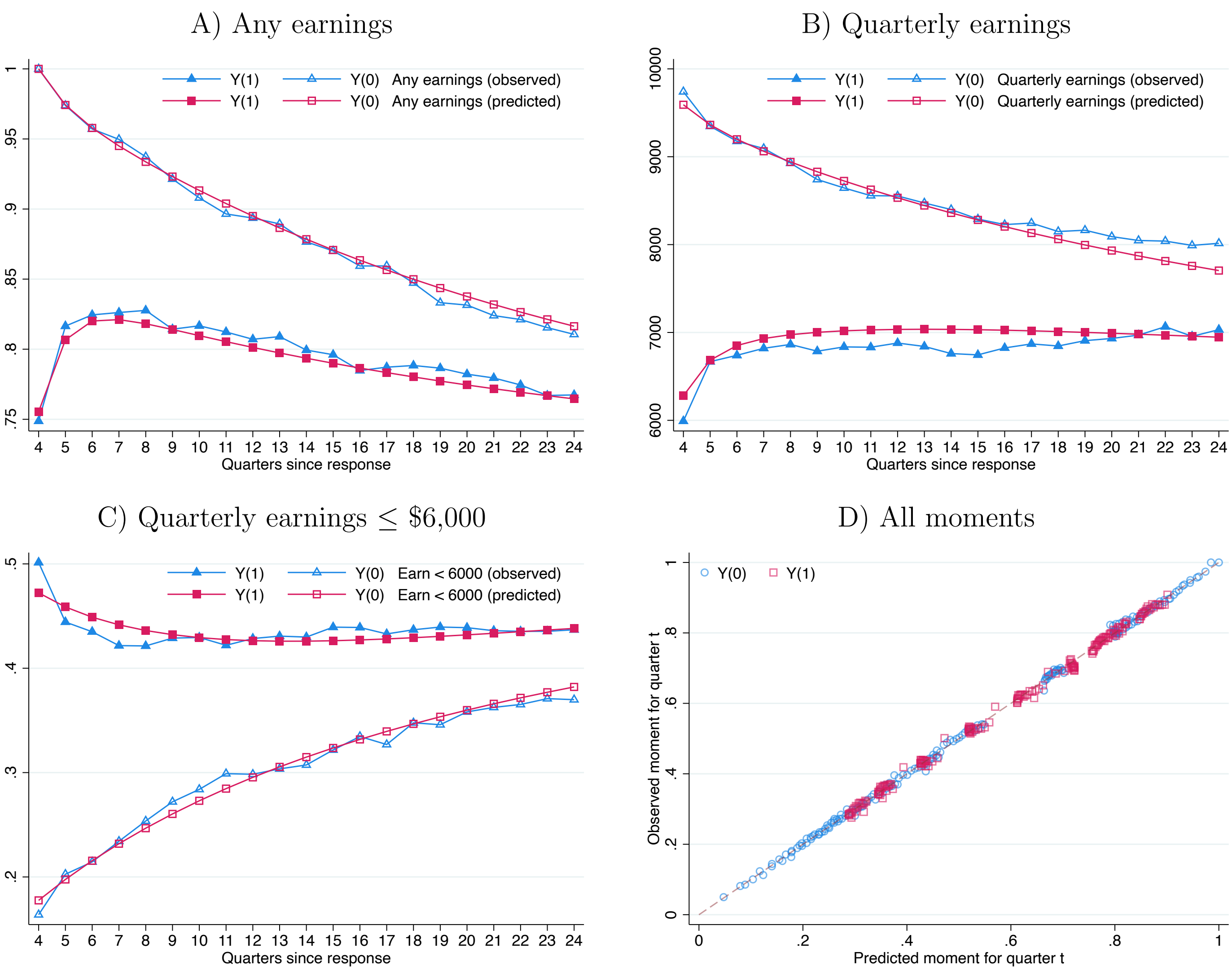
Notes: This figure shows estimates of reduced-form effects of firm-level labor demand shocks on job separations (Panel A) and earnings and employment (Panel B) in the three years prior to and six years after initial ACS response for workers initially earning between \$15 and \$30 per hour. Each coefficient 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 constant 2020 dollars using the CPI. Standard errors are clustered by employer at $t = 0$.

Figure 5: Demographic heterogeneity in long-run effects on earnings



Notes: This figure plots 2SLS effects on long-run (at $t = 24$) 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 constant 2020 dollars using the CPI. Standard errors are clustered by employer at $t = 0$. All models include the baseline set of controls and pool quarters 16 to 24.

Figure 6: 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 $[0, 1]$.

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.

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 $t = 0$.

Table 3: Long-run effects on ACS outcomes

	(1) Mean	(2) Reduced form	(3) 2SLS
Income			
Total income	33,880	-2,763	-5,243
		(865)	(1,399)
Wages	31,710	-2,486	-4,717
		(867)	(1,398)
Household income	76,550	-3,647	-6,919
		(2034)	(3,281)
Employment			
Employed	0.88	-0.031	-0.058
		(0.015)	(0.024)
Unemployed	0.034	0.017	0.032
		(0.009)	(0.014)
Not in labor force	0.082	0.013	0.026
		(0.012)	(0.020)
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 $t = 0$.

Table 4: 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	0.034	0.067
		(0.0062)	(0.0124)
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 $t = 0$.

Table 5: 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 $t = 0$.

Table 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%
Share of earnings impact explained by hourly wage		37.7%		58.3%

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. The final row reports an estimate of the share of reductions in wage earnings that can be explained by impacts on the hourly wage alone. This estimate measures implied earnings reductions if treated compliers had the estimated wage rates conditional on positive but worked as many hours and weeks as untreated compliers. See Appendix G for details. For consistency, total wage earnings are coded here as the product of weeks worked, usual weekly hours worked, and the hourly wage. This definition differs slightly from the wage earnings variable used in prior tables, which is reported by respondents directly. Estimated effects are similar to those in Table 3, however. Note also that employment status in the ACS is not the same as an indicator for any wage earnings. The former relates to employment in the previous week, while the latter captures wage earnings over the previous year.

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 long-run quarterly earnings and cumulative earnings since $t = 0$, 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 constant 2020 dollars using the CPI. Standard errors are clustered by employer at $t = 0$.

Table 8: Parameter estimates and rents

	(1) Est	(2) Weak bound	(3) Better bound	(4) Exact rents
Parameter				
λ	0.29			
	(0.02)			
δ	0.016			
	(0.000)			
π	0.74			
	(0.01)			
Monthly earnings CDF / rents				
$1,333	0.684	0%	0%	0%
	(0.015)			
$1,666	0.786	65%	102%	179%
	(0.011)			
$2,000	0.866	108%	181%	352%
	(0.007)			
$2,333	0.915	138%	242%	543%
	(0.005)			
$2,666	0.945	162%	289%	753%
	(0.003)			
$3,333	0.972	194%	355%	1153%
	(0.002)			
$4,000	0.984	215%	399%	1550%
	(0.001)			
Top earnings	$4,752	232%	436%	1940%
	(4.15)			
Min. dist. criterion	390.075			
Number of moments	378			

Notes: This table shows estimates of parameters from the model described in Section 9. Column one shows estimates of the core parameters of the model, including monthly job arrival ($\lambda = \lambda_u = \lambda_e$) and destruction rates (δ), the population share of active workers (π), 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, since when $\lambda_e = \lambda_u$ reservation earnings levels equal b. Standard errors reported assume a diagonal variance-covariance matrix for the targeted moments.

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 ≤ $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: Model estimates of initial conditions

	(1)	(2)	(3)	(4)
	Untreated		Treated	
	Initial share	Share active	Initial share	Share active
Unemployed	0.000	-	0.348	0.601
$1,333	0.042	0.739	0.000	0.813
$1,666	0.038	0.739	0.044	0.813
$2,000	0.091	0.739	0.081	0.813
$2,333	0.123	0.739	0.096	0.813
$2,666	0.137	0.739	0.102	0.813
$3,333	0.228	0.739	0.150	0.813
$4,000	0.137	0.739	0.078	0.813
$4,751	0.203	0.739	0.100	0.813

Notes: This table shows estimates of initial conditions from model described in Section 9. Columns 1 and 3 report the initial shares of each group by state as of $t = 4$, where a state is either unemployment or employment at one of the eight discrete wage levels. Columns 2 and 4 report the estimated share of workers in each state who are active. As discussed in the main text, initial active shares are constrained to be equal employed workers.

Table A.7: Model estimates allowing for different search productivity on and off the job

	(1) Estimates	(2) Exact rents
Parameter		
λ_u	0.27	
	(0.02)	
λ_e	0.03	
	(0.01)	
δ	0.017	
	(0.001)	
π	0.75	
	(0.01)	
Monthly earnings CDF / rents		
$1,333	0.231	0%
	(0.023)	
$1,666	0.329	488%
	(0.030)	
$2,000	0.453	840%
	(0.035)	
$2,333	0.572	1125%
	(0.036)	
$2,666	0.677	1373%
	(0.033)	
$3,333	0.813	1777%
	(0.023)	
$4,000	0.886	2122%
	(0.015)	
Top earnings	$4,712	2428%
	(4.31)	
Min. dist. criterion	280.476	
Number of moments	378	

Notes: This table shows estimates of parameters from the model described in Appendix D.2 that allows for different job arrival rates on and off the job. Column 1 shows estimates of the core parameters of the model, including monthly job arrival rates in unemployment (λ_u) and while employed (λ_e), job destruction rates (δ), the population share of active workers (π), 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. Column 2 shows exact proportional rent estimates implied by the discrete earnings distribution and assuming a 5% annual interest rate. Rents are the flow utility difference relative to unemployment as a fraction of earnings, with b set to the implied value when $1,333 is the reservation earnings level. Standard errors reported assume a diagonal variance-covariance matrix for the targeted moments.

Table A.8: Parameter and rents for model estimated on higher-wage job losers

	(1) Est	(2) Weak bound	(3) Better bound	(4) Exact rents
Parameter				
λ	0.31			
	(0.02)			
δ	0.007			
	(0.000)			
π	0.66			
	(0.01)			
Monthly earnings CDF / rents				
$1,333	0.608	0%	0%	0%
	(0.028)			
$1,666	0.692	62%	94%	151%
	(0.021)			
$2,000	0.761	104%	164%	283%
	(0.016)			
$2,333	0.807	134%	219%	410%
	(0.013)			
$2,666	0.846	156%	264%	534%
	(0.010)			
$3,333	0.931	187%	340%	767%
	(0.005)			
$4,000	0.960	208%	388%	1151%
	(0.003)			
Top earnings	$6,026	243%	470%	2192%
	(0.16)			
Min. dist. criterion	1306			
Number of moments	378			

Notes: This table shows estimates of parameters from the model described in Section 9 when fit to the post-job loss earnings dynamics of workers initially earning $15-30 per hour. Column 1 shows estimates of the core parameters of the model, including monthly job arrival (λ) and destruction rates (δ), the population share of active workers (π), 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 on 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 earnings. All rent calculations assume a 5% annual interest rate and set b equal to the lowest earnings mass point, $1,333 per month, since the model implies reservation earnings levels equal b. Standard errors reported assume a diagonal variance-covariance matrix for the targeted moments.

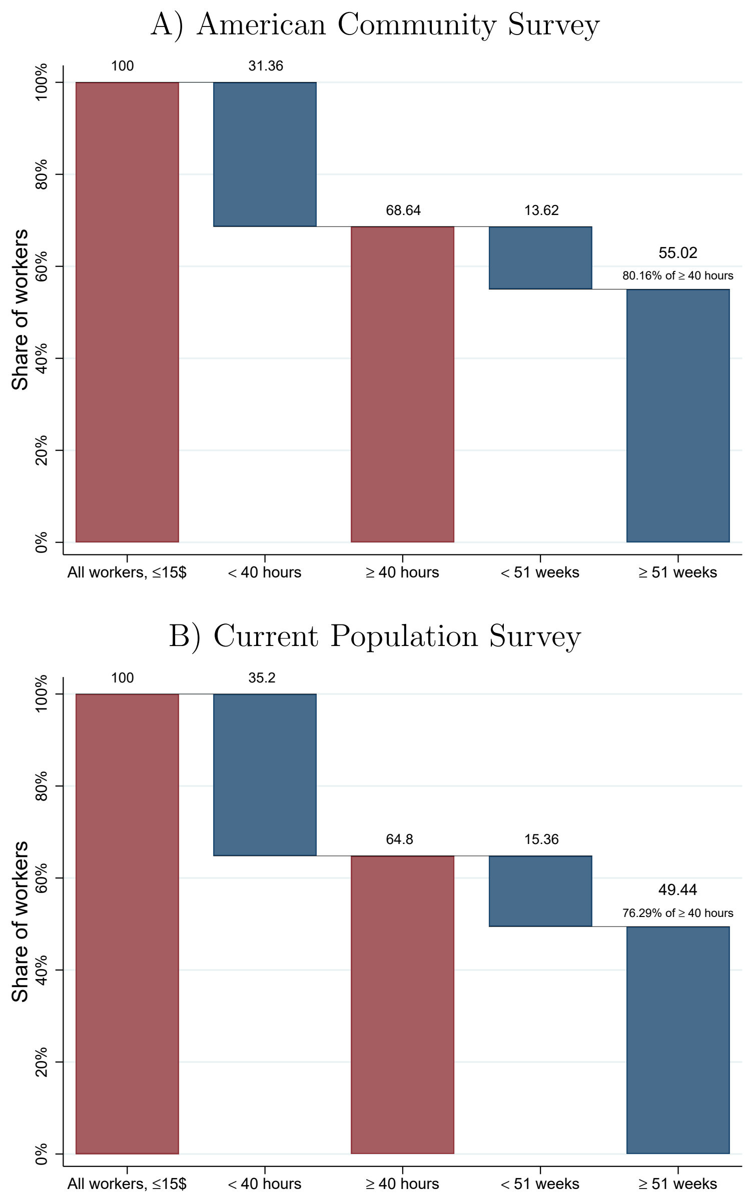
Table A.9: Estimates of the surplus and rents from employment when workers have non-linear utility from earnings

Earnings level	(1) Wage	(2) Hours	(3) MVT	(4) AVT	(5) Rents
\$1,333	27.71	5.47	652.31	7.97	0%
\$1,666	30.79	5.72	757.83	7.97	225%
\$2,000	33.25	7.20	1029.18	14.68	288%
\$2,333	35.27	7.63	1156.39	14.68	533%
\$2,666	36.95	7.80	1239.96	12.00	829%
\$3,333	39.59	8.08	1376.14	12.00	1350%
\$4,000	41.57	8.64	1544.84	21.89	1793%
\$4,752	43.28	9.17	1705.97	21.89	2224%

Notes: This table shows estimates of the surplus and rents from employment at different levels of earnings when workers have non-linear utility from earnings due to disutility from labor or diminishing utility from consumption. The search model parameters are the same as in Table 8 but the utility from earnings is non-linear as described in Section E. Columns 1 and 2 report the hourly wage and weekly working hours associated with each monthly earning level. We calibrate the hourly wage and work hours associated with each earning level according to the following procedure. We first set the hourly wage at the lowest earning job to be the median hourly wage at $t = 0$, $w_{min} = \$11.19$. The bottom rung of the earnings ladder is a job that pays \$1,333 per month, which thus involves $h_{min} = 1333/11.19$ hours per month (roughly 30 hours per week). Let γ be the share of the increase in earnings (above \$1,333) that can be explained by a higher hourly wage, with the remainder attributed to longer work hours, therefore, the hourly wage for earnings e is $w(e) = w_{min} + \frac{\gamma \cdot (e-1333)}{h_{min}}$ and $h(e) = h_{min} + \frac{(1-\gamma)\cdot(e-1333)}{w(e)}$. We use $\gamma = 0.5$. Columns 3 and 4 report the Marginal Value of Time (MVT) and the Average Value of Time (AVT) associated with each earning level based on estimates from Mas and Pallais (2019). Columns 5 reports rents $\left(\frac{(V(e)-V_u)}{e}\right)$ from employment at each earning level relative to unemployment. All rent calculations assume a 5% annual interest rate and set b such that $V_u = V(1,333)$, the implied reservation earnings level.

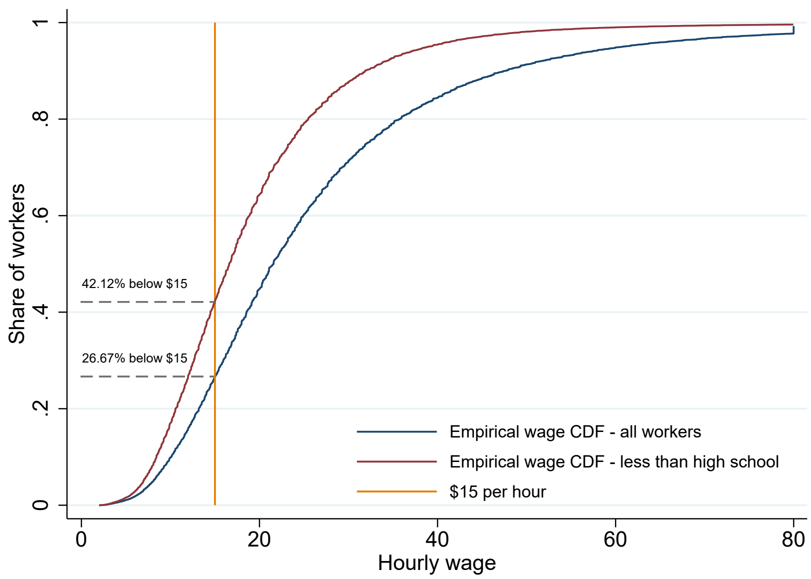
B Appendix figures

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



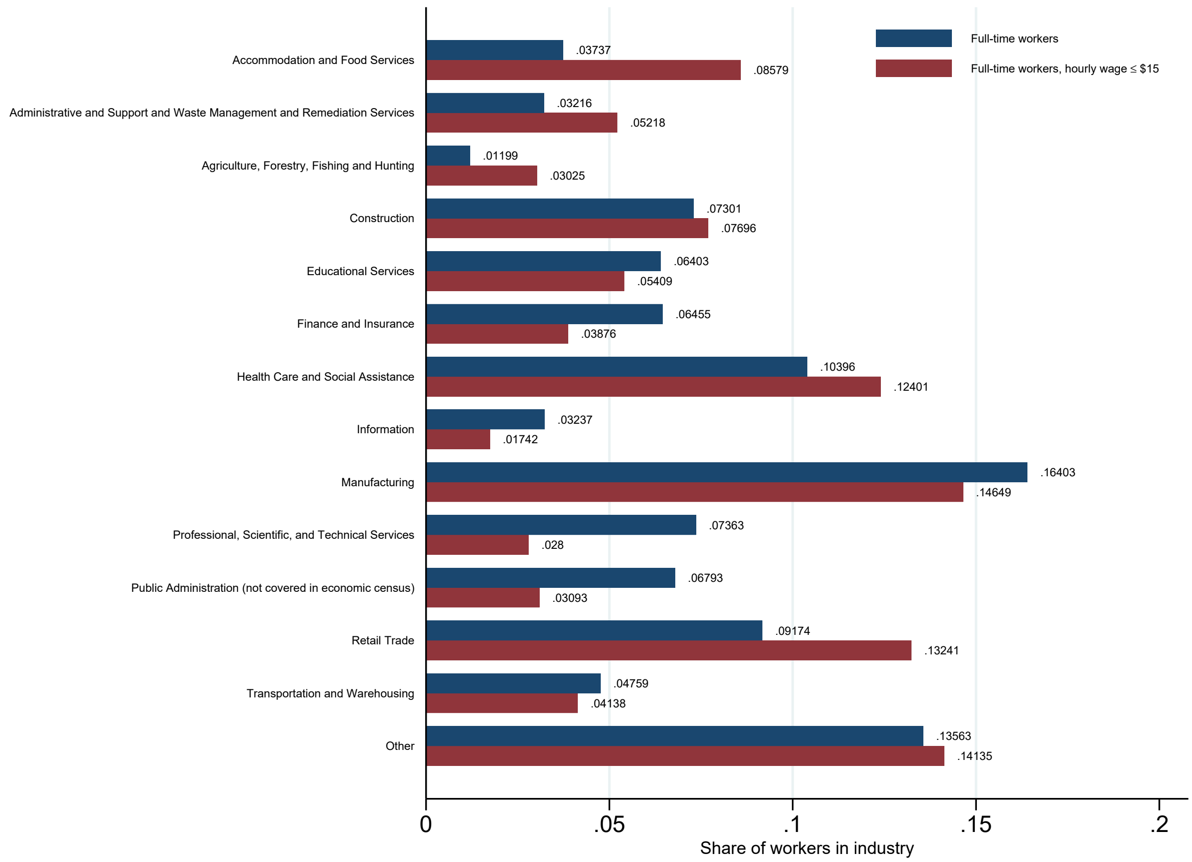
Notes: This figure shows the distribution of usual hours and weeks worked last year among low-wage workers. Panel A shows the results from American Community Survey data, while Panel B uses Current Population Survey data, restricting to participants in the Annual Social and Economical Supplement within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



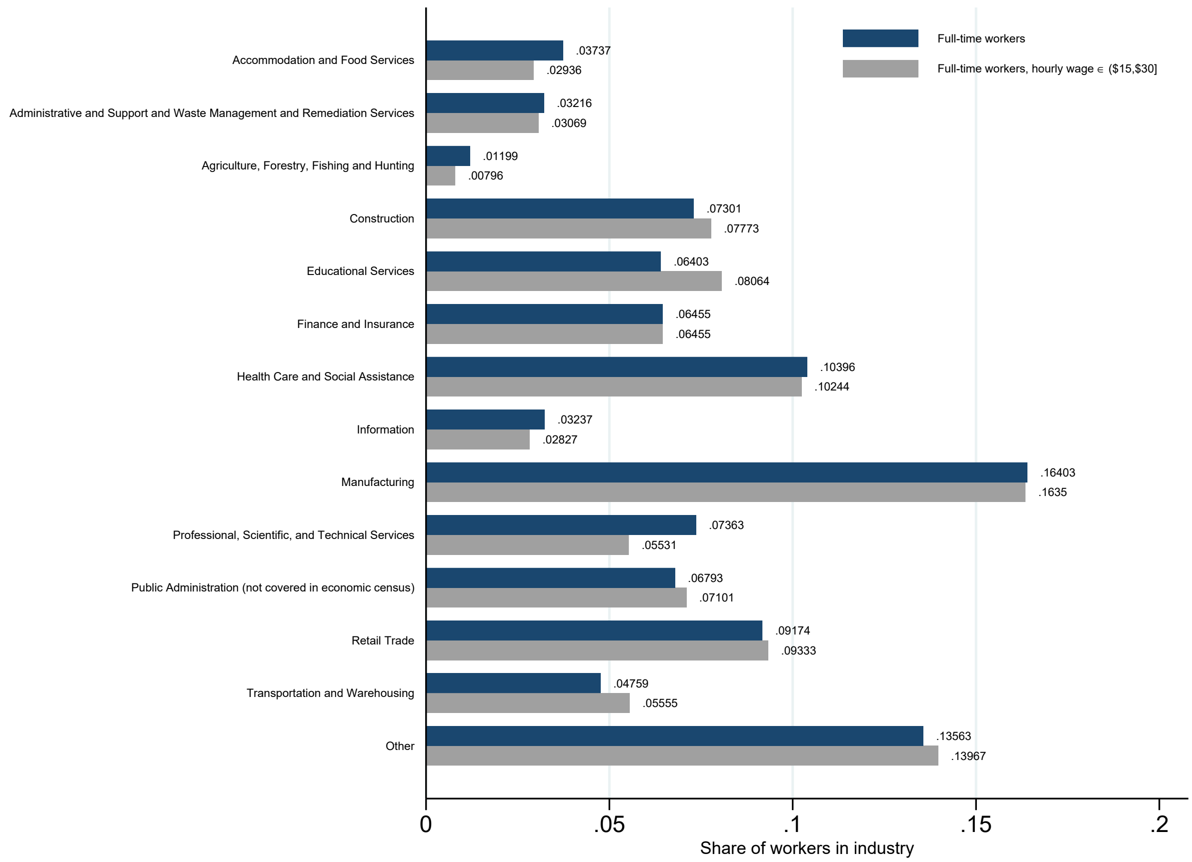
Notes: This figure shows the distribution of hourly wages among employed workers. The figure is based on the authors' calculations using the publicly available American Community Survey, 2001-2008. We restrict attention to ACS respondents between the ages of 22 to 50 who are civilian employees, at work,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



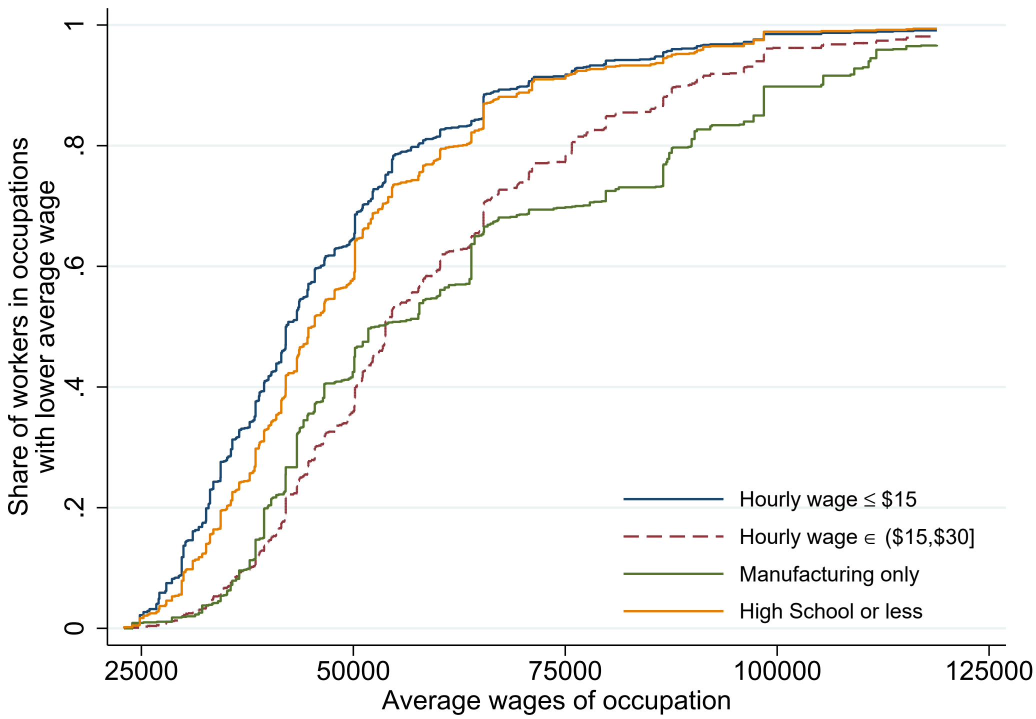
Notes: This figure shows the distribution of employed workers across industries based on 2-digit North American Industry Classification System (NAICS) codes. The figure includes two samples. All workers who are employed full-time in the last year are defined as individuals who worked for at least 51 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



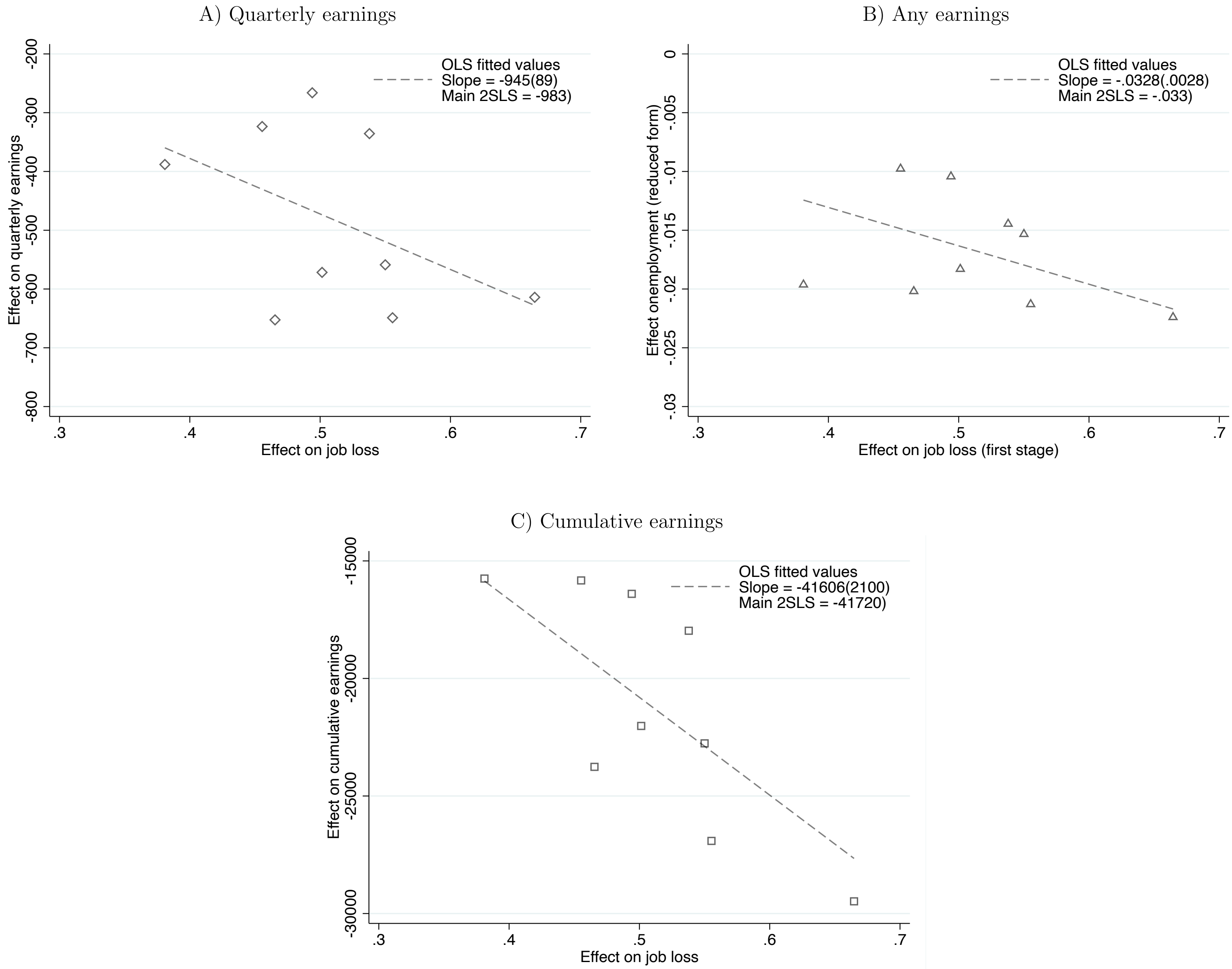
Notes: This figure shows the distribution of employed workers across industries based on 2-digit North American Industry Classification System (NAICS) codes. The figure includes two samples. All workers who are employed full-time in the last year are defined as individuals who worked for at least 51 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



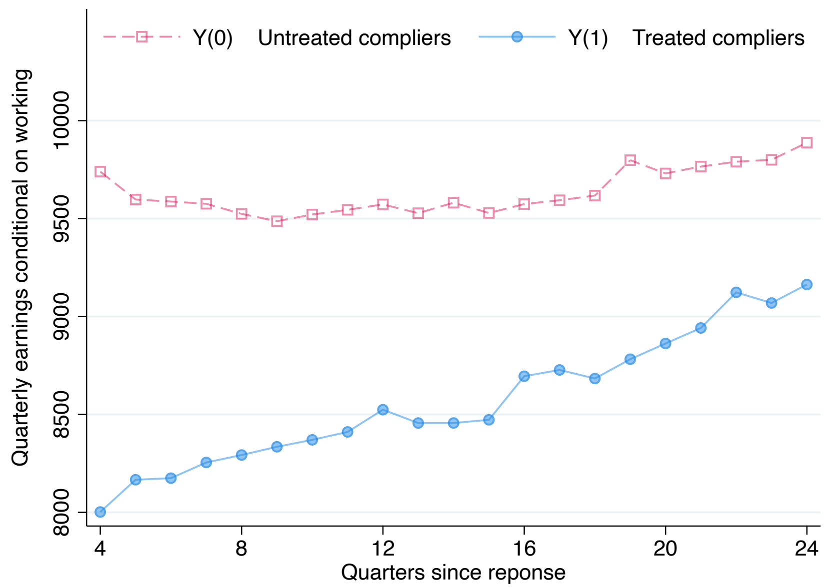
Notes: This figure shows the distribution of full-time employed workers across occupations (based on 2010 occupation codes). The x-axis reports the average wage of full-time workers in each occupation using ACS surveys from 2001 to 2020. The y-axis reports the share of workers working in occupations 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: Effects on job loss vs. earnings and employment across demographic groups



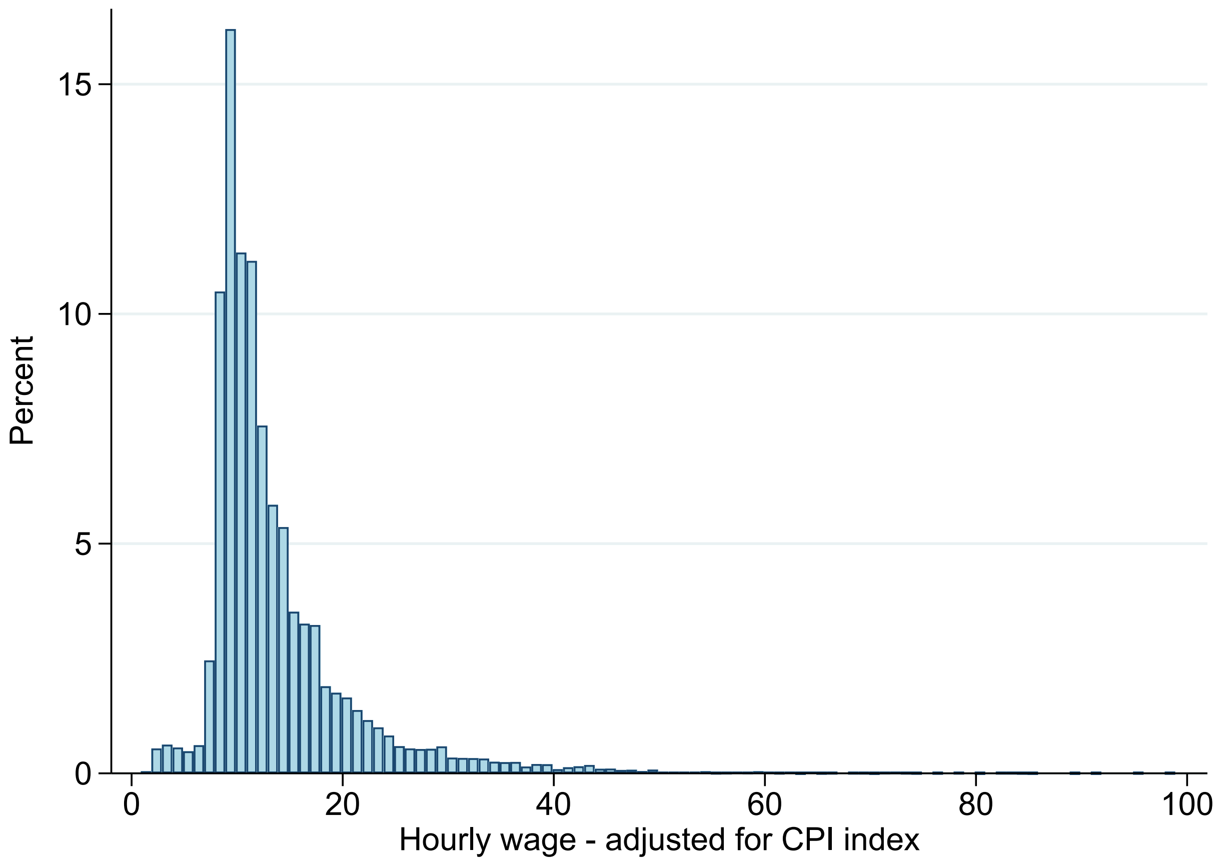
Notes: This figure plots first-stage effects on job loss and reduced-form effects on long-run quarterly earnings (Panel A), employment (Panel B), and cumulative earnings (Panel C). Each point corresponds to the estimated effect on job loss (x-axis) and the estimated effect on a long-run outcome (y-axis) 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.7: 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.

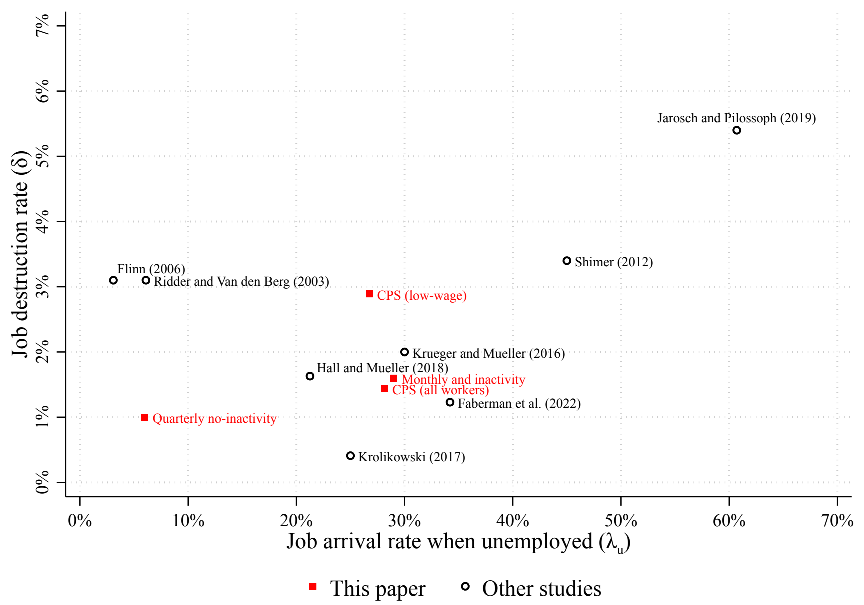
Figure B.8: Share of involuntarily part-time workers by wage in the CPS



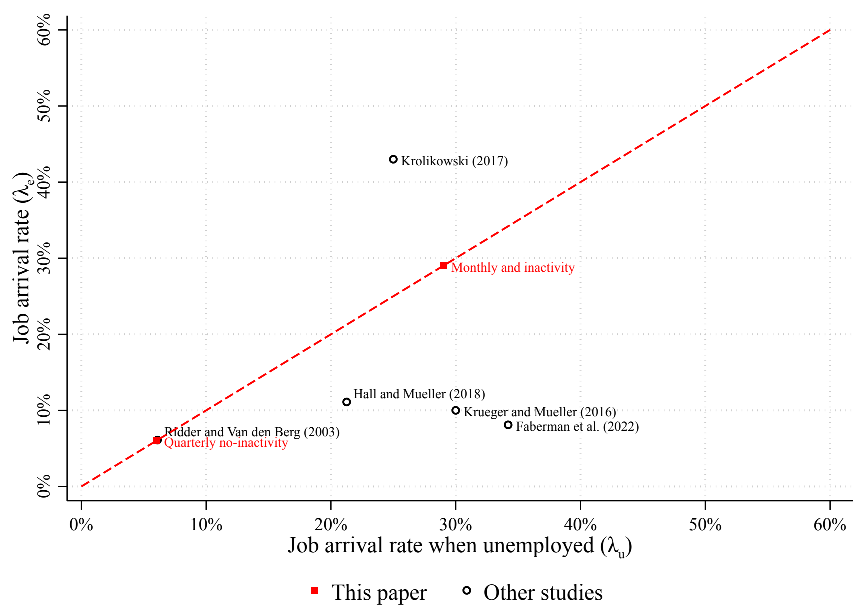
Notes: This figure shows incidence of involuntarily part-time employment by wage. Each bar reports the share of workers in a $1 wage bin whose employment status is part-time for involuntary reasons, coded using the standard Bureau of Labor Statistics definitions. The sample includes the Outgoing Rotation Groups of the Current Population Survey. The sample cover years 2001-2014 and respondents between the ages of 22 to 50, and in one of our LEHD approving states. We inflation adjust hourly wages to constant 2020 dollars using the CPI. We restrict the graph to individuals with wages below $100.

Figure B.9: Search model parameters in the literature

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

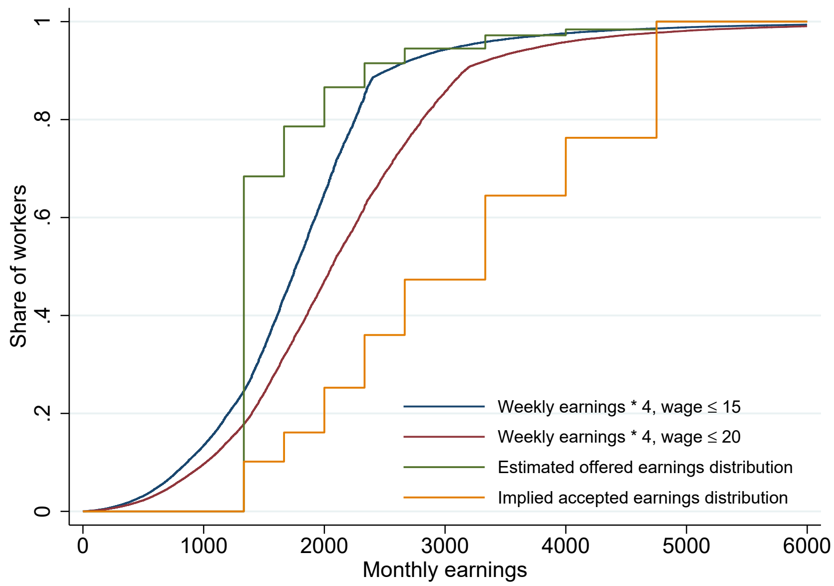


B) Job arrival rates from unemployment (λ_u) and employment (λ_e)



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 F. The CPS estimates are based on the transition probabilities in Appendix Table F.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.

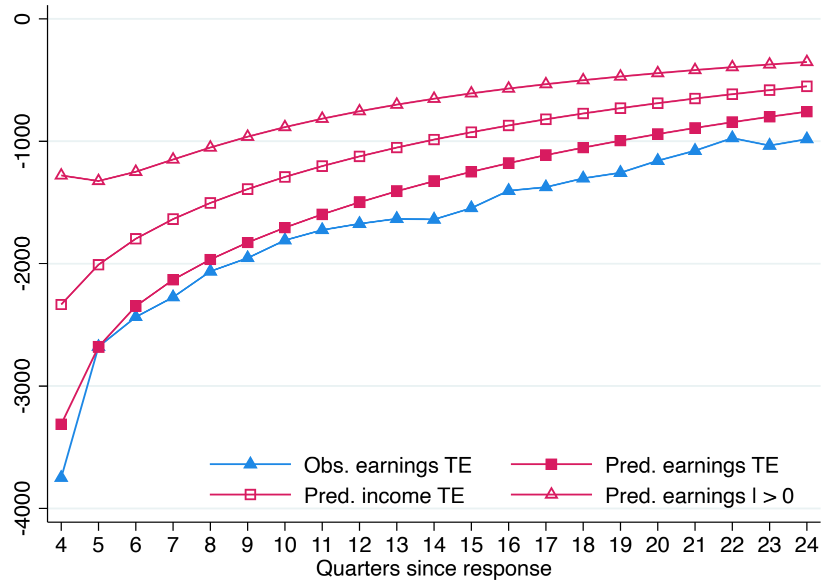
Figure B.10: Offered and accepted wage distribution vs. the CPS



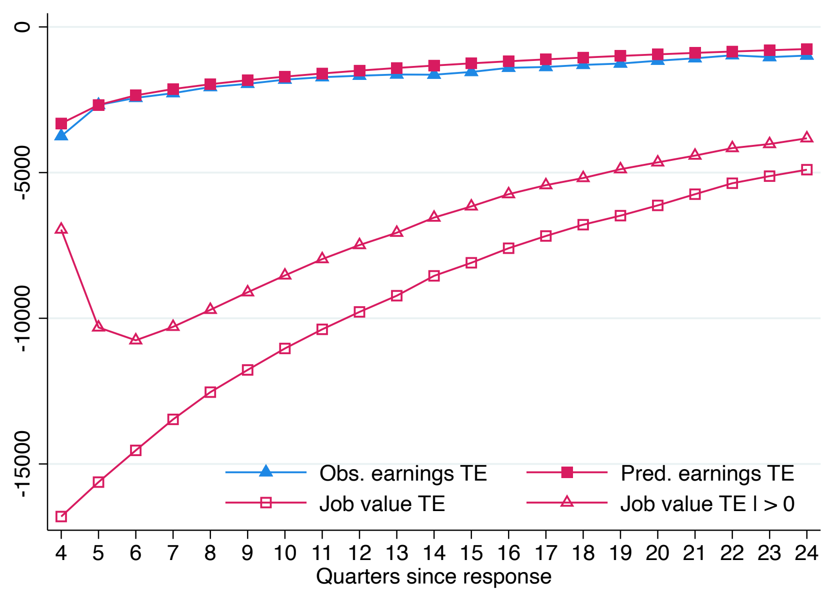
Notes: This figure plots offered and accepted wages from the estimated offer distribution described in Section 9. The figure also plots two benchmarks from the CPS outgoing rotation groups. The blue line plots the cumulative distribution of implied monthly earnings for all workers with wage last week 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 F.

Figure B.11: Model-based treatment effects

A) Earnings and income effects



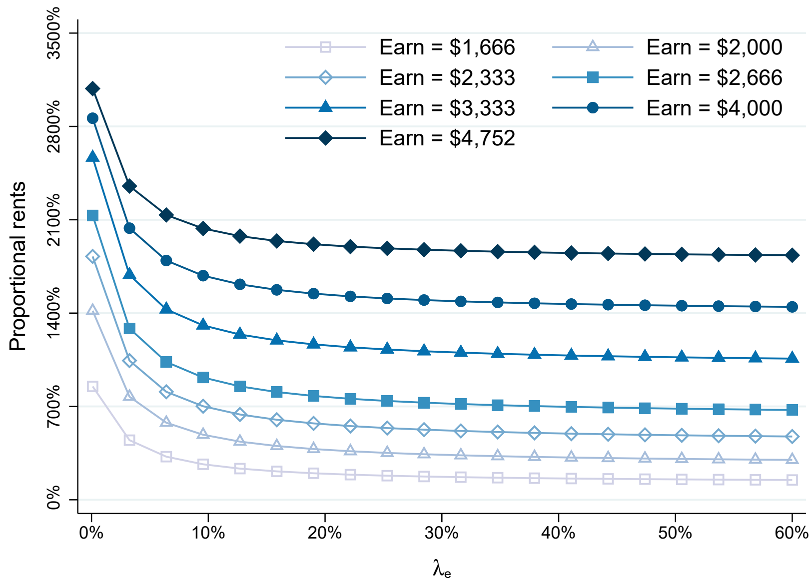
B) Job value functions



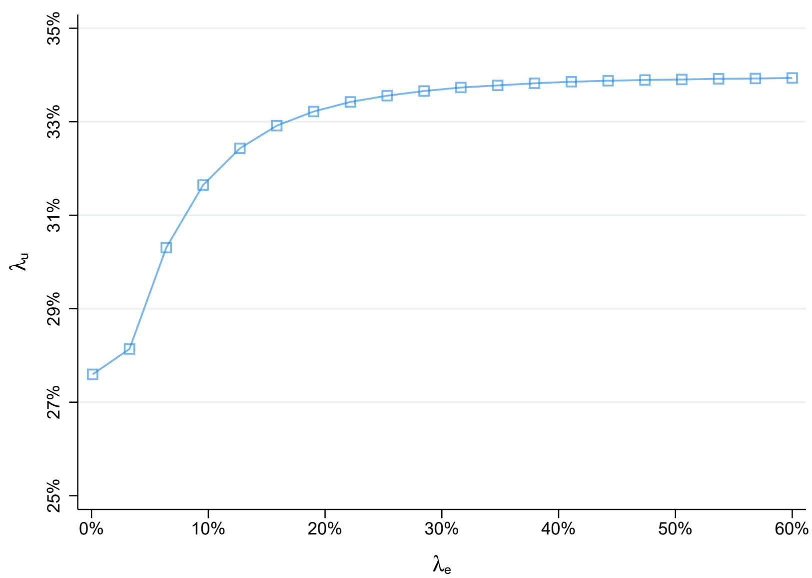
Notes: This figure presents decompositions of the earnings and income effects of job loss using estimates from the job-ladder model described in Section 9. Panel A show effects on total wage earnings and income (including unemployment benefits b), as well as earnings conditional on holding any job. Panel B shows observed and predicted estimates on total earnings, as well as estimated effects on average job values $V(e)$. Unemployed workers (including inactive workers) are assigned V_u.

Figure B.12: Rents and job arrival rates fixing on-the-job search productivit

A) Exact rent estimates



B) Job arrival rates in unemployment (λ_u)



Notes: This figure shows estimates of rents (Panel A) and job arrival rates in unemployment (Panel B) from the model described in Appendix D.2, which allows for different job arrival rates on and off the job. Panel A plots exact proportional rent estimates implied by the discrete earnings distribution and assuming a 5% annual interest rate for each level of earnings. Rents are the present value utility difference relative to unemployment as a fraction of earnings, with b set to the implied value when $1,333 is the reservation earnings level. Panel B reports estimates of λ_u. Each estimate in both panels fixes the value of λ_e at the value listed on the x-axis.

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,[39] 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

[39]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 Derivation of earnings-ladder model

D.1 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:

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

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:

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

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

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

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 Different search productivity on and off the job

If job offers arrive at different rates on and off the job, flow rents can be written as:

$$\begin{aligned} r\bar{V}_u &= b + \lambda_u \int_{e^*}^{\infty} [\bar{V}(x) - \bar{V}_u] dF(x) \\ rV(e) &= e + \lambda_e \int_{e}^{\infty} [\bar{V}(x) - \bar{V}(e)] dF(x) + \delta(\bar{V}_u - \bar{V}(e)) \end{aligned}$$

where λ_u and λ_e are the arrival rate of offers on and off the job, respectively.

Some algebraic manipulation shows that flow rents can be expressed in this case as:

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

which naturally collapses to the prior case when $\lambda_u = \lambda_e = \lambda$.

With differential search productivity on and off the job, rents involve an extra term capturing the added benefit (or costs) of being able to further climb the job ladder beyond earnings level e while employed vs. unemployed.

Because it remains the case that $\bar{V}'(e) = 1/(r+\delta+\lambda_e(1-F(e))) > 0$, both the integral terms are positive. Thus whenever on-the-job search is at least as productive as off-the-job search, a similar bound to the case where $\lambda_u = \lambda_e = \lambda$ can be obtained by ignoring the second integral term:

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

When $\lambda_e < \lambda_u$, rents are lower because the second integral term decreases flow rents—holding a job is less valuable if you cannot continue to advance up the job ladder. In the extreme case where $\lambda_e = 0$, the value of employment can be expressed simply as:

$$V(e) - V_u = \frac{1+r}{r+\delta}(e - r\bar{V}_u)$$

Because it can be shown that when $\lambda_e = 0$ reservation earnings levels respect $e^* = b + \lambda_u \int_{e^*} \frac{1-F(x)}{r+\delta} dx$, we can express flow rents as a portion of earnings in this extreme case as:

$$\frac{V(e) - V_u}{e} = \frac{1+r}{r+\delta}\left(1 - \frac{b + \frac{\lambda_u}{r+\delta}\int_{e^*}(1 - F(x))dx}{e}\right)$$

If F is known, this expression can also be evaluated exactly. If the value of F is known only at M points in the support of e, rents can be bounded using the fact that F is non-decreasing:

$$\frac{V(e) - V_u}{e} \geq \frac{1+r}{r+\delta}\left(1 - \frac{b + \frac{\lambda_u}{r+\delta}\left[\sum_{m=1}^{M}(1 - F(e_{m-1}))(e_m - e_{m-1}) + (1 - F(e_M))(\bar{e} - e_M)\right]}{e}\right)$$

where $\{e_1, \ldots, e_M\}$ are the points where the value of F is known, $\bar{e}$ is the highest level of earnings possible, e_0 can be assumed to be zero, and $F(e_0) = 0$. Note that this bound need not be positive, so a better bound can be found by taking the greater of its value and zero.

D.3 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 $\bar{V}_u = \bar{V}_1$. In the model with equally productive search on and off the job, this implies $b = e^* = e_1$. If search is not equally productive on and off the job, however, one can compute the value of b consistent with the model. 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.

A similar approach can be used when allowing for different search productivity on and off the job by setting $\mathbf{W} = r\mathbf{I}_{M+1} - \tilde{\mathbf{P}} - \delta(\mathbf{I}_{1,M+1})$, where $\tilde{\mathbf{P}}$ is the matrix $\mathbf{P}$ with the first row multiplied by λ_u and all other rows multiplied by λ_e. When $\lambda_e > \lambda_u$, however, it no longer follows that $b = e^*$. Instead, reservation wages respect:

$$e^* = b + (\lambda_u - \lambda_e)\int_{e^*} \frac{1 - F(x)}{r + \delta + \lambda_e(1 - F(x))} dx$$

Setting $e^* = e_1$ implies an implied value for b that can be solved from the discrete wage distribution F.

D.4 Non-participation / inactivity

We assume that there are two types of workers: active and inactive. Active workers seek new jobs when they become unemployed. These jobs arrive at rate λ. Inactive workers do not seek new work after they become unemployed. While the population share of inactive workers is constant, over time inactive workers become increasingly concentrated in non-employment. Let π_{u_t} denote the share of active workers among the non-employed in period t. The average job-finding rate out of non-employment is given by $\lambda\pi_{u_t}$, and the evolution of employment stocks can be written as:

$$\triangle y_t^d = \lambda\pi_{u_t} - y_t^d(\delta + \lambda\pi_{u_t})$$

Likewise, the evolution of earnings distributions can be expressed as:

$$\triangle Q_t^d(e) = \delta - (1 - F(e))[\lambda\pi_{u_t} - \lambda](1 - y_t^d) - [\lambda(1 - F(e)) + \delta]Q_t^d(e)$$

Both expressions clearly collapse to the case discussed in the main text when $\pi_{u_t} = 1$. When some workers are inactive, however, job-finding rates out of non-employment may decay over time as inactive workers become increasingly concentrated among the non-employed. Likewise, $\pi_{u_t} < 1$ generates an implicit difference in the average productivity of search on-and-off the job. The evolution of earnings distributions measures this difference directly through the relative contributions of employment (y_t^d) and the earnings distribution ($Q_t^d(e)$) to its changes.

The evolution of π_{u_t} is also a deterministic function of the parameters of the model and π, the population share of active workers. To derive its law of motion, we need to account for both the likelihood that active workers remain non-employed and the impact of inflows of employed workers who lose their jobs. The first channel includes the $(1-y_t)\pi_{ut}(1-\lambda)$ active workers who do not find jobs. Because the population share of active workers is π, the share of active workers among the employed at time t can be written as: $\frac{\pi-(1-y_t)\pi_{ut}}{y_t}$. Thus the total inflows of active workers due to job loss amounts to $(\pi - (1 - y_t)\pi_{ut})\delta$.

We can therefore express the change in the share of active workers among the non-employed at time t as:

$$\Delta\pi_{ut} = \frac{(1 - y_t)\pi_{ut}(1 - \lambda) + (\pi - (1 - y_t)\pi_{ut})\delta}{1 - y_{t+1}} - \pi_{ut}$$

Taken together, these three equations make it possible to identify and estimate the parameters of this model. Although the dynamics of employment and earnings distributions are linear in the model parameters (or their combinations), the evolution of π_{u_t} is not. Rather than using regression-based approaches as above, we instead fit the model using minimum distance. First, however, we make an additional adjustment to account for the quarterly nature of our earnings data and allow for sub-quarterly job mobility, as discussed in the next subsection.

D.5 Sub-quarterly mobility

Our outcomes measure the probability of observing any earnings and the probability of earning less than particular levels over the course of a quarter, not employment itself or earnings levels in a particular job. These objects can be expressed as functions of model parameters as well with some additional structure. To see how, suppose there are K periods within a quarter. At the start of each period, employed workers collect earnings and unemployed workers collect benefits b. At the end of the period, workers receiving exogenous job offers and job destruction shocks, which determine their state (non-employed vs. employed at each earnings level) at start of the next period. The LEHD outcomes we measure report the sum of earnings from all states within a quarter.

Assume there are $G + 1$ distinct states, including non-employment and G possible earnings levels. Let $g_t \in \{0, \ldots, G\}$ indicate the state occupied at time t, e_g denote earnings associated with occupying state g, and F_g denote the cumulative mass function of job offers evaluated at e_g (because there G earnings levels, F is discrete). Transition probabilities between states are functions of the model parameters and the worker's active/inactive status. Inactive workers never exit non-employment once they enter it, for example, while active workers exit at rate λ. The probability of moving from employment to non-employment is δ for all workers. And the probability of moving up the job ladder can be written as $\lambda(1 - F_g)$. Thus:

$$
\begin{aligned}
Pr(g_{t+1} = 0|g_t = 0, active) &= (1 - \lambda) \\
Pr(g_{t+1} = 0|g_t = 0, inactive) &= 1 \\
Pr(g_{t+1} > k|g_t = k) &= \lambda(1 - F_k) \ \forall\ k > 1 \\
Pr(g_{t+1} = 0|g_t = k) &= \delta \ \forall\ k > 1
\end{aligned}
$$

The conditional likelihood of any K-length sequence of states beginning with g_t, which we denote as $\mathbf{g}_t = \{g_t, \ldots, g_{t+K-1}\}$ can thus be written separately for active and inactive workers as:

$$P^a(\mathbf{g}_t;\Theta) = \prod_{m=1}^{K-1} Pr(g_{t+m}|g_{t+m-1}, active)$$
$$P^i(\mathbf{g}_t;\Theta) = \prod_{m=1}^{K-1} Pr(g_{t+m}|g_{t+m-1}, inactive)$$

Given an initial share of workers in state g in period t, $P_t(g)$, of whom share π_{gt} are active, we can express the unconditional probability of the sequence $\mathbf{g}_t$ as:

$$P(\mathbf{g}_t;\Theta, P_t(g_t), \pi_{g_t t}) = P_t(g_t)\left[\pi_{g_t t}P^a(\mathbf{g}_t;\Theta) + (1-\pi_{g_t t})P^i(\mathbf{g}_t;\Theta)\right]$$

where $\Theta = \{\lambda, \delta, F\}$ collects the parameters of the search model.

Given state shares and active shares in an initial period, say $P_0 = \{P_0(0), \ldots, P_0(G)\}$ and $\pi_0 = \{\pi_{00}, \ldots, \pi_{0G}\}$, state and active shares in all subsequent periods are also determined model parameters. We write these objects as $P_t(g; P_0, \pi_0, \Theta)$ and $\pi_t(g; P_0, \pi_0, \Theta)$. For example, state shares and the distribution of active workers at the start of the second quarter, or $P_K(g)$ and π_{gK}, can be written as:

$$P_K(g) = \sum_{\{\mathbf{g}_0 \ | \ g_K = g\}} P(\mathbf{g}_0;\Theta, P_0(g_0), \pi_{g_1})$$
$$\pi_{g_K} = \frac{\sum_{\{\mathbf{g}_0 \ | \ g_K = g\}} P_0(g_0)\pi_{g_0 0}P^a(\mathbf{g}_0; g_0, \Theta)}{P_K(g)}$$

where with a slight abuse of notation we have extended the sequences $\mathbf{g}_0$ to be length $K+1$ and include the transition into the first period of the next quarter.

We can therefore express the likelihood of earning no more than m dollars over the course of the quarter beginning in period t as the sum of the likelihoods of all possible state sequences from periods t to $t+K-1$ that yield no more than m total earnings:

$$Pr(earn \leq m \ in \ q_t) = \sum_{\{\mathbf{g}_t \ | \ \sum_{g\in\mathbf{g}_t} e_g \leq m\}} P(\mathbf{g}_t;\Theta, P_t(g_t; P_0, \pi_0, \Theta), \pi_t(g_t; P_0, \pi_0, \Theta))$$

The full set of parameters thus consists of the search model parameters Θ, the set of initial conditions P_0 and π_0, and the population active share π. Conditional on these parameters, a full sequence of earnings probabilities, as well as average earnings, can be computed for all periods.

Initial conditions are also allowed to differ for treated and untreated compliers. This is critical, since at $t = 4$ a large share of treated compliers have zero earnings and all untreated compliers are employed by construction. Because job separation is exogenous, the distribution of active workers across initial employed states does not affect predicted earnings and employment. We therefore impose that $\pi_{0g} = \pi_{0g'}$ for all $g, g' > 0$ for both treated and untreated compliers.

E Further extensions to the job ladder model

On- vs. off-the-job search productivity: The model imposes that job offer arrival rates are the same on and off the job. In principle, it is straightforward to relax this assumption by allowing λ_u to capture offer arrival rates when unemployed and λ_e to capture arrival rates when employed. Appendix Section D.2 shows that in this version of the model, the rent bounds established above remain valid when substituting λ_u for λ if $\lambda_e \geq \lambda_u$. If $\lambda_e < \lambda_u$, rent calculations differ because part of the value of being unemployed includes increased access to potentially high-earning job offers. The appendix shows how rents can also be bounded in the extreme case where $\lambda_e = 0$ using partial knowledge of the distribution of job offers.[40]

Using the empirical approach from the previous subsection to estimate differences in λ_e and λ_u is more challenging. Intuitively, because we observe only quarterly totals of earnings, there is no information about the degree of job-to-job mobility within a quarter in our causal effect estimates. A worker who earns \$6,000 over the course of a quarter may have held a single job that pays \$2,000 per month for the entire quarter, or have climbed the earnings ladder from \$1,000 to \$2,000 to \$3,000 each month. In practice this means that differences in λ_e and λ_u combined with shifts in the CDF of the offer distributions provide similar fits to observed moments, with any identification coming from the assumed discrete-time nature of job transitions and the discretization of the offer distribution.

Nevertheless, to examine the sensitivity of rent calculations to different assumptions about on-the-job search, Appendix Figure B.12 presents rent estimates fixing λ_e over a range from 0.1 to 0.6. Rents are computed exactly using the assumed discrete distribution of job offers, as in Column 4 of Table 8. Because when $\lambda_e \neq \lambda_u$ reservation earnings levels need not be equal to b, we compute the level of b implied by reservation earnings being equal to \$1,333 per month, the lowest mass point in the discrete job offer distribution. We view this choice as conservative; it must be an upper bound on possible reservation earnings because workers also take jobs that pay less than this amount.

The results show that in general rents are larger when $\lambda_e < \lambda_u$. Intuitively, this is driven by the fact that low offer arrival rates on the job make unemployment relatively more attractive, since it offers better opportunities to advance into higher paying jobs. This means workers must have lower levels of b in order to rationalize taking jobs paying as low as \$1,333 per month when λ_e is small. Consistent with weak identification, the figure also shows that

[40] Panel B of Appendix Figure B.9 reports estimates of λ_u and λ_e in other studies using U.S. data. Generally, λ_e is smaller than λ_u. However, several recent studies estimate that $\lambda_e > \lambda_u$ in the U.S. and Germany (Krolikowski, 2017; Jarosch, 2021).

similar estimates of λ_u are obtained regardless of how λ_e is chosen.[41]

Non-linear utility over earnings: As noted in the main text, the baseline model features linear utility over earnings. This implies that the flow utility from a job that pays \$2,666 per month is exactly twice as much as that of a job that pays \$1,333 per month. In practice, flow utility from the former may be less than twice as high as from the latter if workers have any disutility from labor or diminishing utility from consumption. To introduce concavity into the utility function over earnings, we use estimates of the marginal value of time from Mas and Pallais (2019), who elicited preferences over hours and wage packages from a set of low-wage workers.

Doing so first requires calibrating the hours and wage combinations at each point on the earnings ladder. We set the hourly wage at the lowest earning job to be the median hourly wage at $t = 0$, $w_{min} = \$11.19$. The bottom rung of the earnings ladder is a job that pays \$1,333 per month, which thus involves $h_{min} = 1333/11.19$ hours per month (roughly 30 hours per week). Let γ be the share of the increase in earnings (above \$1,333) that can be explained by a higher hourly wage, with the remainder attributed to longer work hours.[42] The flow utility from each job on the ladder is given by the surplus over the relevant average value of time reported in Mas and Pallais (2019). That is, $u(e) = h(e)[w(e) - w^*(h(e)]$, where $h(e)$ and $w(e)$ are the hours worked and wages paid at earnings level e and $w^*(h(e))$ is the average value of time (the reservation wage) at hours h. We substitute these values for $u(e)$ into Equation 4 and recompute rents, allowing the value of b in Equation 3 to satisfy $V_u = V(e^*)$ as before.

The results are presented in Table A.9. The marginal value of time increases from less than \$8 per hour at the bottom of the ladder, where our calibration implies roughly 28 hours per week of work, to \$22 per hour at the top, where weekly hours surpass 43. Total utility remains higher for jobs further up the job ladder, however, consistent with the structure of the search model. A job that pays \$2,666 per month implies rents of 3.37 percent and a surplus ($\frac{V(2666)-V_u}{V_u}$) of 2.88 percent. At this earnings level, marginal and average value of time are \$14.7 and \$7.6 per hour, respectively. Rents are larger for higher earnings levels, reaching levels similar to those in Table 8. Incorporating non-linear utility over earnings into the model thus yields similar conclusions about the value of holding a full-time \$15 per hour job.

Higher-wage job losers: It is also possible to estimate the model using the post-job loss

[41] Appendix Table A.7 presents full model estimates for the value of λ_e that minimizes the diagonally-weighted distance between predicted and observed moments.

[42] Hence, the hourly wage for earnings e is $w(e) = w_{min} + \frac{\gamma\cdot(e-1333)}{h_{min}}$ and $h(e) = h_{min} + \frac{(1-\gamma)\cdot(e-1333)}{w(e)}$.

dynamics of workers initially earning between \$15 and \$30 per hour analyzed in Section 4. Appendix Table A.8 presents these estimates. The estimates we disclosed were chosen to cover the earnings activity of our primary sample of low-wage job losers and provide less information on the activity of higher-wage job losers. For example, more than half of higher-wage untreated compliers have $t = 24$ earnings above the top level we consider, \$12,000 per quarter, while only 16% of low-wage untreated compliers meet this benchmark. As a result, the discrete wage approximation used in the model provides a worse fit to higher-wage job losers' earnings activity, as evidenced by the minimum distance criterion reported at the bottom of the table.[43] Nevertheless, the results show that higher-wage job losers face higher monthly job-finding rates, lower job destruction rates, and a job offer distribution with substantially more mass at higher earnings levels. Rents are correspondingly lower.

Other measures of frictional inequality: Various other measures of frictional inequality have been studied in prior work. Hornstein, Krusell and Violante (2011), for example, propose summarizing levels of frictional inequality using the "mean-min ratio," or the ratio of the average wage accepted in a steady state to the reservation wage. In the case where $\lambda_e = \lambda_u$, the mean-min ratio collapses to $1/\rho_e$. If the average replacement rate is 0.5, then the mean-min would be 2. This metric is increasing in λ_e, so that allowing for $\lambda_e > \lambda_u$ would serve to increase frictional inequality. When $\lambda_e = 0$, on the other hand, mean-min ratios are generally small. As discussed above, however, rents in both cases can be non-negligible.

[43]The higher-wage sample is also significantly bigger, so the moments we match are estimated more precisely.

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

Table F.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.

G Oaxaca-Blinder decomposition of job loss effects

The long-run effects of job loss on wage earnings for low-wage and higher-wage workers reported in Tables 3 and 5, respectively, can be decomposed to components attributed to any wages, weeks worked, usual weekly hours, and average hourly wage.

Let e_i denote wage earnings reported in the ACS. Let k_i^d, $\bar{h}_i^d$, $\bar{w}_i^d$ denote average weeks worked, usual weekly hours worked, and average hourly wages of $d \in [0,1]$ type compliers. The effect of job loss on long-run wage earnings reported in the ACS (i.e., the 2SLS estimates in Tables 3 and 5) can be written as:

$$
\begin{aligned}
&E[e_i^1] - E[e_i^0] = \\
&\Pr(e_i^1 > 0) \cdot Cov(k_i^1, \bar{h}_i^1 \cdot \bar{w}_i^1 | e_i^1 > 0) - \Pr(e_i^0 > 0) \cdot Cov(k_i^0, \bar{h}_i^0 \cdot \bar{w}_i^0 | e_i^0 > 0) \\
&+ \\
&\Pr(e_i^1 > 0) \cdot E[k_i^1 | e_i^1 > 0] \cdot Cov(\bar{h}_i^1, \bar{w}_i^1 | e_i^1 > 0) - \Pr(e_i^0 > 0) \cdot E[k_i^0 | e_i^0 > 0] \cdot Cov(\bar{h}_i^0, \bar{w}_i^0 | e_i^0 > 0) \\
&+ \\
&\left[Pr(e_i^1 > 0) \cdot E[k_i^1 | e_i^1 > 0] - Pr(e_i^0 > 0) \cdot E[k_i^0 | e_i^0 > 0] \right] \cdot E[\bar{h}_i^1 | e_i^1 > 0] E[\bar{w}_i^1 | e_i^1 > 0] \\
&+ \\
&\left[E[\bar{h}_i^1 | e_i^1 > 0] E[\bar{w}_i^1 | e_i^1 > 0] - E[\bar{h}_i^0 | e_i^0 > 0] E[\bar{w}_i^0 | e_i^0 > 0] \right] \cdot Pr(e_i^0 > 0) \cdot E[k_i^0 | e_i^0 > 0]
\end{aligned}
$$

The last two expressions can be further decomposed into two extensive margin components and two intensive margin components. And the last term can be decomposed to an hours and hourly wages component. Thus, the above last two terms can be decomposed further into:

$$E[e_i^1] - E[e_i^0] =$$

Selection covariances terms

$$+$$

$$\big[Pr(e_i^1 > 0) - Pr(e_i^0 > 0)\big] \cdot E[k_i^1|e_i^1 > 0]E[\bar{h}_i^1|e_i^1 > 0]E[\bar{w}_i^1|e_i^1 > 0]$$

$$+$$

$$\big[E[k_i^1|e_i^1 > 0] - E[k_i^0|e_i^0 > 0]\big] \cdot Pr(e_i^0 > 0)E[\bar{h}_i^1|e_i^1 > 0]E[\bar{w}_i^1|e_i^1 > 0]$$

$$+$$

$$\big[E[\bar{h}_i^1|e_i^1 > 0] - E[\bar{h}_i^0|e_i^0 > 0]\big] \cdot E[\bar{w}_i^1|e_i^1 > 0]Pr(e_i^0 > 0) \cdot E[k_i^0|e_i^0 > 0]$$

$$+$$

$$\big[E[\bar{w}_i^1|e_i^1 > 0] - E[\bar{w}_i^0|e_i^0 > 0]\big] \cdot E[\bar{h}_i^0|e_i^0 > 0]Pr(e_i^0 > 0) \cdot E[k_i^0|e_i^0 > 0]$$

The estimate in the bottom line of Table 6 reports the share of the total effect of job loss that can be explained by reductions in the average hourly wage if working:

$$\frac{[E[\bar{w}_i^1|e_i^1 > 0] - E[\bar{w}_i^0|e_i^0 > 0]] \cdot E[\bar{h}_i^0|e_i^0 > 0] \cdot E[k_i^0|e_i^0 > 0]Pr(e_i^0 > 0)}{E[e_i^1] - E[e_i^0]} \tag{G.1}$$