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Pay, Employment, and Dynamics of Young Firms*

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Abstract

Why do young firms pay less? Using confidential microdata from the US Census Bureau, we find lower earnings among workers at young firms. However, we argue that such measurement is likely subject to worker and firm selection. Exploiting the two-sided panel nature of the data to control for relevant dimensions of worker and firm heterogeneity, we uncover a positive and significant young-firm pay premium. Furthermore, we show that worker selection at firm birth is related to future firm dynamics, including survival and growth. We tie our empirical findings to a simple model of pay, employment, and dynamics of young firms.

Keywords: Young-Firm Pay Premium, Selection, Worker and Firm Heterogeneity, Firm Dynamics, Startups

JEL classification: J30, J31, D22, E24, M13

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1 Introduction

Young firms account for 11 percent of US employment and are credited with a disproportionate share of total job creation.[1] Given the importance of young firms in generating jobs, extensive literature has focused on the employment dynamics of new businesses. However, much less is known about the quality of jobs at young firms. Previous work has documented that workers earn less at young firms (Brown and Medoff, 2003) and has offered two competing explanations for this correlation. The first explanation pertains to preferences: employees at young firms may enjoy nonpecuniary benefits such as autonomy and flexibility in lieu of monetary compensation (Hurst and Pugsley, 2011). The second explanation pertains to constraints: credit limits may lead young firms to offer lower starting wages (Michelacci and Quadrini, 2009) and asset-poor workers to opt for lower-paying but more readily available jobs at young firms (Dinlersoz et al., 2019). In this paper, we pursue a third and entirely different explanation for low pay at young firms: simultaneous worker and firm selection.

To uncover the relative pay of young firms in the presence of worker and firm selection, we follow a theory-guided empirical approach. We first present a simple model of the labor market, in which young firms, on average, employ lower-ability workers ("worker selection"), have lower productivity ("firm selection"), and have a higher probability of exiting the market that leads them to pay a premium. We then put these model predictions to the test in the data. Using administrative linked employer-employee records from the US Census Bureau that span almost two decades, we track millions of worker careers across young and old firms. The two-sided panel nature of the data allows us to identify permanent worker and firm pay heterogeneity separately from the firm age-pay component. Without additional controls, we confirm lower pay at young firms, in line with previous survey data evidence by Brown and Medoff (2003). However, controlling for time-invariant and time-varying worker and firm characteristics, we find a positive and significant young-firm pay premium. We also show that worker selection at firm birth is related to future firm dynamics, including survival rates and employment growth. We tie these empirical findings back to our model and alternative theories of pay, employment, and dynamics of young firms.

Previous research on the young-firm pay premium has faced two major challenges. The first challenge is related to data availability. Obviously, a study of young firms' pay policies requires

[1] See Decker et al. (2014) for a comprehensive survey of the role of young businesses in the US economy. Haltiwanger et al. (2013) discuss patterns in job creation and exit rates across firm age groups. Young firms are also important drivers of innovation and productivity growth (Acemoglu et al., 2018), resource reallocation (Foster et al., 2008), and trends in business dynamism (Decker et al., 2018).

a reliable measure of firm age. However, measurement of firm age is often problematic because many datasets do not distinguish between establishment and firm births, or between new firm formation and changes in ownership or legal structure. This distinction is critical because the majority of jobs created in new establishments belong to incumbent firms (Haltiwanger et al., 2013). The second challenge is methodological. Going back to the seminal contribution by Abowd, Kramarz, and Margolis (1999, henceforth AKM), it is now well understood that permanent heterogeneity among both workers and firms plays a prominent role in explaining empirical pay dispersion. When using only cross-sectional data, however, it is impossible to distinguish between high-pay workers, high-pay firms, and firm-age-specific pay policies in the presence of selection. We overcome both of these challenges by estimating an augmented model in the spirit of AKM using microdata based on two confidential databases from the US Census Bureau: linked employer-employee data from the Longitudinal Employer-Household Dynamics program (LEHD) and business register data from the Longitudinal Business Database (LBD). To the best of our knowledge, we provide the first estimates of the young-firm pay premium while accounting for worker and firm selection in the US.

Before launching into our empirical analysis, we present a simple theoretical model of worker and firm selection to structure our analysis and aid interpretation of our findings. In the model, workers of different ability are matched to firms of different productivity and age subject to search frictions. When a new firm is started, it receives a productivity draw and recruits workers. But instead of attracting a representative sample of job applicants, firms recruit individuals of a single random ability level. A notable feature of our model is that future productivity of a firm, and hence its survival and growth prospects, may depend on initial workforce composition. Before production occurs, young firms learn about their cost structure, leading some to exit and expose their workers to unemployment risk. Bargained earnings in this environment are log-additively separable into three terms: a worker component, a firm component, and a positive young-firm pay premium that compensates workers for higher exit risk.

Guided by our theoretical model, we turn to measuring the young-firm pay premium in the data. First, confirming previous survey data evidence by Brown and Medoff (2003), we find that young firms up to three years old pay 30.7 log points lower earnings compared to older firms.[2] However, we argue that these estimates are subject to worker and firm selection. To address the

[2]We find consistent results when repeating our analysis using a more narrow age range of 0–1 years to define young firms and under a more gradual definition of firm age using two-year bins from birth to 20 or more years old.

issue of selection, we follow recent work employing the two-way fixed effects framework developed by AKM.[3] This approach relies on following individuals across employers over time in order to separately identify worker and firm fixed effects in earnings. We augment this framework with indicators for firm age groups, which are identified by tracing individual firms as they grow older. We operationalize variants of this augmented AKM equation on our linked employer-employee data by repeatedly estimating the young-firm pay premium with sequentially added controls (Altonji et al., 2005; Oster, 2019).

Our first main result is that the apparent pay penalty at young firms turns into a positive and significant young-firm pay premium after controlling for relevant dimensions of worker and firm heterogeneity. Adding worker fixed effects alone accounts for more than two-thirds of the original point estimate, resulting in a young-firm pay penalty of 8.7 log points. Put differently, by following workers over time, we learn that workers employed at young firms are permanently low-paid, even when employed at older firms. Adding additional time-varying worker controls, including education-specific age profiles, the young-firm pay penalty decreases further in magnitude to 7.8 log points, consistent with young firms' hiring younger and less-educated workers. In our preferred specification, which adds firm fixed effects, we estimate a moderate but significant young-firm pay premium of 0.7 log points. We interpret the increase in the point estimate after inclusion of firm fixed effects as the pool of young firms being skewed toward permanently low-paying firms. Following the pay policy over a firm's life cycle reveals a negative relation between firm age and pay. We conclude from these findings that simultaneous worker and firm selection mask the true young-firm pay premium.

It is well documented that most young firms are born small and that firm size has a significant life cycle component (Bartelsman et al., 2005). At the same time, there is still significant variation in size conditional on age (Pugsley et al., 2018). Consequently, we investigate whether the positive young-firm pay premium could be confounding firm age and size. To this end, we add controls for firm size to our empirical model. In the raw data, firm employment significantly mediates the cross-sectional relation between firm age and pay. Among employers with the same number of workers, young firms pay 13.1 log points less than old firms. However, when controlling for worker and firm heterogeneity as before, we find a positive and significant premium of 1.7 log

[3]Variants of the AKM methodology have been fruitfully employed in a number of contexts, including the sources of earnings heterogeneity (AKM; Abowd et al., 1999b, 2002; Card et al., 2013, 2016; Alvarez et al., 2018; Song et al., 2019; Bonhomme et al., 2019), teacher-classroom assignments (Burke and Sass, 2013), chief executive officers' effect on firm performance (Bertrand and Schoar, 2003), and the formation of bank-firm loan relationships (Gao et al., 2017).

points paid by young firms. This shows that firm age is a pay-relevant characteristic that is distinct from firm size. After accounting for the latter, we find a larger young-firm pay premium.

Our second main result is that initial workforce composition is significantly related to future dynamics of young firms. To arrive at this conclusion, we first compute mean estimated AKM worker fixed effect estimates among the workforce of every firm over time. We find substantial variation in mean worker fixed effects across firms and a clear pattern across firm age groups. Average worker fixed effects are 17.6 log points below the population average at firms 0–1 years old, but around the population average at firms 8–13 years old, and 17.7 log points above the population average at firms with 16–17 years old, before declining again for the oldest age groups. We then show that worker selection and firm dynamics are closely related. While most firms initially hire low-fixed-effect workers and exit within a couple of years, newly established firms that survive for 10 or more years start with workers with significantly higher permanent pay components. We formalize this finding in a regression framework with additional worker and firm controls and confirm that firms with higher worker fixed effects among their initial workforce are significantly more likely to survive and grow in the future.

Guided by our theory, we interpret AKM worker fixed effects as proxying for unobserved worker ability. Through the lens of our model, worker talent is scarce and few firms are lucky enough to assemble a high-quality workforce. Hiring workers of higher ability is doubly beneficial in this environment. On the one hand, it shields those employers from cost shocks and potential firm exit by increasing current output. On the other hand, initial worker ability also feeds positively into future firm productivity, leading to higher growth at those employers. While this story parsimoniously explains all of our empirical patterns, our results leave room for alternative explanations, including reverse causality. In such an alternative story, some firms face inherently higher survival and growth prospects. As a result, they have a higher propensity to hire high-ability workers, possibly due to production complementarities between worker and firm types. But one would expect such a theory to imply strong match components in labor demand and consequently pay, for which we find little evidence. We conclude that the human capital of young firms is an important predictor of young-firm dynamics.

We supplement our analysis with several robustness tests. First, we revisit the hypothesis that young firms offer steeper tenure-earnings profiles (Michelacci and Quadrini, 2009). To separate the mean from the trend effects of young firms, we follow Schmieder (2013) and restrict our sample to only the first quarter of each worker-firm match, resulting in a slightly higher young-firm pay premium of 2.6 log points in our preferred specification. When we control for worker tenure interacted with a young-firm indicator, we find that—consistent with Michelacci and Quadrini (2009)—earnings are more backloaded at young firms, which are, on average, more credit constrained (Evans and Jovanovic, 1989). However, tenure-earnings profiles are actually flatter at young firms when controlling for worker and firm heterogeneity. Thus, to the extent that credit constraints are operative among young firms, rationing seems to occur on the extensive margin.

Second, to probe the generality of our findings, we reestimate our main specification for the subpopulations of college-educated workers and those in high-technology sectors. While we still find selection to be important, the young-firm pay premium is also lower for these subpopulations in line with our theory. This is so plausibly because these workers have higher bargaining power (Cahuc et al., 2006) and face lower employer exit risk (Agarwal and Gort, 1996, 2002).

Finally, we perform a battery of diagnostic tests that lend support to our augmented AKM specification. Specifically, we build on Card et al. (2013) to argue in favor of log-additive separability of the earnings equation and provide evidence against endogenous mobility based on drift or transitory components of the error term. We also show that our results are robust to different sources of worker mobility, including their previous employer status.

Related Literature. Our paper contributes to three strands of the literature. The first strand we contribute to is concerned with the relative pay of young employers. To the best of our knowledge, our paper is the first to estimate the young-firm pay premium while accounting for worker and firm selection in the US, where prior work has been confined to survey data evidence or firm-level analyses. The seminal reference is from Brown and Medoff (2003), who collected information on employer age in a special supplement to the Survey of Consumers that had around 1,000 respondents. They noted that the positive gradient between firm age and pay in the raw data turns insignificant or negative when controlling for worker characteristics, including education, experience, tenure, race, gender, marital status, and occupation. Their finding challenged conventional wisdom, based on previous evidence by Dunne and Roberts (1990), who used the Annual Survey of Manufactures, that older employers pay more, even after controlling for establishment size, industry, and region. Other early works that drew similar conclusions include Davis and Haltiwanger (1991), Doms et al. (1997), and Troske (1998). Brown and Medoff (2003) called attention to the important issue of worker sorting across firms. But they were constrained by their small sample size and cross-sectional nature of their data, making it impossible to distinguish between worker and firm selection, and between selection on observables and unobservables in relation to the young-firm pay premium. This is an important omission given recent evidence of assortative matching between workers and firms in the US labor market (Lamadon et al., 2019). Indeed, we find that in our data most of the cross-sectional young-firm pay penalty is explained by permanent, unobserved worker heterogeneity.

A handful of prior studies have estimated the young-firm pay premium using linked employee-employer data from Europe. An inconsistent picture emerges from these studies due to differences in contexts and methods. For Sweden, for example, Heyman (2007) finds a weak positive relationship between firm age and pay using three years of cross-sectional data, while Nyström and Elvung (2014) find mixed results using a propensity score matching approach. For Denmark, Burton et al. (2018) find a pay premium at young firms in a specifications with worker matching based on age, gender, education, firm size, prior job, and prior earnings. For Germany, Brixy et al. (2007) find an 8 percent young-establishment pay penalty in a cross-sectional study, while Schmieder (2013) finds a 10 percent pay premium after controlling for worker and firm fixed effects. Finally, using a 1 percent random sample of employees in the United Kingdom, Adrjan (2018) finds that young firms pay a small premium for new hires.

One benefit of our approach lies in estimating the young-firm pay premium while accounting for unobserved worker and firm heterogeneity, which have been shown to matter empirically by many studies since AKM. Yet few works have applied this insight to the study of young firms. We augment their classical two-way fixed effects framework by allowing for firm age-pay profiles under the assumption that they remain constant after a certain firm age. In doing so, we provide the first selection-corrected estimates of the young-firm pay premium for the US.

Perhaps the paper closest to ours is Schmieder (2013), which applies a similar AKM methodology to a 2 percent random sample of German administrative data. While the paper documents a pay premium at younger single-establishment firms in Germany, consistent with our results for the US, there are several notable differences. First, our paper focuses on the sorting of workers into young firms and its relation to firm dynamics, which we formalize in a simple model of two-sided selection. Schmieder (2013), on the other hand, is primarily concerned with evidence of upward-slowing labor supply curves in relation to existing monopsony theories of monopsony. Second, without worker and firm fixed effects, Schmieder (2013) documents different establishment age-pay and establishment size-pay relationships than what has been found in the US, pointing toward unique characteristics of the German labor market in this context. Finally, an advantage of our approach vis-à-vis Schmieder (2013) and prior work on the US is that we track a much larger sample, consisting of millions of unique workers and hundreds of thousands of unique firms.

The second strand of literature we contribute to is concerned with theories of who works at young firms and why. Existing work has proposed two reasons for lower pay at young firms. The first reason pertains to preferences. Workers at young firms, like entrepreneurs, may enjoy non-pecuniary benefits (Evans and Leighton, 1989; Blanchflower and Oswald, 1992; Hamilton, 2000; Moskowitz and Vissing-Jørgensen, 2002; Hurst and Pugsley, 2011, 2017; Catherine, 2019) or have a preference for working at young firms (Kraus and Litzenberger, 1976; Roach and Sauermann, 2015). The second explanation pertains to constraints. Credit limits may lead young firms to offer lower starting wages (Azariadis, 1988; Michelacci and Quadrini, 2009; Guiso et al., 2013; Moser et al., 2019) and asset-poor workers to accept low-paying jobs (Herkenhoff et al., 2018; Herkenhoff, forthcoming; Dinlersoz et al., 2019). We pursue simultaneous worker and firm selection as a third and entirely different explanation for low pay at young firms. Contrary to the premise of much previous work, we find that after controlling for appropriate dimensions of worker and firm heterogeneity, the young-firm pay penalty actually turns into a positive young-firm pay premium. Thus, no additional economic justification is required to explain why individuals work at young firms.

The third strand of literature we contribute to studies worker and firm dynamics. The classical firm dynamics model by Hopenhayn (1992) features no between-firm pay differences due to the assumption of competitive labor markets, making it an unsatisfactory point of departure for our analysis. Conversely, many seminal models of wage determination such as Burdett and Mortensen (1998) do not feature any meaningful firm dynamics. A small number of papers study frictional wage dispersion with endogenous firm dynamics (Kaas and Kircher, 2015; Gavazza et al., 2018; Engbom, 2019; Gouin-Bonenfant, 2019). Unfortunately, none of these papers allow for worker heterogeneity, which we find to be important for the young-firm pay premium. Although a quantitative general-equilibrium framework is beyond the scope of our paper, we develop a simple model of the labor market to guide our analysis. A notable feature of our framework is that future productivity of a firm, and hence its survival and growth prospects, may depend on initial workforce composition—a prediction for which we find strong empirical support. In contemporaneous work, Choi et al. (2019) hypothesize that organizational capital may be embodied in a firm's founding members. While they do not study the role of worker and firm selection vis-à-vis the young-firm pay premium, we argue that our empirical findings are consistent with theirs.

2 Why Do Young Firms Pay Less?

In this section, we present a theoretical model of worker and firm selection. The goal is to provide a simple framework that allows us to interpret our main empirical findings in light of this theory.

2.1 A Model of Worker and Firm Selection

Consider a two-period economy with time indexed by $t \in \{1, 2\}$. The economy is populated by two types of agents, a fixed unit mass of heterogeneous workers and a mass of heterogeneous firms. We first describe the two types of agents in the economy, outline the timing, then discuss how matches are formed and how pay is set.

Workers. Workers differ in their ability θ. At any time, they are either employed or unemployed. Employed workers consume some labor income w_t that depends on their own ability as well as the productivity and age of their employer. The unemployed consume an exogenous amount $b\theta$, for some $b > 0$, from home production. Workers have linear period consumption utility.

Firms. Firms differ in their productivity $p_t \sim G_t(\cdot)$ and age group $a_t \in \{\text{young}, \text{old}\}$. New firms can be created in the beginning of a period by paying an entry cost in exchange for a productivity draw. After entering and recruiting l_θ workers of ability θ before production, young firms learn their operating cost $c \sim O(\cdot)$ as in Jovanovic (1982). At this point, firms choose between either paying the cost and continuing operations or shutting down, which results in an ex-ante probability of exit $x_t(p_t, \{l_\theta\}_\theta) > 0$. Continuing firms produce output according to the technology $y(p_t, \{l_\theta\}_\theta) = p_t \int l_\theta \theta d\theta$. Importantly, a firm's future productivity depends on the average ability of its initial workforce, $\overline{\theta}$, through the function $p_2(p_1, \overline{\theta})$, which is increasing in both arguments.

Timing. At the beginning of period 1, all workers are unemployed and there are no old firms. New firms are created with idiosyncratic productivity and match with the unemployed. At this point, worker-firm bargaining and home production occurs. Next, young firms learn about their cost and decide between exiting and continuing. Exiting firms send their workers back into unemployment. Continuing firms produce output and pay their employees. At the beginning of period 2, employed and unemployed workers enter from the previous period. Continuing firms are classified as old and a set of new firms enter as young. Firms hire unemployed workers, bargaining occurs, and home production is realized. Finally, young firms learn about their cost and some young firms exit, output is produced, and the employed are paid.

Matching. In both periods, firms and unemployed workers meet in a labor market characterized by search frictions, which we think of as capturing incomplete information, adjustment costs, or idiosyncratic mobility shocks. New firms (in periods 1 and 2) and old firms (in period 2) are randomly assigned a worker type θ. They then compete with other firms for the $u_{\theta,t}$ unemployed workers in a market specific to that worker type by choosing a recruitment intensity.[4] A firm that posts $v_{\theta,t}$ vacancies subject to convex increasing costs $\phi(v_{\theta,t})$ hires $h_{\theta,t}(v_{\theta,t}) = u_{\theta,t}v_{\theta,t}/V_{\theta,t}$ workers, where $V_{\theta,t} = \int v_{\theta,t}(p)dG_t(p)$ denotes aggregate vacancies posted by all firms in the market.

Pay Determination. Frictions in the labor market imply that there are rents to be shared between matched workers and firms. We assume that pay in both periods is set through intraperiod bargaining that depends on worker ability, firm productivity, and firm age.[5] Specifically, the pay of a worker with ability θ employed at a firm with productivity p and age a in period t solves $w_t(\theta, p, a) = \arg\max_w C(w;\theta, p, a)^{\beta} J(w;\theta, p, a)^{1-\beta}$, where $C(w;\theta, p, a)$ is the expected surplus of a worker paid w, $J(w;\theta, p, a)$ is the expected surplus of a firm paying w, and $\beta \in (0, 1)$ denotes workers' bargaining weight. Appendix A demonstrates that the solution to the bargaining problem results in the following model earnings equation:

$$\ln\left(w_t\left(\theta, p, a\right)\right) = \underbrace{\ln\left(\theta\right)}_{\text{"worker fixed effect"}} + \underbrace{\ln\left(\beta p + \left(1-\beta\right)b\right)}_{\text{"firm fixed effect"}} + \underbrace{\mathbf{1}\left[a = young\right]\ln\left(\frac{\beta p + \frac{(1-\beta)b}{1-x_t(p,\{l_\theta\}_\theta)}}{\beta p + \left(1-\beta\right)b}\right)}_{\text{"young-firm pay premium"}}. \quad (1)$$

The model earnings equation (1) is log-additively separable into three terms: a "worker fixed effect" that depends only on worker ability, a "firm fixed effect" that depends on firm productivity, workers' outside options and their bargaining power, and a "young-firm pay premium."

2.2 Key Theoretical Predictions

We provide further details of the model and its solution in Appendix A. Here, we outline five key theoretical predictions about the pay, employment, and dynamics of young firms. The first

[4]Our framework could accommodate job-to-job transitions without changing the main insight.

[5]Consistent with our model featuring a finite number of periods, we adopt a static bargaining protocol so as to not conflate firm age with time in the model. Exploiting the linearity of the production function across worker types allows us to solve for the bargaining solution separately by worker type.

prediction concerns the heterogeneity of pay across workers and firms:

Prediction 1. *Higher-ability workers command proportionately higher pay at any given firm, and higher-productivity firms pay proportionately more to any given worker.*

While in a frictional labor market workers are paid below their marginal product, Prediction 1 states that both worker and firm characteristics enter into earnings equation (1). The existence of worker and firm pay components is a notable departure from the competitive-markets benchmark in which all individuals get awarded their marginal product, regardless of where they work. Although it is impossible to disentangle these pay components using just cross-sectional data, we will demonstrate that following workers and firms over time allows us to separately identify them.

The second prediction concerns the relative pay of young versus old firms:

Prediction 2. *Young firms pay a premium to workers relative to otherwise identical old firms.*

The young-firm pay premium mentioned in Prediction 2 reflects the compensation that workers receive for unemployment risk due to the higher exit probability of young firms. In anticipation of forgone consumption from future unemployment when starting at a young firm, workers bargain for higher pay. In the data, the young-firm pay premium may be masked by the convolution of worker and firm selection, which we describe in detail below. We can uncover the true firm-age pay premium, however, by controlling for other observed as well as unobserved dimensions of worker and firm heterogeneity.

The third prediction regards young firm dynamics in relation to initial worker composition:

Prediction 3. *Young firms that have higher productivity or initially hire higher-ability workers are more likely to survive and grow.*

A notable feature of our model is the dependence of future firm productivity on initial workforce ability, which shields young firms from cost shocks and gives rise to Prediction 3. Inferring worker quality from data can be a difficult task absent precise human capital proxies. However, through the lens of our model, worker pay fixed effects directly correspond to unobserved worker ability. Consequently, the model suggests a positive relation between the permanent pay component of a firm's initial workforce and its future survival and growth prospects.

The fourth prediction concerns "worker selection" at young versus old firms:

Prediction 4. *Young firms employ workers with lower ability on average.*

The worker selection described in Prediction 4 arises because, as a corollary of Prediction 3, the unemployment pool will be skewed toward lower-ability workers. While surviving old firms on average hired relatively high-ability workers in the past, young firms recruit exclusively from the negatively selected unemployment pool, leading to a positive correlation between firm age and average worker ability. Since most young firms hire low-ability workers who are permanently low-paid, they may misleadingly appear to be low-paying in cross-sectional data.

The fifth prediction concerns "firm selection" over the life cycle of a firm:

Prediction 5. *Young firms have lower productivity on average.*

The firm selection described in Prediction 5 results from dynamic attrition as firms age. Since the least productive among all young firms are most likely to exit when a cost shock materializes, the pool of old firms consists of a positively selected subset of previously young firms. Because most young firms have low productivity, one may erroneously confound the firm age-pay component with permanent firm heterogeneity in the cross-section.

Altogether, the five key theoretical predictions call for an empirical specification with controls for (unobserved) worker and firm heterogeneity in addition to firm age. Beyond these key predictions, the model is consistent with several other empirically relevant patterns, which we will investigate in the data. First, if the initial distribution of job seekers is skewed toward low-ability workers and most new business ideas are not productive, then the majority of young firms will be low-paying and likely to exit. Second, if the unemployment pool composition is constant over time, then there will be mean reversion in average workforce quality over a firm's life cycle due to repeated sampling of worker skills over time. Finally, we expect the young-firm pay premium to be lower for workers with higher bargaining power, such as college graduates, and those in industries with higher firm survival rates, such as workers in high-technology sectors.

3 Data and Methodology

3.1 Data Description

Our analysis is made possible by combining two confidential databases from the US Census Bureau. The first database contains employer-employee matched records including earnings information, while the second database allows us to track businesses over their life cycle. The combined dataset allows us to investigate the sources of pay differentials between young and old firms.

Longitudinal Employer-Household Dynamics program (LEHD). Our analysis crucially relies on the ability to track workers across firms over time. For this, we use restricted-use microdata from the LEHD. Within covered states, this linked employer-employee dataset is constructed from administrative unemployment insurance records of states participating in the program. The data track nearly 100 percent of private employees across employers on a quarterly basis. Data coverage starts in 1990 for some states, while other states' coverage begins later.

We have access to microdata for 31 states covering over 60 percent of US private sector employment, which translates into billions of observations over the data sample period.[6] Our analysis of such a large sample is subject to computational constraints on the Census's administrative system, leading us to seek an adequate sampling procedure. As we explain later, the estimation strategy requires the inclusion of workers at firms that are connected through worker mobility. Instead of randomly selecting workers across 31 states, which would skew our sample toward large firms (Woodcock, 2005) and could exacerbate limited-mobility bias (Andrews et al., 2008), we restrict attention to a representative subset of states, for which we keep the universe of all workers and firms. Specifically, we sort all 50 states and the District of Columbia by the fraction of employment in firms up to three years old and choose the three states that land at the tenth, median, and ninetieth percentiles: Vermont, Maryland, and Colorado.

This sampling procedure ensures that most observations within a state are included in the connected set of workers. Including small firms is crucial for our analysis since most firms are born small. For each individual, we record the logarithm of quarterly real earnings (or, in short, "earnings") at their current employer. Earnings data in the LEHD include all forms of compensation that are immediately taxable. Stock options are typically taxed when exercised and at this point appear in our earnings measure.[7] Because our data do not contain information on equity ownership, we do not separate between founders and nonfounders. Both are included in our data, although most employees are nonfounders: the median new firm has six employees in our sample but according to previous work has only two founders (Parker, 2009).[8] The LEHD also allows us to observe the age, gender, race, place of birth, and education of each employee.[9]

[6]See Abowd et al. (2009) for a detailed description of the LEHD program and the datasets generated from it. A map showing the 31 states available in our data is contained in Figure 4 of Appendix B.

[7]The possibility that young firms offer additional remuneration not reported in administrative data, as documented by Hurst et al. (2014), would further strengthen the main conclusions of our analysis. While there is some evidence that larger firms offer greater fringe benefits such as pensions (Freeman, 1981; Brown and Medoff, 1989), which fall outside of our income concept, our main conclusions are robust to controlling for firm size.

[8]Azoulay et al. (2018) identify founders as the highest earner at the time of firm birth. However, sorting on pay at new firms is problematic in our empirical setting, given that our dependent variable is earnings.

[9]Education is imputed for employees with missing education data (Abowd et al., 2009) .

To construct our baseline sample, we start with all workers ever observed in one of the three selected states. We retrieve these workers' entire work histories in the LEHD from 1990 through 2006. We end our sample in 2006 to leave enough time to measure future firm outcomes. Earnings are normalized to constant 2014 US dollars. As is standard in the literature, for each worker-quarter combination, we keep the observations with the highest earnings. To limit the influence of outliers, we drop observations with earnings growth exceeding 5,000 percent in a given year.

The LEHD data allow us to observe quarterly earnings with no information on weeks worked. To account for the mechanical effect of job mobility within a quarter on earnings, we drop observations that do not have the same worker-firm pair in both the preceding and the subsequent quarter. This adjustment is important, given that worker transitions between jobs not occurring at the exact start of a new quarter would lead to a downward bias in earnings around a job change. A potential downside of this adjustment is that we undersample workers with especially high turnover rates, specifically those who switch jobs twice in two subsequent quarters. Furthermore, to be consistent with previous work that uses annual income reports and to minimize the computational requirements of a large sample size, we retain only the first quarterly earnings report for each worker-year combination.

Longitudinal Business Database (LBD). Our analysis requires us to reliably identify firm age. To this end, we supplement the LEHD data with firm-level information from the LBD. The LBD is a panel dataset that tracks the universe of US business establishments with at least one paid employee (Jarmin and Miranda, 2002). An establishment is any separate physical location operated by a firm with at least one paid employee. In addition, the LBD contains a unique firm-level identifier, which longitudinally links establishments that are part of the same firm. A representative new firm in our sample will be an incorporated business with a few employees and a physical office location. This is distinct from the self-employed entrepreneurs who Hurst and Pugsley (2011, 2017) and Levine and Rubinstein (2017) argue have little desire to grow and are unlikely to create economic benefits beyond themselves. The LBD contains information for all 50 US states and the District of Columbia on the number of employees, total payroll, entry, exit, and age of the establishment. Using the LBD has at two notable advantages.

First, the longitudinal linkage between establishments and their parent firm allows us to measure firm age. This is an important advantage over data that only allow us to infer establishment age. The distinction between firm age and establishment age is quite important in our setting because the majority of new establishments are new branches of incumbent firms. A new establishment of an incumbent firm may resemble a more mature enterprise than the establishment age would suggest. Following Haltiwanger et al. (2013), we define firm age using the oldest establishment that the firm owns in the first year the firm is observed in the LBD. As firm birth is defined at the time of founding, we avoid misclassifying an establishment that changes ownership as a newly born firm. Similarly, we track true firm age for companies that are legally acquired by another company or those that change legal status.

Second, the full geographic coverage of the LBD allows us to measure firms' total employment by summing employment levels at each of its establishments. Many outcomes of interest—including entry, exit, and growth—are at the firm level rather than at the establishment level. But this distinction is often hard to make in data that do not separately identify establishments from firms (Schmieder, 2013).

3.2 Empirical Strategy

Motivated by our theoretical model in Section 2, we identify pay policies of young firms by augmenting the classical two-way fixed effects framework developed by AKM to allow firm pay policies to vary with firm age. We assume that earnings of individual i in year t at employer $j = J(i.t)$ are determined by the following equation:

$$y_{it} = \alpha_i + \psi_{J(i,t)} + \eta_t + X_{it}\beta + FirmAge_{J(i,t)t}\gamma + \varepsilon_{it}, \tag{2}$$

where y_{it} denotes earnings, α_i are worker fixed effects, $\psi_{J(i,t)}$ are firm fixed effects, η_t are year fixed effects, X_{it} is a matrix of time-varying observable individual characteristics, $FirmAge_{J(i,t)t}$ is a vector of indicators for firm age groups, and ε_{it} is an error term. We are interested in the coefficient vector γ on different firm-age groups. As a baseline, we will consider $FirmAge_{J(i,t)t}$ to contain an indicator for firms up to three years old, which we will classify as "young firms," while other firms are classified as "old firms." In alternative specifications, we vary this firm age cutoff and also estimate a specification containing separate indicators for two-year bins between firm birth and 20 or more years of age.

In equation (2), worker fixed effects capture the time-invariant component of pay due to individual heterogeneity rewarded proportionately across employers, which could be due to innate ability and other individual characteristics. In comparison, firm fixed effects capture the time-invariant component of pay due to employer heterogeneity awarded equally to all employees, which could be driven by differences in productivity, rent sharing agreements, or workplace amenities. Abowd et al. (2002), Sorkin (2018), and Song et al. (2019) find significant between-firm pay differentials for identical workers in the US labor market. Year fixed effects control for time-varying changes in earnings common to all workers at a given point in time, including the mean shift in earnings due to business cycle fluctuations. The set of time-varying worker controls, which include squared and cubic terms of age interacted with education levels, capture skill-specific human capital accumulation over an individual's lifetime.[10]

Building on the insight by Abowd et al. (2002), our augmented AKM specification is identified only within a set of firms and workers connected through worker transitions. The largest connected set of workers in our data comprises the vast majority of worker-years and appears otherwise similar to the complete data in terms of observable worker and firm characteristics. Worker and firm effects are separately identified for observations contained in the (largest) connected set by using the information on wage changes of workers moving between employers during the sample period. Not all workers are required to move, but for each firm, at least one worker is required to join from or else leave for another firm in the connected set.

By augmenting the standard AKM equation with an additional set of firm age controls, we effectively allow for a common age effect in firm pay policies across employers. For the coefficients on firm age to be identified separately from the other AKM components, we require that age buckets be connected through surviving firms. Identification of the age indicators cannot be achieved if each firm is observed for only one period, as the firm fixed effect would then account for all firm-pay variation. A sufficient condition for identification is for each age bucket below the highest one to contain at least one firm that is observed after graduating into the next higher age bucket. No further restriction on worker mobility between young and old firms is required.

As discussed in Schmieder (2013), identification of a linear firm age coefficient or more flexible specifications is not possible in the presence of firm (cohort) fixed effects and time effects. Therefore, it is not possible to include a fully flexible set of firm age dummies. Instead, we assume that the firm-age-pay profile is flat after a certain cutoff firm age, which we vary between four

[10] Without further restrictions it is not possible to separately identify linear age effects in the presence of individual (cohort) effects and year effects due to the well-known collinearity problem. Following Card et al. (2018), we omit the linear age term and normalize the earnings profile to be flat around age 40 to obtain identification. Note that the inclusion of worker fixed effects will subsume time-invariant worker controls such as education and gender. Similarly, the inclusion of firm fixed effects will subsume time-invariant firm controls such as industry and state fixed effects.

and 20 years.[11] We later show that, consistent with this assumption, raw firm-pay profiles are approximately flat after 20 years of age, with and without various controls.

We estimate equation (2) by ordinary least squares (OLS). To recover unbiased estimates of worker and firm fixed effects, we require that the error term ε_{it} satisfy a strict exogeneity condition:

$$\mathbb{E}\left[\varepsilon_{it}|\, \alpha_i, \psi_j, \eta_t, X_{it}, FirmAge_{J(i,t)t}\right] = 0.$$

As shown by Card et al. (2013), a sufficient condition for this to hold under the usual assumptions is that the assignment of workers across (young and old) firms obeys a strict exogeneity condition:

$$\mathbb{P}\left[J\left(i,t\right)=j|\, \varepsilon_{it}\right] = \mathbb{P}\left[J\left(i,t\right)=j\right] = G_{jt}\left(\alpha_i, \left\{\psi_j\right\}_j\right) \quad \forall i,t.$$

This condition rules out "endogenous mobility" based on the error term ε_{it}, including any match-specific wage component. However, important for our application, this condition is entirely consistent with worker mobility based on worker identity and the identity of all firms in the economy, as captured by the function $G_{jt}(\alpha_i, \{\psi_j\}_j)$. That is, our OLS estimates of pay premia will not be biased by systematic mobility of certain workers across certain (young and old) firms. Specifically, equation (2) is consistent with, say, low-α_i workers' being disproportionately attracted to low-ψ_j or young firms. To test for endogenous mobility, we employ a battery of tests proposed by Card et al. (2013) and used in subsequent work by Card et al. (2016) and Alvarez et al. (2018).[12]

In a second step, we will relate future firm dynamics of employer j to their initial worker composition at firm birth. As we will shortly see, unobservable time-invariant worker heterogeneity, which could be seen as a proxy for worker ability and human capital, will play an especially important role in explaining cross-sectional pay differences. Consequently, we explore how the initial workforce composition is related to future firm performance of firm j between its birth year t and some future date t' by running the following regression:

$$z_{jt'} = \delta(\overline{\hat{\alpha}_i})_j + Z_{jt}\zeta + \omega_{jt'}, \tag{3}$$

where $z_{jt'}$ is a firm dynamics outcome, such as survival or employment, $(\overline{\hat{\alpha}_i})_j$ is the mean estimated

[11] Schmieder (2013) follows a similar identification strategy, assuming that firm age-pay profiles are flat from age 30 onward. Technically, we also obtain identification of firm age effects separately from firm fixed effects and year effects by grouping firm age into two-year intervals and assuming constant firm-age effects within those intervals.

[12] See Section 5.2 for an event-study analysis of pay changes in relation to worker mobility and invariance of our results to different sources of worker transitions.

AKM worker fixed effect among firm j's initial workforce, Z_{jt} is a vector of observable employer characteristics, and $\omega_{jt'}$ is an error term. We are interested in estimates of the coefficient on the mean worker fixed effect, δ, in equation (3). To the extent that worker fixed effects proxy for ability, δ captures the relation between initial workforce quality and future business performance.

3.3 Summary Statistics

Our baseline sample is a panel of 48.4 million worker-year observations over 1990–2006. This includes 7.1 million unique workers and 345,000 unique firms.[13]

To motivate our analysis, we plot average earnings of employees in our sample by two-year bins from firm birth to 20 or more years of age in Panel A of Figure 1. Consistent with the findings in Brown and Medoff (2003) from survey data, we document that employees at young firms receive lower earnings as compared to employees at older firms. Specifically, employees at firms 0–1 years old have mean earnings almost USD 2,500 or 20 percent below the sample mean of USD 8,536. For intermediate groups, mean earnings grow monotonically with firm age.

An immediate suspicion is that low-paid workers at young firms may be compensated with faster future earnings growth. To test for this, Panel B of Figure 1 plots the one-year earnings growth rates for new hires as well as stayers, by employer age. Inspection of the figure shows a nuanced pattern of mean wage growth across firm age groups. Earnings growth is close to the sample mean of 4.6 percent for the subpopulation of firms up to three years old. Overall, the variation in earnings growth rates as well as the gradient across firm age is rather small compared to the large cross-sectional pay difference we document.

In Table 1, we report summary statistics for firms in Panel A and for workers in Panel B. Starting with the firm-level analysis in Panel A, column 1 reports mean values and standard deviations calculated across all firm-years in our sample. In column 2, we report statistics for old firms that we define to be firms at least four years old. In column 3, we report the same statistics for young firms, which are firms aged three years or less. As expected, new firms employ significantly fewer workers, a median (mean) of 6 (15) employees compared to nearly 13 (210) employees at old firms.[14] In terms of observable worker composition, young and old firms employ a similar-looking pool of employees. New firms are slightly more likely to employ male workers, with 54.5 percent of males compared to 52.8 percent males at old firms, and are less likely to employ college-

[13] All observation counts and estimates are rounded according to the US Census Bureau's disclosure policies.

[14] In accordance with the US Census Bureau's confidentiality rules, medians are calculated as the average over observations within the interquartile range.

educated workers, with 32.3 percent compared to 36.4 percent of college-educated workers at old firms. Altogether, the raw data suggest a large pay gap between young and old firms that is not readily explained by worker composition based on observable worker attributes, leading us to investigate the role of unobservable firm characteristics.

Turning to the worker-level analysis, column 1 in Panel B of Table 1 reports summary statistics for all worker-years in our sample. Column 2 pertains to employees at old firms, while column 3 provides statistics for employees at young firms. We find that earnings are substantially lower but earnings growth is essentially no greater at young firms relative to old firms. Employees at young firms have lower mean tenure at 3.2 years compared to 5.9 years, but have a similar representation of male and college-educated workers relative to old firms.[15] While there are some notable differences in observable worker attributes across young and old firms, this leaves room for sorting on unobservable characteristics.

In Table 2, we report summary statistics separately for employees who ever move between firms (column 2) and those who stay at the same firm throughout the sample period (column 1). These statistics are informative of the observable attributes of job switchers, including those who will start work at a new firm. Importantly, our identification strategy does not rely on movers being similar to stayers in either observable or unobservable characteristics. However, we rely on the identifying assumption that the expected gains from moving between firms are the same for movers and stayers. Job movers tend to be younger, have less tenure, earn less, and have higher earnings growth, consistent with findings in Topel and Ward (1992). During the 17-year time window we study, most workers make at least one job transition, with job movers making up 74 percent of all worker-year observations.

4 Pay and Dynamics of Young Firms

4.1 The Young-Firm Pay Premium

We now turn to the results from estimating the augmented AKM equation (2). Our focus is on estimates of the coefficient γ on a young-firm indicator, which equals one for employers up to three years of age.[16] Guided by our theory in Section 2, we postulate that specifications without the full

[15]For this statistic only, we define tenure as the completed length of employment at the current firm, including future employment. Hence, mean tenure may exceed young firms' mean age, which is mechanically capped at three years.

[16]Our results are not sensitive to the exact definition of what constitutes a "young" firm. We find consistent results when defining a young firm as being up to one year old (Table 11 in Appendix B) or using a more gradual definition of

set of controls for unobserved worker and firm heterogeneity are misspecified, leading to biased estimates of young firms' pay policies. We uncover the true young-firm pay premium (if positive) or penalty (if negative) by simultaneously controlling for both worker and firm heterogeneity.[17] All standard errors are double-clustered at the firm and worker level.

We begin by estimating the young-firm pay premium with only year fixed effects as controls. We then add worker fixed effects to control for time invariant worker heterogeneity. Next, we add controls for time-varying worker characteristics to account for life cycle patterns in pay. In our preferred specification, we add firm fixed effects to control for permanent firm heterogeneity. We then repeat our estimates with additional controls for firm size. Finally, we repeat our estimates while allowing for a more flexible firm age profile of pay.

Cross-Sectional Estimates. Column 1 of Table 3 shows that workers at young firms earn 30.7 log points less compared to those at old firms, suggesting a sizable pay penalty at young firms consistent with the results in Brown and Medoff (2003) and Ouimet and Zarutskie (2014).

Controlling for Permanent Worker Heterogeneity. In column 2 of Table 3, we include worker fixed effects to control for time-invariant worker characteristics by following individuals who move across employers. As a result, the estimated pay penalty of young firms drops by more than two-thirds of its original magnitude, to around 8.7 log points. We interpret this as young firms' disproportionately employing workers who are low paid regardless of where they are employed. For workers who switch between old and young firms, there is a sizable, yet significantly smaller pay penalty than the cross-sectional comparison would suggest. We also note a dramatic increase in the R^2 of this regression, to 75 percent, suggesting that time-invariant worker traits explain a sizable share of the earnings variation.

Additional Controls for Observable Worker Characteristics. In column 3 of Table 3, we add controls for observable time-varying worker characteristics, including squared and cubic terms of age interacted with education levels. Similar to our findings on young firms' propensity to hire workers with lower-paid time-invariant characteristics, we find that young firms also dispro-

firm age using two-year bins from birth to 20 or more years of age (Table 5).

[17]We follow Altonji et al. (2005) and Oster (2019) in presenting estimation results with sequentially added controls. As is well-known, even under the assumption that earnings equation (2) is the true data-generating process, coefficient estimates will remain biased when not all relevant controls are included. The size and direction of the bias will depend on the covariance structure of the included and omitted variables.

portionately employ workers with lower-paid time-varying observable characteristics. Indeed, after controlling for time-invariant and time-varying worker characteristics, the young-firm pay penalty is further reduced to 7.8 log points, while the R^2 increases to 77 percent.

Additional Controls for Permanent Firm Heterogeneity. In column 4 of Table 3, we add firm fixed effects to our previous specification. By simultaneously including worker and firm fixed effects, we are effectively estimating an AKM equation, augmented with a control for firm age. As a result of controlling for time-invariant characteristics by following workers and firms over time, we estimate a moderate but statistically significant pay premium of 0.7 log points. Thus, a worker who switches from an old firm to a young firm of otherwise identical characteristics experiences, on average, a small increase in earnings. Adding controls for time-invariant firm characteristics further increases the R^2 to 81 percent.

Additional Controls for Firm Size. Most young firms are born small, with the median startup employing six workers. Bartelsman et al. (2005) find that employer size varies systematically with employer age in a sample of OECD countries. At the same time, there is ample variation in firm size conditional on firm age (Pugsley et al., 2018). Related work has documented a positive firm size-pay premium, even conditional on worker composition (Brown and Medoff, 1989; Idson and Oi, 1999; Bloom et al., 2018). Furthermore, there is some evidence that larger firms offer hire fringe benefits such as pensions (Freeman, 1981; Brown and Medoff, 1989), which fall outside of our income concept. Consequently, one may wonder how our finding of a young-firm pay premium is related to differentials in pay and benefits across small and large firms.

To distinguish between firm age and size, we add controls for firm employment to our baseline specification in equation (2). Table 4 shows our estimation results. In column 1, controlling only for firm size and year fixed effects, we find a negative and significant young-firm pay penalty of 13.1 log points. The relatively smaller point estimate compared to our baseline result suggests that the relation between firm size and earnings partly mediates the young-firm pay penalty. After adding further controls for unobserved time-invariant worker heterogeneity (column 2), observable worker attributes (column 3), and time-invariant firm heterogeneity (column 4), we confirm that young firms pay a small but significant positive pay premium of around 1.7 log points.

Extension to Firm Age-Pay Profile. While the classification of firms into "young" and "old" is a useful abstraction, it may seem natural to consider a specification that allows for a continuous firm age-pay profile (Haltiwanger et al., 2013). We pursue this by allocating all employers into two-year bins based on firm age between birth and up to 18–19 years old, the omitted category is firms 20 years or older. As the omitted category formerly consisted of firms at least four years old, this leads to a change in interpretation of all coefficients in the current specification, which are now relative to the reference group of mature firms at least 20 years old. Following an identification strategy similar to Schmieder (2013) and discussed in greater detail in Section 3.2, we allow for firms' pay policies to vary flexibly across two-year bins for the first 20 years of firm age, assuming that the firm pay policies remain constant thereafter.

Three key insights emerge from the results presented in Table 5. First, controlling for only year fixed effects (column 1), workers at young firms are paid significantly less than those at older firms, and earnings are monotonically increasing with firm age. For example, workers at firms 0–1 years old earn 19.8 log points less than those at firms 10–11 years old, and 38.2 log points less than those at firms at least 20 years old.

Second, adding controls for permanent worker heterogeneity (column 2) and time-varying observable worker heterogeneity (column 3) explains around two-thirds of the raw pay gap at young firms. For example, the pay penalty relative to firms at least 20 years old decreases from 38.2 log points to 12.6 log points at firm age 0–1 and from 18.4 log points to 7.9 log points at firm age 10–11 after accounting for worker heterogeneity (comparing columns 1 and 3).

Third, with a full set of controls including those for permanent firm heterogeneity, we find a statistically significant young-firm pay premium, which is essentially monotonically decreasing with firm age. For example, relative pay compared to firms with at least 20 years of age is 6.4 log points higher at firms 0–1 years old and 2.7 log points at firms 10–11 years old.

Summary. It is well-known that workers at young firms have significantly lower earnings. However, we find that the bulk of the young-firm pay penalty is explained by young firms' disproportionately hiring low-pay workers based on unobservable and observable characteristics. Moreover, young firms are more likely to be permanently low-paying relative to the pool of old firms. Together, these two dimensions of selection explain essentially all of the young-firm pay penalty in the raw data, turning it into a positive and significant young-firm pay premium.

4.2 Initial Worker Composition Predicts Young-Firm Dynamics

In the previous section, we documented that young firms disproportionately hire low-paid workers and that the bulk of young firms are permanently low-paying. In this section, we explore the relation between firms' initial worker composition and future survival and growth prospects. Our theory in Section 2 suggests that the quality of the set of initial employees affects the dynamics of young firms by imbuing in their organizational capital certain qualities of the initial worker pool. This theory predicts that young firms that start out with initially high-ability workers are more likely to survive and grow to old age.

We begin by providing descriptive evidence that initial worker composition matters for future firm dynamics. We then test whether this suggestive evidence is robust to the inclusion of other firm-level controls that may be related to future firm survival and employment.

Descriptive Evidence. As a first step, we compute for each firm the average AKM worker fixed effect of its workforce, based on our main regression specification (see column 4 of Table 3). We then tabulate the mean worker fixed effect for each two-year bins from birth to age 20 or greater, shown in Panel A of Figure 2. Note that by construction, the average worker fixed effect across the whole sample is zero. Related to our previous finding, we find that firms 0–1 years old employ individuals who have, on average, almost 18 log points lower worker fixed effects than firms 10–11 years old. Average worker fixed effects values increase in firm age up until 16–17 years old, then start to decline again somewhat. This pattern could be consistent with either average worker fixed effects increasing across firm age, or alternatively, the pool of older firms may be skewed toward a subset of firms with relatively higher worker fixed effects.

Next, in Panel B of Figure 2, we show mean AKM worker fixed effects by two-year firm age bins and whether the firm survives till 10 years of age. We find that surviving firms (green solid bars) hire above-average worker fixed effect workers at birth. Among surviving firms, those 0–1 years old have worker fixed effects that are on average 5.8 log points higher than the population average. Firms that exit within nine years of birth (red striped bars) hire individuals with below-average worker fixed effect at birth. Among all dying firms, employers that are 0–1 years old have worker fixed effects that are on average 18.9 log points lower than the population average. The same figure demonstrates that the difference in mean worker fixed effects between surviving and failing firms shrinks toward higher firm ages, possibly driven by the fact that the survival criterion becomes less distinct conditional on higher firm age.

Regression Analysis. Based on the descriptive evidence, we now formally examine whether new firms' initial worker composition predicts future survival and growth outcomes. To this end, we project metrics of future firm dynamics on the mean AKM worker fixed effect of the initial workforce at the time of a firm's birth, while controlling for other relevant firm-level characteristics. Specifically, we use as outcome variables an indicator for a young firm's five-year exit rate and the logarithm of five-year employment.[18] In all regressions, we control for the logarithm of initial firm employment, year of firm birth, state dummies, and industry fixed effects. In an additional set of regressions, we also control for firm-level moments of the distribution of observable worker characteristics, including the logarithm of the mean years of education and the logarithm of the mean age of all employees in the first year of firm birth.

Our baseline estimates in Table 6 (columns 1 and 3) show that initial worker composition, through mean AKM worker fixed effects, is positively and significantly related to future firm survival and employment growth.[19] Moreover, the relationship is also economically significant. Conditional on initial employment and observable worker characteristics (columns 2 and 4), a one standard deviation increase in worker fixed effects is associated with a 9 percent higher survival rate, relative to the mean survival rate of 42 percent, and 10 percent higher employment growth.

Summary. We find that initial worker composition, as measured by the average AKM worker fixed effect, is significantly positively associated with future firm survival and employment. On average, young firms are significantly more likely to hire low-pay workers. Also, as is well-known, most young firms exit within few years of their formation. However, among firms that survive, the initial workforce is skewed toward individuals with high fixed effects in earnings. Young firms that hire more highly paid workers initially are less likely to exit and more likely to increase in employment over the subsequent years.

4.3 Discussion

Through the lens of the theoretical model in Section 2, our empirical findings are informative about the underlying selection of workers across young and old firms, about the selection of firms over their life cycles, and the link between worker quality and firm dynamics. Combining theory

[18]We find similar results when using a logit specification to predict exit probabilities or when measuring exit after four or six years instead of the five-year cutoff. We also find similar results when predicting future employment growth in a restricted sample of surviving firms, so our results on employment growth are not driven by firm exit decisions.

[19]Since we control for the logarithm of initial firm employment, we interpret the coefficients on future employment as capturing employment growth.

and data, we briefly discuss some plausible economic interpretations of our empirical findings in relation to structural determinants of worker and firm pay heterogeneity.

Our first main empirical finding is that young firms pay a positive pay premium relative to old firms after controlling for worker and firm heterogeneity. Thus, we provide definitive evidence from administrative data that extends earlier, tentative conclusions based on survey data that worker characteristics explain part of the lower pay at younger firms in the US (Brown and Medoff, 2003). That the cross-sectional young-firm pay penalty is largely explained by worker fixed effects, and to some extent by observable worker characteristics, suggests that young firms disproportionately hire low-ability workers ("worker selection"). That the young-firm pay penalty turns into a positive pay premium after additionally controlling for firm fixed effects suggests that most young firms have permanently low productivity ("firm selection"). Our model interprets this pay premium as compensation for higher earnings risk at young firms due to higher firm exit risk, consistent with the evidence by Abowd and Ashenfelter (1981), Diamond and Simon (1990), Mayo and Murray (1991), and Moretti (2000). While our simple two-period model features only "young" and "old" firms, we find that pay decreases gradually with firm age, consistent with the gradual negative relation between firm exit risk and firm age in the data, as documented by Haltiwanger et al. (2013).[20]

Alternatively, our first finding could also be consistent with the an upward-sloping labor supply curve, as in Schmieder (2013). This would be expected in an environment in which firm-level labor supply elasticities are finite and a higher firm pay rank leads to greater recruiting intensity, possibly due to monopsony power in the labor market (Burdett and Mortensen, 1998; Engbom and Moser, 2018). This mechanism is absent from our simple theoretical model. Such a story seems plausible for the parts of the economy for which a monopsony model provides a good description of labor markets. Interestingly, as we demonstrate in Section 5.1, the young-firm pay premium is smaller and statistically indistinguishable from zero for college-educated workers and those in high-technology sectors. Our model can rationalize these observations as workers in those skill groups and sectors have higher bargaining power (Cahuc et al., 2006) and face lower firm exit risk (Agarwal and Gort, 1996, 2002).

[20] A back-of-the-envelope calculation suggests that the 1.7 log points pay premium at young firms, with controls for worker and firm heterogeneity including firm size, is of a reasonable magnitude given the extra unemployment risk that working at a young firm entails. First, firms up to four years old, on average, have 5.0 percentage points higher annual exit rates than older firms (Haltiwanger et al., 2013). Second, the mean duration of unemployment between 1990 and 2006 was around 16.0 weeks (Bureau of Labor Statistics, 2019). Consequently, the expected foregone income that we would expect young firms to compensate their workers for is around 1.5 percent $(= 0.05 \times 16/52)$ per year.

Our second main empirical finding is that young firms that initially hire more highly paid workers are more likely to survive and grow in the future. While this empirical relationship need not be causal, it is interesting to think about the fundamental economic forces giving rise to such positive assortative matching. An often cited reason for the negative relation between firm age and firm exit probability (Evans, 1987) is the low productivity of young exiting firms (Foster et al., 2001, 2006). Our theory suggests a related but distinct explanation: the quality of young firms' initial workforces. A notable feature of our model is that the average ability of the initial workforce codetermines a young firm's survival and growth prospects by helping it to absorb cost shocks and by feeding into future firm productivity. While worker quality is sought after by everyone, in a frictional labor market only some firms get lucky and hire a high-ability workforce. Therefore, our findings contribute to a recent strand of literature arguing that business performance depends on the quality of the CEO (Bertrand and Schoar, 2003) and the entrepreneur (Parker, 2009).

An alternative interpretation of our second finding outside of our model would be that some firms face inherently higher survival and growth prospects and as a result hire high-ability workers, possibly due to production complementarities between worker and firm types. While our results leave room for alternative stories of this kind, one would expect such a theory to imply strong match components in labor demand and consequently pay, for which we find little evidence (see Section 5.2). Instead, our findings contribute to a growing list of evidence that a log-linear earnings specification, such as the one predicted by our model, provides a good fit to labor market data from the US and other countries (AKM; Abowd et al., 1999b; Abowd et al., 2002; Card et al., 2013; Card et al., 2016; Alvarez et al., 2018; Bonhomme et al., 2019). Our model, which delivers an earnings equation consistent with our empirical specification, provides a parsimonious explanation for positive assortative matching and a rich set of other empirical patterns we document. It is worth noting that in spite of the log-linearity of the wage equation, positive assortative matching between high-ability workers and high-productivity firms emerges from our structural model because of the notable feature that initial workforce quality feeds into firm productivity.

Our findings also speak to other, alternative explanations of the firm-age pay differential and the link between initial workforce composition and business dynamics. Having demonstrated that young firms pay a positive premium makes it no longer necessary to appeal to theories of higher nonpay amenities at startups (Hamilton, 2000), differences in individual risk preferences (Roach and Sauermann, 2015), backloaded tenure-pay profiles (Michelacci and Quadrini, 2009), or household borrowing constraints (Dinlersoz et al., 2019) to rationalize observed employment patterns. After including relevant controls for worker and firm heterogeneity, no additional economic justification is required to explain positive employment at young firms. If anything, our findings suggest there is a positive pay premium at young firms, which our model interprets as compensation for the expected cost of unemployment (Jacobson et al., 1993; Jarosch, 2015).

The results of our investigation lend no support to alternative interpretations of high AKM worker fixed effects as representing worker who are "more expensive" or "overpaid," possibly because of their higher bargaining power. That young firms with higher average worker fixed effects among their workforce are more likely to survive and grow is consistent with worker fixed effects reflecting a sought-after worker quality such as ability, not just pure rent extraction.

Finally, our analysis points toward an interesting link between firm and worker characteristics, which is closely related to the distinction between the quality of a business ("horse") and its founder ("jockey") made by Kaplan et al. (2009). In contemporaneous work, Choi et al. (2019) empirically test their hypothesize that organizational capital is embodied in an enterprise's founding members, by considering the premature deaths of those members. That firm quality is embedded in human capital in their model is an interesting mechanism linking the fate of a firm and its workforce. Conversely, in our model, initial workforce quality is embedded in firm productivity. Both views are consistent with findings by Becker and Hvide (2017) and Choi et al. (2019) of a negative effect of founding team member death on future business performance. Likewise in our model, where recruiting is slow and costly, losing some of its initial workers would make a young firm more prone to exit.

5 Robustness

In the previous section, we presented evidence that worker selection largely explains the young-firm pay penalty and that firm selection explains it to a lesser extent. After controlling for differences in permanent worker heterogeneity and permanent firm heterogeneity, we found that young firms actually pay a positive premium relative to old firms. In this section, we provide several robustness checks to corroborate this finding.

5.1 Alternative Samples

To probe the generality of our finding, we reestimate our main specification in equation (2) while limiting the sample to subsets of high-skill workers. By focusing on certain proxies for ability, we implicitly test for the strength of selection on unobservables within more narrow skill groups.

College-Educated Workers. We start by looking at the subsample of college-educated workers, defined as employees with sixteen or more years of education. The results in Table 7 show a similar conclusion to that of the baseline sample of all workers. As for the overall sample, college-educated workers earn significantly less at young firms compared to older employers, around 27.5 log points in magnitude (column 1). Controlling for permanent individual heterogeneity through the addition of worker fixed effects (column 2) and time-varying worker characteristics (column 3), the young-firm pay penalty is reduced by almost two-thirds, with a resulting estimate of around 9 log points. Moreover, controlling for permanent employer heterogeneity by adding firm fixed effects (column 4) delivers a precisely estimated zero pay difference between young and old firms in this subsample. That pay at young firms is not significantly different from that at old firms for college graduates is interesting and consistent with our model prediction, given that college graduates have been shown to hold greater bargaining power (Cahuc et al., 2006).

College-Educated Workers in High-Technology Sectors. Next, we further restrict our subsample to college educated workers in high-technology sectors. We define high-technology sectors to include Standard Industrial Classification (SIC) codes corresponding to computers, biotechnology, electronics, and telecommunications. We assign each worker to an industry according to their first employer's SIC code in the data. As in our previous findings, the results in Table 8 show a significant young-firm pay penalty of around 20.6 log points that goes to zero after adding controls for worker and firm heterogeneity. That workers in these sectors are not paid a young-firm pay premium is also consistent with our model prediction, given that technology-heavy businesses have been shown to have higher survival rates (Agarwal and Gort, 1996, 2002).

5.2 Support for AKM Assumptions

In this section, we present a battery of robustness checks related to the augmented AKM specification we employed in our main analysis. Following Card et al. (2013), we first provide empirical support for the log-additive separability in the AKM earnings equation and evidence against endogenous mobility based on drift or transitory components of the error term. We then provide further evidence that our results are robust to different sources of mobility, including more plausibly exogenous variation in workers' propensity to move.

A key concern in estimating an augmented AKM specification like equation (2) is that worker mobility between (young and old) firms may be endogenous to the error term in earnings. To investigate this, we write the error term, ε_{it}, as the sum of three separate mean-zero components:

$$\varepsilon_{it} = \lambda_{iJ(i,t)} + \mu_{it} + \nu_{it},$$

where $\lambda_{iJ(i,t)}$ is a worker-firm match-specific component, μ_{it} is a persistent unit-root component, and ν_{it} is a transitory error component.

Match Effects. We first address the match component, $\lambda_{iJ(i,t)}$, which could derive from the classical Roy (1951) framework or a model of entrepreneurial selection as in Hvide and Oyer (2018). In our setting, this could be problematic if workers joined a young firm based on a great personal fit between the worker's individual characteristics and those of the firm, beyond what is captured by the worker- and firm-specific components α_i and $\psi_{J(i,t)}$. If the majority of worker transitions were guided by such match-specific considerations, then we would expect little systematic correlation in gains for any two workers moving between the same firm pair. Conversely, absent match-specific considerations, the relative earnings gains from moving between two firms would be of the same magnitude and of the opposite sign as those from making the opposite move for all workers.

We test for this symmetry by constructing an event-study analysis that examines the earnings evolution around the time of switching employers. Specifically, we categorize employers into quartiles based on mean coworker earnings. We then assign all workers to one of 16 groups based on the quartiles of mean coworker earnings at the origin and destination firms around a move between firms. For each of these 16 mover categories, we calculate mean earnings in the two years before and after the job change.

Figure 3 plots a selection of the transitions from this event study. Panel XXX figure shows the mean earnings profiles for workers leaving quartile 1 and quartile 4 employers for an employer in the same or an adjacent category. The figure shows clear evidence that moving to a job with higher-paid coworkers raises pay and vice versa. The bottom of the figure presents a similar event study for workers switching between quartiles 1 and 4, with remarkable symmetry between the two groups. The gains and losses for other mover categories exhibit a similar degree of symmetry.[21]

[21] Both subfigures show means of unadjusted earnings. In untabulated results, we find similar patterns when adjusting earnings with age polynomials interacted with educational attainment.

This symmetry suggests that a simple model with additive worker and firm effects but without match effects provides a reasonable approximation of the average worker experience across firms in the US labor market.

Drift and Transitory Shocks. We next address the possibility of endogenous worker mobility based on the drift μ_{it} or transitory component ν_{it} of earnings . If a drift component of the error were correlated with firm fixed effects, then job transitions would follow a systematic pattern of either increasing or decreasing earnings at the prior employer. This would be the case, for example, in learning models with comparative advantage (Gibbons, 2005) and models in which wages are set under Bertrand competition (Postel-Vinay and Robin, 2002; Dey and Flinn, 2005). Under either scenario, workers' propensity to move would generally depend on their current drift component, which would lead to biased estimates of the AKM earnings components. We find little evidence of such patterns in the data. The event-study graphs in Figure 3 show not much of a differential trend by switcher category around the time of transition. Similarly, we find little evidence of a dip or a bump in earnings right around the time workers move between employer groups.

Sources of Mobility. Related to the endogenous mobility bias addressed above, a general concern is that workers move as a function of the error term in earnings, ε_{it}. Such moves would appear relatively more likely after workers and firms have had ample opportunity to learn about potential future match effects between them as well as between themselves and other potential match partners. We proxy for the ability to learn about match effects with the reason a worker left their previous employment. Specifically, we distinguish between movers whose previous employer exited and consequently had to lay off all its workers, those whose previous employer did not exit, and those who joined a firm from no previous employment recorded in the data. Table 9 reports the resulting young-firm pay premium estimates for each subsample, first without firm fixed effects (column 1) and then in our preferred specification with firm fixed effects (column 2). Consistent with our identifying assumption of exogenous mobility, we find little evidence of a differential young-firm pay premium across categories of movers.

5.3 Mean or Trend Pay Premium?

Given our finding of a positive young-firm pay premium after controlling for worker and firm heterogeneity, one may wonder if this is the result of a differently sloped wage-tenure profile at young firms. For example, some theories predict that young and financially constrained firms may provide relatively backloaded compensation in order to relax credit constraints (Michelacci and Quadrini, 2009; Guiso et al., 2013). We address tenure effects in two ways.

First, we follow Schmieder (2013) in restricting our sample to only the first observation for each worker-firm match. By dropping subsequent earnings observations, we effectively estimate the young-firm starting-pay premium. This restriction reduces our sample size from around 48.4 million to 13.7 million observations. Table 9 shows results from estimating our main specification in equation (2) with all controls other than firm fixed effects (column 5) and full controls including firm fixed effects (column 6). Broadly consistent with our previous result, we find a slightly lower young-firm pay penalty of 5.9 log points (compared to 7.8 log points previously) without firm controls and a slightly higher young-firm pay premium of 2.6 log points (compared to 0.7 log points previously) with firm controls.

Second, we estimate an extended specification with worker tenure dummies interacted with a young-firm indicator, where young-firm status is defined as previously to include employers in business up to three years old. For old firms, we include dummies for 0, 1, 2, 3, and 4 or more years of tenure. For young firms, we include dummies for 0, 1, 2, and 3 years of tenure. It is important to note that tenure will be mechanically lower and capped at 3 years for workers at young firms. As a result, this specification may confound worker-tenure heterogeneity across young and old firms with young firm–specific firm age–pay profiles. For example, a worker transitioning from 0 to 1 year of tenure at a young firm will experience wage growth that may come from either a change in the firm age–specific pay component or from the worker tenure–specific component specific to young firms. It should be noted, however, that this is less of an issue in our specification that includes a young-firm dummy, which is constant for the first three years of a firm's life. With this in mind, we estimate the same specification as in equation (2).

The estimated tenure profiles at young and old firms are reported in Table 10. Column 1 shows tenure profiles at old and young firms with only year fixed effect controls. We find positive tenure-earnings relations at both old and young firms, although the profile is steeper at young firms. Subsequent columns repeat estimates with additional controls for permanent worker heterogeneity (column 2), time-varying worker controls (column 3), and our preferred specification with permanent firm heterogeneity (column 4). In the latter, we find a significant tenure-earnings profile for workers at old and young firms. At old firms, pay increases monotonically up to 8.7 log points after three years of tenure, relative to zero tenure years. At young firms, zero-tenure workers start out earning 3.4 log points more than their counterparts at old firms but the relative pay premium of young firms shrinks monotonically to 1.4 log points for workers with three years of tenure. Consistent with our previous finding, these results suggest that young firms pay a small premium at every tenure level, although they offer a less steep tenure-earnings profile compared to old firms. Comparing columns 1 and 4, we conclude that the flatter tenure-earnings profile at young firms is driven by worker and firm selection over time.

6 Conclusion

In this paper, we take a theory-guided empirical approach to studying the sources of the young-firm pay penalty in the data. Combining two confidential databases from the US Census Bureau, we confirm earlier survey data evidence (Brown and Medoff, 2003) of lower earnings at young firms by 30.7 log points. We then exploit the two-sided panel dimension of the data to separately identify unobserved worker and firm pay components. Controlling for time-invariant and time-varying worker and firm heterogeneity, we find a significant positive young-firm pay premium of 0.7 log points (1.7 log points with additional firm size controls) at young firms. Our results highlight the role of simultaneous worker and firm selection. We also show that worker selection at firm birth is significantly related to future business dynamics, including firm survival rates and employment growth. Finally, we tie our empirical findings to a simple model of pay, employment, and dynamics of young firms.

Our findings definitively overturn conventional wisdom, based on previous evidence using survey and employer-level data, that older employers pay more. Our findings also suggest that, after including relevant controls for worker and firm heterogeneity, no additional economic justification is required to explain positive employment at young firms. Instead, we draw three conclusions. First, selection of heterogeneous workers across heterogeneous employers is of paramount importance to understanding the pay structure across young and old firms. Second, selection of highly paid workers into young firms is a strong predictor of future business success, although the direction of causality remains an open question. Third, more extensive data infrastructure along the lines of Davis et al. (2007) and Goetz et al. (2015) is needed and new theories of joint worker and firm dynamics with two-sided heterogeneity should be developed in order to further our understanding of important labor market outcomes.

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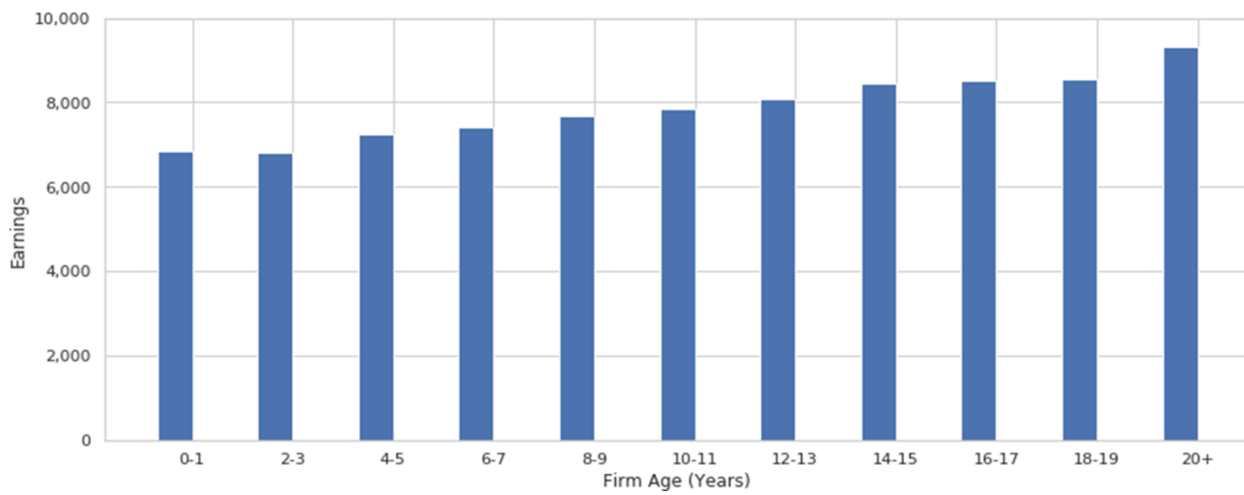
Figure 1. Mean Earnings and Average Earnings Growth by Firm Age

Figure shows mean worker earnings in Panel A and mean worker earnings growth in 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, earnings are quarterly and normalized to real 2014 dollars. In Panel B, earnings growth is the log differences between the current and the previous year's quarterly earnings.

Panel A. Average Earnings



Panel B. Average Earnings Growth

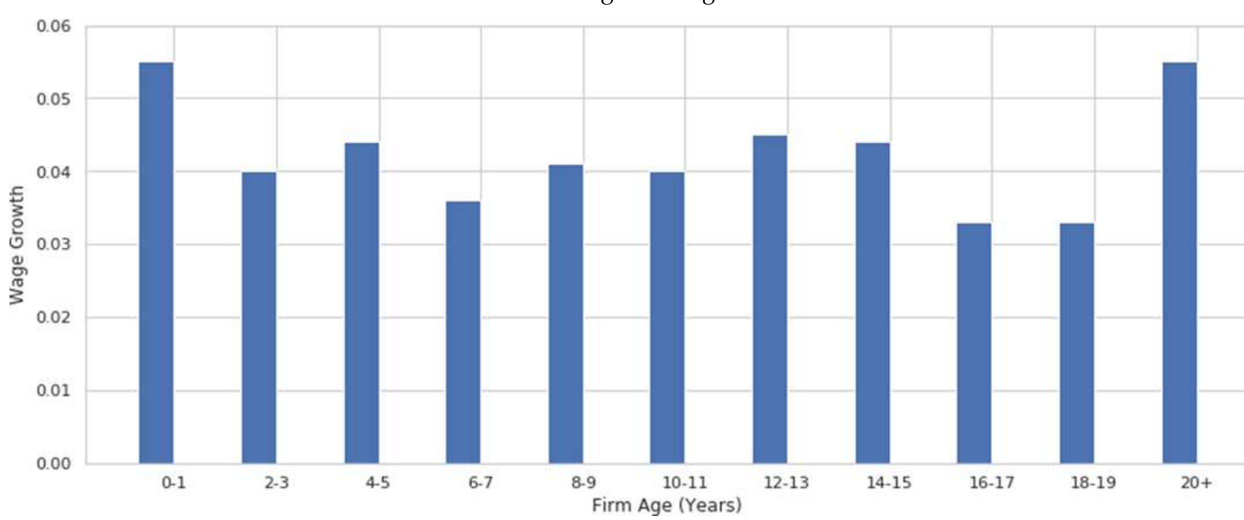
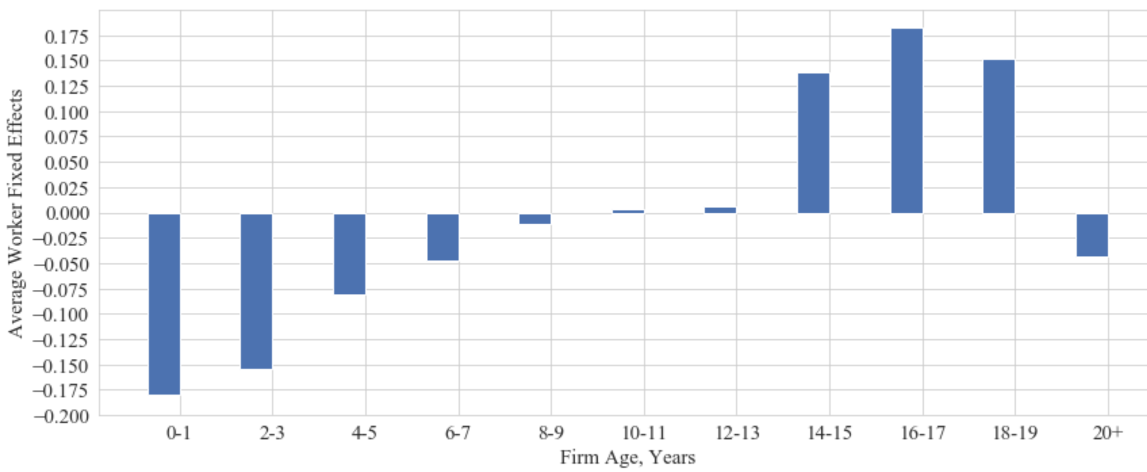


Figure 2. Mean Worker Fixed Effects by Firm Age and Survival Status

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

Panel A. All Firms



Panel B. Surviving and Dying Firms

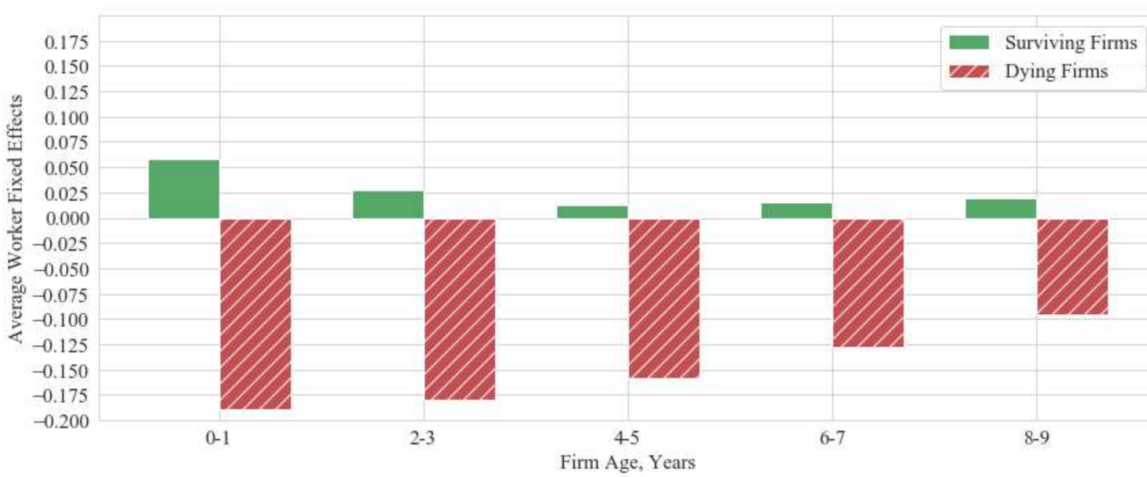
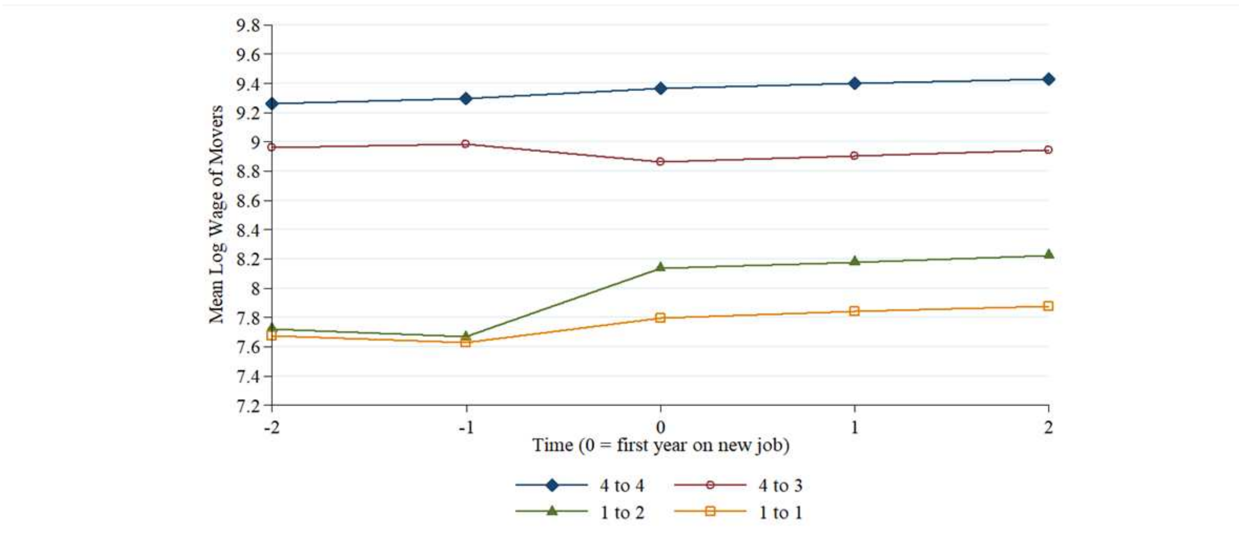


Figure 3. Mean Earnings of Job Changers Classified by Quartile of Mean Earnings of Coworkers at Origin and Destination Firm

Figure shows mean earnings of workers from the baseline sample who change employers in 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 earnings of coworkers. Earnings are log normalized to real 2014 dollars.

Panel A. Switchers from top and bottom quartiles



Panel B. Switchers between top and bottom quartiles

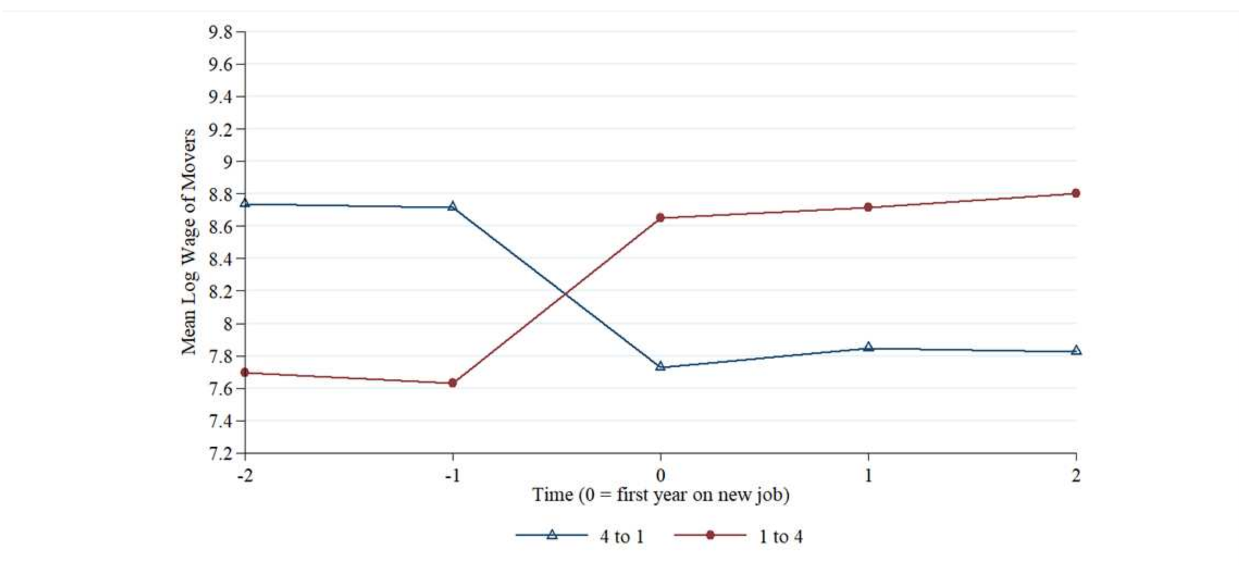


Table 1. Summary Statistics for Young and Old Firms

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 old firms (young firms). Old firm is a firm four or more years old; young firm is a firm three or fewer years old. In Panel A, workforce statistics are calculated at a unique firm-year level in the 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) Old Firms	(3) Young 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	0.5

Panel B. Worker-year level variables

	(1) All Firms	(2) Old Firms	(3) Young Firms
Quarterly Earnings (2014$)	8,536	8,673	6,818
	(7,602)	(7,643)	(6,839)
Earnings 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 baseline sample is a worker-year panel from 1990 through 2006. Statistics are means, followed by standard deviations in parentheses.

	(1) Stayers	(2) Movers
Quarterly Earnings (2014$)	10,020	8,002
	(8,905)	(6,999)
Earnings 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. Young-Firm Pay Premium Estimates for All Workers

Table reports baseline results of earnings at young 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 earnings. Earnings are in real 2014 dollars. Young firm is defined as a firm three or fewer years old. 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 normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Young Firm	-0.307***	-0.087***	-0.078***	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	0.771	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. Young-Firm Pay Premium Estimates After Controlling for Firm Size

Table reports baseline results of earnings at young firms after controlling for firm size. The baseline sample is a worker-year panel from 1990 through 2006. The baseline sample of workers which consists of a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly earnings. Earnings are in real 2014 dollars. Young firm is defined as a firm three or fewer years old. Firm employment is log transformed. 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 normalized by 40 and log transformed. Education is measured as years of schooling and is log transformed. Note, worker age and education are not included as linear controls in regressions with worker fixed effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Young Firm	-0.131***	-0.014***	-0.008***	0.017***
	(0.007)	(0.002)	(0.002)	(0.002)
Ln(Firm Employment)	-0.010	0.104***	0.118***	0.087***
	(0.032)	(0.006)	(0.005)	(0.008)
Ln(Firm Employment)^2	0.018***	-0.006***	-0.009***	-0.005**
	(0.006)	(0.001)	(0.001)	(0.002)
Ln(Firm Employment)^3	-0.001***	-0.000	0.0002***	0.000
	(0.000)	(0.000)	(0.000)	(0.0001)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.040	0.750	0.775	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. Firm-Age Group Pay Estimates for All Workers

Table reports baseline results of earnings at firms of different ages. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly earnings. Earnings 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 normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Firm Age 0–1	-0.382***	-0.127***	-0.126***	0.064***
	(0.025)	(0.005)	(0.004)	(0.014)
Firm Age 2–3	-0.387***	-0.138***	-0.140***	0.042***
	(0.025)	(0.005)	(0.004)	(0.014)
Firm Age 4–5	-0.303***	-0.105***	-0.118***	0.039***
	(0.032)	(0.009)	(0.006)	(0.014)
Firm Age 6–7	-0.258***	-0.089***	-0.105***	0.032***
	(0.030)	(0.006)	(0.005)	(0.012)
Firm Age 8–9	-0.208***	-0.072***	-0.09***	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***	-0.049***	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	0.771	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 6. Young-Firm Dynamics as a Function of Initial Worker Composition

Table shows cross-sectional OLS results from predicting young firm exit (columns 1–2) and future employment (columns 3–4) as a function of worker fixed effects estimated from the earnings regression in Table 3, column 4. The sample is a cross-section of young firms from the baseline sample. The baseline sample is a worker-year panel from 1990 through 2006. In columns 1–2, the dependent variable, Young Firm Exits in 5 Years, equals one for young firms that exit by year five since founding. In columns 3–4, the dependent variable, Young Firm 5-year Employment, is the log of a young firm's employment at age five. Mean Worker Fixed Effects is the mean of worker fixed effects of workers at the young firm in its first year of existence. State FE and Industry FE refer to the industry of the young firm. Industry fixed effects are at the SIC-3 digit level. Standard errors are clustered at the firm level, and reported in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Young Firm Exits in 5 Years		Ln(Young Firm Employment in 5 Years)	
	(1)	(2)	(3)	(4)
Mean Worker Fixed Effects	-0.063***	-0.054***	0.120***	0.145***
	(0.004)	(0.005)	(0.009)	(0.011)
Observations (thousands)	205	205	205	205
R-squared	0.036	0.036	0.098	0.099
Log Young 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

Table 7. Young-Firm Pay Premium Estimates for College-Educated Workers

Table shows results from regressions of worker earnings on the young 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 earnings. Earnings are in real 2014 dollars. Young firm is defined as a firm three or fewer years old. 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 normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Young Firm	-0.275***	-0.092***	-0.089***	0.001
	(0.016)	(0.002)	(0.002)	(0.003)
Observations (millions)	18.3	18.3	18.3	18.3
R-squared	0.008	0.751	0.762	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 8. Young-Firm Pay Premium Estimates for College-Educated Workers in High-Technology Sectors

Table shows results from regressions of worker earnings on the young firm indicator variable for college-educated workers in high-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 earnings. Earnings are in real 2014 dollars. Young firm is defined as a firm three or fewer years old. 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 normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Young Firm	-0.206***	-0.095***	-0.091***	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	0.738	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 9. Robustness: Sources of Mobility and First Quarter Earnings at New Job Sample

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 earnings. Earnings are in real 2014 dollars. Young firm is defined as a firm three or fewer years old. Columns 1–2 show results for workers who have moved after plant closure. Move to Young Firm from Closed Plant (Open Plant) shows results for workers who move to young firms from closed plants (open plants). Plant is defined as closed when the employment is zero either in the year of the move to a young employer or the year prior to the move. Columns 3–4, First Earnings Sample, refers to the subset of observations from the baseline sample restricted to the first observed full-quarter earnings at a new employer. 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 normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	Moves From Closed vs. Open Establishment		First Earnings Sample	
	(1)	(2)	(3)	(4)
Move to Young Firm from Closed Establishment	-0.056***	0.027***		
	(0.002)	(0.003)		
Move to Young Firm from Open Establishment	-0.077***	0.019***		
	(0.003)	(0.002)		
Worker Starts Employment in Young Firm	-0.095***	-0.015***		
	(0.003)	(0.002)		
Young Firm and Low Unemployment				
Young Firm and High Unemployment				
Young Firm			-0.059***	0.026***
			(0.0032)	(0.0024)
Observations (millions)	48.4	48.4	13.7	13.7
R-squared	0.771	0.810	0.710	0.770
Time-Varying Worker Controls	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 10. Robustness: Young-Firm Tenure–Pay Profile Estimates

Table reports results of earnings 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 earnings. Earnings 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. Young firm is defined as a firm three or fewer years old. Worker controls include worker age squared and age cubed, and their interactions with worker education. Worker age is normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
1 Year of Tenure	0.165***	0.041***	0.011***	0.008***
	(0.005)	(0.002)	(0.002)	(0.002)
2 Years of Tenure	0.370***	0.109***	0.064***	0.062***
	(0.007)	(0.003)	(0.002)	(0.002)
3 Years of Tenure	0.477***	0.128***	0.083***	0.087***
	(0.009)	(0.003)	(0.003)	(0.003)
4+ Years of Tenure	0.686***	0.117***	0.119***	0.133***
	(0.014)	(0.004)	(0.004)	(0.003)
0 Years of Tenure * Young Firm	-0.086***	-0.050***	-0.047***	0.034***
	(0.014)	(0.003)	(0.003)	(0.002)
1 Year of Tenure * Young Firm	-0.087***	-0.058***	-0.049***	0.027***
	(0.016)	(0.003)	(0.003)	(0.002)
2 Years of Tenure * Young Firm	-0.071***	-0.059***	-0.044***	0.026***
	(0.016)	(0.003)	(0.002)	(0.0024)
3 Years of Tenure * Young Firm	-0.030	-0.064***	-0.040***	0.014***
	(0.019)	(0.004)	(0.003)	(0.003)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.079	0.749	0.772	0.811
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

A Model Appendix

Details on Pay Determination. The bargagining problem between a worker with ability θ and a firm with productivity p and age a is

$$w_t(\theta, p, a) = \arg\max_w C_t(w;\theta,p,a)^\beta J_t(w;\theta,p,a)^{1-\beta}.$$

Then the surplus function for young firms can be written

$$J_t(w;\theta,p,young) = (1 - x_t(p, \{l_\theta\}_\theta))(p\theta - w)\, l_\theta,$$

while for old firms the surplus function is

$$J_t(w;\theta,p,old) = (p\theta - w)\, l_\theta.$$

Note that in formulating the surplus function for old firms, we exploited the linearity of the production function, which allows us to solve the bargaining problem separately by worker type.

The surplus function for workers joining at a young firm is then

$$C_t(w;\theta,p,young) = (1 - x_t(p, \{l_\theta\}_\theta))\, w - b\theta,$$

while that for workers remaining at or joining old firms is

$$C_t(w;\theta,p,old) = w - b\theta.$$

Bargaining Solution. First, note that we can rewrite the bargaining program in logarithms as

$$w_t(\theta,p,a) = \arg\max_w \beta \ln[C_t(w;\theta,p,a)] + (1-\beta)\ln[J_t(w;\theta,p,a)].$$

For young firms, the bargaining problem becomes

$$w_t(\theta,p,young) = \arg\max_w \left\{ \begin{array}{c} \beta \ln[(1 - x_t(p,\{l_\theta\}_\theta))\, w - b\theta] + (1-\beta)\ln[p\theta - w] \\ + (1-\beta)\ln[(1 - x_t(p,\{l_\theta\}_\theta))\, l_\theta] \end{array} \right\},$$

with associated FOC

$$\frac{\beta\left(1-x_t\left(p,\{l_\theta\}_\theta\right)\right)}{\left(1-x_t\left(p,\{l_\theta\}_\theta\right)\right)w-b\theta}-\frac{1-\beta}{p\theta-w}=0.$$

Solving this, we find the bargained earnings at young firms to be

$$w_t\left(\theta,p,young\right)=\beta p\theta+\frac{\left(1-\beta\right)b\theta}{1-x_t\left(p,\{l_\theta\}_\theta\right)}. \tag{4}$$

For old firms, the bargaining problem becomes

$$w_t\left(\theta,p,old\right)=\arg\max_w \beta\ln\left[w-b\theta\right]+\left(1-\beta\right)\ln\left[p\theta-w\right]+\left(1-\beta\right)\ln\left[l_\theta\right],$$

with associated FOC

$$\frac{\beta}{w-b\theta}-\frac{1-\beta}{p\theta-w}=0.$$

Solving this, we find the bargained earnings at young firms to be

$$w_t\left(\theta,p,old\right)=\beta p\theta+\left(1-\beta\right)b\theta. \tag{5}$$

Young-Firm Pay Premium. Comparing bargained earnings for young firms in equation (4) and old firms in equation (5), we see that $w_t\left(\theta,p,young\right)>w_t\left(\theta,p,old\right)$ whenever $\beta<1$ and $x\left(p,\{l_\theta\}_\theta\right)>0$ for all p. In other words, young firms pay a premium over otherwise identical old firms.

Relation to AKM Earnings Equation. Finally, taking logs of equations (4) and (5), we see that earnings in the model are log-separable between a worker component, a firm component, and a component specific to employer age:

$$\begin{aligned}
\ln\left(w_t\left(\theta,p,young\right)\right) &= \underbrace{\ln\left(\theta\right)}_{\text{"worker fixed effect"}}+\underbrace{\ln\left(\beta p+\left(1-\beta\right)b\right)}_{\text{"firm fixed effect"}}+\underbrace{\ln\left(\frac{\beta p+\frac{(1-\beta)b}{1-x_t(p,\{l_\theta\}_\theta)}}{\beta p+\left(1-\beta\right)b}\right)}_{\text{"young-firm pay premium"}}\\
\ln\left(w_t\left(\theta,p,old\right)\right) &= \underbrace{\ln\left(\theta\right)}_{\text{"worker fixed effect"}}+\underbrace{\ln\left(\beta p+\left(1-\beta\right)b\right)}_{\text{"firm fixed effect"}}
\end{aligned}$$

Firm Exit Threshold. At the point when the fixed cost shock is realized, the firm weighs the value of continuing operations in the current and next period against the value of exiting, which is normalized to zero. Clearly, for large enough cost shocks it will always be optimal to exit.

Conversely, for low enough fixed costs, firms must optimally decide to stay, or else their entry and hiring decision cannot have been optimal. It is straightforward to see that the optimal exit policy must follow a threshold rule: there exists a cost cutoff function $\bar{c}(p_1, \{l_\theta\}_\theta)$ that depends on firm productivity and initial worker composition such that the firm decides to stay for fixed cost realizations below the threshold, $c \leq \bar{c}(p_1, \{l_\theta\}_\theta)$, while the firm decides to exit for fixed cost realizations above the threshold, $c > \bar{c}(p_1, \{l_\theta\}_\theta)$.

Relation Between Firm Productivity and Firm Survival. Since current and future profits are strictly increasing in firm productivity p, the threshold function inherits the same property. Therefore, more-productive firms have a higher exit threshold compared to less-productive firms:

$$\frac{\partial \bar{c}(p_1, \{l_\theta\}_\theta)}{\partial p} > 0.$$

Consequently, for a fixed distribution of fixed cost shocks across firm productivity levels, the probability of firm exit is decreasing in firm probability:

$$\frac{\partial x(p_1, \{l_\theta\}_\theta)}{\partial p} \begin{cases} < 0 & \text{whenever } \frac{dO(\bar{c}(p_1, \{l_\theta\}_\theta))}{dc} > 0. \\ = 0 & \text{otherwise} \end{cases}$$

Note that the inequality is strict whenever there is a positive probability of a cost shock at the point that defines the exit threshold for the current productivity.

Impact of Initial Worker Composition on Firm Survival and Growth. Recall that the average ability of a firm's initial workforce, $\bar{\theta}$, affects its future productivity $p_2(p_1,\bar{\theta})$,which is strictly increasing in both its arguments. Given that higher $\bar{\theta}$ implies higher p_2 and the value of not exiting is clearly increasing in p_2, then a higher $\bar{\theta}$ also implies a lower firm exit rate. Furthermore, conditional on firm survival, a higher value of p_2 together with the convex increasing nature of $\phi(v)$ implies that the optimal vacancy posting amount in period 2 will be higher as well. As a result of the linearity of new hires in the number of individual vacancies, higher p_2 then implies higher firm size in period 2. Conditional on current firm size in period 1, therefore, higher $\bar{\theta}$ implies higher firm growth in the future.

B Empirical Appendix

Figure 4. Map of US States Available in the LEHD

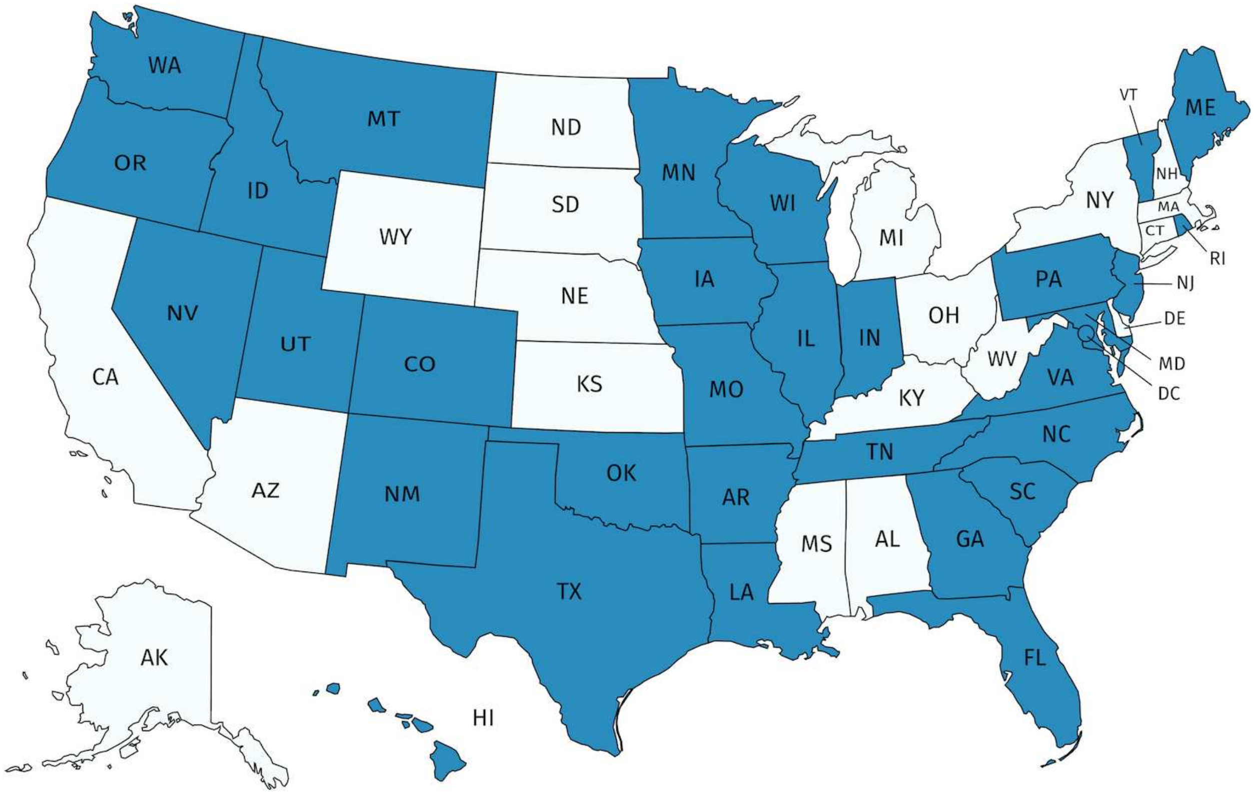


Table 11. Robustness: Defining Young Firms as Age 0–1

Table reports baseline results of earnings at young firms, where young firm is defined as younger than two years. The sample is a worker-year panel from 1990 through 2006. In all columns, the dependent variable is the log of worker total quarterly earnings. Earnings are in real 2014 dollars. 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 normalized by 40 and 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 effects 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 at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Young Firm (Age 0-1)	-0.290***	-0.065***	-0.056***	0.021***
	(0.017)	(0.003)	(0.002)	(0.002)
Observations (millions)	48.4	48.4	48.4	48.4
R-squared	0.005	0.748	0.770	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