Entrepreneurial Wages

Tania Babina, Wenting Ma, Paige Ouimet, and Rebecca Zarutskie

November 2018

Abstract

Why do young firms pay less? Previous studies have argued that employees willingly accept lower wages at new firms in response to offsetting benefits. A second literature argues that lower wages at new firms are driven by selection of lower quality workers into new firms, firms which are likely to be of lower productivity or financially constrained. Using US Census employer-employee matched data, we show new evidence consistent with the selection argument. After including worker fixed effects, nearly three quarters of the new firm wage difference disappears. 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 working at the new firm is, on average, economically insignificant. Moreover, young firms that can hire high quality workers have higher future survival rates and total employment, suggesting that human capital is an important predictor of young firm success.

Tania Babina, Columbia University, Tania.Babina@gsb.columbia.edu. Wenting Ma, University of North Carolina at Chapel Hill, wma7@live.unc.edu. Paige Ouimet, University of North Carolina at Chapel Hill, Paige_Ouimet@unc.edu. Rebecca Zarutskie, Federal Reserve Board, Rebecca.e.Zarutskie@frb.gov. We thank Michael Ewens, Neville Francis, Ross Levine, Song Ma, Chris Moser, Olav Sorenson, Christopher Stanton, Boris Vallee as well as participants at the 28th Mitsui Finance Symposium, Columbia Junior Entrepreneurship and Innovation Workshop, UNC Kenan Institute Frontiers of Entrepreneurship, Dartmouth, and HEC for helpful suggestions. The views and conclusions are those of the authors and do not necessarily indicate concurrence by the members of the Federal Reserve System. We thank Chris Galvan for his 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. Research results and conclusions 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, supported by the 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.

Young firms account for 11% of US employment and are credited with a disproportionate share of total job creation (Haltiwanger, Jarmin, and Miranda, 2013). Given the importance of young firms in generating jobs, an extensive literature has explored the drivers of these new firms. However, the question of why workers join new firms remains controversial. On average, employees 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). One literature has interpreted this fact as evidence of a willingness of employees at new firms to accept below-market wages due to offsetting attributes from working at new firms. A second, mostly theoretical, literature has instead argued that lower wages at new firms reflect selection: Young firms employ disproportionately more lower quality workers, either because new firms are lower quality or financially constrained. In this paper, we revisit this debate to separate between the wage penalty and selection interpretations of lower wages at new firms.

Using US Census employer-employee matched data over almost two decades, we confirm that new firms, defined as three years of age or younger, pay 31% lower wages, on average. 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 differences in worker quality and time invariant firm quality. Our findings suggest that a given worker considering joining either a new or established firm of equivalent quality would receive equivalent wages at both employment opportunities, supporting the selection interpretation of the new firm wage discount.

To reach these conclusions, we start by including worker fixed effects. Previous studies do not usually include worker fixed effects either because of the cross-sectional nature of the data (Brown and Medoff, 2003) or due to a different focus (Burton, Dahl and Sorensen, 2017). With these controls for time invariant differences in worker quality, we find the new firm wage differential declines by almost three fourths. Adding controls for time varying observable worker characteristics further reduces the magnitude of the new firm wage discount. These results indicate that new firms, on average, employ workers who receive lower market wages due to differences in skills or talent. A disproportionate matching of low-skill workers to new firms is consistent with positive assortative matching. New firms in our data include a large representation of low quality firms, which are unlikely to succeed over the long run, as well as higher quality new firms with greater survival potential. Alternatively, new firms may not have the financial resources necessary to employ high-wage workers.

Moreover, once we add firm fixed effects, absorbing time invariant firm quality, the wage difference between new and mature firms becomes positive, although economically small. In our setting, firm fixed effects reflect any time-invariant wage premium or discount paid to all employees of a given firm above and beyond the person-specific component of pay, captured by the worker fixed effects. Abowd, Kramarz and Margolis (1999) shows a positive correlation between this firm-specific component of pay and firm-level productivity. Equivalent workers will, on average, have relatively higher individual output in more productive firms. Our finding of a reduction in the new firm wage discount with firm fixed effects support this argument assuming new firms have lower initial quality, as compared to the set of established firms which have successfully survived to maturity.

After controlling for differences in employee and firm quality, the expected wage penalty of working at a new firm is, on average, economically insignificant. Earlier conclusions that new firms pay lower wages still hold. However, this fact is explained by the types of workers new firms employ and by the variety of firm quality represented by new firms. Assumptions regarding preferences or biased beliefs are not required to understand why workers join new firms. Instead, the difference in wages is explained by the lower mean quality of new firms or a higher likelihood of financial constraints which limit the ability to hire high skill workers.

We find similar results if we instead use a sample of only college educated workers or a set of college educated workers employed in the technology sectors. Documenting equivalent results in these samples indicates that new firms hire relatively more lower-skill workers, as compared to established firms, even within sets of high-skill workers.

To further support our argument that employee quality reflects firm quality, we do two additional tests. First, we show that firms that survive for at least 10 (5) years (firms that are likely born more productive) have higher quality initial workforces, as compared to firms which exit prior to year 10 (5). Second, we document that firms which employ higher quality workers at birth have higher 5-year total employment after their creation. These results show that the human capital of young firms is an economically important predictor of the new firms' performance. Hence, a new firm's ability to hire a high quality team matters: If firms are financially constrained or otherwise unable to secure talent, they are less likely to survive and grow.

Firm size has also been used as a proxy for firm quality, and given that young firms also tend to be small, it is important to document that our effect is distinct from the firm-size wage premium documented in Oi and Idson (1999). As expected, controlling for firm size reduces the new firm wage differential, even in the absence of worker and firm fixed effects, as size proxies for firm quality. However, we continue to observe a significant coefficient on firm age, indicating firm age is a distinct firm characteristic from firm size. Moreover, as in the baseline results, we observe a decline in the magnitude of the new firm wage differential with the addition of worker fixed effects, working time varying controls and firm fixed effects. In addition, with both sets of fixed effects and controls for firm size, we now document a significant new firm wage premium of nearly 2%. These results show that firm age is unique and not fully captured by firm size.

One important caveat to our analysis is that we do not observe exogenous movement between firms. While this is a common feature in papers that include worker fixed effects, it 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 economically similar results when estimated using only exogenous job switchers, workers who had to change jobs following establishment closure.

Our paper is the first to use a large sample of employee-employer matched data for US firms over nearly two decades to examine the underlying drivers of new firm wages. A handful of prior studies have also examined the new firm wage penalty, primarily using 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. The closest paper to ours is Burton, Dahl and Sorensen (2017), which uses Danish data and differs from our work in other 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. Our main contribution to this literature is two-fold. First, by showing the impact of controlling for time invariant worker characteristics, time varying worker characteristics and time invariant firm characteristics - separately, we provide strong evidence that selection mechanisms explain the difference in mean wages at new and established firms. Second, we provide new evidence using a large sample of US employer-employee matched data.

We reach our results using the AKM method, an approach developed by Abowd, Kramarz, and Margolis (1999) and used in Card, Heining and Kline (2013). This approach uses workers who change jobs to simultaneously isolate employer and employee fixed effects. We contribute by focusing on employees at young firms and show that young firm's ability to attract high quality workers is an important predictor of the future firm performance. Understanding wages and potential employment frictions present at young firms is important. If young firms cannot hire desirable workers, they cannot grow.

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). 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 providing a better estimate of the wage consequences of joining a new firm, as compared to an established firm, after controlling for a given worker's opportunity set.

1 Why New Firms Pay Lower Wages?

In this section, we briefly describe the key arguments pertaining to new firms and wages which have previously been made in the existing literature. We group these arguments into two broad categories. The first group argues that workers voluntarily accept below market wages at new firms due to offsetting benefits associated with employment at a new firm. In effect, these papers argue the new firm wage discount is driven by a supply of workers willing to accept a wage penalty to be employed at a new firm. The second group argues that the wage differential is driven by selection. In effect, these papers argue the new firm wage discount is driven by selection of low-skill, low-wage workers into new firms.

A number of papers have argued that employees at entrepreneurial firms accept lower wages due to the presence of offsetting attributes from working at new firms. Evans and Leighton (1989) find greater autonomy to be a benefit to entrepreneurial work, and Blanchflower and Oswald (1992) find higher self-reported satisfaction among these workers. Hamilton (2000) suggests non-pecuniary benefits explain the wage difference among self-employed. Although most of these early papers focus on the self-employed, the same arguments can be applied among all workers at new firms. Alternatively, employees may join new firms due to greater tolerance of risk, as in Roach and Sauermann, (2015), preferences for skewness (Kraus and Litzenberger, 1976), or due to overconfidence in the expected benefits, as in Bernardo and Welch, (2001). Finally, workers may accept lower earnings at young firms because they are willing to learn about their own abilities in entrepreneurship (Manso, 2016; Dillon and Stanton, 2018).

The second strand of literature focuses on sorting and argues that the wage differential between young and established firms is in fact a proxy for worker ability or differences in quality across firms. Young firms are born with a given time-invariant draw of productivity, driven by differences in initial ideas, technology or resources. Failure rates among young firms are high. Better firms survive, as in Baker and Kennedy (2002) and, older firms (i.e., surviving firms) are more productive as in Pakes and Erickson (1998), Hopenhayn (1992), and Oi and Idson (1999a, 1999b). Assortative matching on firm and worker productivity would then suggest that low productivity, and hence low wage workers, would disproportionately match to young firms.

Young firms are also more likely to face financial constraints as in Evans and Jovanovic (1989), Petersen and Rajan (1994), and Hadlock and Pierce (2010). These financial constraints can be driven by higher opacity at young firms, making access to external finance more costly (Berger and Udell, 1998). As such, young firms may not be able to afford high-wage workers and disproportionately employ low-skill, low-wage workers. Moreover, low productivity firms are relatively more likely to be financially constrained, further reinforcing the correlation between low wages and low productivity young firms (Evans and Jovanovic, 1989).

2 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 employer-employee matched 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.[1] The data start in 1990 for several states and coverage of states increases over time. The data coverage ends in 2008. While our project has access to 31 states, we observe nearly 100% of private employment for these states This comprehensive data coverage means that we cannot include all of the available states in our estimation sample due to computational constraints: The 31 states cover over 60% of the US private sector employment, which translates into billions of observations over the data sample period – an infeasible sample for a regression analysis.[2] As we explain later, the estimation strategy requires the inclusion of firms that are connected through worker mobility across firms. For that reason, instead of randomly selecting workers across 31 states, for our main analysis, we chose a few states with high, average, and low population of young firms. A random sample generates selection

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

[2] The map of the 31 states available in the data are shown in Appendix, Figure 1. We are currently seeking disclosure permission to provide more details about the exact states used in the analysis of this paper.

towards large firms, as explained and shown in Woodcock (2005).[3] Selecting all workers within a state ensures that almost all observations within a state are included. Including small firms is crucial for our analysis since most firms are born small. For each individual we observe total quarterly wages at the current place of employment. Although the LEHD does not contain equity ownership, 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, but are counted at the time of the exercise. Because our data does not have information on equity ownership, we do not separate between founders and non-founders. Both are included in our data, although most employees are non-founders: On average, a new firm has 15 employees in our sample, and an average firm has two founders (Parker, 2009).[4] The LEHD also allows us to observe the age, gender, race, place of birth, and education of each employee.[5]

To construct our baseline sample, we start with all workers ever observed in the state. For these workers, we retrieve their entire work history and wages in the LEHD from 1990 through 2006. We stop in 2006 to have enough time passed to obtain future performance outcomes for new firms. 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

[3] In untabulated results, we find qualitatively similar results if we draw a random 10% sample of all workers or if we use a subsample of employees across all states who were ever employed at US public firms.

[4] One approach used in the existing literature, such as Azoulay, Jones, Kim, and Miranda (2018), is to identify founders as the highest wage earner at the time of founding. They show that in 60 to 70 percent of cases the top three earners capture the founders. However, sorting on wages at the new firm is problematic in our empirical setting, given our dependent variable is wages.

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

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 the LEHD data, we observe wages over a full quarter with no information on weeks worked. We follow the literature and drop observations for workers with incomplete quarters of employment, defined as employee-firm quarters where we do not observe both 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 downwards bias in wages around a job change. The implications of such a step is that we under-sample workers with especially high turnover rates. Furthermore, to minimize the computing requirements of a large sample size, we retain only the first quarterly wage estimate for each employee.

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 and described in Jarmin and Miranda (2002). 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 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. 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 or moves to work for another firm.

3 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:

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

where y_{it} are log quarterly real wages of individual i in year t and $\propto_i$ 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 and X'_{it} is a vector of time-varying observable individual characteristics. $newfirm_{Jt}$ is an indicator variable which assumes the value of one if in year t the worker is employed in a firm *J* that is three years of age or younger in that year. 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. 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 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 worker 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).

For this model to be estimated, the analysis must be run on a connected set, a subset of the full data. To be in the connected set, a firm must be linked to at least one other firm in the connected set by worker mobility. We use the largest connected set available. Consistent with other studies that use a universe of all workers within a state (Woodcock 2005), our connected set contains nearly all observations and appears otherwise similar to the full set of firms.

4 Summary Statistics

Our baseline sample is a panel of 48.4 million worker-year observations over 1990-2006, which includes 7.1 million unique workers and 345 thousands unique firms. All observation counts and estimates are rounded according to the US Census disclosure policies. To motivate our analysis, in Figure 1, Panel A, we plot average wages of employees in our sample by firm age for each two-year firm age cohort from firm birth to firms 18-19 years old and for firms 20 years or older. As in Brown and Medoff (2003), employees at young firms receive lower wages as compared to employees at older firms. Specifically, employees at firms aged 0-1 receive quarterly wages which are, on average, almost $2,500 lower as compared to employees at firms with age 20 years or older. This is a wage difference of 29%, as compared to the sample mean. In Panel B, we plot a one-year wage growth of employees by employer age. Wage growth is measured for all employees at the firm, including new joiners and employees who were employed at the same firm in the last period. In contrast to Panel A, we observe no clear pattern in wage growth across firm age. The fact that employees at firms aged 0-1 (a group disproportionately composed of employees who recently switched to a young firm) do not realize wage declines is inconsistent with theories of workers at new firms accepting a wage discount due to their offsetting attributes. Instead, this group experiences an average wage growth of 5.5%, a year on year wage growth rate that is above the sample average of 4.6%.

In Table 1, we report summary statistics for firms (in Panel A) and workers (in Panel B) in our sample. In Panel A, column 1, we report mean values and standard deviations, in parentheses, calculated across all firm-year observations in our sample. In column 2, we report statistics for established firms, defined as firms four years of age or greater. In column 3, we report statistics 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 15 employees, as compared to nearly 210 employees at established firms.[6] However, in terms of percent of male employees and percent of college educated workers, both samples are similar economically. In Panel B, column 1, we report summary statistics calculated across all worker-year observations in our sample. Column 2 contains all employees at established firms and column 3 samples all employees at new firms. As in Figure 1, wages at young firms are lower and wage growth is similar, as compared to established firms. As expected, employees at older firms have longer tenures, but economically similar representations of males and college educated workers.[7]

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

---

[6] The median new (established) firm has an employment of 6 (13). Due to the US Census confidentiality rules, the medians are calculated as an average of observations within an interquartile range.

[7] In Panel B, tenure is measured as maximum numbers of years the worker is employed by the current firm. We report that the average tenure of workers at new firms is 3.2 years. This is longer than the reported average firm age at new firms in Panel A of 1.8 years. The difference reflects the fact that some employees stay with a young firm after the employer matures and becomes an established firms.

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, earn lower wages and have higher wage growth. These results are consistent with a finding that younger and shorter tenure workers switch jobs more frequently as in Topel and Ward (1992). The table also reveals that jobs switchers are more common that non-switchers and account for almost three quarters of all observations, further mitigating any representatives concerns of the switchers.

5 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 with previous work, we first estimate the new firm wage penalty using a simple OLS with year fixed effects. We then add individual fixed effects to control for time invariant worker quality. We next add controls for employee characteristics to control for time-varying observable employee characteristics. Finally, we add firm fixed effects to control for time invariant firm quality. We discuss the interpretation of each regression next.

5.1 OLS Estimation

As reported in column 1, new firms pay wages that are 31% lower as compared to established firms, after controlling for year fixed effects. This is consistent with results in Brown and Medoff (2003) and Ouimet and Zarutskie (2014). This wage gap may be due to employees accepting a wage penalty in return for compensating differentials or due to differences in the types of employees hired at new firms.

5.2 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 almost three fourths. In this specification, a worker who switches between an established and new firms will earn, on average, an 8.7% lower wage at the new firms. The difference in the magnitudes of the coefficient on new firm between columns 1 and 2 tells us that young firms employ, on average, workers who earn less – workers who presumably have lower time-invariant skill. 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.

Young firms may disproportionately hire low-wage workers because less productive workers match to new firms or because they are financially constrained and cannot afford to pay the high wages necessary to attract high skill workers. The set of new firms in our sample includes a mix of both low quality new firms – that are unlikely to survive beyond four years – as well as high quality young firms with strong growth potential. Under an assumption of positive assortative matching, lower quality employees will match to lower quality firms and receive lower wages. Financial constraints, common at new firms, will likely reinforce this relationship as lower quality firms will have more difficulty raising capital.

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 similar summary statistics reported for job switchers and non-job switchers in Table 2.

In column 3, we add controls for observable time-varying employee characteristics associated with wages. We control for age, 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 the employee education level to allow for the fact that more educated workers can have different wage patterns across time (Card, Heining and Kline, 2013). Given 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 (Ouimet and Zarutskie, 2014). Indeed, after controlling for time-invariant worker characteristics, the coefficient on the new firm wage penalty is further reduced in magnitude to -0.077, consistent with new firms hiring workers at points in time in their career where they would command lower wages.

5.3 Individual and Firm Fixed Effects (AKM)

In Table 3, column 4, we add firm fixed effects, thereby estimating an AKM regression. The firm fixed effects capture the firm-specific and time-invariant component of compensation above and beyond the person-specific component of pay, as captured by the worker fixed effects. The coefficient on new firm is now positive and statistically significant, equals to 0.7%, and economically small. The time-invariant and firm-specific component of compensation captured by the firm fixed effects is correlated with firm productivity (Abowd, Kramarz and Margolis, 1999). As such, the change in the coefficient on new firm with the addition of firm fixed effects is consistent with new firms, on average, being of lower quality. This result suggests that for a given worker who has job opportunities from a similar quality new and established firm, the expected wage differences of going to work at the new firm are, on average, economically insignificant.

The worker fixed effects from this specification are an estimate of time invariant worker quality after controlling for time-varying worker characteristics and time-invariant firm differences. We plot the mean worker fixed effect (as estimated in this specification) by firm age in Figure 2, Panel A. By construction, the average worker fixed effect across the whole sample is zero. We observe that the mean worker fixed effect is negative for firms aged 0-1, increases with firm age and turns positive for firms aged 12-13, peaks for firms aged 16-17, then goes down, becoming slightly negative for firms aged 20 years and older. These novel statistics are consistent with the argument that young firms employ lower quality workers. These statistics also are inconsistent with another set of theories that explain lower wages in new firms due to learning and skill accumulation. They suggest that workers might accept lower wages at new firms because they learn skills that allow them to experience faster wage growth down the road.

Adding firm fixed effects changes the sample used to estimate the coefficient on new firm in a manner similar to adding person 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, we estimate the same regression but define new firms as ages zero to one. The results are reported in Appendix Table 1. We find qualitatively similar results.

Our estimates are based on a sample of employees who endogenously match to firms. While the presence of this type of endogenous mobility does not invalidate AKM assumptions, it does impact the interpretation of our findings.[8] Our sample of employees who move to new firms is likely biased towards employees who specifically anticipate relatively higher productivity at these new firms. While it is true that employees who match to established firms are likely to exhibit a similar bias, this is unlikely to fully offset the effect at new firms. Wage gains associated with this type of endogenous mobility should be impounded into any new hire wage bump. Given new hires are relatively more common at new firms, this could impact the estimate of the new firm coefficient. However, the effect, if any, appears to be modest. First, as discussed earlier, in Appendix Table 1, we define new firms as firms aged zero to one. In this specification, new hires should be an even

[8] Section 9 discusses in more detail the type of endogenous mobility which can invalidate AKM assumptions.

larger part of total firm employment, yet, we observe no meaningful change in the estimate of the new firm coefficient.

Second, we find no difference in the new firm wage estimate when controlling for the one form of endogenous mobility we can directly observe in the data, the decision to leave the current employer. Workers who leave an established firm voluntarily to join a new firm presumably anticipate especially higher productivity gains, as revealed by the preference for employment at the new firm compared to the set of other new employment opportunities and the existing job. On the other hand, workers who are required to leave their existing position at an established firm due to a firm closure and then select to join a new firm are choosing from a smaller set of options. If employees facing a relatively more constrained choice set experience significantly lower new firm wage differentials, then this would suggest that endogenous mobility has important implications for our results. To test this prediction, we separately estimate the new firm wage differential for two sets of employees, employees departing a continuing establishment and employees departing a closing establishment. As reported in Appendix Table 2, columns 1 and 2, workers leaving a closed establishment experience a similar new firm wage differential.

Likewise, our results are not driven by workers moving between new and young firms. In Table 4, we expand Equation 1 to include ten dummy variables capturing firms in each two-year age cohort from birth to firms 18-19 years old. Firms aged 20 or greater are the excluded set. We then repeat the same specifications as in Table 3, starting with an OLS estimation and then adding worker fixed effects, controls for time-varying characteristics and firm fixed effects. The pattern which emerges is consistent with the earlier table. In an OLS framework, young firms pay lower wages. However, as we add controls which absorb differences in worker and firm quality, the wage discount at younger firms becomes a wage premium.

In conclusion, on average new firms pay lower wages. However, the large wage difference observed when just looking at simple averages is driven by the fact that new firms hire disproportionately more workers who command lower wages 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 time invariant lower quality. These firms are likely to always pay lower wages, even if they are able to survive to maturity. Controlling for individual time invariant and observable time varying characteristics as well as firm time invariant characteristics largely explains the difference in wages between new and established firms.

6 Alternative Samples

In the previous section, we show that new firms disproportionately hire low-skill, low-wage workers when using the full sample of employees. In this section, we re-estimate the same specifications but limit the sample to sets of high-skill workers, namely college educated workers or college educated workers employed in the high tech sector. If our results hold in these two alternative samples, then we can conclude that new firms hire relatively more low quality workers, as compared to established firms, across different distributions of skill.

6.1 College Educated Employees

We start by looking at the subsample of college-educated workers, 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. In Table 5, we repeat the same empirical specifications as used in the baseline sample but applied to the sample of college educated workers. We find similar results to the baseline sample with all workers. In a univariate setting, we still observe a significantly lower wage at new firms, as reported in column 1. As in Table 3, employee fixed effects (column 2) and worker time varying characteristics (column 3) continue to be important explanatory variables of wages in new firms, even within the more homogenous set of college-educated workers. Moreover, adding firm fixed effects (column 4) lowers the new firm wage difference to be statistically zero.

6.2 High Technology Firms

In Table 6, 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, and use the first industry observed for a given worker.[9] 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.

7 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

[9] 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.

regressions. Specifically, we measure firm size as log employment and the second and third order transformations of log employment.

In column 1, after controlling for firm size and year fixed effects, we find a negative and significant coefficient on new firms. However, the coefficient on new firms is significantly smaller, as compared in the baseline estimation suggesting that the relation between firm size and wages can explain some of the baseline finding. Moreover, as in Oi and Idson (1999), firm size is a significant predictor of wages. This result is inconsistent 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, likely reflecting differences in the samples of new firms across the two countries.

After controlling for individual fixed effects in column 2, the coefficient on new firm is further reduced, but remains negative and significant. Furthermore, with the addition of time-varying controls for observable worker characteristics in column 3, the magnitude of the new firm wage differential is again further reduced.

In column 4, with the addition of firm fixed effects, we report a positive and significant coefficient on new firms. The coefficient is now an economically modest but not insignificant 1.7%. 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).

8 Tenure Wage Relationship

Another common belief is that young firms offer higher wage gains for retained employees. Such a pattern would be consistent with financial constraints, where firms essentially borrow from workers in the early years and then repay them with higher wage growth as the firm becomes less financially constrained (Guiso, Pistaferri and Shivardi, 2013). To test this, we create a set of dummy variables for employees with 0, 1, 2, 3 or 4 or more years of tenure with their given firm and interact these variables with our new firm dummy variable. We then repeat the same specifications as in the baseline and report the results in Table 8.

We show a dramatic increase in wages at established firms with greater tenure. The omitted group is workers with 0 tenure at established firms. These regressions are estimated on all workers and, as such, the sample of workers used to estimate wages with 0 tenure is different from the sample of workers used to estimate wages with 1 year of tenure. In column 1, the large difference in wages between the 0 tenure and 1 year tenure is primarily driven by a selection effect. Workers who leave within a year are, on average, less productive workers. Alternatively, by including worker fixed effects in column 2, we can now observe the impacts of tenure on wages after controlling for differences in worker quality across the different tenure groups. As predicted, the wage increases with tenure are now much more gradual at established firms. Adding time varying controls or firm fixed effects has relatively little impact on the tenure wage gradient at established firms.

In column 1, with just year fixed effects, there is a significantly less positive tenure-wage relation at new firms. This is consistent with results in Brown and Medoff (2003) who show that in a cross-sectional analysis wages grow faster with tenure at more established firms. However, this appears to be driven by the higher proportion of lower quality firms in the set of new firms. These firms may not be able to increase wages over time as they are less productive and already starting to fail. Once we include firm fixed effects, in column 4, there is now a more positive tenure-wage relation at new firms, consistent with a financial constraints mechanism.

9 Does Worker Quality Matter for New Firm Performance?

In the previous sections, we document that young firms disproportionally hire low quality workers. We argue that this selection can be attributed to a matching of less productive workers to less productive new firms or by the fact that new firms are financially constrained and less able to pay for high skill workers. The two explanations are inter-related: financial constraints will likely reinforce assortative matching as lower quality firms will have more difficulty raising capital. Because the two explanations are difficult to separate empirically, we do not try to distinguish between them, leaving this for future research. Instead, as a means to further support both selection mechanisms, we explore the relation between worker quality and new firm future performance, which should correlate with intrinsic firm quality. We explore two tests. First, we plot mean worker fixed effects using a two by two sort on firm age and whether or not the firm survives to year 10. Then, in a regression setting, we investigate whether firms with higher quality workers at birth perform better ex-post. Results from both test a mechanism where low quality workers disproportionately match to low quality firms and financially constrained firms cannot hire high skill workers and underperform (Evans and Jovanovic, 1989).

In Figure 2, Panel B, we plot mean worker fixed effects from the baseline AKM wage regression specification in Table 3, column 4. We measure the quality of a firm's workforce in a given year as the firm's mean worker fixed effect, using all workers employed by the firm in that year. In Figure 2, Panel B, we look at the variation within firm age group by whether or not the firm survived to year 10. Firms that survive to year 10 (in green) have above average worker quality at birth: Among surviving firms, firms with age 0 to 1 have worker fixed effects equal to 4.7% higher than the average worker's fixed effect (which includes workers from all firms: new and established). It also shows, that firms that exit within 9 years of birth (in red) have well below average worker quality at birth: among these firms, the firms with age 0 to 1 have worker fixed effects 9.4% lower than average. The difference between the mean fixed effect for surviving and failing firms aged zero to one is 21% of the standard deviation of the worker fixed effect across the full sample (the standard deviation is 0.66). These univariate results support our argument that young firms which disproportionately hire low-wage workers are themselves worse quality, on average. The figure also shows that the difference in worker fixed effects between surviving and failing firms shrinks as firms age: the differences goes down from 14.1% to 6.5% for firms with age 8 to 9, however, this could also be driven by the fact that the surviving to year 10 or not is less informative of firm quality at older firms.

In a second set of tests, we examine whether the ability of young firms to attract a high quality workforce at birth predicts young firm performance post-birth. In these tests, we use the initial workforce quality of a firm, or the average worker fixed effects using only workers employed by that firm in its first year of existence. Then, in OLS regressions, we predict a new firm's 5-year exit rate and 5-year employment as a function of these worker fixed effects. Five-year employment is set to zero if the firm fails before year 5.[10] In all these regressions, we also control for initial firm employment, birth year, and state and industry fixed effects. We report the results in Table 9. The workforce quality at firm birth is positively associated with the firm survival and future employment. Moreover, the relationship is economically significant. When all controls are included (columns 2 and 4), one standard deviation increase in the worker fixed effect predicts an 8% higher survival rate (from the mean survival rate of 41.5%) and a 9% higher employment. The caveat is that these estimates do not present a causal relation between the worker intrinsic quality and the new firm future performance (which should correlate with firm quality). However, they do contribute to the

[10] We find similar results if we use a logit specification to predict startup exit or if we measure startup exit in 6 or 4 years instead of 5 years. We also find similar results if we predict firm future employment, conditional on survival.

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. Hence, a new firm's ability to hire a high quality team matters: If firms are financially constrained or otherwise unable to secure talent, they are less likely to survive and grow.

9 Validating AKM Assumptions and Endogenous Mobility

In order to interpret the regression coefficients from an AKM specification in an unqualified manner, certain conditions must be met. In this section, we show the validity of our empirical approach by repeating the diagnostic tests used in Card, Heining and Kline (2013). 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. All three terms must be uncorrelated with the firm fixed effects and 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 a 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, but not by other employers. It is possible to test for such sorting in two ways.

First, if employees tend to move to jobs based on the match component, then people who exchange workplaces will not necessarily experience systematic wage changes. Alternatively, in the absence of worker-firm specific matches, the wage gains will be symmetric to the losses. More precisely, if an average worker gains when moving from firm A to firm B, then an average worker moving from firm B to firm A should realize symmetric loses. This symmetry is due to differences in wage premiums across firms. An individual who joins a workplace where other employees are highly paid will, on average, experience a wage gain, whereas an individual who joins 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. For each job change, we classify the origin and destination firms into quartiles, based on the mean wages of co-workers in the firm at that point in time. We then assign job changes to one of 16 groups 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 for each group and plot in Figure 3.

For clarity, Figure 3 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 provides strong evidence that moving to a job with higher paid coworkers raises pay and *vice versa*. Most importantly, the figure shows the 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 4th to the 1st quartile realize wage losses that are similar in magnitude to the wage gains of workers who move from the 1st to the 4th 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 firm 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 explain much variation in wages. However, across all our AKM specifications, the R-squared ranges from 80 to 82 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 violates 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.[11] However, we find that the data does not support the existence of such a pattern. In Figure 3, 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). Most importantly, even if the estimates of the fixed effects themselves were biased, (which would change the interpretation of the R^2 of regressions), the interpretation of the estimate on the new firm dummy should not be affected. For example, with the inclusion of worker fixed effects, the estimate on the new firm dummy is measuring relative wage growth for a given person who switches between young and old employers.

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

[11] 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).

be more likely to move to higher (lower) wage firms, leading to attenuation of the estimated 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 mitigate this concern we explore a highly relevant shock in our setting, unemployment rates. It would be concerning if workers are more likely to transition to young firms during periods of high unemployment and receive lower wages. However, we find no such evidence, as reported in Appendix Table 2, columns 3 and 4.

10 Conclusion

In this paper, we use US Census administrative data to report important facts regarding wages at young firms. As in earlier studies, we confirm a lower average wage at new firms. We document that nearly three quarters of this wage difference can be attributed to differences in worker quality at new firms. These results mitigate the common perception that employees joining new firms accept a wage penalty. 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 also control for time varying observable worker characteristics and firm fixed effects, the wage penalty disappears and is instead replaced by a statistically significant but economically small wage premium at new firms of 0.7%. In this saturated specification, we control for time invariant differences in firm quality. This is important as 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. As such, our results can be interpreted as saying that a given worker with job opportunities at a new and established firm of equivalent quality will expect to earn equivalent wages at both.

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 join young businesses. These results no longer require that employees of new firms offset a wage penalty with a compensating differential associated with working at a new firm. Instead, the new firm wage difference appears to be driven by selection. New firms disproportionately hire lower wage workers due to either positive assortative matching or higher likelihood of financial constraints at new firms which limit the ability to hire high wage workers.

One important implication of these results is that initial worker quality at a new firm is a proxy for firm quality and an economically important predictor of future firm performance. We show that initial worker quality at a new firm can predict 5- and 10-year survival rates and future employment. Hence, a new firm's ability to hire a high quality team matters: If firms are financially constrained or otherwise unable to secure talent, they are less likely to survive and grow.

References

Abowd, Kramarz, and Margolis, 1999. High wage workers and high wage firms. *Econometrica* 67: 251-333.

Abowd, Stephens, Vilhuber, Andersson, McKinney, Roemer, and Woodcock, 2006. The LEHD infrastructure files and the creation of the quarterly workforce indicators, CES Paper 2006-01.

Azoulay, Jones, Kim, and Miranda, 2018. Age and high-growth entrepreneurship, Working paper, National Bureau of Economic Research.

Baker and Kennedy, 2002. Survivorship and the economic grim reaper. *Journal of Law, Economics, and Organization* 18: 324-361.

Berger and Udell, 1998. The economics of small business finance: The roles of private equity and debt markets in the financial growth cycle. *Journal of Banking and Finance* 22: 613-673.

Bernardo and Welch, 2001. On the evolution of overconfidence and entrepreneurs, *Journal of Economics & Management Strategy* 10: 301–330.

Bertrand and Schoar, 2003. Managing with style: The effect of managers on firm policies. *The Quarterly Journal of Economics* 118: 1169–1208.

Brixy, Kohaut, and Schnabel, 2007. Do newly founded firms pay lower wages? First evidence from Germany. *Small Business Economics* 29: 161– 171.

Brown and Medoff, 2003. Firm age and wages. *Journal of Labor Economics* 21: 677-698.

Burdett and Mortensen, 1998. Wage differentials, employer size and unemployment. *International Economic Review* 39: 257–273.

Burton, Dahl, and Sorenson, 2017. Do startups pay less? *Industrial and Labor Relations*, forthcoming.

Card, Heining and Kline, 2013. Workplace heterogeneity and the rise of West German Inequality. *Quarterly Journal of Economics* 128: 967-1015.

Chemmanur, Ertugrul, and Krishnan, 2017. Is it the investment bank or the investment banker? A study of the role of investment banker human capital in acquisitions. *Journal of Financial and Quantitative Analysis*, forthcoming.

Chemmanur, Loutskina, and Tian, 2014. Corporate venture capital, value creation, and innovation. *The Review of Financial Studies* 27:2434–2473.

Dillon and Stanton, 2017. Self-employment dynamics and the returns to entrepreneurship, *NBER Working Paper No. 23168.*

Dinlersoz, Hyatt, and Janicki, 2016. Who works for whom? Worker sorting in a model of entrepreneurship with heterogeneous labor markets. *US Census Bureau Center for Economic Studies Working Paper No. CES-WP-15-08R.*

Evans and Jovanovic, 1989. An estimated model of entrepreneurial choice under liquidity constraints. *Journal of Political Economy* 97: 808-827.

Ewens and Rhodes-Kropf, 2015. Is a VC partnership greater than the sum of its partners? *The Journal of Finance* 70: 1081–1113.

Gao, Martin, and Pacelli, 2017. Do loan officers impact lending decisions? Evidence from the corporate loan market, working paper.

Graham, J. R., Li, S., and Qiu, J. 2011. Managerial attributes and executive compensation. *The Review of Financial Studies* 25: 144-186.

Graham, Kim, Li, and Qiu, 2017. Employee costs of corporate bankruptcy, working paper.

Guiso, Pistaferri and Shivardi, 2013. Credit within the firm. Review of Economic Studies 80: 211-247.

Haltiwanger, Jarmin, and Miranda, 2013. Who creates jobs? Small versus large versus young. *The Review of Economics and Statistics* 95: 347-361.

Hamilton, 2000. Does entrepreneurship pay? An empirical analysis of the returns of self-employment. *Journal of Political Economy* 108: 604-631.

Hopenhayn, 1992. Entry, exit and firm dynamics in long run equilibrium. *Econometrica* 60: 1127-1150.

Hurst, Li, and Pugsley. 2014. Are household surveys like tax forms? Evidence from income underreporting of the self-employed. *Review of Economics and Statistics*, 96: 19-33.

Hurst and Pugsley, 2011. What do small business do? *Brookings Papers on Economic Activity*, Fall 2011: 73-118.

Kraus and Litzenberger, 1976. Skewness preference and the valuation of risk assets. *Journal of Finance* 31: 1085–1100.

Levine and Rubinstein, 2017. Smart and illicit: who becomes an entrepreneur and do they earn more? *Quarterly Journal of Economics* 132: 963-1018.

Manso, 2016. Experimentation and the returns to entrepreneurship. *Review of Financial Studies*, forthcoming.

Moskowitz and Vissing-Jorgensen, 2002. The returns to entrepreneurial investment: A private equity premium puzzle. *American Economic Review* 92: 745-778.

Oi and Idson, 1999. Firm size and wages. *Handbook of Labor Economics* 3: 2165-2214.

Ouimet and Zarutskie, 2014. Who works for startups? The relation between firm age, employee age and growth. *Journal of Financial Economics* 112: 386-407.

Pakes and Ericson, 1998. Empirical implications of alternative models of firm dynamics. *Journal of Economic Theory* 79: 1-45.

Parker, 2009, The Economics of Entrepreneurship. *Cambridge University Press.*

Peterson and Rajan, 1995. The effect of credit market competition on lending relationships. Quarterly Journal of Economics 110: 407–44.

Roach and Sauermann, 2015. Founder or joiner? The role of preferences and context in shaping different entrepreneurial interests. Management Science 61: 2160-2184.

Schmieder, 2013. What causes wage dispersion? Evidence from new firms, working paper.

Song, Price, Guvenen, Bloom and von Watcher, 2017. Firming up inequality, working paper.

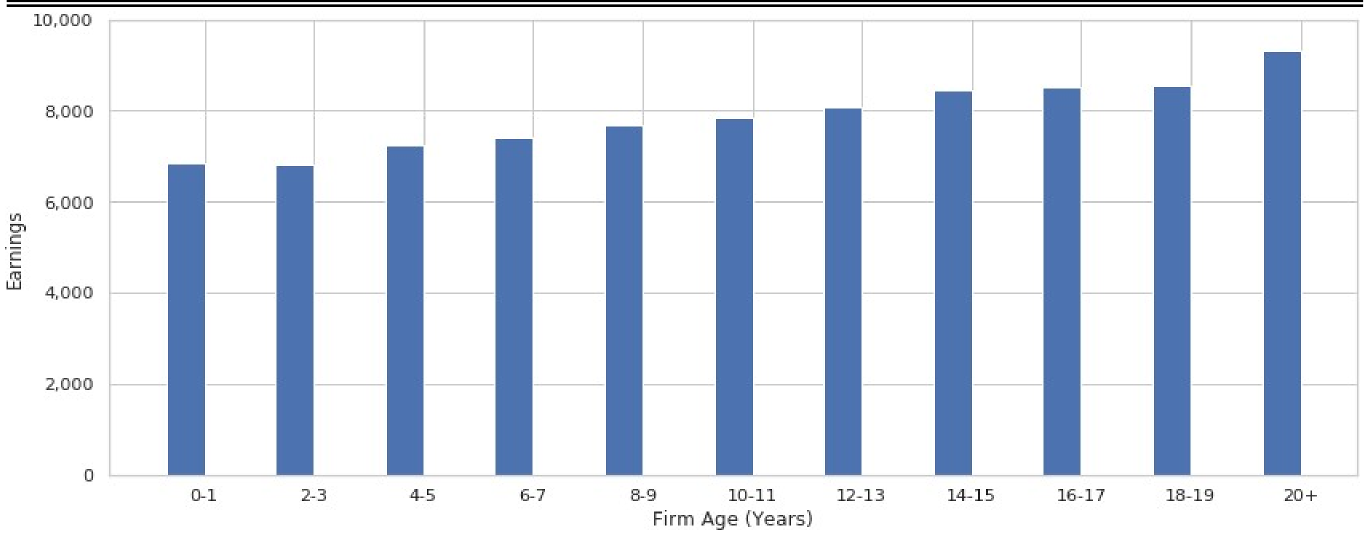
Topel and Ward, 1992. Job mobility and the careers of young men, *Quarterly Journal of* Economics 107: 441-79.

Woodcock, 2005. Sampling connected histories from longitudinal linked data, working paper.

Figure 1. Average Wages and Average Wage Growth by Firm Age

Figure shows mean worker wages (Panel A) and mean worker wage growth (Panel B) by employer age of all worker-years in the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. In Panel A, wages are quarterly and normalized to real 2014 dollars. In Panel B, wages growth is the log differences between the current and the previous year quarterly wages.

Panel A. Average Wages



Panel B. Average Wage Growth

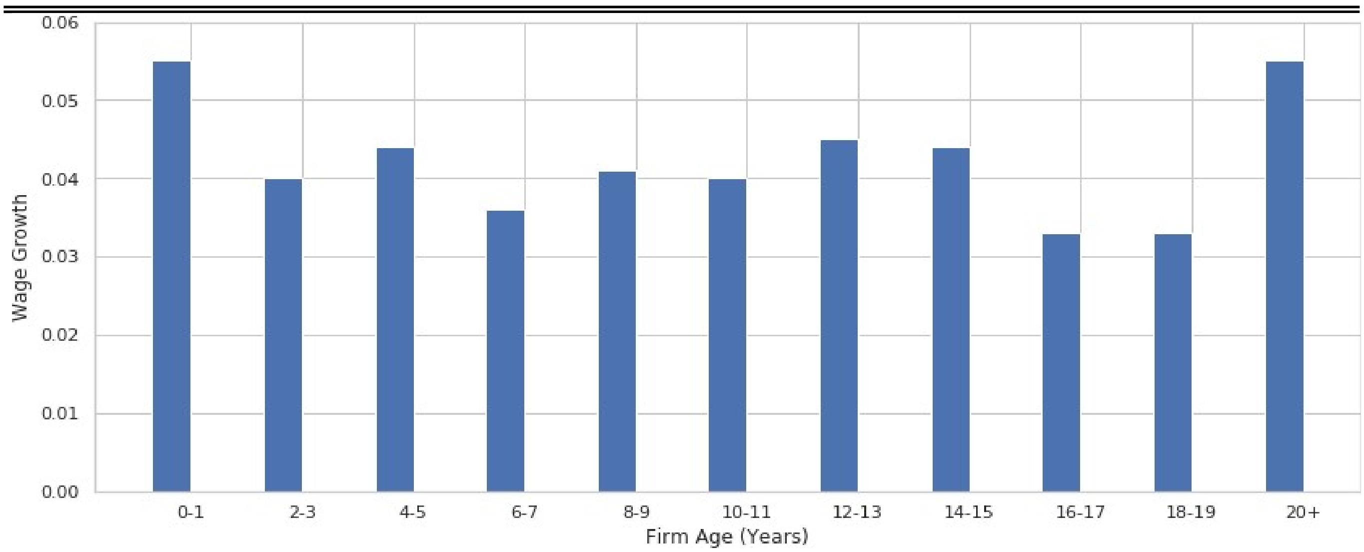
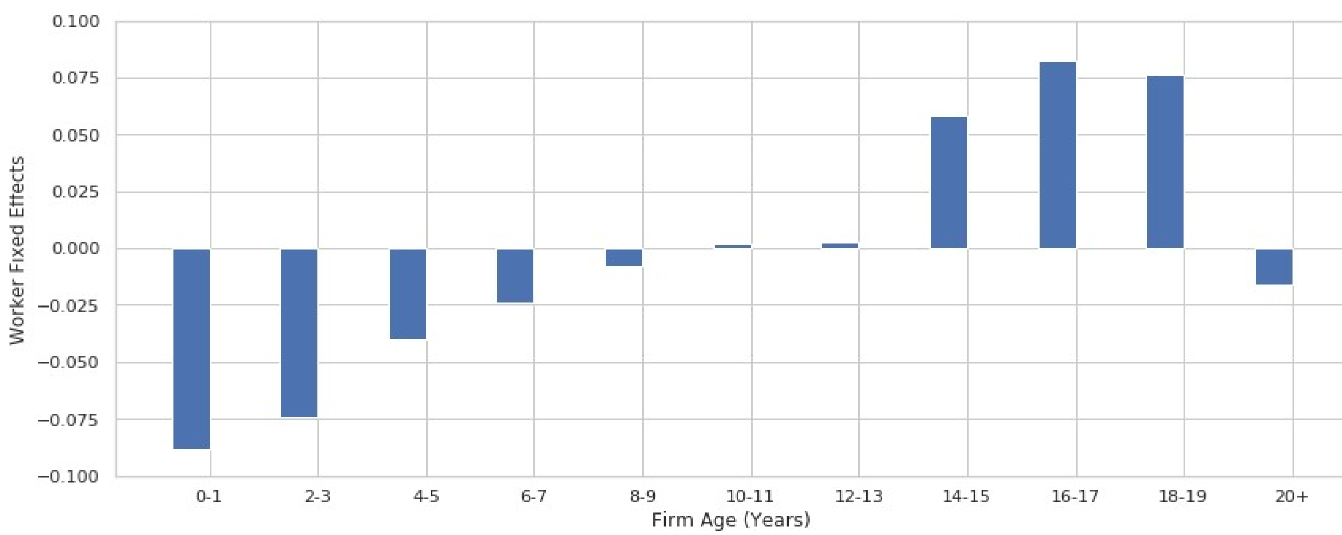


Figure 2. Worker Fixed Effects by Firm Age

Figure shows mean of worker wage fixed effects by employer age for workers in the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. Wages are log normalized to real 2014 dollars. Worker wage fixed effects are estimated from the baseline wage regression in Table 3, column 4. Panel A reports the statistics for all worker-years from the base sample. Panel B shows the statistics for a sub-sample of worker-years at firms that survive for at least ten years (in green) and for a sub-sample of worker-years at firms that exit within nine years of the firm birth (in red).

Panel A. All Firms



Panel B. Surviving and Dying Firms

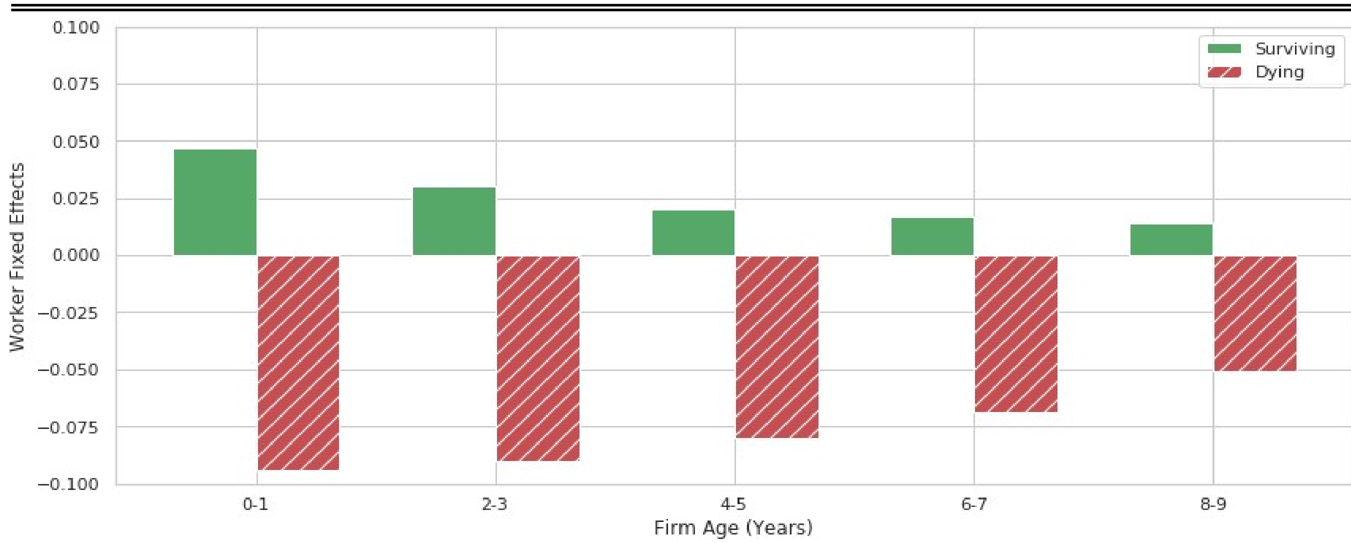


Figure 3. Mean Wages of Job Changers Classified by Quartile of Mean Wages of Coworkers at Origin and Destination Firm

Figure shows mean wages of workers from the baseline sample who change jobs (i.e., employers) in the year zero, and held the preceding job for two or more years (years -2 and -1), and the new job for two or more years (years 1 and 2). The baseline sample is a worker-year panel from 1990 through 2006. Each job is classified into quartiles based on mean wage of coworkers. Wages are log normalized to real 2014 dollars.

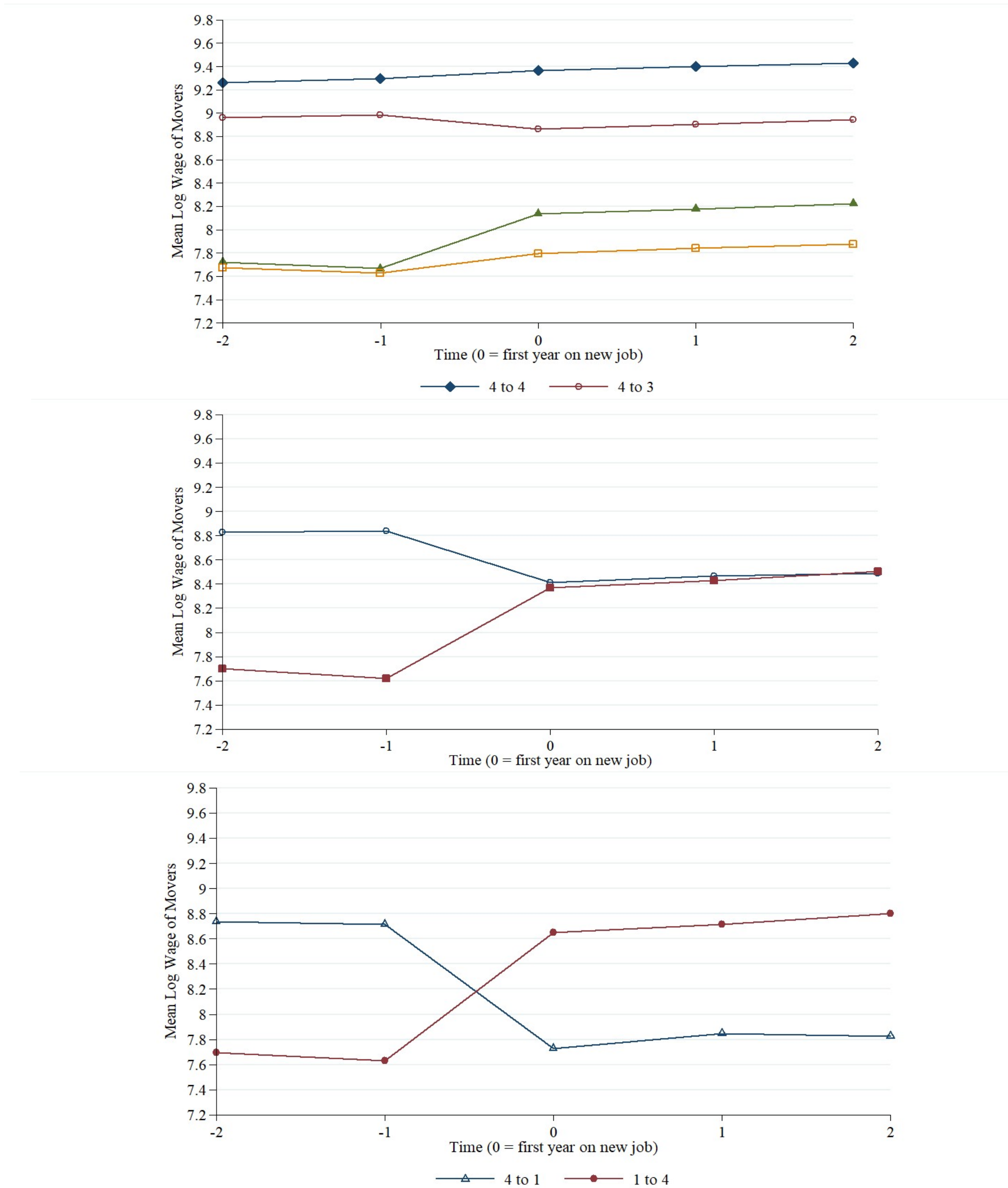


Table 1. Summary Statistics for New and Established Firms

Panel A shows mean (standard deviation) statistics at the firm-year level, and Panel B at the worker-year level for the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. 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 Age	11.1	13.7	1.8
	(8.1)	(7.3)	(1.0)
Firm Employment	167	210	14.6
	(965)	(1,084)	(131)
Percent Male Employees	0.532	0.528	0.545
	(0.330)	(0.325)	(0.349)
Percent College Educated Employees	0.355	0.364	0.323
	(0.254)	(0.246)	(0.276)
Number of Observations (millions)	2.1	1.6	0.45

Panel B. Worker-year level variables

	(1) All Firms	(2) Established Firms	(3) New Firms
Quarterly Earnings (2014$)	8,536	8,673	6,818
	(7,602)	(7,643)	(6,839)
Wage Growth	0.046	0.046	0.046
	(0.485)	(0.476)	(0.582)
Tenure (years)	5.7	5.9	3.2
	(4.4)	(4.5)	(2.7)
Age	38.7	38.9	36.0
	(12.8)	(12.7)	(12.7)
Male	0.524	0.523	0.535
	(0.499)	(0.499)	(0.498)
Education (years)	13.9	13.9	13.6
	(2.6)	(2.6)	(2.5)
Number of Observations (millions)	48.4	44.8	3.6

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 at least once (Column 2) for the workers in the baseline sample. The base sample is a worker-year panel from 1990 through 2006. Statistics are means and standard deviations (in parenthesis).

	(1) Do Not Move	(2) Move
Quarterly Earnings (2014$)	10,020	8,002
	(8,905)	(6,999)
Wage Growth	0.018	0.056
	(0.372)	(0.519)
Tenure (years)	8.3	4.7
	(5.3)	(3.6)
Age	41.9	37.5
	(13.4)	(12.3)
Male	0.558	0.512
	(0.496)	(0.500)
Education (years)	14.2	13.8
	(2.5)	(2.6)
Number of Observations (millions)	12.8	35.6

Table 3. New Firm Wages for All Workers

Table reports baseline results of wages at new firms. The baseline sample is a worker-year panel from 1990 through 2006. 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. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. 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	-0.307***	-0.087***	-0.077***	0.007***
	(0.017)	(0.003)	(0.002)	(0.002)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.009	0.748	0.772	0.810
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 4. New Firm Wages by Firm Age

Table reports baseline results of wages at firms of different age. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. Worker controls include worker age squared and age cubed, and their interactions with worker education. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. 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)
Firm Age 0-1	-0.382***	-0.127***	-0.126***	0.063***
	(0.025)	(0.005)	(0.004)	(0.015)
Firm Age 2-3	-0.387***	-0.138***	-0.139***	0.041***
	(0.025)	(0.005)	(0.004)	(0.014)
Firm Age 4-5	-0.303***	-0.105***	-0.117***	0.038***
	(0.032)	(0.009)	(0.006)	(0.014)
Firm Age 6-7	-0.258***	-0.089***	-0.105***	0.031**
	(0.030)	(0.006)	(0.005)	(0.012)
Firm Age 8-9	-0.208***	-0.072***	-0.090***	0.028**
	(0.033)	(0.007)	(0.005)	(0.011)
Firm Age 10-11	-0.184***	-0.064***	-0.079***	0.027***
	(0.036)	(0.008)	(0.005)	(0.010)
Firm Age 12-13	-0.157***	-0.052***	-0.064***	0.028***
	(0.039)	(0.008)	(0.006)	(0.009)
Firm Age 14-15	-0.128***	-0.035***	-0.048***	0.025***
	(0.035)	(0.008)	(0.008)	(0.007)
Firm Age 16-17	-0.081***	-0.016**	-0.034***	0.018***
	(0.029)	(0.007)	(0.006)	(0.005)
Firm Age 18-19	-0.049*	-0.011***	-0.025***	0.009**
	(0.025)	(0.004)	(0.004)	(0.004)
Firm Age 20+	(omit)	(omit)	(omit)	(omit)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.018	0.748	0.772	0.810
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 5. 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 2006. 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. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. 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	-0.275***	-0.092***	-0.088***	0.002
	(0.016)	(0.002)	(0.002)	(0.003)
Observations (millions)	18.3	18.3	18.3	18.3
R-squared	0.008	0.751	0.763	0.809
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 6. 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 2006. 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. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. 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	-0.206***	-0.095***	-0.090***	0.005
	(0.027)	(0.004)	(0.004)	(0.005)
Observations (millions)	6.29	6.29	6.29	6.29
R-squared	0.014	0.724	0.739	0.804
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

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 2006. 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. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Firm employment is log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. Estimates marked "Included" are not reported due to the US Census disclosure limits on the number of estimates that can be cleared. 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	-0.131***	-0.014***	-0.008***	0.017***	0.015***	0.017***
	(0.007)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Firm Employment	-0.010	0.104***	0.119***	0.087***	Included	Included
	(0.032)	(0.006)	(0.005)	(0.008)		
Firm Employment ^2	0.018***	-0.006***	-0.009***	-0.005**	Included	Included
	(0.006)	(0.001)	(0.001)	(0.002)		
Firm Employment ^3	-0.001***	-0.000	0.0002***	0.000	Included	Included
	(0.000)	(0.000)	(0.0001)	(0.000)		
Observations (millions)	48.4	48.4	48.4	48.4	18.3	6.29
R-squared	0.04	0.751	0.775	0.811	0.81	0.8
Time-Varying Worker Controls	No	No	Yes	Yes	Yes	Yes
Worker FE	No	Yes	Yes	Yes	Yes	Yes
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8. Wages by Worker Tenure and Firm Age

Table reports results of wages by worker tenure at an employer and by the employer's age. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly wages. Wages are in real 2014 dollars. Tenure 1 (2) (3) (4+) equals one for workers who were at the employer for one (two) (three) (four or more) years. New firm is defined as a firm of three years of age or less. Worker controls include worker age squared and age cubed, and their interactions with worker education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. 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)
Tenure 0	(omit)	(omit)	(omit)	(omit)
Tenure 1	0.165***	0.041***	0.010***	0.007***
	(0.005)	(0.002)	(0.002)	(0.002)
Tenure 2	0.370***	0.109***	0.063***	0.061***
	(0.007)	(0.003)	(0.002)	(0.002)
Tenure 3	0.477***	0.128***	0.082***	0.085***
	(0.009)	(0.003)	(0.003)	(0.003)
Tenure 4+	0.686***	0.117***	0.119***	0.133***
	(0.014)	(0.004)	(0.004)	(0.003)
Tenure 0 * New Firm	-0.086***	-0.050***	-0.047***	0.033***
	(0.014)	(0.003)	(0.003)	(0.002)
Tenure 1 * New Firm	-0.087***	-0.058***	-0.048***	0.027***
	(0.016)	(0.003)	(0.003)	(0.002)
Tenure 2 * New Firm	-0.071***	-0.059***	-0.043***	0.027***
	(0.016)	(0.003)	(0.002)	(0.002)
Tenure 3 * New Firm	-0.026	-0.064***	-0.038***	0.015***
	(0.019)	(0.004)	(0.003)	(0.003)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.079	0.749	0.773	0.812
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

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

Table shows cross-sectional OLS results from predicting new firm exit (Columns 1-2) and future employment (Columns 3-4) as a function of worker fixed effects estimated from the wage regression in Table 3, column 4. The sample is a cross-section of new firms from the base sample. The base sample is a worker-year panel from 1990 through 2006. In Columns 1-2, dependent variable, New Firm Exits in 5 Years, equals one for new firms that exit by year five since founding. In Columns 3-4, dependent variable, New Firm 5-year Employment, is the log of a new firm's employment at age five. Mean Worker Fixed Effects is the mean of worker fixed effects of workers at the new firm in its first year of existence. 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	
	(1)	(2)	(3)	(4)
Mean Worker Fixed Effects	-0.077***	-0.065***	0.177***	0.180***
	(0.006)	(0.005)	(0.013)	(0.013)
Observations (thousands)	205	205	205	205
R-squared	0.035	0.036	0.099	0.1
Log New Firm Employment in First Year	Yes	Yes	Yes	Yes
Log Mean Worker Education in First Year	No	Yes	No	Yes
Log Mean Worker Age in First Year	No	Yes	No	Yes
Year of Firm Birth FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Appendix.

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

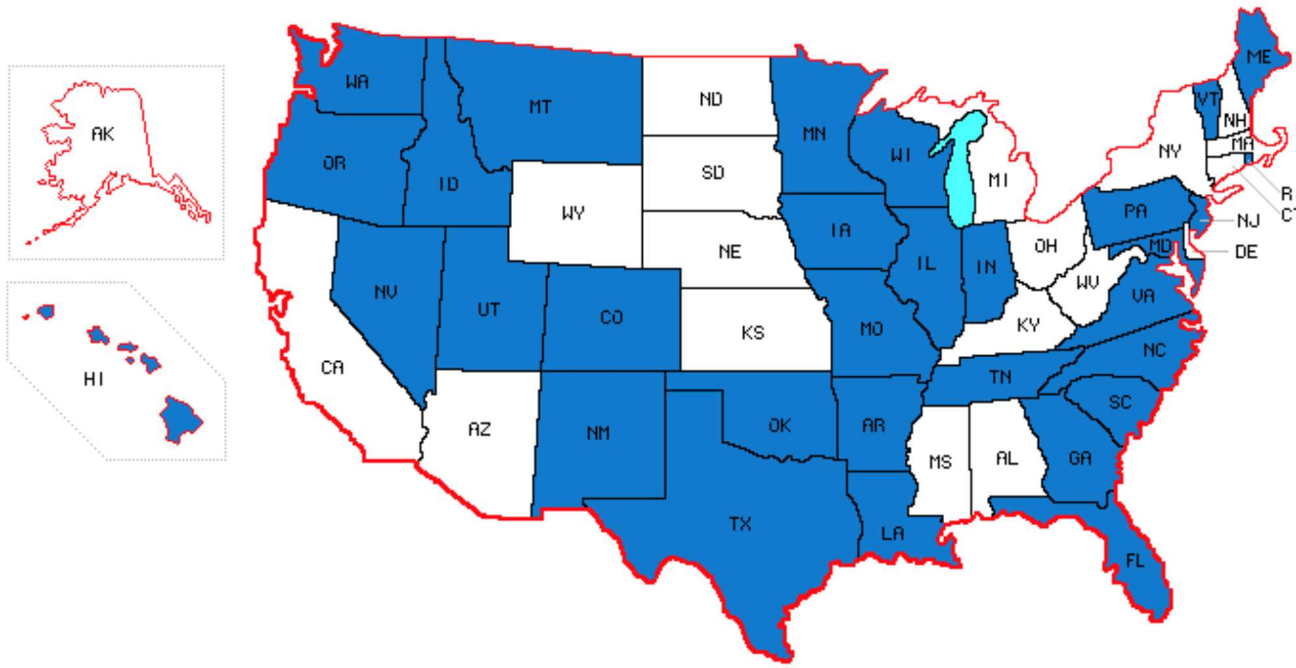


Figure 2. Mean Adjusted Wages of Job Changers Classified by Quartile of Mean Wages of Coworkers at Origin and Destination Firm

Figure shows mean adjusted wages of workers from the baseline sample who change jobs (i.e., employers) in the year zero, and held the preceding job for two or more years (years -2 and -1), and the new job for two or more years (years 1 and 2). The baseline sample is a worker-year panel from 1990 through 2006. Each job is classified into quartiles based on mean wage of coworkers. Wages are log normalized to real 2014 dollars. 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.

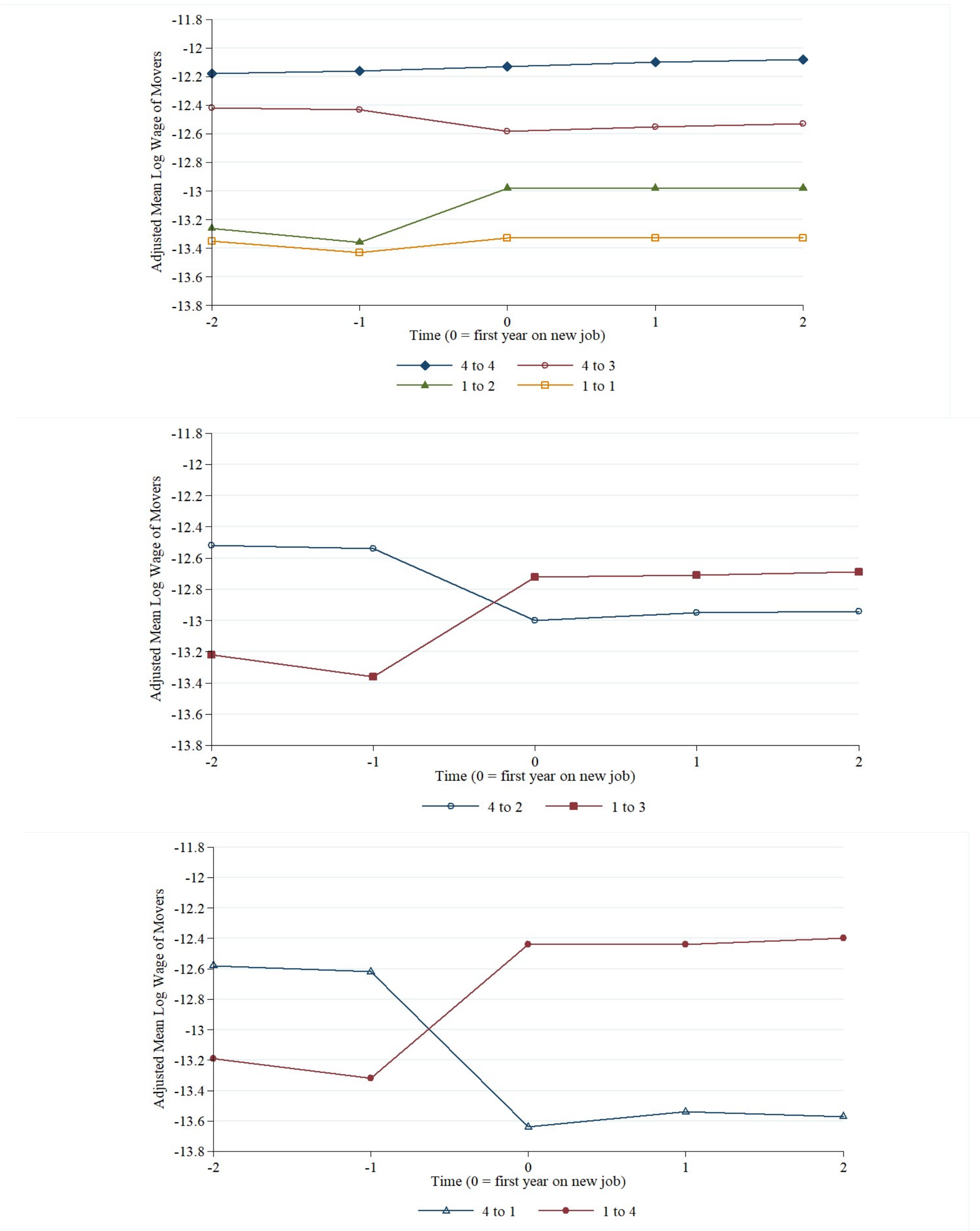


Table 1. New Firm Wages: Define New Firm as Aged Zero or One

Table reports baseline results of wages at new firms, where new firm is defined as aged zero or one. The sample is a worker-year panel from 1990 through 2006. 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 one or less. Time-varying worker controls include worker age squared, worker age cubed, worker age times education, worker age squared times education, worker age cubed times education. Worker age is log transformed. Education is measured in years of schooling and log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effect since they are collinear with the fixed effect. 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 (Age 0-1)	-0.290***	-0.065***	-0.056***	0.021***
	(0.017)	(0.003)	(0.003)	(0.002)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.005	0.748	0.772	0.810
Time-Varying Worker Controls	No	No	Yes	Yes
Worker FE	No	Yes	Yes	Yes
Firm FE	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes

Table 2. Robustness

Table reports results of robustness tests for the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. 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. Columns 1-2 show results for workers who have moved after the plant closure. Move to New Firm from Closed Plant (Open Plant) show results for workers who move to new firms from closed plants (open plant). Plant is defined as closed when the employment is zero either in the year of the move to a new employer or the year prior to the move. Columns 3-4 show results for workers who are at new firms during low unemployment (New Firm and Low Unemployment) and high state-level unemployment (New Firm and High Unemployment). High Unemployment is one for firms in states with unemployment rate above the national unemployment rate-year. Worker age is log transformed. Education is measured as years of schooling and is log transformed. Estimates marked "Included" are not reported due to the US Census disclosure limits on the number of estimates that can be cleared. 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.

	Moves From Closed vs. Open Establishment		Moves During Low vs. High Unemployment	
	(1)	(2)	(3)	(4)
Move to New Firm from Closed Establishment	-0.056***	0.026***		
	(0.002)	(0.003)		
Move to New Firm from Open Establishment	-0.076***	0.017***		
	(0.003)	(0.002)		
Worker Starts Employment in New Firm	-0.100***	-0.013***		
	(0.003)	(0.002)		
New Firm and Low Unemployment			-0.077***	0.007***
			(0.002)	(0.002)
New Firm and High Unemployment			Included	Included
Observations (millions)	48.4	48.4	48.4	48
R-squared	0.77	0.81	0.77	0.8
Time-Varying Worker Controls	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
P-value from *t*-test	0.00	0.00	NA	NA