Entrepreneurial Wages

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Abstract

We use US Census administrative data to document important facts about wages at entrepreneurial firms. As in earlier studies, we confirm lower average wages at new firms. However, nearly two thirds of this decline can be attributed to 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 further declines. This suggests that while new firms pay lower wages, on average, there is no dramatic increase in wages across the first years of a firm's lifetime. Finally, with the addition of controls for observable time varying worker characteristics, we show that there is no economically significant difference in wages at new firms. These findings suggest 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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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)[2], small firms (Oi and Idson, 1999) and during self-employment spells (Hamilton, 2000; Manso, 2016).[3] Moskowitz and Vissing-Jorgensen (2002) also document low returns for owners of small businesses. The belief that new firms pay lower wages is troubling given that new firms account for approximately twenty percent of new job creation in the United States (Haltiwanger, Jarmin and Miranda, 2013).

Instead, using US Census administrative data, we find no evidence of a wage penalty for workers who match to entrepreneurial firms. We confirm that new firms indeed pay lower wages, 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 could have earned at other firms, i.e. market wages. We document that two thirds of the wage difference at new firms can be explained by differences in intrinsic worker quality. On average, new firms employ workers who command lower market wages due to time-invariant differences in skills or talent. In addition, new firms employ workers at points in their careers when they expect to earn lower wages, i.e. at younger ages. After controlling for time invariant and observable time-varying worker characteristics, the new firm wage gap approaches zero.

[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.

[3] Hamilton (2000) finds a 35% reduction in wages for self-employed. Manso (2016) finds a 5-10% decrease in wages for the 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. This suggests that while new firms pay lower wages, on average, these firms do not significantly increase wages as they mature from new to more established firms. New firms in our data include an assortment 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 anticipate lower wages. The presence of low quality new firms will depress mean wages at new firms, unless firm fixed effects are included. With firm and employee fixed effects, as well as controls for time-varying employee characteristics, there is no significant difference in wages at new and more established firms.

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. One important caveat to our analysis is that we do not observe exogenous movement between firms. This limits the generalizability of our results. Our conclusions are specific to the real world setting where employees who chose to match to new firms presumably do so in anticipation of productive matches.

We reach these conclusions using an AKM method, an approach widely used in labor economics and developed by Abowd, Kramarz, and Margolis (1999) and further adapted by Card, Heining and Kline (2013). This approach uses workers who changes jobs to isolate employer and employee fixed effects simultaneously. We identify the wage penalty specific to firm age by including indicator variables. We define new firms as firms under four years of age. 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. In some specifications, we also control for other firms between four and ten years of age to ensure that our results are not driven by employees moving between new and young(ish) firms. Again, our findings are robust.

In separate tests, 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. These employees may be relatively more likely to be compensated with stock options, a form of compensation underestimated in our definition of wages.[4] 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. Alternatively, high skill, high tech workers may have a preference for skewness and trade-off lower average compensation for a greater probability of very high future compensation.

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

Overall, these results dispel commonly held beliefs that employees who work at startups face a significant wage penalty as compared to the wages the employee could have earned at a more established firm. However, these results leave open several puzzles and raise additional questions. First, it remains unclear why employees at startups don't receive a wage premium given the higher risk of job loss following bankruptcy. This is in sharp contrast to standard theories in labor

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

economics which predict higher wages for workers exposed to greater risk of termination due to a higher probability of financial distress or bankruptcy (Berk, Stanton, and Zechner, 2009). Numerous studies have documented the high employment risk associated with working at a new firm. And studies such as Dore and Zarutskie (2016) and Graham et al (2016), have shown that firm characteristics which change the probability of bankruptcy risk are associated with higher wages. It may be that these employees receive compensation for their greater risk in the form of stock ownership or options, but we cannot observe this type of compensation in our data.

Second, the findings highlight that matching between workers and firms is important in understanding wage dynamics at young firms and raise questions about the underlying matching mechanism. Do new firms disproportionately match with low wage workers due to lower firm-level productivity? Do financial constraints at new firms play a role in hiring and wage-setting decisions?

Our paper is the first to use a very large sample of employee-employer observations for U.S. firms over nearly two decades to examine the underlying drivers of the new firm penalty documented in prior studies such as Brown and Medoff (2003). A handful of prior studies have also examined the new firm penalty using samples of employee-employer matched data in Europe. For example, Nyström and Zhetibaeva (2015) examine whether the new firm wage penalty persists for labor market entering workers, and finds that the penalty still persists. Burton, Dahl and Sorensen (2017) find evidence in Denmark that after controlling for firm size, younger firms do not pay a significant wage penalty. Our study is the first to analyze to what extent lower wages at new firms can be explained by the type of workers joining new firms and by the type of new firms being founded. By employing both worker and firm fixed effects as well as worker time varying controls in the U.S., we document that the new firm wage penalty is negligible in a sample of job switchers and that the cross-sectional new firm wage penalty can be explained by the types of workers who match to new firms.

Our paper also serves to complement an expanding and important body of research which relies on Census data, and in particular employer-employee matched data, to examine the features and dynamics of new firms (e.g., Goetz et al, 2016). For example, Haltiwanger et al (2012) use these data to show that the startup wage penalty increases from 2000 to now. About 40-50% of the difference is explained by the differences in the industrial composition of entrants, which tend to be in the lower wage industries. Dinlersoz, Hyatt, and Janicki (2016) develop a model of worker sorting to firm based on differential ability and firm age, which they calibrate using Census data, and find that lower ability workers and younger workers match to younger firms.

Finally our paper adds to the literature seeking to understand the drivers and implications of working for or starting a new firm. A number of studies have argued that the choice to work for an entrepreneurial firm poses a puzzle in light of relatively lower wages and returns for those investing in the equity of such firms (e.g., Hamilton (2000), Moskowitz and Vissing Jorgensen (2002)) . However, recently, studies such as have begun to show that such a penalty not always be present and that controlling for more precisely for outside options and ability is important in generating such estimates (e.g., Kartashova (2014), Manso (2016), Dillon and Stanton (2017)). Our study adds to such recent endeavors by estimating worker-firm level regressions which control for many previously unobservable characteristics of the workers joining young firms.

1 Data

We combine confidential databases from the US Census Bureau to form our estimation sample. We use both the Longitudinal Employer-Household Dynamics data (LEHD) and the Longitudinal Business Database (LBD) in our analysis.

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 employer establishments on a quarterly basis. LEHD data are collected from the unemployment insurance records of states participating in the program.[5] Data start in 1990 for several states and coverage of states increases over time. The data coverage ends in 2008. Our 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, for any state in the program, we observe all employees at firms with at least one paid employee. For each individual we observe quarterly wages and current place of employment. The LEHD also allows us to observe the age, gender, race, place of birth, and imputed education of each employee.

We supplement the information contained in the LEHD with firm-level information from the Longitudinal Business Database (LBD). The LBD is a panel dataset, also maintained by the Census Bureau, that tracks all US business establishments.[6] 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 annual establishment payroll. In addition, the LBD also contains a unique firm-level identifier, *firmid*, which links establishments that are part of the same firm. We observe the LBD for all 50 states and the District of Columbia.

We link the LEHD to firm identifiers in the LBD using the employer identification numbers (EIN). Matching between the LBD and the LEHD is an imperfect process because the LBD infrastructure is based on physical establishments while the LEHD infrastructure uses reporting

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

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

units for a given firm which are defined at the state level and called State Employer Identification Numbers (SEIN). SEINs may or may not match the physical establishments identified in the LBD. As such, we do not track whether an individual stays at the same physical establishment over time, only if the individual remains at the firm.

Wages are measured at the quarterly level and we do not observe the number of weeks worked. Thus, to ensure that we are not observing a quarter in which the employee was only partially employed at the given firm, we limit the sample to employee-firm quarters where we observe a full quarter of employment prior to and a full quarter of employment after the sample period for the given employee-firm pair. 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 minimize the probability of data errors in our sample, we drop all observations for individuals where wages change by 5,000% in one year. Wages are normalized to year 2014 constant dollars. In addition, we use log wages in the regressions to address the skewed distribution of wages as well as to minimize the role of outliers.

In Table 1, we report summary statistics for firms in our sample. In column 1, we report mean values for all firms in our sample calculated as an average of firm-year observations. In column 2, we report mean values for established firms, defined as firms four years or greater in age. In column 3, we report mean values for new firms, defined as firms less than four years of age. As expected, 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.[7] As in Brown and Medoff (2003), we also find that younger firms pay lower wages. Looking at the worker characteristics, new firms employ slightly less educated workers and fewer male employees.

[7] 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.

In Table 2, we report summary statistics for the employees in our sample. Given 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 full set of employees in the sample (column 1) and for those employees who never switch jobs during our sample (column 2) and employees who switch jobs (column 3). 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 likely to receive lower wages.

2 Empirical Strategy

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

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

where y_{it} are log quarterly real wages of individual i in year t,[8] $\propto_i$ 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. The firm fixed effect reflects any time-invariant wage premium or discount paid to all employees of

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

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 the 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).

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. If all three components are mean zero and orthogonal to the fixed effects, then the interpretation of the regression coefficients can be unqualified. However, three types of endogenous mobility can violate this assumption. We discuss each in turn.

Endogenous employee mobility can occur if employees sort into firms based on the firm-employee match component. An example of this type of endogenous mobility follows when employee job transitions are motivated by an expectation that employee-specific traits will be relatively more valued at the new firm. This type of endogenous mobility will leave a distinct pattern in the data of wage increases following the average job transition.

In Figure 1, we graph an event study of the effect of job changes on wages. The sample is limited 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. The figure plots wages over time for these employees, where the job transition occurs between year -1 and year 0. We separately plot workers who 1) begin at an established firm (firm aged four or older) and move to a different established firm; 2) who begin at an established firm and move to a new firm (firm aged three or younger); 3) who begin at a new firm and move to a different new firm; and, 4) who begin at a new firm and move to an established firm.

In Figure 2, we instead plot abnormal wages, to remove any trends in wages due to age, education and the interaction of the two employee characteristics. For all groups, we observe modest wage increases before the job transition, a jump in wages at the point of the job transition, and generally flat wages after the job transition. Figures 1 and 2 suggest that there is a significant but economically modest trend in the data of increasing worker-firm specific matches following job transitions. Moreover, these results echo the finding in the summary statistics that employment at a new firm is associated with lower wages, in the absence of additional controls.

A pattern of rising wages following a job transition 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. The presence of this type of endogenous mobility impacts the interpretation of our findings. Our results are specific to employees who endogenously match to new firms. In other words, our sample of employees who move to new firms is likely biased towards employees who are specifically productive at new firms. We estimate the new firm wage differential using workers who switch between employment at new firms and employment at more established firms. These workers may have multiple employment opportunities and, hence, realized job transitions observed in the data are likely to reflect an anticipated wage increase.

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.

While we find evidence of endogenous mobility along worker-firm matches, we find no evidence that the unit root and transitory components of the error term violate the AKM assumptions. A unit root error component could be correlated with the firm fixed effects. If so, job transitions would systematically occur following a pattern of either increasing or decreasing wages at the prior employment. Such a pattern if 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.[9] However, again, we find that the data does not support the existence of such a pattern. In Figure 2, we find no evidence of trends in the wages of workers pre-transition based on the future transition (e.g. to startup or established firm).

Finally, our results would be biased if fluctuations in the transitory error term was correlated with mobility patterns between startups and established firms. Essentially, this would predict that transitory shocks are followed by a systematic pattern of job changes to one specific type of firm, startup or established. One example could be that workers are more likely to transition to startups during periods of high unemployment and, hence, lower wages. We find no such evidence.

In addition, it is worth emphasizing a few additional issues specific to the AKM methodology as applied in our setting. First, we estimate the AKM using a subset of the full data, a set of firms

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[9] 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).

connected through switching workers. This assumption 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.

Computing limitations prohibit us from using the full set of workers in our estimations. We, therefore, start with workers who have worked at a public firm at some point in their observed work history. This sample is selected to sample workers who are known to work at established firms and hence are likely not shut off from established firms labor markets. This sample also facilitates estimation of workers who transition to and from established firms and minimizes any potential bias from the connected set limitation due to the modest dropping of observations. In unreported results, we have used a random 10% sample of the LEHD and found similar results.

Second, by including the firm fixed effect along with the startup variable, we are only able to estimate the coefficient on startup for the subset of startups which survive for four or more years. In robustness tests, we find qualitatively similar results when we define our startup variable to only include firms under two years of age. Dropping the set of firms which survive for only two or three years does not significantly alter our findings, suggesting that the results are not critical on firms surviving beyond a of minimum two years. However, we cannot speak to wages at startup firms which survive for less than two years.

Third, 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 for the first and last quarters around a job change. However, the cost of such a step is that we under-sample workers with especially high turnover rates.

Forth, employees with missing data are dropped, as required by the AKM methodology. Missing data occurs when an employee is unemployed and, hence, is unmatched to a firm and wages are unobserved for a period of time. 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 addition, Employees may also be dropped if data is missing due to other issues, such as the imperfect match between the LBD and LEHD.[10]

3 Baseline Results

We report our baseline estimations in Table 3. 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 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.

All regressions in Table 3 are estimated using the connected set of employees who have worked at a public firm at some point during their observable tenure. We also require that any worker transitioning in the data is observed employed at the new and old firm for a minimum of two years and drop workers with an unemployment spell greater than one year, as in Card, Kline and Henning

[10] Matching issues between the LBD and LEHD are further discussed in Babina, Ouimet and Zarutskie (2016).

(2013). All standard errors are double clustered at the firm and at the worker level. All observation counts are rounded due to Census disclosure policies.

3.1 OLS Estimation

As reported in column 1, new firms on average, 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 to differences in the types of employees hired at new firms, the characteristics of new firms or by differences in the compensation practices at new firms.

3.2 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 with 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.[11]

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.

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[11] 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.

3.3 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. We find similar effects if we use an alternative approach to controlling for firm quality. In untablulated results, we find that if we limit the sample to only firms which survive for at least ten years, then the wage penalty at new firms is further decreased.

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.

3.4 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 or if we instead use a random ten percent sample of all individuals in the LEHD. 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, this wage difference can be entirely explained by controlling for 1) individual time invariant characteristics; 2) time invariant firm characteristics and 3) time varying observable employee characteristics. The large wage difference observed when just looking at simple averages is explained by the fact that new firms hire more workers who command a 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.

4 Alternative Samples

Having shown no evidence of a new firm wage discount after controlling for time varying and time invariant worker characteristics and time invariant firm characteristics using a diverse sample of workers and firms, we now consider if the results are different when considering specific subsets of employees. Specifically, we are interested 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.

4.1 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 U.S. 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.

4.2 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.[12] 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,

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

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

4.3 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 whose average wage across years in that new firm is the highest and 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 5%. This result suggests that instead of a wage discount, founders instead receive a wage premium when they join 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 Annette Vissing-Jorgensen (2002) for pursuing self-employment, then again this underestimates the total gains founders realize upon joining the new firm.

However, we must caveat these results with the acknowledgement that by sorting on wages at the new firm, we are mechanically increasing the probability that we observe a wage increase. However, in the absence of alternative means to identify the founder, we argue these results suggest that joining a new firm can be associated with gains, especially for founding employees.

5 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).

6 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 decline 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. 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%. This suggests that while new firms pay lower wages, on average, there is no dramatic increase in wages across the first years of a firm's lifetime. 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. However, these results still leave open the questions. First, why workers do not receive a wage premium upon joining a new firm given the inherently higher employment risk associated with these firms? Second, what is the underlying mechanism by which lower quality workers are matched to young firms?

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Figure 1. Average Wage Changes for Job Switchers

Figure 1 shows the average change in raw wages (log normalized to real 2014 dollars) around job changes (normalized to occur at year 0) by type of job change. We separately plot workers who 1) begin at an established firm (firm aged four or older) and move to a different established firm (plotted in green, marked "Est to Another Est"); 2) who begin at an established firm and move to a new firm (firm aged three or younger; plotted in blue, marked "Est to Startup"); 3) who begin at a new firm and move to a different new firm (plotted in yellow, marked "Startup to Another Startup"); and, 4) who begin at a new firm and move to an established firm (plotted in red, marked "Startup to Est").

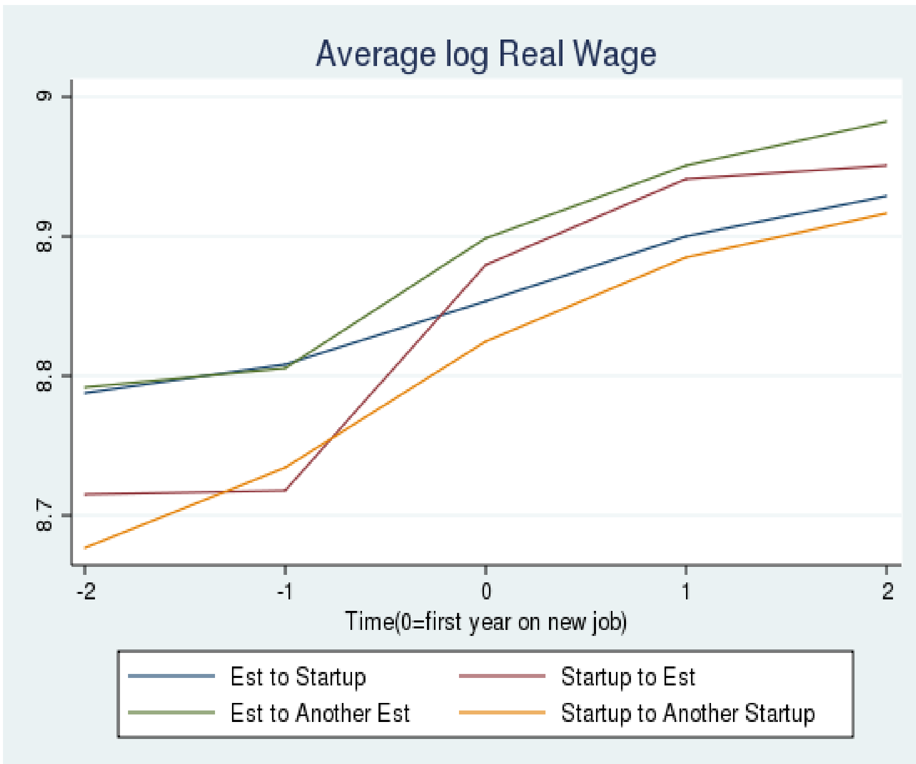

Figure 2. Average Adjusted Wage Changes for Job Switchers

Figure 2 shows the average adjusted change in 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 workers who 1) begin at an established firm (firm aged four or older) and move to a different established firm (plotted in green, marked "Est to Another Est"); 2) who begin at an established firm and move to a new firm (firm aged three or younger; plotted in blue, marked "Est to Startup"); 3) who begin at a new firm and move to a different new firm (plotted in yellow, marked "Startup to Another Startup"); and, 4) who begin at a new firm and move to an established firm (plotted in red, marked "Startup to Est").

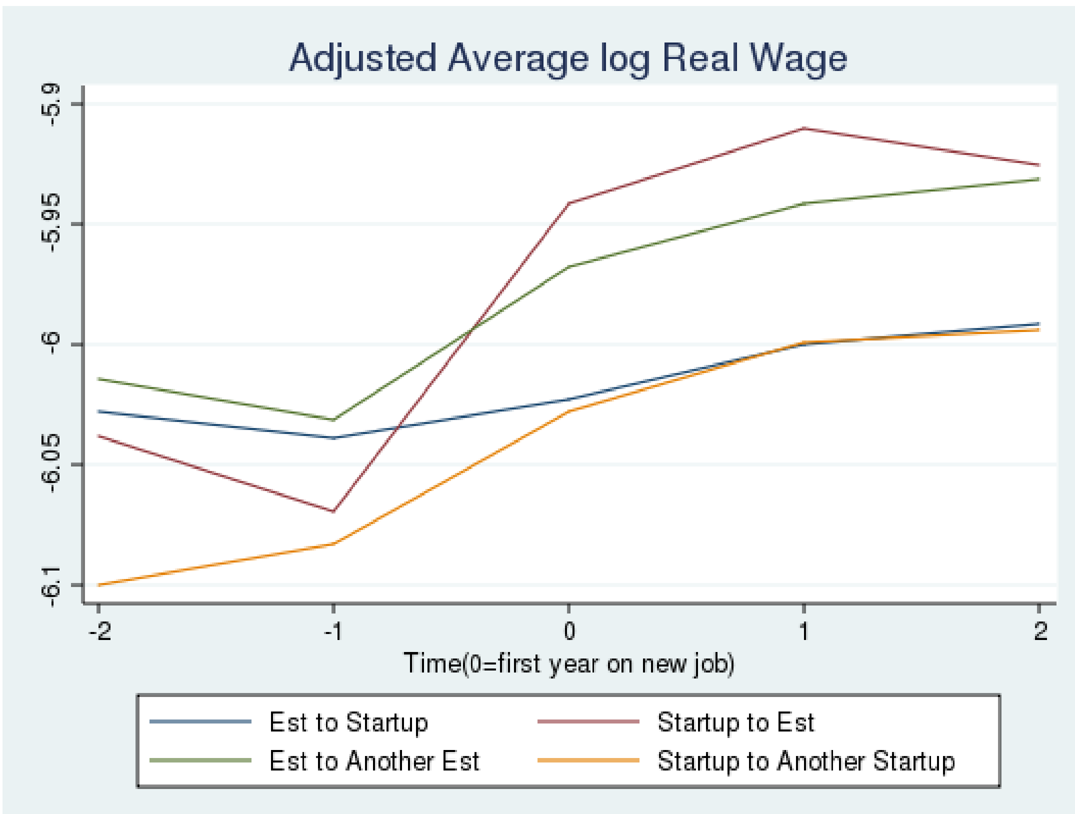

Table 1. Firm-Level Summary Statistics

Summary statistics for firms in our sample. Column 1 reports mean (standard deviation) 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. Each statistic is calculated at a unique firm level in a following way: first, for each variable the average is calculated for each firm-year across all workers employed by that firm-year; second, means and standard deviations reported in this table are calculated across all firm-years.

	(1)	(2)	(3)
Firm Average Quartly Earnings (2014$)	7,675	7,704	7,315
	(7,969)	(7,808)	(9,765)
Firm Employment	17.67	18.91	2.095
	(277.50)	(288.30)	(11.20)
Percent Male Employees	0.518	0.519	0.504
	(0.43)	(0.43)	(0.47)
Percent College Educated Employees	0.273	0.274	0.26
	(0.37)	(0.37)	(0.40)
Number of Observations (thousands)	640	592	47

Table 2. Worker Summary Statistics

Summary statistics for workers in our sample. Column 1 reports mean (standard deviation) using the sample of all workers. Column 2 (3) reports statistics for workers who never change jobs (change jobs) in the sample. Mover is an indicator variable which takes the value of 1 if the worker ever changed employers in our sample. 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)	(2)	(3)
Age	41.01	41.78	39.6
	(12.91)	(13.29)	(12.06)
Education (years)	13.44	13.42	13.47
	(2.41)	(2.40)	(2.41)
Quarterly Earnings (2014$)	10,280	10,550	9,777
	(17,550)	(18,955)	(14,606)
Tenure (years)	6.657	7.175	5.7
	(4.22)	(4.79)	(2.65)
Male	0.548	0.552	0.539
	(0.50)	(0.50)	(0.50)
Mover	0.351	0	1
	(0.48)	(0.00)	(0.00)
Number of Observations (thousands)	1,330	861	467

Table 3. New Firm Wages for All Workers

Table 3 reports baseline results using the sample of all workers who are ever observed at a public firm. The dependent variable is quarterly earnings, log transformed. 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 they are 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 4 reports results using the sample of college educated workers who are ever observed at a public firm. The dependent variable is quarterly earnings, log transformed. 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 they are 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)	3.587		3.587		3.587		3.587	
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 5 reports results using the sample of college educated workers who are ever observed at a public firm and work in the technology sector. We identify worker industry as the first industry observed for a given worker. The dependent variable is quarterly earnings, log transformed. 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 they are 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)	1.163		1.163		1.163		1.163	
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

Table 6 reports results using the sample of all workers who are ever observed at a public firm. The dependent variable is quarterly earnings, log transformed. New firm is defined as a firm of three years of age or less. Founder is identified as the employee whose average wage across years in that new firm is the highest and who was in the firm in the firm's first year of existence. 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 they are reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	(1)		(2)		(3)		(4)	
Founder at New Firm	-0.3428	***	-0.1183	***	0.0261	***	0.0478	***
	(0.042)		(0.005)		(0.005)		(0.004)	
Non-founder at New Firm	-0.202	***	-0.0629	***	-0.0473	***	-0.0177	***
	(0.054)		(0.007)		(0.003)		(0.003)	
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							0.2716	***
							(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	
P-value from T-test	0.00		0.00		0.00		0.00	

Table 7. New Firm Wages After Controlling for Firm Size

Table 7 reports baseline results of wages at new firms after controlling for firm size. Columns 1-4 use the sample of all workers who are ever observed at a public firm. Column 5 uses the sample of college educated workers observed at public firms. Column 6 uses the sample of college educated workers observed at a public firm and employed in the tech sector. The dependent variable is quarterly earnings, log transformed. 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 they are reported in parentheses. ***, **, * indicate statistical significance as the 1%, 5%, and 10% level, respectively.

	All								College		College & Hi-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)	
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)	
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)	
Observations (millions)	11.3		11.3		11.3		11.3		3.587		1.163	
R-squared	0.028		0.841		0.864		0.872		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	