Entrepreneurial Wages

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June 2018

Abstract

Do young firms pay less? Previous studies have argued that employees earn less when they join young firms. Using US Census employer-employee matched data, we confirm lower average wages at new firms. However, after including worker fixed effects, nearly two thirds of this decline disappears, suggesting differences in worker quality at new firms. Moreover, once we control for firm fixed effects, absorbing time invariant firm quality, the wage difference between new and established firms becomes economically unimportant. Overall, our findings indicate that, for a given worker who has job opportunities at similar quality new and established firms, the expected wage penalty of going to work at the new firm are, on average, economically insignificant.

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We thank Michael Ewens, Neville Francis, Ross Levine, Chris Moser, Olav Sorenson, Christopher Stanton, Boris Vallee as well as seminar participants at UNC Kenan Institute Frontiers of Entrepreneurship, Dartmouth, HEC, and University of North Carolina for helpful suggestions. The views and conclusions are those of the authors and do not necessarily indicate concurrence by the Federal Reserve Board or other members of the Federal Reserve System. We thank Chris Galvan for his diligent assistance with the data and clearance requests. The research in this paper was conducted while the authors were Special Sworn researchers of the US Census Bureau at the Triangle Census Research Data Center. Research results and conclusions expressed are those of the authors and do not necessarily reflect the views of the Census Bureau. This paper has been screened to ensure that no confidential data are revealed. This research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants SES-9978093, SES- 0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation.

What do we really know about wages at new firms? The common portrayal in the media is that employees at new firms accept lower wages as entrepreneurial firms lack the financial liquidity to offer workers competitive salaries.[1] This viewpoint is supported by empirical evidence that employees, on average, earn lower wages at young firms (Brown and Medoff, 2003), small firms (Oi and Idson, 1999), and when self-employed (Hamilton, 2000; Moskowitz and Vissing-Jorgensen, 2002).[2] Common explanations for lower entrepreneurial wages involve either differences in preferences associated with working at new firms (Blanchflower and Oswald, 1992; Kraus and Litzenberger, 1976), biased beliefs about future wage gains (Bernardo and Welch, 2001), or the exclusion of the option value to abandon entrepreneurship (Manso, 2016). These explanations assume that workers joining new firms would have expected to earn higher wages at older firms.

In this paper, we challenge the underlying belief that joining a new firm entails accepting a wage penalty. Using US Census administrative data, we confirm that new firms indeed pay lower wages on average, approximately 26 percent lower in our sample. However, we disprove the assumption that these workers are accepting lower wages, i.e. a wage penalty, as compared to the wages they would have earned at established firms, i.e. market wages. New firms pay economically identical wages after controlling for worker quality and opportunity sets.

What explains the existence of a significant average differences in raw wages at young firms compared to established peers? The key driver is that new firms, on average, employ workers who receive lower market wages due to time-invariant differences in skills or talent. In fact, when we include worker fixed effects, we show that the new firm wage differential declines by two thirds.

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[1] See for example, "10 Reasons Why You Shouldn't Join a Startup" Entrepreneur, May 22, 2014. The article is available at https://www.entrepreneur.com/article/233831.

[2] Brown and Medoff (2003) find that a 1 SD increase in firm age leads to a 7% increase in wages. Hamilton (2000) finds a 35% reduction in wages for self-employed.

Moreover, once we control for firm fixed effects, absorbing time invariant firm quality, the wage difference between new firms and mature firms becomes positive and insignificant. Controlling for time invariant firm quality absorbs differences in the opportunity sets of given employees and allows us to characterize whether workers who join new firms expect to earn a wage penalty compared to what they could have earned at an established firm of equivalent quality.[3] These results suggests that differences in firm quality are another key to explaining the new firm wage differential observed in the raw data. New firms in our data include a large representation of low quality new firms which are unlikely to succeed over the long run as well as higher quality new firms with greater growth potential. Assuming positive assortative matching, lower quality employees will match to lower quality firms and anticipate lower wages. The presence of disproportionately more low quality new firms will depress mean wages at new firms, on average. However, to understand the expected wage gap a given employee should expect if joining a new firm as compared to employment options at more established firms, we need to absorb these differences in quality so that we can compare otherwise equivalent established and young firm employment opportunities.[4]

Earlier conclusions that new firms pay lower wages still holds. However, this fact is explained by the types of workers new firms employ and by the variety of firm quality represented by new firms. Taken together, these findings suggest that for a given worker who has job opportunities from a similar quality new and established firm, the expected wage penalty of going to work at the new firm will, on average, be economically insignificant. Assumptions regarding preferences or

[3] Such differences in opportunity sets are not absorbed by the employee controls to the extent that they are time varying and unobservable.

[4] Firm fixed effects also control for differences in bankruptcy risk, an important determinant of wages in Berk, Stanton, and Zechner, (2009), and supported empirically in Dore and Zarutskie (2016) and Graham et al (2016).

biased beliefs are not required to understand why workers join new firms. Workers join new firms because they expect to receive similar wages as compared to offerings by established firms in their given opportunity sets.

We reach these conclusions using an AKM method, an approach developed by Abowd, Kramarz, and Margolis (1999) and used to explore CEOs (Bertrand and Schoar, 2003; Graham et al, 2011), investment bankers (Chemmanur, Ertugrul, and Krishnan, 2017), venture capitalists (Chemmanur, Loutskina, and Tian, 2014; Ewens and Rhodes-Kropf, 2015), and loan officers (Gao, Martin, and Pacelli, 2017). This approach uses workers who change jobs to isolate employer and employee fixed effects simultaneously. We identify the new firm wage penalty by including an indicator variable for firms three years or younger. Given the lack of a consistent definition of a new firm in the literature, we also consider a robustness test where we define new firms as firms aged zero to one. The results are qualitatively similar.

While we observe no wage penalty in the full sample, we document an economically modest wage penalty of one percent associated with employment at a new firm for the set of college educated workers and, a more pronounced wage penalty of four percent for the set of college educated workers employed in the technology sectors. A greater wage penalty for high skill and high tech workers would be consistent with greater use of stock option compensation for these types of workers at new firms.[5] High skill, high tech workers may trade-off lower average compensation for a greater probability of high future compensation.

In contrast to the greater new firm wage penalty for high skill and high tech workers, we document a modest wage premium at new firms for founders. These results suggest that founders realize no wage penalty when joining a new firm. In fact, if we were to include founder's equity

[5] Wages in our data include all forms of compensation that are immediately taxable. Stock options are typically not taxed until exercised and, as such, are unlikely to be counted in wages at the time of the grant.

(unobserved in our wage data) and non-pecuniary benefits of being the boss, these workers appear to gain upon joining the new firm.

One important caveat to our analysis is that we do not observe exogenous movement between firms. This potentially limits the generalizability of our results. Our conclusions apply to the real world setting where employees who chose to match to new firms presumably do so in anticipation of productive matches. However, we also find an economically small new firm wage premium of one percent when estimated using only exogenous job switchers, workers who had to change jobs following the closure of an establishment.

Overall, our results imply that young firms disproportionally employ lower skill workers. Given that young firms important contributors to productivity growth, an economically relevant question then becomes whether the ability of young firms to hire human capital matters for the new firm future performance. To explore this, we measure a new firm's worker quality with the AKM's worker fixed effects of people employed by the new firm in its first year of existence. We then predict the new firm's 5-year exit rate and employment growth as a function of the worker fixed effects. We find that the worker fixed effects are an economically important predictor of the new firm survival and growth. While these estimates do not necessarily present a causal relation between the worker quality and the new firm future performance, they do contribute to the literature that used similar methodology to estimate effect of CEOs' (Bertrand and Schoar, 2003) on firm performance. We document that the human capital of young firms is an economically important predictor of new firm performance.

Our paper is the first to use a large sample of employee-employer observations for US firms over nearly two decades to examine the underlying drivers of the new firm wage penalty documented in prior studies such as Brown and Medoff (2003). A handful of prior studies have also examined the new firm wage penalty, primarily using samples of employee-employer matched data in Europe.

However, there are inconsistent findings across these European studies, likely driven by differences in empirical specifications or in country-level factors. For example, looking at young establishments in Germany, Brixy, Kohaut, and Schnabel (2007) find an 8% wage penalty, while Schmieder (2013) instead finds a 10% wage premium. Nyström and Zhetibaeva (2015) document a 2.9% wage penalty associated with new firms in Sweden, when looking at a sample of new entrants to the labor market. Finally, Adrjan (2018) documents that young firms pay modestly higher starting wages in the UK, but that wage growth at these new firms is lower compared to their established peers. Our main contribution to this literature is to show the separate contributions of time invariant worker characteristics, time varying worker characteristics and time invariant firm characteristics in explaining the wage difference between new and established firms.

The closest paper to ours is Burton, Dahl and Sorensen (2017), which differs from our work in substantial ways. While Burton, Dahl and Sorensen (2017) focus primarily on disentangling the effects of firm age on wages from the effects of firm size on wages, our paper focuses on the question of whether a given worker will receive a wage penalty when joining a new firm, given the employee's opportunity set. The papers also differ methodologically. Burton, Dahl and Sorensen (2017) use matching methods to absorb worker selection, which, by design, only control for observable characteristics. We use worker and firm fixed effects to quantify impact of time invariant worker and firm quality on new firm wage penalty.[7] Finally, we use US data and, interestingly, document similar summary statistics as found in Burton, Dahl and Sorensen (2017) in Denmark. They find that new firms have 20 percent lower wages on average, compared to 26 percent in our

[7] Schmieder (2013) also uses an AKM approach which controls for firm and worker fixed effects. However, the sample is skewed towards large establishments of incumbent firms, as the German data used in the study only identify new establishments and do not identify new firms. Additionally, young firms are defined as being 10 years of age or less which makes comparisons more difficult.

sample. Our paper contributes to our understanding why individuals participate in entrepreneurship. Workers join new firms because they expect to receive similar wages as compared to offerings by established firms in their job opportunity sets.

Our paper also adds to the literature's understanding of why people found or join new firms, given they provide lower earnings than incumbent firms (Hamilton, 2000; Brown and Medoff, 2003; and Moskowitz and Vissing-Jørgensen, 2002). Prior studies have argued that people select into entrepreneurship due to non-pecuniary benefits (Moskowitz and Vissing-Jørgensen, 2002; Hurst and Pugsley 2011), a preference for skewness (Kraus and Litzenberger, 1976), preferences for attributes of entrepreneurial firms, such as autonomy and tolerance of risk (Roach and Sauermann, 2015), overconfidence in expected benefits (Bernardo and Welch, 2001), learning about one's own abilities through experimentation (Manso, 2016; Dillon and Stanton, 2018), measurement issues (Hurst, Li, and Pugsley 2014; Levine and Rubinstein, 2017), and sorting based on personal assets (Dinlersoz, Hyatt, and Janicki, 2016). We contribute by challenging the underlying belief that workers anticipate lower wages when joining a new firm.

1 Data

We combine confidential databases from the US Census Bureau to form our estimation sample. Our primary database is the Longitudinal Employer-Household Dynamics data (LEHD) maintained by the US Census Bureau. This matched employer-employee database tracks employees and their wages with various employers on a quarterly basis. LEHD data are collected from the unemployment insurance records of states participating in the program.[8] The data start in 1990 for several states and coverage of states increases over time. The data coverage ends in 2008. Our

[8] See Abowd et al. (2006) for a more detailed description of the program and the underlying data sets that it generates.

project has access to 25 states: Arkansas, Georgia, Hawaii, Iowa, Idaho, Illinois, Indiana, Louisiana, Maryland, Maine, Montana, North Carolina, New Jersey, New Mexico, Nevada, Oklahoma, Oregon, Rhode Island, South Carolina, Tennessee, Utah, Virginia, Vermont, Washington and Wisconsin. While we do not observe data for all states, we observe almost 100% of private employment for any state in the program (Abowd et al. 2006). We map states available in our LEHD sample in Appendix Figure 1. For each individual we observe total quarterly wages at the current place of employment. Although LEHD does not contain equity ownership or hours worked, wage data include all forms of compensation that are immediately taxable. Stock options are typically not taxed until exercised and, as such, are unlikely to be counted in wages at the time of the grant. The LEHD also allows us to observe the age, gender, race, place of birth, and education of each employee[9].

We supplement the information in the LEHD with firm-level information from the Census's Longitudinal Business Database (LBD). The LBD is a panel dataset that tracks all US business establishments.[10] An establishment is any separate physical location operated by a firm with at least one paid employee. The LBD contains information on the number of employees working for an establishment and total establishment payroll. In addition, the LBD contains a unique firm-level identifier, *firmid*, which longitudinally links establishments that are part of the same firm. We observe the LBD for all 50 states and the District of Columbia, which allows us to measure firms' total employment across all 50 states.

We also use the LBD to measure firm age. Firm age is equal to the age of the oldest establishment that the firm owns in the first year the firm is observed in the LBD (Haltiwanger, Jarmin, and Miranda, 2013). This definition of firm age will not misclassify an establishment that changes

[9] Education is imputed for employees with missing education data (Abowd et al. 2006).

[10] See Jarmin and Miranda (2002) for more information.

ownership through M&As as a firm birth, since a firm is defined as a new firm only when all the firm establishments are new establishments and establishment age should remain the same in the LBD regardless of ultimate ownership. Given that the LBD covers employer firms with at least one physical establishment, a representative new firm in our sample will be an incorporated business with a few employees and a physical office. This is a distinction from the self-employed definition of entrepreneurship who Hurst and Pugsley (2011) and Levine and Rubinstein (2017) argue have little desire to grow and are unlikely to create economic benefits beyond the self-employed.

We link the LEHD to firm identifiers in the LBD using the employer identification numbers (EIN). We then track whether an individual stays at the firm over time or moves to work for another firm.

2 Empirical Strategy

To identify wage patterns specific to new firms, we adapt the AKM method as developed by Abowd, Kramarz, and Margolis (1999). We use the following specification:

$$y_{it} = \propto_i + \delta_{J(i,t)} + \eta_t + X'_{it}\beta + \gamma * newfirm_{it} + \varepsilon_{it} \qquad (1)$$

where y_{it} are log quarterly real wages of individual i in year t,[12] $\propto_i$ are employee fixed effects. $\delta_{J(i,t)}$ are firm fixed effects where *J(i,t)* gives the identity of the unique firm that employs employee i in year *t*. η_t are year fixed effects, X'_{it} is a vector of time-varying observable individual characteristics, $newfirm_{it}$ is an indicator variable which assumes the value of one if worker i is employed in a firm three years of age or younger in year *t* and ε_{it} is an error term.

Employee fixed effects capture the time-invariant fraction of individual pay driven by innate skill and other individual and time-invariant attributes which are rewarded equally across employers.

[12] In consideration of computing limitations, we use only quarterly wages from the first full-time quarter observed for each employee in that year.

Firm fixed effect reflects any time-invariant wage premium or discount paid to all employees of a given firm. Abowd, Kramarz, and Margolis (1999) and Song et al (2017) find significant inter-firm wage differentials. These firm-specific premiums or discounts may be explained by differences in intrinsic marginal productivity or rent-sharing across firms. We add year fixed effects to control for time varying changes in wages across the economy. Finally, we include the set of time-varying controls, age and squared and cubed terms of age (to allow for a non-linear trend in wages over an employee's lifetime) and education interacted with employee age and all nonlinear terms of age (to allow for variation in the returns to skill over an employee's lifetime). This is the same specification as used in Card, Heining and Kline (2013), which discusses and provides diagnostic tests of the identifying AKM assumptions.

We repeat diagnostic tests in Card, Heining and Kline (2013) to verify the validity of AKM assumptions in our setting. In the AKM model, the error term consists of three separate random effects: 1) a firm-employee match component; 2) a unit root component; and 3) a transitory error. The identifying assumption is that all three terms are uncorrelated with the firm fixed effects. Three types of endogenous mobility can violate this assumption. We discuss each in turn.

One problematic type of endogenous employee mobility would occur if employees sort into firms based on the firm-employee match component. An example of this type of mobility follows when employee job transitions are motivated by an expectation that employee-specific traits will be specifically valued and compensated by the new employer. In other words, an expected employee wage at the new job will not be so much a function of his skills that would be rewarded equally by all employers (worker fixed effects). Nor will it be the function of how big of a premium the new employer pays to all its employees (firm fixed effects). But rather the wage will be unique to that employer-employee match and reflect how productive the worker is at that particular firm. It is possible to test for such sorting in two ways.

First, if employees tend to get jobs based on the match component, then people who exchange workplaces will not necessarily experience systematic wage changes. More precisely, the expected wage *gains* for workers who move from a firm A to a firm B would be likely different from the wage *losses* for those who move from the firm B to the firm A. In the AKM setting however, the wage gains will be symmetric to the losses. This is because, if different firms pay different average wage premiums, then individuals who join a workplace where other employees are highly paid will on average experience a wage gain, whereas those who join a workplace where others are poorly paid will experience a wage loss. To test for this symmetry, we present event-study analyses that examines the wage effects of switching employers, as in Card, Heining and Kline (2013). Specifically, we begin by calculating the distribution of mean co-worker wages across all person-year observations. We then classify the old employer based on the quartile of coworker mean wages in the last year at that firm, and the new employer based on the quartile of coworker mean wages in the first year at that firm. We then assign job changers to 16 cells based on the quartiles of coworker wages at the origin and destination workplaces. Finally, we calculate mean wages in the two years before and after the job change event in each cell and plot them in Figure 1.

For clarity, Figure 1 only shows the wage profiles for workers leaving quartile 1 and quartile 4 employers (i.e., those with the lowest- and highest-paid coworkers). The figure highlights that within mobility groups there is a strong evidence that moving to a job with higher paid coworkers raises pay and *vice versa*. Most importantly, the figure shows approximate symmetry of the wage losses and gains for those who move between quartile 1 and quartile 4 firms. Namely, workers who move from the 4^{th} to the 1^{st} quartile realize wage losses that are similar in magnitude to the wage gains of workers who move from the 1^{st} to the 4^{th} quartile. The gains and losses for other mover categories exhibit a similar degree of symmetry, particularly after adjusting for trend growth in wages (see Online Appendix Figure 2). This symmetry suggests that a simple model with additive

worker and establishment effects may provide a reasonable characterization of the mean wages resulting from different pairings of workers to firms.

Second, if wages tend to be set at the worker-firm match level, then the implication of such a wage setting mechanism is that neither worker nor firm fixed effects would be able to explain much variation in wages. However, across all our AKM specifications, the R-squared ranges from 84 to 87 percent, suggesting that firm and worker fixed effects explain large fraction of variation in wages.

We find no evidence that the unit root component of the error term violate the AKM assumptions. If a unit root error component were correlated with the firm fixed effects, then job transitions would systematically occur following a pattern of either increasing or decreasing wages at the prior employment. Such a pattern is best motivated by a mechanism where worker ability is revealed slowly over time. Under this scenario, a high ability worker could realize wage increases at her current employer before making the transition to a firm with a relatively greater density of high-ability workers, a firm which is likely to also be a high wage firm. If true, the individual fixed effect would be biased low due to the years before the high quality was revealed. Moreover, this would lead to an over-estimation of the firm fixed effect for high quality worker/high wage firms due to the bias in the individual fixed effects.[13] However, we find that the data does not support the existence of such a pattern. In Figure 1, we find no evidence of trends in the wages of workers pre-transition based on the future transition (e.g. low to high wage firm or high to low wage firm).

Finally, our results would be biased if fluctuations in the transitory error term were correlated with mobility patterns between higher and lower wage firms and, potentially, with new firm status. In other words, workers who have recently received a positive (negative) transitory wage shock will be more likely to move to higher (lower) wage firms, leading to attenuation of the estimated

[13] Likewise, this same pattern would lead to an under-estimation of the firm fixed effect for low wage firms if low ability is revealed slowly over time. For reference, please see Card, Heining and Kline (2013).

employment effects. Essentially, this would predict that transitory shocks are followed by a systematic pattern of job changes to one specific type of firm: (1) high vs. low wage; or (2) new vs. established firm. To test (1), in Figure 1 we examine wage changes before the job switch. If people move after they experience (transitory) shock before the move, then we should observe significant changes in wage trends before the move. However, we see relatively flat wage profiles before the move (at t=-1), suggesting that transitory shocks do not generally predict job transitions. Another example of transitory shock could be that workers are more likely to transition to young firms during periods of high unemployment and, hence, receive lower wages. We find no such evidence (see columns 3-4 in table 8). To test (2), that transitory shocks predict departures to new or established firms, in Figure 2, we plot wage changes around a job switch by the type of origin and destination employer, namely established (aged more than three years) or new (aged three years or less) firm. Again, we do not see economically large trends in wages before job transitions.

In summary, the results of the diagnostic tests for the validity of AKM assumptions suggested by Card, Heining and Kline (2013) show no evidence of endogenous mobility patterns that might invalidate AKM estimation. While it is helpful to verify what types of mobility would invalidate AKM, it is also helpful to recognize types of endogenous mobility that do *not* violate the AKM assumptions. For example, the possibility that high-skilled workers are more (or less) likely to transition to firms with higher wage premiums is fully consistent with the AKM assumptions. Also fully consistent is the possibility that mobility may be related to fixed or time-varying non-wage characteristics of firms (Card, Heining and Kline, 2013). Similarly, an expected bump in wages after a job change is not a problem either. We bring this up because Figure 2 shows a pattern of rising average wages following a job transition, which suggests that job changes are, at least partially, motivated by an expectation that the given worker will be relatively more productive at the new firm, and hence, realize a wage increase. While the presence of this type of endogenous mobility

does not invalidate AKM assumptions, it impacts the interpretation of our findings. Our results are specific to employees who endogenously match to new firms. Our sample of employees who move to new firms is likely biased towards employees who are specifically productive at new firms. Given, the AKM procedure depends on switchers to identify the firm fixed effects, the new firm wage differential will be estimated using workers who may have multiple employment opportunities and chose to join new firms.

While this type of endogenous mobility is intuitive, it does color the interpretation of the new firm wage differential. Workers at new firms are, by definition, new hires. If new hires realize, on average, a wage increase and new hires are relatively more common at new firms, then this will be reflected in the new firm wage differential. The wage implications of a labor force composed disproportionally of new hires cannot be separated from the overall estimate of the new firm coefficient. Moreover, from an employee's point of view, there is no distinction. Employees working at entrepreneurial firms are both employees of a new firm and new employees.

To better understand any bias stemming from voluntary vs non-voluntary job changes, we explore a sample of workers who have to find new employment following the closure of their physical location of employment. We then estimate a separate new firm wage differential using only these workers. The new firm wage differential, estimated only on the set of exogenous job changers, is similar to our baseline specification with all movers (see columns 1-2 of table 8).

We next discuss how we construct our baseline sample underlying Figures 1 and 2 as well as the main AKM regression sample. We start with workers who have worked at a public firm at some point in their observed work history. This sample is selected to choose workers who are known to work at established firms and hence are likely to not be shut off from established firms' labor markets. For these workers, we retrieve their entire work history and wages in the LEHD from 1990 through 2008. Wages are normalized to year 2014 constant dollars and measured at the quarterly

level. Following Card, Heining and Kline (2013), we also minimize part-time jobs in our sample by keeping only the observations with the highest paid wage when a given worker reports wages at multiple firms in a given quarter. To limit the probability of data errors in our sample, we drop all observations for individuals where wages change by 5,000% in one year. We use log wages in the regressions to address the skewed distribution of wages as well as to minimize the role of outliers.

In addition, it is worth emphasizing a few additional sample restrictions specific to the AKM methodology (Card, Heining and Kline 2013). First, we estimate the AKM using a subset of the full data, a set of firms connected through switching workers. This restriction is necessary for the model to be estimated. To be in the connected set, a firm must be linked to at least one other firm in the connected set by worker mobility. Our connected set contains nearly all observations and appears otherwise similar to the full set of firms.

Second, computing limitations prohibit us from using the full set of workers from the LEHD in our estimations. We select a subsample of workers with personal identifying numbers (PIK) ending with even numbers. In unreported results, we have used a random 10% sample of the LEHD and found similar results.

Third, employees with missing data are dropped, as in Card, Heining and Kline 2013. Missing data occurs when an employee is not observed in the employment sample. We attempt to minimize such cases by using only wages from one quarter of each year and replacing missing data in a given quarter by firm and wage data from a subsequent quarter in the same year, when possible. (Specifically, we use the quarter one of data for each employee-year, if available. If missing, we then use the first available quarter in that calendar year.) This approach under samples employees with sustained periods of unemployment. In robustness tests, we included workers with non-employment spells and found qualitatively similar results (see columns 5-6 in table 8).

Fourth, the fact that we observe wages over a full quarter with no information on weeks worked limits our sample to workers with a minimum tenure of over three months. To avoid noise introduced by including incomplete quarters of employment, we drop employee-firm quarters if we do not observe a previous and subsequent quarter of employment at the same firm. This step is acutely important in our setting as worker transitions between jobs are unlikely to occur at the exact start of a new quarter, leading to a systematic bias downwards in wages around a job change. The implications of such a step is that we under-sample workers with especially high turnover rates.

Fifth, we limit the sample to workers who switch jobs after at least two full years of employment and then remain at their new employer for at least two full years. This restriction is necessary to test the validity of the AKM assumptions discussed earlier in this section.

Finally, the implication of the AKM estimation is that by including the firm fixed effect along with the *New Firm* indicator, we are only able to estimate the coefficient on *New Firm* for the subset of new firms which survive for four or more years. In robustness tests, we find qualitatively similar results when we define our new firm variable to only include firms with age zero or one, suggesting that the results are not critical on firms surviving beyond a minimum of two years. However, we cannot speak to wages at young firms which survive for less than two years.

3 Summary Statistics

In Table 1, we report summary statistics for firms (in Panel A) and workers (in Panel B) in our sample. All observation counts and estimates are rounded according to Census disclosure policies. In column 1, we report mean values for all observations in our sample. In column 2, we report mean values calculated for established firms, defined as firms four years or greater in age. In column 3, we report mean values calculated for new firms, defined as firms less than four years of age. As expected, Panel A shows that new firms are significantly smaller, in terms of employee counts. New

firms in our sample have an average of 2 employees, as compared to nearly 19 employees at established firms.[15] Panel B contains statistics for person-level variables. While column 2 contains employees at established firms, column 3 samples both employees and founders at new firms. As in Brown and Medoff (2003), Panel B shows that wages at young firms are lower than at established firms: by 22% in our sample. Employees also have longer overall tenures with older employers by working for a total of 8.7 years, on average, compared to 3.9 years at young firms. Looking at the worker characteristics, new firms employ younger workers and slightly fewer male employees, but similarly educated workers.

In Table 2, we report summary statistics for the employees who switch and do not switch employers. Given that our estimation strategy depends on the assumption that employees who switch jobs are representative of the overall sample, we report these summary statistics for the set of employees who never switch employers during our sample (column 1) and employees who switch employers (column 2). We find workers are economically similar in the two groups in terms of education and gender. However, job switchers are younger, have lower tenure and earn lower wages. These results are consistent with a finding that younger and lower tenure workers switch jobs more frequently as in Topel and Ward (1992). These employees are also likely to receive lower wages.

4 Baseline Results

We report our baseline estimations in Table 3. All standard errors are double clustered at the firm and at the worker level. To facilitate interpretation, we first estimate the new firm wage penalty using a simple OLS, using only year fixed effects. We then add individual fixed effects to control

[15] Average firm size is small when estimated using the mean across all firm-year observations. If we instead calculate firm size using an employee-weighted mean, we find an average of 98,000 workers per firm.

for time invariant worker quality. Then, we add firm fixed effects to control for time invariant firm quality. Finally, we add time varying observable employee characteristics. In the following paragraphs, we discuss the interpretation of each regression in turn.

4.1 OLS Estimation

As reported in column 1, new firms pay lower wages. As compared to established firms, new firms pay, on average, 26% lower wages. This is consistent with results in Brown and Medoff (2003) and Ouimet and Zarutskie (2014). This wage gap may be due the characteristics of new firms, due to differences in the types of employees or compensation practices at new firms.

4.2 Worker Fixed Effects

In column 2, we include worker fixed effects. By controlling for time invariant worker quality, the coefficients on new firms is cut by more than two thirds. In this specification, a worker who switches from an established firm to a new firm will experience, on average, an 8.5% wage decline. The decline in the magnitude of the coefficient on new firm from column 1 tells us that young firms employ, on average, workers who earn less at established firms – workers who presumably have lower time-invariant skill. Young firms may do this because they need less skill, can't successfully find or hire high-skill workers, or because they are financially constrained and this reduces total payroll. There is also a dramatic increase in the R-squared of this regression, suggesting that time invariant worker traits explain most of the wage variation.

By adding worker fixed effects, we can identify the new firm wage penalty which is not driven by employing workers of lower intrinsic quality. However, by adding the worker fixed effect, we now estimate the new firm dummy variable using only the sample of workers who switch jobs. We argue that this limitation does not skew the results given the generally economically similar summary statistics reported for job switchers and non-job switchers in Table 2.

Alternatively, we do acknowledge that endogenous matching can limit the generalizability of our results. Given job changes are, on average, followed by a wage increase as seen in Figures 1 and 2, we assume the majority of job changes are worker-initiated. To the extent that job changes to new firms are worker-initiated, then presumably, the workers who are initiating these specific job transitions are workers who anticipate being relatively more productive at new firms. Moreover, in our sample, we drop all long-term unemployed workers, the set of workers who might be most willing to accept a poor match due to limited options. Assuming employees are paid based on productivity, this suggests that the new firm wage difference may be underestimated, as calculated using our real-world sample of endogenously matched employees.[16]

The existence of an upward bias in wages at new firms rests on the assumption that the average job change to a new firm is worker-initiated. However, even if worker job changes are worker-initiated on average, the same pattern may not hold in the smaller sample of job transitions from established firms to new firms. If true, this would bias the coefficient on new firm down, as compared to a theoretical setting where worker job changes are fully exogenous. In the absence of a direct measure of any bias in the new firm wage estimate, we restrict our interpretation to observed wages in a real world setting. Our results reflect the expected wage change a given worker should anticipate if making an endogenous job transition to a new firm.

[16] Theoretically, this same reasoning should apply to workers observed at established firms as well as workers at new firms. However the bias will be specific to wages at new firms as new firms by definition have more new employees. If new employees are paid a premium, on average, due to a better match in terms of productivity and new firms have more new employees, there will be an upward bias in the estimate of wages at new firms or a downward bias in the new firm wage penalty.

4.3 Individual and Firm Fixed Effects (AKM)

In column 3, we now add firm fixed effects, thereby estimating an AKM regression. The coefficient on new firm is further reduced and now equals 2.4%, suggesting an economically small new firm discount. Adding firm fixed effects changes the sample used to estimate the coefficient on new firm in a manner similar to adding employee fixed effects. With firm fixed effects, the coefficient on new firm is only estimated for the set of firms which survive for four or more years. To ensure that this is not introducing a significant bias, in untabulated results we estimate the same regression but define new firms as ages zero-one. We find qualitatively and statistically similar coefficient on new firm.

The set of new firms in our sample includes a mix of both low quality new firms that are unlikely to survive much beyond four years as well as high quality young firms with strong growth potential. Alternatively, the pool of established firms is likely to be relatively more weighted towards successful firms. Firms which only survive four years will be observed only one time in the established firm sample (in year 4). Alternatively, firms which survive for fifteen years could be observed for ten or more unique years. As such, the average firm captured by the new firm indicator variable is likely to be of relatively lower quality. Under an assumption of positive assortative matching, lower quality employees will match to lower quality firms and receive lower wages.

Firm fixed effects controls for time invariant firm quality. The fact that the coefficient on new firm is lower with the addition of firm fixed effects suggests that some of the new firm wage penalty observed in the prior two columns is due to the fact that some new firms are low quality firms, paying low wages. These firms are unlikely to pay significantly higher wages in later years even if they were able to survive to maturity.

In sum, wages at new firms may be lower due to differences in intrinsic firm quality or intrinsic employee quality. After controlling for both worker and firm fixed effects, the wage penalty associated with new firms declines dramatically.

4.4 AKM with Time Varying Worker Characteristics Controls

In column 4 we add controls for time varying and observable worker characteristics. We control for age squared and age cubed to control for typical non-linear patterns in wages over the career of a typical employee. We also interact the age terms with employee education level to allow for the fact that more educated workers can have different wage patterns across time. Given the evidence in column 2, that new firms disproportionately employ time invariant lower quality workers, it is reasonable to expect that young firms may also disproportionately employ workers at points in their career where they would expect lower wages. Such an assumption is also consistent with the findings in the summary statistics and reported in Ouimet and Zarutskie (2014) that young firms employ more young workers.

The coefficient on new firm in column 4 is further reduced as compared to column 3. This result suggests that new firms indeed hire workers at points in time in their career where they would command lower wages. In fact, with the added controls for time varying worker quality, the coefficient on new firms is no longer negative or statistically significant. This result suggests that for a given worker who has job opportunities from a similar quality new and established firm, the expected wage penalty of going to work at the new firm will, on average, be economically insignificant.

These results are robust to the choice of other broad samples of workers. In untabulated results, we find qualitatively similar results if we limit the sample to just men; if we instead use a random ten percent sample of all individuals in the LEHD; or if we limit the sample to only workers who have ever changed employers. Likewise, our results are not driven by workers moving between

new and young firms. We include an indicator variable for firms aged four to ten and find a qualitatively similar the coefficient on *New Firm*.

In conclusion, on average new firms pay lower wages. However, large wage difference observed when just looking at simple averages is explained by the fact that new firms hire more workers who command lower wage due to lower intrinsic quality as well as more workers at a point in time when they are commanding relatively lower wages due to youth or inexperience. Moreover, some new firms are of inherently and time invariant lower quality. These firms are likely to always pay lower wages, even if they are able to survive to a greater maturity. Controlling for individual time invariant and observable time varying characteristics as well as firm time invariant characteristics explains the difference in wages between new and established firms.

5 Alternative Samples

We now consider if the results are different when considering specific subsets of employees. Specifically, we are interested in whether the same patterns are observed in subsets of employees who are particularly critical to firm growth, educated workers and founders as well as employees of high technology firms, a sector where startups play an especially critical role in overall firm growth.

5.1 College Educated Employees

We start by looking at the subsample of college-educated workers, as defined as employees with sixteen or more years of education. A large literature in economics shows that highly educated workers are also relatively more skilled, compared to the general population. Therefore it is important to understand if new firms are able to employ these high skill employees at market wages or if they pay them a discount or premium.

In Table 4, we repeat the same empirical specifications as used in the baseline sample but applied to the sample of college educated workers. It is interesting to note that even after limiting the sample to college educated workers, we still observe a significantly lower wage at new firms in an univariate setting, as reported in column 1. As in Table 3, employee fixed effects continue to be important explanatory variables of wages, even within the more homogenous set of college-educated workers, as reported in column 2. Moreover, adding firm fixed effects (column 3) and worker time varying characteristics (column 4) lowers the new firm wage penalty.

As compared to the results using all workers, the key difference is that there is a slightly larger wage penalty associated with working at a new firm for college educated workers. College educated employees at new firms earn, on average, 1.3% lower wages. These results could be driven by the fact that these workers are relatively more likely to receive compensation that is not captured in our measure of wages, as compared to their less educated peers. For example, college educated workers at new firms may receive stock options. Stock option based compensation will be reflected in our measure of wages, but only when the options are exercised. Given results in Ouimet and Tate (2017) that only 15% of employees receiving stock options exercise any of these options within three years, suggests that such compensation is unlikely to be reflected in wages of firms three years of age or younger. Moreover, Oyer and Schaefer (2005) use a BLS survey and report that "just 2.7% of US establishments granted stock options to non-owners in 1999." Thus, while unexercised stock options are unlikely to explain differences in wages between new and established firms using broad samples, they can possibly explain some of the differences when looking specifically at college educated workers (those more likely to receive options) or for employees in the high tech sector (the industry where option use is more common).

Alternatively, college educated workers may expect more or value more greatly other benefits from working at a new firm as compared to their peers. For example, college educated workers

may be more aware of large payouts to employees at some young firms following IPO events and are, thereby, willing to accept a lower mean wage for greater skewness in future expected wages. However, in untabulated results we test this prediction and find no supporting evidence. We score industries based on the skewness in returns and then estimate separately the new firm wage discount for high skew and low skew industries. We find no meaningful difference between the two industries.

5.2 High Technology Firms

In Table 5, we further restrict the sample to just college educated workers at high technology firms. We define the high technology sector to include firms in computers, biotech, electronics and telecom.[17] Specifically, we define a firm as being in the "Computer" industry if its primary SIC code is 3570-5379, 5044, 5045, 5734, or 7370-7379. A firm is in the "Biotech/Medical" industry if its primary SIC code is 2830-2839, 3826, 3841-3851, 5047, 5048, 5122, 6324, 7352, 800-8099, or 8730-8739 excluding 8732. A firm is in the "Electronics" industry if its primary SIC code is 3600- 3629, 3643, 3644, 3670-3699, 3825, 5065, or 5063. A firm is in the "Telecom" industry if its primary SIC code is 3660-3669 or 4810-4899. We focus on these industries given the concentration of high value startups in these industries.

Overall, the pattern of wages is similar for college educated workers in high technology areas as compared to college educated workers in the full sample. However, with the high technology workers, there is an even more pronounced wage discount for employees of new firms, as shown in the regression reported in column 4 with firm and individual fixed effects and time varying employee controls. These results suggest that potential non-wage benefits suggested for college educated workers at new firms may be especially important within this sector of high growth firms.

[17] We identify worker industry as the first industry observed for a given worker.

College educated workers employed in the technology sector, on average, realize 4% lower wages at new firms.

5.3 Firm Founders

In Table 6, we look specifically at new firm founders. We do not directly observe the job title of employees in our data. Instead, we identify a founder for each new firm as the employee who was in the firm in the firm's first year of existence. We then create two indicator variables for employees of new firms. *New Firm Founder* is defined as 1 if the firm is three years of age or less and the employee is identified as a founder. *New Firm Employee* is defined as 1 if the firm is three years of age or less and the employee is not identified as a founder.

We run the same specifications as in the earlier tables. It is striking to note in column 4, that new firm founder has a positive and significant coefficient equal to 1%. This result suggests that instead of a wage discount, founders instead receive a small wage premium at a new firm. Moreover, to the extent that founders are more likely to receive equity in the new firm, as compared to their previous employer, this premium will be underestimated. Furthermore, if founders receive non-wage perks from being the boss, one of the key justifications in Moskowitz and Vissing-Jorgensen (2002) for pursuing self-employment, then again this underestimates the total gains founders realize upon joining the new firm. In column 4, we now observe a small wage penalty of 2.9% for new firm employees. This penalty might stem from preferences for working at new firms.

6 Controlling for Firm Size

In the previous analysis, we do not control for firm size. Firm size is positively correlated with firm age and negatively correlated with wages. As such, the exclusion of this variable is biasing our coefficient on "new firm" downwards, or making the wage penalty for working at new firms appear more negative. We chose not to include firm size in the baseline estimation to capture the

typical wage implications for a given employee joining a new firm, which in almost all likelihood will also be a small firm. However, there is value in understanding how much of the wage penalty associated with new firms is driven by firm size. Hence, in Table 7, we add firm size to the baseline regressions. Specifically, we measure firm size as log employment and the second and third order transformations of log employment.

In column 1, we find no significant difference in wages at new firms, after controlling for firm size. This result is consistent with Burton, Dahl and Sorenson (2017) which finds that firm age has no bearing on wages, after controlling for firm size in a sample of Danish firms. As in Oi and Idson (1999), firm size is a significant predictor of wages. Firm size has a non-linear relation with wages. The net effect of the three terms enters as a positive relation between firm size and wages for firms up to 33 employees.

After controlling for individual fixed effects in column 2, the coefficient on new firm is now negative and significant. Moreover, individual fixed effects have a pronounced impact on the non-linear relation between firm size and wages. For all firm employment sizes, the relation between firm size and wages is now strictly positive.

In column 3, with the addition of firm fixed effects, the coefficient on new firm is comparable to the baseline results without controls for firm size in Table 3. The similarity between the two different specifications suggests that after controlling for time invariant firm characteristics, the added effect of controlling for firm size is marginal. Most firms in our sample experience modest change in employment over the sample, thereby limiting the ability to estimate the effect of firm size after controlling for firm fixed effects.

In column 4, with the addition of time varying worker characteristics, we report a positive and significant coefficient on new firms. These results suggest that employees at larger new firms realize a wage premium. Likewise, adding controls for firm size increases the coefficient on new

firm if we use just the sample of college educated workers (column 5) or college educated workers in the tech sectors (column 6).

7 Do Worker and Firm Quality Matter for New Firm Outcomes?

In the previous sections, we documented that young firms disproportionally hire low quality workers. An economically interesting question is then: does the ability of young firms to attract high quality workforce matter for the young firm performance going forward? On the one hand, performance of a startup might be mostly driven by the quality of an idea behind the new firm, i.e. firm intrinsic quality. Alternatively, the human capital of a new firm might be a critical input for the new firm to grow and succeed. Kaplan, Sensoy, Stromberg (2009) call the former a "horse" and the latter a "jockey" and in a sample of 50 VC-backed firms find that the idea is more important for a new firm success than its management team.

One limitation of Kaplan, Sensoy, Stromberg (2009) study is that their sample is limited to VC-backed firms that managed to go public. This means that the startup ideas for these firms were likely quite good to start with, so the impact of the team's human capital on firm performance might be less important for this sample of firms. We revisit this question using our sample of new firms. Our advantage is that we have a large number of new firms with large variation in their outcomes. We have 22 thousand new firms with 55% percent 5-year exit rate and 5-year employment growth rate with a mean of 34% and a standard deviation of 75%. We also provide a methodological contribution to measure worker vs startup quality. We measure a new firm's human capital with the worker fixed effects of workers employed by a new firms in its first year of existence. We measure a new firm quality with the firm fixed effect (measures the new firm's "abnormal" wages paid to employees after adjusting for the worker quality). Labor economics literature has argued that the firm fixed effect from AKM regressions might reflect intrinsic firm quality (Burdett, and Mortensen. 1998;

Abowd, Kramarz, and Margolis, 1999). Both fixed effects are estimated from the baseline AKM wage regression specification in column 4, table 3.

In OLS setting, we predict a new firm 5-year exit rate and employment growth as a function of the worker and the firm fixed effects.[18] We report results in Table 9. Both the worker and the firm fixed effects are positively associated with the new firm survival and employment growth. Moreover, the relationship is economically significant. When all controls are included (columns 2 and 6), doubling of the worker fixed effect predicts a 20% higher survival rate and a 28% higher employment growth. Similarly, doubling of the firm fixed effect is associated with a 17% higher survival rate and a 22% higher employment growth (columns 4 and 8). While both the worker and the firm fixed effects are positively associated with new firm outcomes, hiring of the high quality employees predicts economically larger positive effects for the new firm's outcomes than the new firm's wage policies. The caveat is that these estimates do not present a causal relation between the worker/firm intrinsic quality and the new firm future performance. However, they do contribute to the literature that used similar methodology to estimate an effect of CEOs' (Bertrand and Schoar, 2003) on firm performance. Our results show that the human capital of young firms is an economically important predictor of the new firms' performance.

8 Conclusion

In this paper, we use US Census administrative data to report important facts regarding wages at entrepreneurial firms. As in earlier studies, we confirm a 26% lower average wage at new firms. Two thirds of this wage difference can be attributed to differences in worker quality at new firms. These results mitigate the perception that employees joining new firms must accept a wage penalty.

18 We find similar results if we use logit to predict startup exit. We also find similar results if we measure startup exit in 6 instead of 5 years.

Instead, most of the observed wage difference is due to the fact that these new firms are employing relatively more workers who command lower wages on the market due to differences in inherent skills or experience.

Moreover, once we control for firm fixed effects, absorbing time invariant firm quality, the wage penalty further drops to 2.4%. New firms in our data will include a varied group of both low quality new firms, which are unlikely to succeed over the long run, as well as high quality new firms with tremendous potential. Assuming positive assortative matching, lower quality employees will match to lower quality firms and receive lower wages.

Finally, if we also control for observable time-varying worker characteristics, we now observe a positive and statistically insignificant wage premium at new firms. New firms disproportionally hire workers at points in their careers when they expect to earn lower wages, due to limited experience or tenure. Taken together, these findings suggest that for a given worker who has job opportunities from a similar quality new and established firm, there will be no expected wage penalty of going to work at the new firm.

Using subsets of just college-educated workers or just college educated workers employed in the high technology sectors, we find a modest wage penalty associated with employment at new firms. These high skill workers may be willing to match to new firms due to the expectation of receiving stock options, which are typically not reflected in our measure of wages, or due to preferences for skewness. Alternatively, we find a wage premium associated with the transition to new firms by founding employees. This is a striking result given that we are likely underestimating this gain due to the fact that owner's equity is not included in our wage measure.

These results contradict the earlier assumptions that workers had to accept a wage penalty, on average, when joining a new firm and add to our understanding of why individuals chose to become entrepreneurs. However, these results still leave open the questions. If wages are similar across new

and older firms, then what drives preferences? Second, what is the underlying mechanism by which lower quality workers are matched to young firms?

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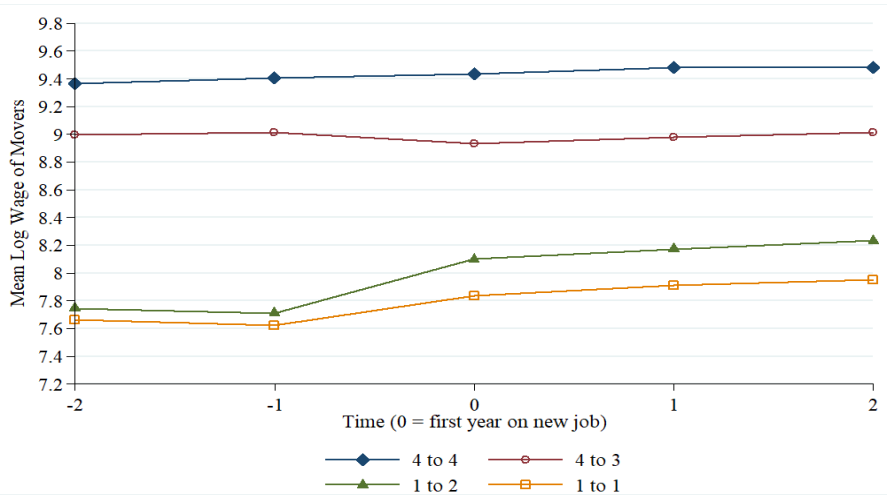
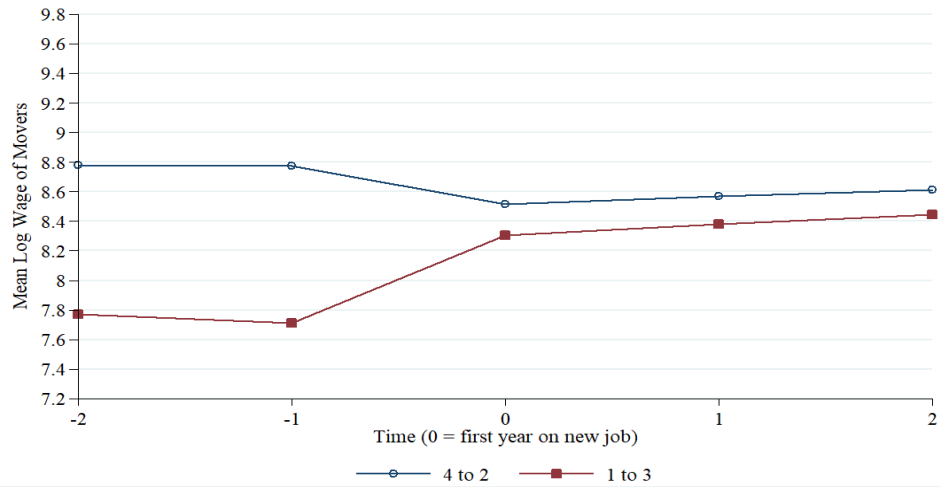
Figure 1. Mean Wages of Job Changers Classified by Quartile of Mean Wages of Coworkers at Origin and Destination Firm

Figure shows mean wages of workers who change jobs (i.e., employers) in the respective interval, and held the preceding job for two or more years, and the new job for two or more years. Job refers to firm with most wages in the quarter. Each job is classified into quartiles based on mean wage of coworkers. Wages are log normalized to real 2014 dollars.





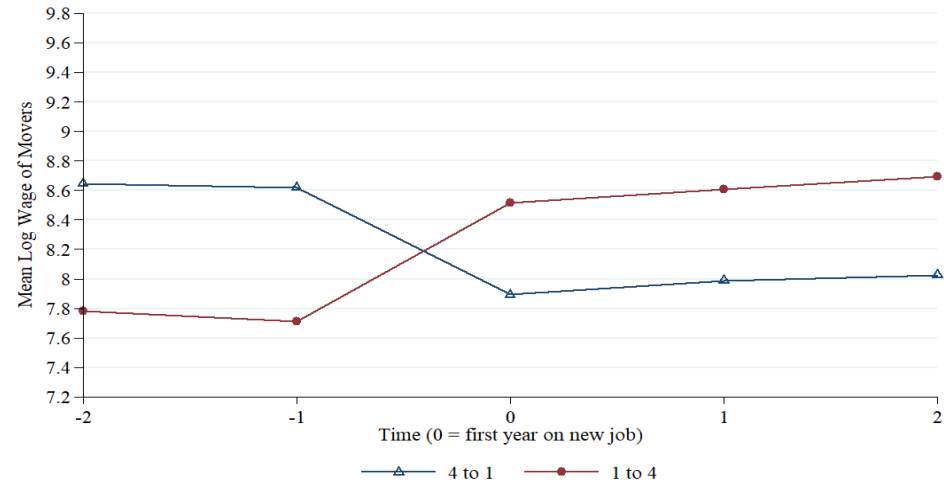


Figure 2. Mean Wages for Job Switchers by Switch Type

Figure shows mean wages around job changes (normalized to occur at year 0) by type of job change. Wages are log normalized to real 2014 dollars. We separately plot mean wages for workers who 1) begin at an established firm (firm aged four or older) and move to a different established firm (marked "Established to Established"); 2) who begin at an established firm and move to a new firm (firm aged three or younger; marked "Established to Startup"); 3) who begin at a new firm and move to a different new firm (marked "Startup to Startup"); and, 4) who begin at a new firm and move to an established firm (marked "Startup to Established").

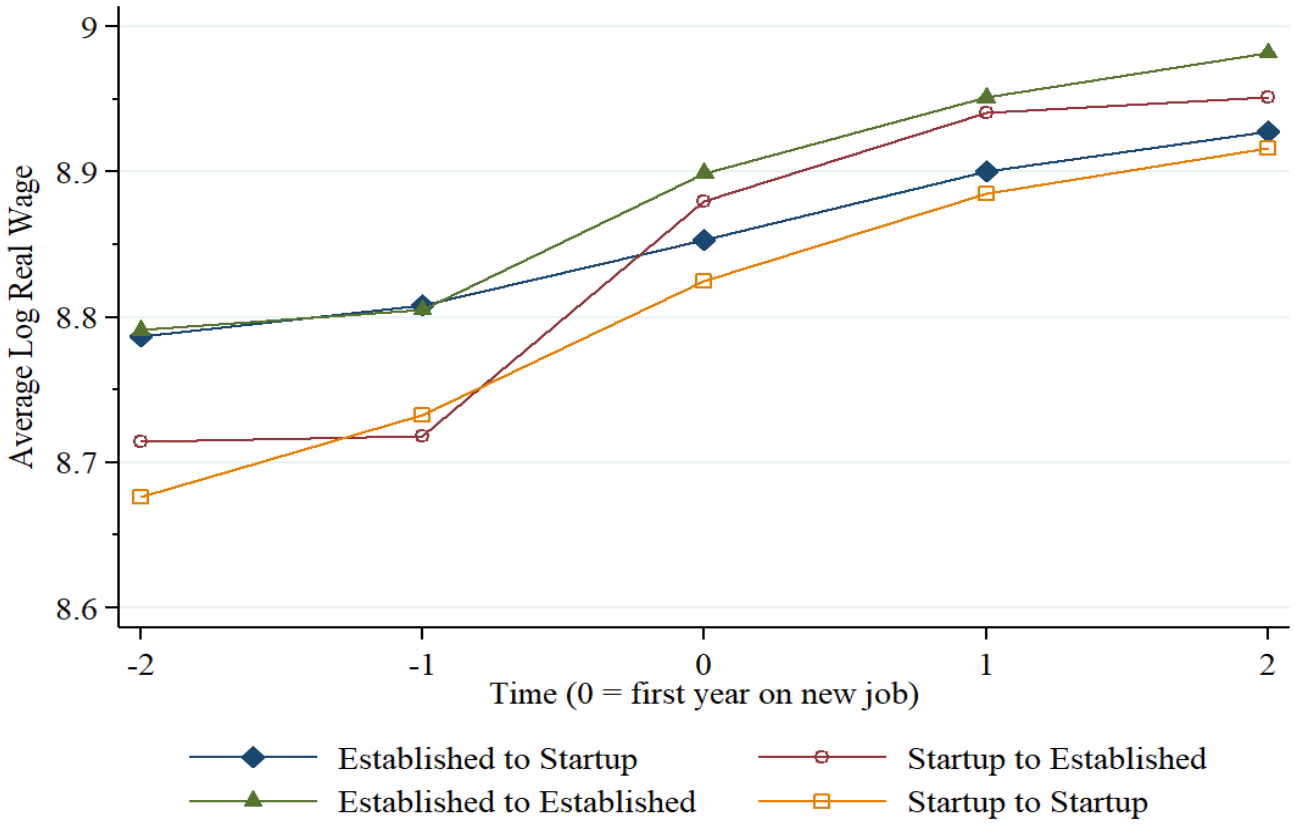


Table 1. Summary Statistics

Panel A shows mean (standard deviation) statistics at the firm-year level, and Panel B at the worker-year level. Column 1 reports statistics using the sample of all firms. Column 2 (3) reports statistics for established firms (new firms). Established firm is a firm aged four or older; new firm is aged three years or less. In Panel A, workforce statistics are calculated at a unique firm-year level in a following way: first, for a given variable the average is calculated for each firm-year across all workers employed by that firm-year; second, reported means and standard deviations are calculated across firm-years.

Panel A. Firm-year level variables

	(1) All Firms	(2) Established Firms	(3) New Firms
Firm Employment	17.7	18.9	2.1
	(278)	(288)	(11.2)
Percent Male Employees	0.52	0.52	0.50
	(0.43)	(0.43)	(0.47)
Percent College Educated Employees	0.27	0.27	0.26
	(0.37)	(0.37)	(0.40)
Number of Observations (thousands)	640	592	47

Panel B. Worker-year level variables

	(1) All Firms	(2) Established Firms	(3) New Firms
Quartly Earnings (2014$)	11,320	11,340	8,862
	(26,750)	(26,830)	(15,000)
Tenure (years)	8.7	8.7	3.9
	(4.8)	(4.8)	(2.5)
Age	42.1	42.1	38.1
	(12.1)	(12.1)	(12.6)
Male	0.56	0.56	0.53
	(0.50)	(0.50)	(0.50)
Education (years)	13.6	13.6	13.5
	(2.40)	(2.40)	(2.43)
Number of Observations (thousands)	11,300	11,200	99

Table 2. Summary Statistics for Workers Who Change and Do Not Change Employers

Table shows summary statistics for workers who never change employers in the sample (Column 1) and change employers (Column 2). Statistics are means and standard deviations (in parenthesis). Each statistic is calculated at a unique worker level in a following way: first, for each variable the average is calculated for each worker across all worker-years; second, means and standard deviations reported in this table are calculated across all workers.

	(1) Do Not Move	(2) Move
Quarterly Earnings (2014$)	10,550	9,777
	(18,955)	(14,606)
Tenure (years)	7.18	5.70
	(4.79)	(2.65)
Age	41.8	39.6
	(13.30)	(12.10)
Male	0.55	0.54
	(0.50)	(0.50)
Education (years)	13.4	13.5
	(2.4)	(2.4)
Number of Observations (thousands)	861	467

Table 3. New Firm Wages for All Workers

Table reports baseline results of wages at new firms. The sample is a worker-year panel from 1990 through 2008 of workers who are ever observed at a public firm during this period. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Worker age is log transformed. Education is measured in years of schooling and log transformed. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
New Firm	-0.257***	-0.0851***	-0.0241***	0.0030
	(0.047)	(0.005)	(0.002)	(0.002)
Worker Age ^2				2.534***
				(0.072)
Worker Age ^3				-0.417***
				(0.013)
Worker Age * Education				7.545***
				(0.147)
Worker Age ^2 * Education				-2.558***
				(0.050)
Worker Age ^3 * Education				0.272***
				(0.006)
Observations (millions)	11.3	11.3	11.3	11.3
R-squared	0.01	0.84	0.86	0.87
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 4. New Firm Wages for College Educated Workers

Table shows results from regressions of worker wages on new firm indicator variable for college educated workers from our baseline sample. The baseline sample is a worker-year panel from 1990 through 2008 of workers who are ever observed at a public firm during this period. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
New Firm	-0.195 ***	-0.0749 ***	-0.0278 ***	-0.0126 ***
	(0.040)	(0.010)	(0.004)	(0.004)
Worker Age ^2				3.387 ***
				(0.400)
Worker Age ^3				-0.548 ***
				(0.070)
Worker Age * Education				3.272 ***
				(0.310)
Worker Age ^2 * Education				-1.731 ***
				(0.160)
Worker Age ^3 * Education				0.225 ***
				(0.030)
Observations (millions)	3.6	3.6	3.6	3.6
R-squared	0.01	0.84	0.86	0.86
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 5. New Firm Wages for College Educated Workers at Technology Firms

Table shows results from regressions of worker wages on new firm indicator variable for college educated workers in technology sector from our baseline sample. The baseline sample is a worker-year panel from 1990 through 2008 of workers who are ever observed at a public firm during this period. We identify worker industry as the first industry observed in his employment histroy. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
New Firm	-0.199 ***	-0.0856 ***	-0.0516 ***	-0.0409 ***
	(0.050)	(0.010)	(0.010)	(0.010)
Worker Age ^2				3.493 ***
				(0.890)
Worker Age ^3				-0.557 ***
				(0.160)
Worker Age * Education				-0.95
				(1.090)
Worker Age ^2 * Education				-0.54
				(0.470)
Worker Age ^3 * Education				0.112 *
				(0.070)
Observations (millions)	1.2	1.2	1.2	1.2
R-squared	0.02	0.81	0.84	0.85
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 6. New Firm Wages for Founders and Employees

Table reports wage results for founders and employees of new firms. The sample is a worker-year panel from 1990 through 2008 of workers who are ever observed at a public firm during this period. In all columns, the dependent variable is the log of worker total quarterly wages.Wages are in real 2014 dollars. Founder is identified as the employee who is at the new firm in its first year of existence. New firm is defined as a firm of three years of age or less. P-value is from *t*-test of difference between coefficient on founder at new firm and non-founder at new firm. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
New Firm Founder	-0.254***	-0.0863***	-0.0140***	0.0114***
	(0.048)	(0.006)	(0.003)	(0.003)
New Firm Employee	-0.278***	-0.0779***	-0.0621***	-0.0288***
	(0.073)	(0.010)	(0.006)	(0.005)
Worker Age ^2				2.534***
				(0.070)
Worker Age ^3				-0.417***
				(0.010)
Worker Age * Education				7.544***
				(0.190)
Worker Age ^2 * Education				-2.558***
				(0.050)
Worker Age ^3 * Education				0.272***
				(0.010)
Observations (millions)	11.3	11.3	11.3	11.3
R-squared	0.01	0.84	0.86	0.87
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
P-value from *t*-test	0.70	0.41	0.00	0.00

Table 7. New Firm Wages After Controlling for Firm Size

Table reports baseline results of wages at new firms after controlling for firm size. Columns 1-4 use the baseline sample of workers which consists of a worker-year panel from 1990 through 2008 of workers who are ever observed at a public firm during this period. Column 5 uses the sub-sample of workers from the baseline sample who are college educated. Column 6 uses the sub-sample of workers from the baseline sample who are college educated and are in tech sector. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. New firm is defined as a firm of three years of age or less. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Firm employment is log transformed. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	All				College	College & Tech
	(1)	(2)	(3)	(4)	(5)	(6)
New Firm	-0.0302	-0.0152 ***	-0.0126 ***	0.0124 ***	0.0018	-0.0214 ***
	(0.020)	(0.004)	(0.003)	(0.002)	(0.004)	(0.010)
Firm Employment	-0.127	0.152 ***	0.122 ***	0.104 ***	0.0403 ***	-0.292 ***
	(0.180)	(0.027)	(0.006)	(0.005)	(0.010)	(0.020)
Firm Employment ^2	0.0363	-0.0125 ***	-0.0077 ***	-0.0069 ***	0.0063 ***	0.0598 ***
	(0.024)	(0.004)	(0.001)	(0.001)	0.000	0.000
Firm Employment ^3	-0.00198 **	0.0003 *	0.0003 ***	0.0002 ***	-0.0005 ***	-0.00288 ***
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Worker Age ^2				2.541 ***	3.336 ***	3.626 ***
				(0.060)	(0.480)	(0.960)
Worker Age ^3				-0.418 ***	-0.538 ***	-0.587 ***
				(0.011)	(0.090)	(0.170)
Worker Age * Education				7.508 ***	3.255 ***	-0.88
				(0.159)	(0.390)	(0.710)
Worker Age ^2 * Education				-2.556 ***	-1.711 ***	-0.62
				(0.049)	(0.230)	(0.430)
Worker Age ^3 * Education				0.272 ***	0.221 ***	0.128 *
				(0.006)	(0.040)	(0.070)
Observations (millions)	11.3	11.3	11.3	11.3	3.6	1.2
R-squared	0.03	0.84	0.86	0.87	0.86	0.85
Worker FE	No	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8. Robustness

Table reports results of robustness tests. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. Columns 1-2 show results for workers who move to new firms from closed plants (Move to New Firm from Closed Plant) and from open plants (Move to New Firm from Open Plant). Plant is defined as closed when the employment is zero. New firm is defined as a firm of three years of age or less. Columns 3-4 show results for workers who are at new firms during high state-level unemployment (New Firm and High Unemployment) and low unemployment (New Firm and Low Unemployment). High Unemployment is one for firms in states with unemployment rate above the national unemployment rate-year. Columns 5-6 show results when we add workers with long periods of non-employment to our base sample. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Standard errors are clustered at the firm and the worker level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	Forced vs. Voluntary Moves		Moves During Low vs. High Unemployment		Include Workers with Non-employment Spells	
	(1)	(2)	(3)	(4)	(5)	(6)
Move to New Firm from Closed Plant	0.011**	0.015***				
	(0.005)	(0.005)				
Move to New Firm from Open Plant	-0.001	0.010				
	(0.004)	(0.005)				
New Firm and High Unemployment			-0.024***	0.003		
			(0.003)	(0.003)		
New Firm and Low Unemployment			-0.024***	0.003		
			(0.003)	(0.003)		
New Firm					-0.039***	-0.008*
					(0.004)	(0.005)
Worker Age ^2		2.534***		2.534***		2.604***
		(0.080)		(0.072)		(0.060)
Worker Age ^3		-0.417***		-0.417***		-0.427***
		(0.010)		(0.013)		(0.011)
Worker Age * Education		7.544***		7.545***		7.700***
		(0.150)		(0.147)		(0.178)
Worker Age ^2 * Education		-2.558***		-2.558***		-2.603***
		(0.050)		(0.050)		(0.054)
Worker Age ^3 * Education		0.272***		0.272***		0.277***
		(0.010)		(0.006)		(0.006)
Observations (millions)	11.3	11.3	11.3	11.3	13.6	13.6
R-squared	0.86	0.87	0.86	0.87	0.85	0.86
Worker FE	No	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
P-value from *t* -test	0.07	0.24	0.93	0.78	NA	NA

Table 9. New Firm Outcomes as a Function of Worker and Firm Fixed Effects from Wage Regressions

Table shows cross-sectional OLS results from predicting new firm exit (Colmns 1-4) and employment growth (Colmns 5-8) as a funciton of worker and firm fixed effects estimated from the wage regression in column 4 of table 3. In Columns 1-4, dependent variable, New Firm Exits in 5 Years, equals one for new firms that exit by year five since founding. In Columns 5-8, dependent variable, New Firm 5-year Employment Growth, is the log-difference between a new firm's employment at age five and its employment in the first year of existance (calculated for firms that survive for five years). Worker Fixed Effects is the mean worker fixed effects of workers at the new firm in its first year of existance. Note, to avoid loosing observations due to negative values in fixed effects, both worker and firm fixed effects are transformed as the log of the fixed effect minus the minimum value of the fixed effect plus 1. Estimates for control variables (Log New Firm Employment in First Year, Log Mean Worker Education in First Year, and Log Mean Worker Age in First Year) are not reported due to the US Census disclosure limits on the number of estimates that can be cleared. State FE and Industry FE refer to the industry of the new firm. Standard errors are clustered at the firm level, and reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	New Firm Exits in 5 Years				New Firm 5-year Employment Growth			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Worker Fixed Effects	-0.249***	-0.204***			0.465***	0.279***		
	(0.020)	(0.030)			(0.080)	(0.090)		
Firm Fixed Effects			-0.204***	-0.167***			0.375***	0.222***
			(0.020)	(0.020)			(0.070)	(0.070)
Log New Firm Employment in First Year	Included	Included	Included	Included	Included	Included	Included	Included
Log Mean Worker Education in First Year		Included		Included		Included		Included
Log Mean Worker Age in First Year		Included		Included		Included		Included
Observations (thousands)	22	22	22	22	8.3	8.3	8.3	8.3
R-squared	0.49	0.51	0.49	0.51	0.09	0.13	0.1	0.13
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Industry FE	No	Yes	No	Yes	No	Yes	No	Yes

Appendix.

Figure 1. Map of the US states available in the LEHD

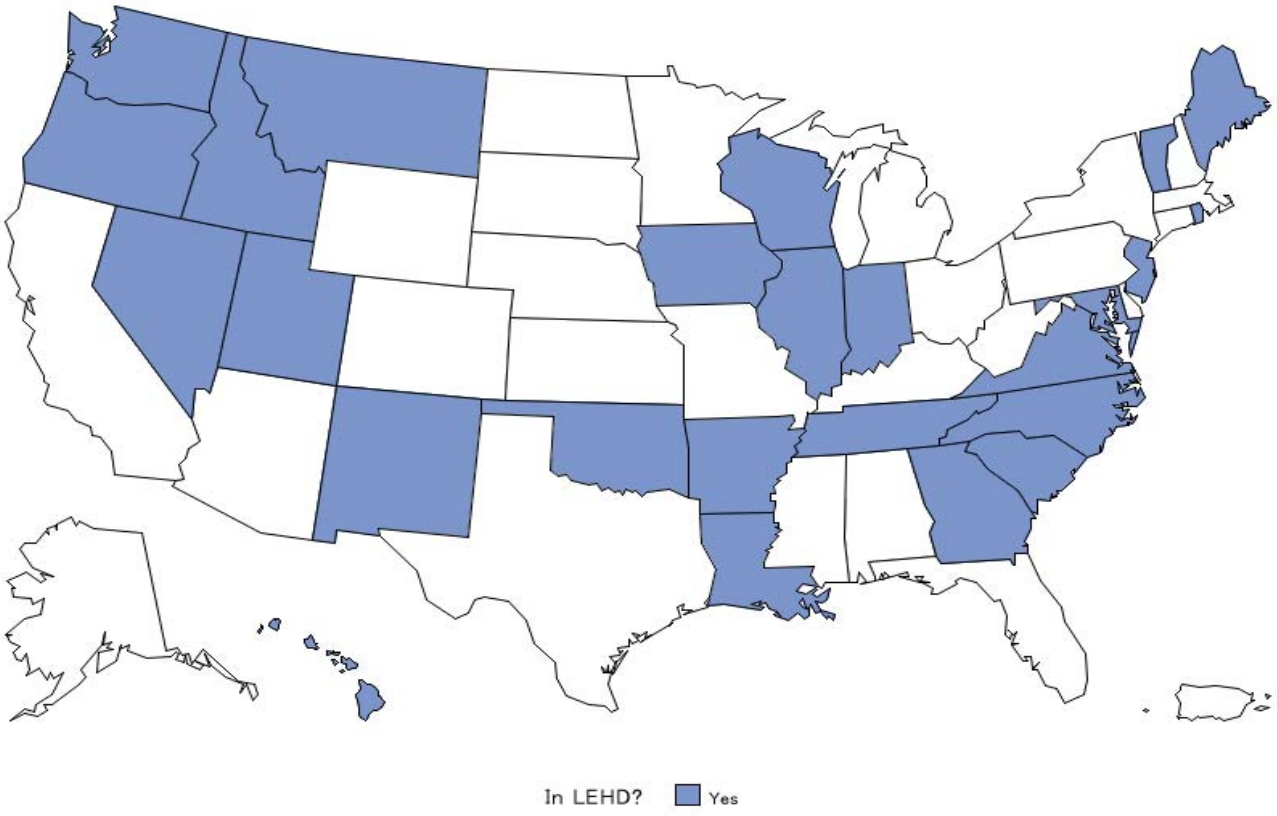
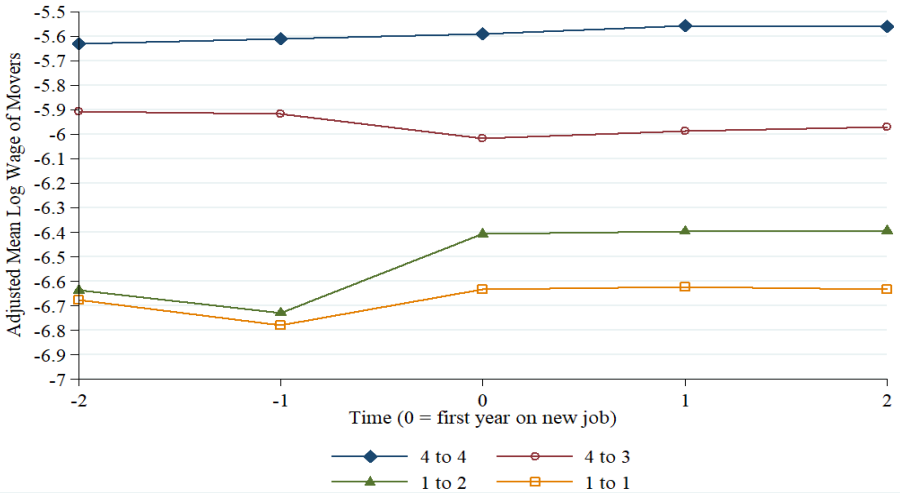
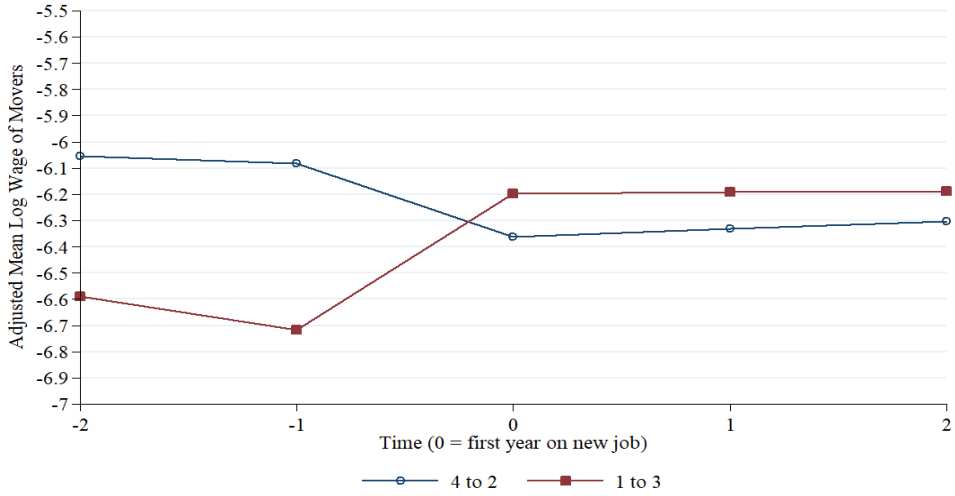


Figure 2. Mean Adjusted Wages of Job Changers Classified by Quartile of Mean Wages of Coworkers at Origin and Destination Firm

Figure shows mean adjusted wages of workers who change jobs (i.e., employers) in the respective interval, and held the preceding job for two or more years, and the new job for two or more years. Job refers to firm with most wages in the quarter. Each job is classified into quartiles based on mean wage of coworkers. Wages are adjusted by employee age squared and cubed and employee age*education, employee age squared*education and employee age cubed*education. Age, education and wages are log normalized.





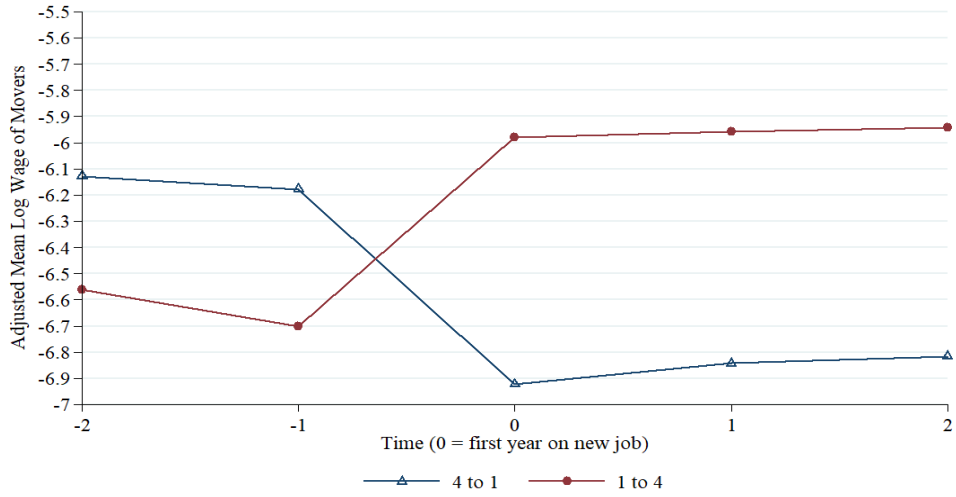


Figure 3. Mean Adjusted Wages for Job Switchers by Switch Type

Figure shows mean adjusted wages around job changes (normalized to occur at year 0) by type of job change. Wages are adjusted by employee age squared and cubed and employee age*education, employee age squared*education and employee age cubed*education. Age, education and wages are log normalized. We separately plot mean wages for workers who 1) begin at an established firm (firm aged four or older) and move to a different established firm (marked "Established to Established"); 2) who begin at an established firm and move to a new firm (firm aged three or younger; marked "Established to Startup"); 3) who begin at a new firm and move to a different new firm (marked "Startup to Startup"); and, 4) who begin at a new firm and move to an established firm (marked "Startup to Established").